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Nonlinear Dynamics From a Physical Point of View

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This commentary sketches some history of the use of nonlinear dynamics in homeokinetics and contrasts this physical approach, grounded in statistical mechanics and hydrodynamics, with more prominent mathematically driven approaches, especially as described by Abraham (1987), and some other groups.

A few years ago in *Physics Today* (Iberall, 1994), we had occasion to comment on versions of the history of complex systems research being written today. In that note, I took issue with the history described by P. W. Anderson (1992). Because of the current prominence of the Santa Fe Institute, Anderson's account by now represents one "received view." Another version of the "received view" may be found in Abraham (1987). Many readers of this journal are familiar with developments in nonlinear dynamics as an approach to complexity theory. Most have used Abraham and Shaw (1983) as their first introduction to dynamics. Some readers are familiar with the efforts of Haken, in his synergetics. Some may have started from linear control theory and passed on to modern control theory. Some may be familiar with bond graph theory, which is associated with the name of H. Paynter. Some may know the work of Katchalsky and the Russian school of Rashevsky. We can name perhaps a half dozen more efforts, of a mathematical nature, that attempted to begin some physical engineering version of a relatively complex form of systems science. In physical engineering circles, particularly as pursued in control theory, it wasn't until the Russian school of nonlinear mechanics (e.g., Andronov and Chaiken in the early 1940s) that we got a broad taste of the nonlinear problem and its history, which we needed for fluid mechanics, and which also pushed us to bring the nonlinear theme to the automatic regulation and control community.

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A number of people identified with ecological psychology have actively used the concepts and methods of nonlinear dynamics (e.g., Beek, 1989; Beek, Turvey, & Schmidt, 1992; Kelso, 1996; Kugler & Turvey, 1987; Saltzman & Munhall, 1989). (An illustrative range of references that may be consulted is Kugler & Turvey, 1987; Kugler, Turvey, Schmidt, & Rosenblum, 1990; Proceedings of VIIth International Conference on Event Perception and Action, Vancouver, Canada, 1993; and Kadar, 1996.)

Although the community of ecological psychologists also has been warming up to homeokinetics (Iberall, 1992; 1995), there has not been a clear statement of similarities and differences in the treatments of nonlinear dynamics in homeokinetics compared with the currently fashionable approaches described by most commentators (Anderson and Abraham happen to be two that would serve the memory banks of current audiences).

Homeokinetics treats the problems of complexity and self-organization permeating complex systems in physics, biology, psychology, and social sciences as hydrodynamic processes, bounded below by kinetics and above by continuum-like fields.

The still easily available forms of the literature from which I learned about nonlinear processes were den Hartog (1944), the Russian influx of Andronov and Chaiken (1949), and Cunningham (1958). The Russian work was pulled together somewhat later in Minorsky (1962). More current literature is enormous. All these sources traced their development back to the earlier Poincaré program in nonlinear dynamics. I was immersed in the experimental practice of these theoretical issues from the earliest days of my working career in everyday hydrodynamic problems. By 1945–1950, I was up to the problems of the complete Navier-Stokes equations of hydrodynamics, their irreversible thermodynamics base, problems of metastability, of creep and other rheological processes, of nonlinearity, of critical points, turbulence, two or more phase flow, and instability in both fluid and solid material fields. My colleagues and I investigated the dynamics of biological–biophysical physiological systems in humans (e.g., respiratory problems in high altitude and high speed flight, thermoregulation in space suits, and high G-exposure). Thus, we had ample opportunity for studying those systems' internal dynamics, which continued but extended a comparable range of advanced dynamical problems in the complex living system. Repeatedly discovering periodic processes in all the systems studied led us to the theme and thesis of homeokinetics by 1964, as a complex of dynamic processes whose periodic characteristics themselves served as the basis for the dynamic regulation of the interior, of its so-called homeostasis or homeostatic regulation. Our first round of work can be found in the following summary sources: Iberall and Cardon (1964, 1964/1965, 1965); Iberall and McCulloch (1969); Iberall (1969a; 1969b; 1970; 1972a); Bloch et al. (1971); Iberall et al. (1972); and Iberall, Schindler, and Cardon (1973).

Much of the work done up to this time was both experimental and applied. We had real data on real systems. I stress that here because so much fashionable, mathematically driven modeling seems to ignore existing data and experience. The fact is that we had shown autonomously and volitionally generated thermodynamic fields—periodic processes from the 1940s on. In our 1960 work, we showed an entire spectrum of periodic thermodynamic engine processes within the autonomous human organism as real measures of its power and communication engineering processes that could be called up in the mammal. We had been involved all through the 1940s in the development of breathing apparatus for high altitude flight. I didn't start that research. My much senior colleagues had put an oxygen supply in aircraft as early as 1925. Even they didn't start the study. Experimenters in the 19th century in caissons and in underwater diving gear showed that they could supply an autonomous human with a gas source and that subject could use it to sustain life. (Much later, I returned our more modern developments to the underwater fraternity in the form of the mouth regulator of modern scuba usage. But I note that a hydronaut has returned the favor, having just resuscitated the rebreather for underwater breathing apparatus as long as 16 hr, when we thought we had comfortably helped put the Navy's use of rebreathers to bed for aircraft use in the mid-1940s). But the significance of our continued development, from flow regulators to demand flow regulators, to diluter-demand regulators, to space suit regulators, was that they furnished clear evidence that mechanical adjuncts could really support periodic organism processes of a thermodynamic nature, whether autonomic or volitional. These were not point attractors. They were at least Poincaré limit cycles, processes that had long become accepted in engineering. The spectra that I showed in the 1950s were clearly continued periodic or near-periodic processes, captured within operational basins for indefinite oscillating performance, and in accord with thermodynamics. (Surely the fact that thousands of flyers during WWII were able to maintain their life processes from such nonlinear dynamics strongly suggests that we did not need to wait for physicists and topologists around 1970 to rediscover classical physics for us.)

CONTRASTING WITH ABRAHAM

The nonlinear dynamics themes building up in homeokinetics were pointedly expressed in Iberall et al. (1972). This report, when it came out, did not cite the Ruelle & Takens (1971) article from which the dynamics of strange attractors generally became widely known. Had we known about the article and the impact it was to have, we would happily have addressed it then. As students of the algebraic topologist, René Thom, Ruelle and Takens (1971) helped bring topological dynamics into prominence. Abraham (1987), also long a student of topology, provides the mathematician's view of the history of development of the subject. He regards the modern era (after Poincaré) as beginning with the 1963 work of Lorenz, and Thom's morphogenetic or catastrophe theory. We, on the other hand, think of our modern history as developing in engineering and engineering physics, going back to Maxwell, Rayleigh, Gibbs, Prandtl, G. I. Taylor, von Karman, and Steinmetz.

Abraham (1987) asserts that his account of dynamics is largely in accord with Thom's but he distinguishes that from parallel development by the Russians. He states that dynamical bifurcation refers to the continuous deformation of one dynamical system into another inequivalent one, through structural instability. One should not confuse this theory—created by Poincaré in 1885—with similar subjects of elementary catastrophe theory, or classical bifurcation theory. Abraham presents the basic concept of dynamics: first from the Greeks to Galileo, from Galileo to Newton, from Newton to Poincaré, and from Poincaré to Thom. Mathematical dynamic systems consist of deterministic equations, including ordinary differential equations, partial differential equations of the evolutionary type, or finite difference equations. Because our training was largely in analysis, we are comfortable with that treatment and Abraham really gives us permission to pursue those kinds of arguments, even if Poincaré also provided means for treating dynamical systems using topological and geometric methods with a divergence from the classical methods of analysis. We are required to translate logical language very carefully. Thus we need to regard the state space as a domain in some tensor transforming coordinate space that is the domain of a dynamical system. Each point of the state space, with its many degrees of freedom, in a Gibbsian sense, corresponds to a "virtual state" of a system being modeled. A dynamical system on or in a state space consists of a vector (by us a tensor) assigned to each point. We accept that sort of picture in a Gibbsian sense with the freedom to elect either a gamma (gas) space or a mu (molecular) space (how the degrees of freedom in a single system pursue their space–time dynamics among all those degrees of freedom or how a class of all similar systems with the same number of degrees of freedom and some very similar or related total energy constraint cycles through all the degrees of freedom).

Rather than continuing to outline all of Abraham's presentation in chapters 29 and 30, we ask the reader to compare our brief summary of the Russian topological views (Iberall, 1972a) and Abraham's (1987) chapter 30 summary. I believe we made it clear that our concern was with nonlinear nonconservative physical systems, such as the Navier-Stokes flow field, or a complete set of meteorological equations. By our referencing Hirschfelder, Curtiss, and Bird (1964), including its near 1,000-page ending in which the positive definite character of the atomistic transports is what makes up the irreversible thermodynamics of physical dynamic fields, and which prevents their description only by continuum-like fields without extension into their atomistic components, and that it is Onsager's linear law that permits that Eulerian equation description to extend to a limited number of atomistic relaxation times. This furnishes the precise limit for our flow equations (Iberall, 1950). We completed that program of specification by the use of experimental data in 1963 (Iberall, 1963), and more completely in theory in Yates's (1987) same book. We proceeded to offer a first opening theory for real turbulence in 1969 (Iberall,1971), using the data sets available from flow in pipes and tubes, one of, if not the, classic problem begun by Reynolds that opened laminar and turbulent flow. We pointed out the significance of limit cycles in our study, and we referenced Minorsky's statement about Poincaré's limit cycles, "Limit cycles, and in particular stable limit cycles [if trajectories spiral toward the limit cycle from both inside and outside, the limit cycle is orbitally stable], are fundamental in the theory of oscillations, of nonlinear, nonconservative systems—the only kinds of systems in which they can arise. A stable limit cycle represents a stationary oscillation of a physical system that a stable singular point represents in a stable equilibrium." What was our common problem when we examined real limit cycles in physical systems, particularly when we had gone to inordinately severe measuring conditions to guarantee nearly noise-free isopotential boundaries in these high ordered systems that we explored experimentally? Clearly, our data sets were not stationary. Clearly we had established that at the atomistic scale, the interactive atomistic processes were not at or near thermodynamic equilibrium. However, if we integrated over a characteristic number of atomistic fluctuations, in accord more with Einstein's theory of Brownian motion, we would find that the description of the fluctuations approached near constant steady state parametric values (e.g., as diffusivities). That is what we decided a theory of the transport coefficients would have to approach. And the issue was not the mathematical one of approach in an infinite time. Rather, as Greenspan showed (1954, 1956, 1959), the approach was by a characteristic number of interactions.

Equally to the point, the Langevin derivation of Einstein's Brownian motion was faulty. It assumed the continuity of uniform convergence from a zeroth collision on up, whereas it was clear in Einstein's transparent derivation that one had to integrate over back and forth random fluctuations that could not begin to converge in say less than a few even numbered collisions. Thus, our remarkable result was 10 mean free paths-relaxation times. This is explored at greater length in Iberall and Schindler (1973) and Soodak and Iberall (1987). Now what our dirty limit cycle report showed was that at the atomistic scale in which equipartitioning of energy has to be established among conflicting thermodynamic degrees of freedom, the approach toward a "cyclic" equilibrium like that discussed by Tolman (1938) would take appropriate time periods. Thus, by the use of our cylindrical space definition (with time the longitudinal cylinder axis and its two cross-sectional axes x_i and dx_i/dt), we hoped to show the level at which limit cycle nonstationarity was to be found, and that it did not require an infinite time for the closed cycling to emerge. We did not invent the problem. It can be found also in Jeffreys's (1968) model of boys shooting peas at an oscillating pendulum in which the isochronism isn't greatly affected but the phasing per oscillation is spoiled. This creates two different views of the line width of the process, whether it simply is broadened in stationary fashion or is a time-evolving process scale for that width. Another view of the same problem is contained in various temperature-dependent coefficients. One finds that many are really not smooth but broken and quantized. Thus, the mathematics to the convergence of transport coefficients is not uniformly convergent; rather it involves jumps. Are we to say that smoothness and chaos are not really distinguishable in these high ordered systems? That is what we believe that Greenspan and we both proved in common.

Abraham (1987) sums up by saying, "I now complete my case for the complex dynamical system scheme, by relating it to specific models emerging in self-organization theory. My scheme is an extension of Thom's, and so his morphogenetic field is automatically accommodated. The proposal of Turing (1952), the dissipative structures of Prigogine (1978), and the synergetics of Haken (1977) are all based on partial differential equations of the reaction-diffusion type; they all fit directly into this scheme. I suspect that the homeokinetics of Iberall & Soodak (Chapter 27) and the models of Winfree (1980) also fit . . . ." He continues on, naming other aspects that he encompasses: fluctuation, irreversibility, coherence, order parameters, symmetry breaking bifurcations, the hypercycle, complementarity. He highlights one particular remark, *"But here I see the need for the further development of an extensive statistical and information theory of dynamical systems, along with near-Hamiltonian dynamics, to place thermodynamical laws on a firmer foundation"* (p. 605), before he rests his case.

But we believe Abraham would have done well to reread our chapters 24 and 27 a number of times, as well as our Iberall and Soodak (1978) article. Our work has been devoted precisely to what he calls for, and it goes back to the 1940s, 1950s, and 1960s. It is not based on reaction–diffusion processes, but on a fluctuation–dissipation theory, nonlinearity of convection, and on the Boltzmann program and how one evolves theory from the Euler equations by including the nature of the specific transports engendered by the field atomisms. This is of course intrinsically embodied in Navier-Stokes theory, but then in complete agreement with Abraham, that is the direction we have been taking our study since the 1950s, when—as we stated the thesis to the control engineering community—we said you have a theory for control, but not for regulation, and you have no theory for the factory and its self-organization (by its own development of autoregulation)! The problem as we understand it from people like Abraham is that logic offers us the pathway of mathematics to identify properly our physical theoretic. But then it turns out that mathematicians cannot solve our mathematics, so they invent a second mathematical game to reduce our game, and then proceed, properly to solve that second mathematics. But the logic for that second mathematics is no longer properly the logic of physics. I first grasped a very primitive form of that at a famous first nonlinear symposium at Princeton in the 1950s. It was clear to me that their interests were not in the mathematics we needed for physical systems then, and now in a second inappropriate form.

At the time of our 1972 report, Thom's morphogenesis and its catastrophe theory details, its followers, strange attractors, and the like were beginning to get attention from the popular press. I received a number of calls myself for comment. I said that these issues would be resolved, not in the field of social or any other phenomena as the popular media were touting widely for the field of application of Thom's morphogenesis, but in the subject of hydrodynamics, in which the physical theory was quite mature. I still believe that and think that the ultimate truth has yet to emerge. The issues that I think have to be resolved are physical, not mathematical ones. When we attempt to be very strict, we ask what physically real assumptions are being made?

THE LORENZ EQUATION SET

When Thom began to define the topology for morphological forms, he elected to study the catastrophes associated with spherical topology. When Ruelle and Takens

(1971) began on the hydrodynamic field, they elected the catastrophes associated with the torus. Note that their election for non-singly connected topologies was very useful for the problem that Lorenz had tackled, nonlinear singularities in ordinary differential equation sets. But consider the question: What was the general problem under attack physically? In my mind, picturing MIT with its two towers devoted to meteorology, the two chief figures sitting there were Lorenz, and my friend Jule Charney. They each took turns in running the meteorology show. Charney had worked with von Neumann in attempting to solve or resolve the equations of the hydrodynamic atmosphere, a problem that had been tackled earlier by Richardson in the 1920s. His failure, Richardson said, was that his solutions showed weather systems going the wrong way around the Earth and at the speed of sound. The von Neumann-Charney computations with their new high speed computer gave the same results in 1955. You will find me teasing my friend about those results in Iberall (). So they "invented" synoptic meteorology. They chopped off those hydrodynamic terms they didn't like, and examined those that they did like until it gave reasonable answers. As one with great respect for the precision of the Navier-Stokes equations, I could not accept that sort of solution, neither for turbulence, nor for atmospheric turbulence. But their 1955 attack still left great problems (e.g., not only for these, by then much better known, meteorologists, Charney and Lorenz). Lorenz then tried to find even simpler sets of equations, no longer nonlinear partial differential equation sets but simple ordinary differential equation sets that he hoped might be able to exhibit some more startling behavior, more like what they wanted for noisy meteorology. Thus, the Lorenz attractor set was presented in 1963, and it was very suitably taken up within the Ruelle-Takens program when it came along. I do not quarrel with their mathematics. I quarrel with whether it is the proper mathematics, as a physically relevant topology for complex systems. I think we are in complete agreement that the rudiments of the issues will be decided in fluid mechanical fields, but it will have to deal both with the mathematical complexity of turbulence and the physical complexity of what we call complex systems (e.g., those with stormy weather processes like those found in brain, society, meteorology, Earth's interior rheology, galactic and cosmic storminess, or the storminess out of the vacuum). We wanted to pursue these themes with Charney, who also reviewed our Soodak and Iberall (1978a) article on complexity, but he died. At least we had budged Charney somewhat in the direction of our Iberall (1971) article on a deterministic theory for turbulence. He began to examine a fuller atmospheric set for their low order modes that began to provide some idea like that for the El Niño transformation, and the differences in the temperate and equatorial weather and wave systems. In particular, Charney deepened the understanding of the connection between the meteorological system and the deep water oceanic hydrological system. His studies also helped cast some light on air mass stability motions, such as those developed by G. I. Taylor and Pekaris, and the character of high altitude waves. The topologists claim a theory for turbulence, but they really do not have one for strong turbulence.

To grasp the issue more specifically, examine Sagdeev, Usikov, and Zaslavsky (1988). It is part of the hypermodern revolution in the mathematics–physics associated with dynamical analysis. But let us note the following slip. The particular case is their exposition of the Lorenz attractor (p. 244) in a chapter on strong turbulence. They present the Lorenz set

$$\begin{aligned} dx/dt &= -\sigma x + \sigma y \\ dy/dt &= rx - y - xy \\ dz/dt &= -bz + xy \end{aligned}$$

If we just consider the small amplitude linear terms

$$\begin{aligned} dx/dt &= -\sigma x + \sigma y \\ dy/dt &= rx - y \\ dz/dt &= -bz \end{aligned}$$

we find that if r (proportional to the Rayleigh number), is greater than 1 (σ, the Prandtl number for gases is about 1), that the linear equations are unstable and they grow. It is that property that forces the nonlinear equation set to be unstable and produce the modern explosion associated with chaos. But this is not part of the much more old fashioned mathematical physical theory embedded in homeokinetics. Our basic assumption is that the small amplitude equations of the complex fields that concern us are thermodynamic—namely, they do not and cannot diverge. The small amplitude equations are stable. They converge to a rest point. Only the convective nonlinearities in the Navier-Stokes equations drive them into instability. This is often also tied up with the issue of nonuniform convergence. The equation set used is not the true physical thermodynamic equation set, but one that assumes if we have solved the true thermodynamic set and reached a point of instability, one can then use an approximation to continue further. An equivalent linearization of an equation set within the nonlinear region is not the same physics as a set that is thermodynamic at its foundations. Thus, the mathematics of dynamics being pursued in hypermodern form is not that of physics, but approximation ideas that produce mathematics that then strictly mathematically produce these processes—of chaos, of fractality. That same sort of attack has been used classically for turbulence, namely the authors—ever since Reynolds—give up on continuing to find the unstable solutions within the rigorous Navier-Stokes equation set. They unfortunately wind up with Richardson's sort of result. The "weather" goes the wrong way and at the wrong speed. That is fine mathematics, but it does not fully serve physics. Thus, to correct their kind of derivation, one has to go back to the Navier-Stokes derivation, nominally from whence Rayleigh and Richardson and von Neumann started, and pursue the argument in all of its detail, to show why weather doesn't go backward with or at the speed of sound securely rather than by an insecure synoptic meteorology or Lorenz type of dynamics. Of course, we may be wrong, and there is a higher ordered mathematical or logical derivation in which both points of view are equivalent, but until that is forthcoming, the logical basis for chaos and so on remains in some doubt.

What is the underlying logical point at issue? That from one false premise any result can be proved. Thus, the Sagdeev et al. (1988) argument has to be examined very carefully both from a mathematical and physical point of view. The situation that is dubious physically is that both $r > 1$ can be true (leading to the nonlinear stability of the Lorenz set) and the small amplitude stability of the Rayleigh set. For small Rayleigh numbers, the field has to converge to a fixed thermal conductivity, not Bénard rolls or the like. By the way, I don't have any difficulty with the problem. As our 1971 article in Rome showed, the issue depends on not throwing away the compressibility, which leads to a much higher ordered mathematical set. It is only by increasing the mathematical complexity that permits viewing the entire field that one can find the solutions that work, rather than by an equivalent linearization.

A Little More History

Garfinkel (1983)—attempting a short history of science via attractors—states that thermodynamics, equilibrium thermodynamics, is a point attractor theory: all systems moving toward a heat death. Oscillatory phenomena in an autonomous system were not a theoretical possibility until "the surprisingly recent development of nonequilibrium thermodynamics." This is startling. The issue became quite clear with Chapman and Cowling's *The Mathematical Theory of Non-Uniform Gases* in the 1930s (1952). The progress made in many communities, particularly during WWII, led many groups to grasp their own home versions of irreversible thermodynamics. We demonstrated that in showing the theoretical foundation for flow through a nozzle as explosive discharge of a shock wave and the ability to compute the final near-equilibrium conditions of temperature and pressure after the shock. This flow field passing through Mach number 1 is a severe test of thermodynamic theory. Other issues of stability theory existed in den Hartog's *Mechanical Vibrations* from 1940, and the Routh-Hurwicz criteria of stability dating back much further. The regulation and control engineering community had developed its notions from the Nyquist stability theory on. In 1955 (not published until 1960) we proved, by direct experiment, that the human mammalian organism obeyed the Second Law of thermodynamics by its cycling through an extensive spectrum of periodic processes, equilibrating in Second Law closure at a period like 3½ hours. Our presentation at Waddington's meeting (Iberall, 1969a; 1969b) claimed them as limit cycles (see also Iberall & Cardon, 1964b, 1965, for our earlier assertions about biodynamics).

In a later chapter in Yates (1987), Garfinkel is much more reasonable, taking the issues for dynamical foundations for biology back to Turing and Rashevsky in the 1950s. All told, we find that Waterman and Morowitz, (1965), as editors, have managed to assemble a much more telling history of theory in biology by many of its distinguished practitioners at that time in the early 1960s. One will see clearly that their book was an elegant forerunner to Waddington's Lake Como meetings.

We add one more final detail from Yates' book. That is the chapter by Stear, a friend and control engineer, formerly head of the department of Electrical Engineering at UCLA, then at UC Santa Barbara, then chief scientist of the US Air Force AFOSR, and now planning vice president of Boeing. I had urged that both Abraham and Stear attend our Dubrovnic meeting. Stear, I believe, gets our construct right. After he presents his control view, he stresses that the greatest difficulty in applying it to biology was the fundamental nonlinearities in biological systems. I was a figure in the 1940s, 1950s, 1960s, and 1970s stalking the control engineering circles in ASME, AACC, and IFAC, stressing the fundamental need to develop a nonlinear understanding of control systems, which did not arrive there at all until the 1950s with bang-bang control and an infusion of European, including Russian, control engineers. I was chairman of a theory committee of my older peers in the ASME division devoted to regulation and control in systems and I put the name dynamical systems theory on the work that our ASME division in instrumentation, regulation, and control was going to pursue from mid-1950s or so onward. I was also the one who put biological control in those control engineering circles of AACC because of its unsatisfactory nature in the then biomedical engineering circles, and brought it into respectful association with the leaders of the American and international physiological community. Stear was one of the first on my committee (from IEEE). Thus, I have always respected his opinion. In his article, he then offers views that he believes have interesting connections with control theory. These are our views and Haken's synergetics views. He identifies our hierarchical view of systems, each with its atomisms engaged in persistent oscillatory motions. In our context, the central doctrine of homeokinetic regulation is modified to encompass operation by dynamic regulators as thermodynamic engines that maintain the mean operating states. The continuum or near-continuum collective is usefully described in hydrodynamic terms. Dynamic instability at each level as sufficiently large fluctuations can produce superatomisms at successive levels. Because the thermodynamic engines making up the dynamic regulators are sufficiently nonlinear, any relevant feedback control concepts or circular regulating chains must involve nonlinear behavior. Within side–side physics arises the up–down physics of hierarchy. I believe that is a very clear short version that acknowledges what we do.

REVEALING PRACTICAL ("DIRTY") LIMIT CYCLES

Our 1972 study of what I called "practical limit cycles" (especially chaps. 7 and 12) was based on the following observation. When we disturbed thermodynamically real physical systems that had been forced or found to be operating in a nonlinear fluctuating or oscillating autonomous mode (e.g., my heart rate or metabolism disturbed by a small burst of activity), we almost invariably found that if the system were disturbed from "below" so as to slow down or reduce the amplitude of the cycles, or from above so as to speed up or accentuate the fluctuating amplitude, that the fluctuating process would return in a short term limit to the original fluctuating cycle.

This we thought of as providing necessary and sufficient conditions for a limit cycle, namely the concurrence between its cycling from inside to cycling from outside. We had to determine what might be the steady energy source to rule out the fluctuations or oscillations of that process as the source for observed fluctuations. We could think about what we saw in terms of the well-developed nonlinear theory of the clock, that according to a Poincaré specification as modified by Airy, would require both a near-linear isochronous oscillator as a timekeeper for each cycle, a low dissipative noise environment involving its thermodynamic losses, and a nonlinear escapement coupled to a steady energy source that would inject a makeup for the lossy energy per cycle, at the least disturbing phase of each oscillation.

To help think about what is involved in understanding a real cycling system, some simple but appropriate examples are needed. A very typical system we and others had often used to illustrate dirty limit cycles is a mechanical one. Consider a uniform rate sanding belt with a flat disk mass attached to a horizontal spring resting on it. The moving sanding belt "grabs" the mass by dry friction, stretching the spring. When the tugging force exceeds the friction force, the mass disengages and is dragged back quickly, generally in a somewhat stuttering fashion, but then it re-engages and it pulls out again. This process continues indefinitely, creating a somewhat dirty but reliable limit cycle. One can find this discussed in Saaty and Bram (1964, p. 215). Other examples of common nonlinear limit cycles are found in den Hartog (1944).

But we wanted an illustrative hydrodynamic field example instead of the mechanical one. Note the problem. A limit cycle is a closed, singly connected contour in a phase space of x and dx/dt, for a one degree of freedom system x that is defined by the two equations

$$dx/dt = P(x,y)$$
$$dy/dt = Q(x,y)$$

We are concerned with solutions of such physical systems as $x(t)$ and $y(t)$ with some initial condition for x and y at $t = t_o$. If the curve corresponding to this solution is closed, then for some time T

$$x(T) = x(O); y(T) = y(O)$$

Thus, the solution is periodic. Conversely, every periodic nonequilibrium solution gives rise to a closed curve. In nonlinear systems of equations, some closed curves are limiting sets for the trajectory corresponding to a solution as $t \to \pm \infty$. In that case, they are known as *limit cycles*. Thus, limit cycles are stable closed cycles independent of the initial conditions toward which the solutions tend as $t \to \pm \infty$.

Now our concern is with complex systems in which each degree of freedom is physical, and the system overall has a mechanical inertia or electric inertia (inductance), and is thermodynamic. It cannot be adiabatically shielded indefinitely, but it can be isothermally bounded with very little noise. The conjecture is that the system overall is nonlinear and that the coupling between degrees of freedom cannot prevent each one from demonstrating periodic process solutions. Furthermore, what we had found in the persistent fluctuating near-periodic processes that we had demonstrated in the human mammalian system was that the binary coupling between cyclic processes was almost negligible. We thus had no basis on which to establish the nature of their process coupling except that it was likely very complex and probably not too strong. However, as in most nonlinear systems in nature, they are coupled, frequently, to moderate sources of external noise also (e.g., so-called Johnson-Nyquist noise, that has an Einsteinian–Brownian motion theory for its foundation). Thus, we surmised that the individual degrees of freedom in the complex biological systems are coupled to many other internal degrees of freedom, but—although nonstationary—in time each one will stay effectively bounded within a limited basin of x, dx/dt (e.g., x, y). Thus, we are not looking for a simple two-dimensional limit cycle, but an extended idea, a cylindric space between two cylinders that in cross section furnish the x,y basin, and the third linear dimension is indefinite time (Figure 1). In the basin between the cylinders, a closed curve winds, always or almost always in that basin, being intermittently disturbed into nonstationary orbits but always returning in various long-term time scales to a winding limit cycle. Each independent physical degree of freedom, in its turn, also has such a cylindrical space basin. Thus, whether isolated intermittent disturbances from outside throw the system into an orbit within the limit contour or out of it, as long as the orbits stay within the basin, each degree of freedom remains a limit cycle variable independent of the coupling. These were the properties we found in all the degrees of freedom we investigated in complex autonomous systems with little noise from outside and isopotential boundary conditions using essentially temporally

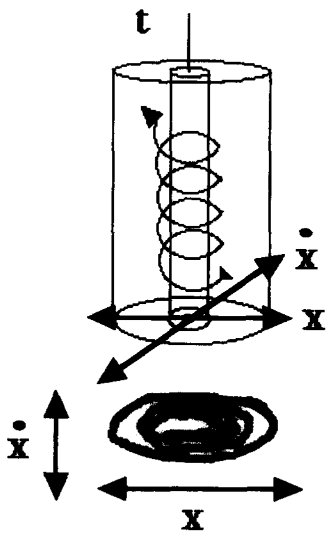

FIGURE 1 Cylindrical space illustrating a "practical" or "extended" limit cycle.

independent energy sources (e.g., constant infusion or isolated periodic or random infusions). Note that these ideas are not inconsistent with the strange attractors notions, but they have a different physical base.

Case 1: A Lava Lamp

What system would demonstrate those characteristics? We wanted something both simple and hydrodynamic, something short of the usual examples like the atmosphere, turbulent fields, living organisms, or social systems. A delayed flight overseas, hours of waiting in an arcade of closed stores, a wrist watch, a captive but willing observer, and a lava lamp on display provided the opportunity to explore a test system. The following description is drawn from the 1972 Iberall et al. report.

A lava lamp is a cylinder holding fluid, with a bottom heater and a quantity of insoluble wax of comparable density to the fluid. The device is used for display purposes as a dynamic ongoing eye-attracting process. Wax spheres settle, fall into a blob, reheat to reduce their surface tension, tear off into near-spherical shape, rise, cool off, change density, and settle back. The process is neither random nor fully determinate. Namely, it is not clear what slug of wax will tear off, but it is clear that some piece will invariably tear loose. Thus, it is a nonlinear lossy system that must provide some resolution of the instability it is presented with over and over again. My colleague, Soodak, has often used the dipping duck system for related demonstrations. The issue with regard to its plausible limit cycle behavior, in the sense that we wish to illustrate our notion here, is how good and reliable a "clock" does it form?

The lava lamp we could observe (June 16, 1972, 5:00 p.m local time, at Orly) had a cylinder diameter of about 4 in. with rising wax sphere diameters of about 1 in. An imaginary surface was noted approximately ½ in. from the top surface, and those wax spheres that passed that surface were timed and counted cumulatively. This excluded very small droplets that were occasionally noted, and some few small spheres that didn't make it to the top (i.e., they cooled off prematurely). Data were gathered over a 20 min period (although the lamp was repeatedly examined over a period of a few hours to assure that no significant change in performance had taken place). The data thus gathered over the 20-min period are shown in the following plot as the cumulative number of wax blobs versus the time of each unit of cumulation (see Figure 2).

The data are boxed by two parallel lines that contain every point observed. One sees a fluctuation "discrete" response that warbles from side to side of those two lines. The mean interval between blobs (taken from the differential statistics) is about 10 ± 5 sec (mean and average deviation). The clocking is not high quality (e.g., errors of about 1 sec per 300 sec of observation), but on the other hand, it is not an indifferent timekeeping device. One notes considerable sequential correlation in the data, namely individual epochs of perhaps 50–100 sec seem smoothly coherent, even if discrete. But the derivatives of smoothly connected lines among those points seem only piecewise continuous. There is that second group of segments that exhibit more ragged performance.

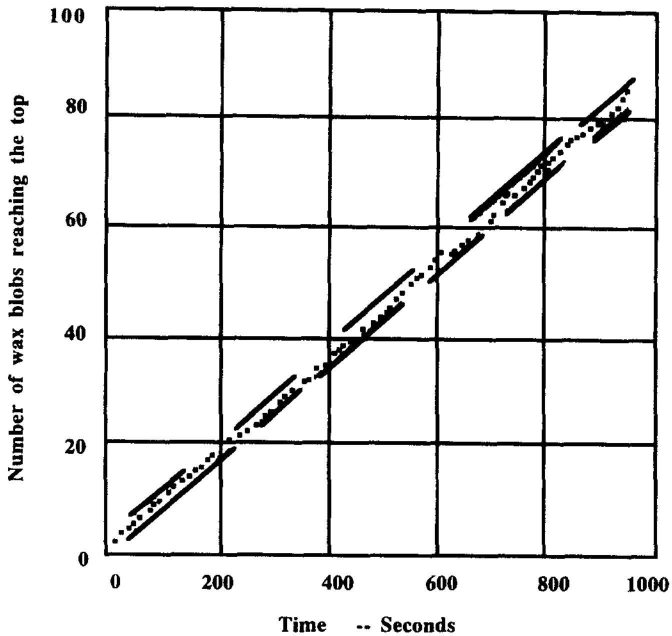

FIGURE 2 Cumulative time of arrival of wax blobs at designated point in lava lamp.

Limit cycle or not? By timekeeping standards, one would say yes. One would say that this is a sloppy timekeeper. Its isochronous element is dirty. But its statistical properties, except for its specific accuracy range, are essentially similar to those of very much higher precision clocks. One detects the segments of piecewise continuity that suggest local coherence; yet in the long run, there is no extended drift from a mean performance. Every class of clocks, up through laser clocks, illustrate similar performance. Thus, even our most precise standards are no different, except for their specific accuracy and precision ranges.

The dirty limit cycle basin, in the sense we use it here, is given by a presentation of $n/t - N/T$, where n is the number of the nth blob counted, t is the cumulative time at which the nth blob was counted, and their long time "averaging" value is the large number N of blobs counted at large time T. This represents the thermodynamic precision in measuring the individual blob, or if there is a thermodynamic engine cycle over which cyclic coherence is achieved, then it will reveal the cyclic fluctuation phase. The rate change is measured by the shorter time derivative (or change) dn/dt. That also is the out-of-phase measurer of any coherent cycling time.

Insofar as there is such a bounded wandering process, the system is bounded to a limit cycle in that cylindrical space. The testing proof would be in speeding up the cycle or slowing it down and showing that it comes back to the same averaging time.

Case 2: The Grandfather Clock

Every academic field seems to want to discover its own version of chaos, without recognizing other plausible accounts, such as "dirty limit cycles." We felt it desirable to attempt another physical experimental test of chaos versus dirty limit cycles in real physical systems. A marvelous repeat of a 17th century physical issue struck us as one easily done. We own a decorative grandfather clock, of the $1,000 variety. These all more or less have the same kind of clockwork of a pendulum and escapement, and weights to be lifted weekly to furnish energy to keep the clock ticking at a near-1 sec compound pendulum cycle. The escapement, mounted on a direct numbered disc 1 to 60, furnishes the two per cycle impulses to the pendulum near its top. Thus, the drop of one of the weights, with the chain driving the escapement, allows the escapement to measure the 1 sec timing phase. The hands of the gears on the clock face then register time by the impulse cycling of the pendulum. One thus has 1 sec marking in the escapement, and 60 min, 12 hr data from the clock plus the visual observation of the pendulum. The cost is in the woodwork. Whereas the pendulum is temperature compensated by alternating bimetalic strips, the weight is largely in the bottom heavy disc. The drive weights are even heavier. At the bottom of the approximately 1 m pendulum (the length of a 1 sec cycle simple pendulum), there is an approximate No. 8 size nut on a screw that is the adjustment weight to "shorten" or lengthen the pendulum period. One might expect that a one-turn adjustment might change the clock timing perhaps of the order of 1 min (40 sec.) per day. The pivot is rather crude, a simple bent wire hanger. The pendulum is not very stiff, so that fluctuations can easily creep into the plane orthogonal to the pendulum. The clock stands simply on a floor, leveled by screws. This is in Southern California so that a considerable amount of microseismic vibrations creep into the clock. There occasionally have been earthquakes at the 3–6 Richter scale (e.g., one yesterday at the 3 scale), but I neglect these. Those tremors have been powerful enough at the higher levels to throw the clock out of kilter (e.g., to start wild oscillations that throw the pendulum into violent out-of- plane motion that most often brings the clock to a halt). The more "normal" operation, whose properties we have tested for the past year, has had a "normal" exogenous noise level. The clock requires oil servicing perhaps once a year or at worst up to once every five years. I have not been meticulous and have only serviced it perhaps every three years. It is not neglected, but neither is it being cared for very well.

Now clearly, there are quite a few mechanical degrees of freedom in the clock, there is a fluid-like process in the real damping and lubricating character of a boundary lubricant. There is a thermal and a mechanical seismic "random" distribution all in a system that is not tremendously high Q. The pendulum traces a rather loping slightly oval path rather than a precise planar motion. Ordinarily, it had been fun attempting to keep resetting the correction weight so as to try to achieve a closure of near 1 min per week. To do that, all of a sudden one has to have a number of sources of higher precision calibration accuracy. Thus, one looks through the common crystal controlled clocks (e.g., a Braun), and then one has to find a suited NBS referral to check on that. One finds that the hysteresis in the nut does not offer any precision in its sensitivity. So one chases back and forth every few weeks trying to stay near the elected goal.

But then it dawned on us, suppose we do not adjust the pendulum "length," only introduce the minimal disturbance of a one-week wind, and check the clock against an NBS referred source a number of discrete times daily. Is that many degree of freedom system thermodynamic (e.g., not exceptional high Q) though largely mechanical system with a considerable random-like noise input chaotic or a noisy limit cycle? That is like a system topologically on a sphere or torus, although there is more than one kind of cycle that exists and is visited in turn, does the system continue to return to a limit cycle in a limited basin for each thermodynamic singular state of motion? In this clock system, we want to know whether the "major" pendulum cycle is in fact contained in a basin for an indefinitely long time (until the atomistic state of its components change). Recall that is what we showed for the lava lamp. It had a noisy limit cycle that stayed within its basin with a complex dynamic of two different sorts. Now we inquire, by examining the changing clock time, only rewinding once a week with little disturbance, what sort of performance do we get in such a 17th century pendulum clock with nonlinear weight driven escapement? Can we achieve the standards that were wanted then and that led to the design of the chronometer? Can we meet the required standards just by this clock and perhaps a clock platform mounting that limits outside disturbances? This has to be an account at changes less than 1 sec per day for a period on the order of a month. We ask, can it be done or not?

After about a year, we had our answer. In its simplest form, we could say that if we judge the timekeeping each week just before the time we rewind the clock, we had the statistics through the entire week of performance including the winding cycle change, of a loss in clock time of a "trend" of −2.8 sec per day with a variance or average uncertainty of ±0.23 sec per day. That slope and band width represent 6 months of performance. It is a "calibration" number that we were able to establish early on and represents an estimate of how close we could estimate time (e.g., to about an uncertainty of a few seconds per week). But that is not really the whole story. Most of the trend error, which does not really concern us in our report of reliablilty, is caused by the peculiar behavior of the escapement. It is a poor escapement, and we would have spent a few years in its redesign if we were trying to get the kind of performance obtained at NBS in a pendulum clock standard that gave the 980.665 measure for Earth gravity g. Each time the clock was wound, the escapement would show, in addition to its tick-tick-tick performance, stops, slips, even though the whole number of intervals covered was 60 per min. That slovenly performance would result in a fallback of about 15 sec the first day, really a portion of a day. Had we interlaced timekeeping with a second clock wound three or so days out of phase with the first clock, we would have found that the "trend" loss was more like –0.1 sec per day than –2.8, namely, the clock itself with the "working" escapement, for 6 days of the week showed a performance more like –0.1 ±0.2 or so seconds per week, namely between 2 and 3 sec per week "accuracy" in time keeping. We consider that very good, if not downright remarkable. It shows the centrality of the timing phase of the pendulum, the relatively minimal nature of the thermodynamic loss of the oil and pivots, and the need for very clean escapement design. The other degrees of disturbance freedom counted little.

So it was time to get the clock oiled. The "expert" craftsman did his job carefully, and was told to make no pendulum length adjustments. He pointed out that the manufacturer knew he had some faults in the escapement design, so that the subsequent behavior might very well change and reflect those faults. As an experimental fact, it did. Instead of keeping time in accordance with the nominal pendulum length, set to near 0.003 in. in the nut position—0.1th of a nut turn—the timekeeping jumped to a gain of about 1 min or so per day, 70 sec per day. But the "trend" was not linear. After two weeks, the gain rate, mostly a daily jump of the clock time by about 1 min per day, began to flatten off. We predicted that it would flatten off toward the few seconds per day—gain or loss, we were still not sure—and we will find—we expect—to show how "nutty" the escapement design is. The only alternative explanation is that the craftsman "bent" the hanger pivot down about 0.030 in. (a shortened pendulum), or that the nut slipped a full turn. We cannot believe those results. Thus, we expect to see a long time drift to back to its earlier pendulum rate. If so, there is no chaos, only poor design of a mechanical element that should be simple to understand. More in time. Experimentally, we never cheat. So, all that we can report here, at this time, is our progress.

It turned out that after about 5 weeks of "curvature" in its timekeeping, the clock performance has "finally" returned to a "linear trend" beginning to approach the same sort of precision that we had obtained before. We have already tested this result with a nut adjustment that brings us reasonably close to a near-zero linear trend. We are getting closer to the previous variance level of about ±0.2 sec per week. Thus, once again, there is no chaos in this second year of performance. We will continue our testing for a while yet.

Case 3: Illustrative Human Measurements

I, at this point, would call attention, less than modestly, to our Iberall and Guyton (1973) regulation and control conference. The last article in that book, on Transcendental Meditation, for example, was included to offer that community a chance to show if it had any data of significance. It did not. I put this note in because on this date of editing (September 4, 1995), I overheard a 1-hr program on "inner calm," a meditation program at a northeatern institute devoted to mind and brain therapy being promoted from Harvard-derived roots of biofeedback from the 1960s on as influencing or producing "significant" changes in long-term inner variables and behaviors regardless of whether one accepted it as science or faith (e.g., as techniques purported to influence blood pressure reduction, headaches, back pain, sleeping problems, and other stress problems). I was shocked to realize how little our homeokinetic results had made in any mark in physiology or medicine, and that only such exaggerated results, mixing of science and faith healing all into a—to us—indigestible package could win out. What is the basis of our criticism? It is that basal operation of metabolic variables requires extreme quietude in all motor activities. However, the various temporal processes that are dynamically involved all the time do not stop nor is there any significant binary correlation among those processes. Thus, the actual physiology of the rest and the activity states is still far from understood. That some such quietude is endowed with the mysticism of internal meditative states, and special faith-driven processes that have to be specially mastered is endowed with considerable nonsense. The entire content of this article stands in contrast to such a program. On one hand, we have the significance of Selye's endocrine proposal of a generalized stress syndrome governing much of the autonomic response of the body. On the other hand, as we have found out, illustrating problems just in the cardiovascular system, there are problems interrelated among the kidney, the cardiovascular system, the cardiopulmonary system, the endocrine system, the biochemical systems from the microvascular level on up, and the brain and its so-called mind. Another message that we had tried to deliver to endocrinologists was that they would have to consider mammalian metabolic regulation as relating to the dynamics of the pituitary, pancreas, adrenals, and liver as a constellation of organs and glands. Here we have been involved in exhibiting the dynamics of blood constituents, and pulmonary, tissue, and glandular constituents. What is our overall point here? It is precisely the same one that is highlighted in this section. In one case, it is the physics versus the mathematics of complex—here living—systems; in this second case, it is the physics–physiological dynamics versus the range of medical–physiological thought that goes from faith to engineering to still very small touches of science.

It is perhaps useful to elaborate a little on the dynamics involved in homeokinetics for complex systems. I will use a few illustrations to make my points more obvious. When I first started on the thermodynamics of the human system, in the mid-1950s, as a study in thermoregulation, I chose to measure surface and interior potentials in the human (see Iberall, 1960). To avoid any confusion with imperfectly defined physiological variables, I chose to measure temperature fluctuations and intake fluctuations in metabolic gases. I had to run the human at a rest or basal condition, and chose stretched out rest on an unyielding plane surface involving little or no heat mass. What I chose for a support surface was a netted surface of high strength textile material. Using myself and then other scientific colleagues as subjects, the question I had to explore was whether a person could so rest with effectively no motion for 5 hr. It was in such studies that we began to learn about the homeokinetic physics–chemistry of near-regular thermodynamic engine processes running both the autonomic and the volitional processes. However, we learned how difficult it was to suppress almost all small movement that we could claim basal processes down to near 1% levels of "volitional" noise. We succeeded. There was nothing mysterious or mystical in that study. It required a great deal of comfortable relaxation to reduce the volitional noise. It did involve getting away from disturbing trains of thought that could and did stir up some low level metabolic activity.

Recall that we also claimed very low or weak correlation among various temporal spectral processes we uncovered. Thus, a number of years later, when we sought to find out whether there was any truth to the claim that some physiologists made that there was a significant circadian rhythm in temperature between waking and sleeping, we were in a position to test the hypothesis ourselves. We had offered various investigators the chance to go into a common experimental test but had no takers. An opportunity for a low-cost experiment arose when I was admitted to the hospital for a few days of observation. The object was to learn what the average temperature cycles were, in particular those late at night at rest before sleeping, to fall asleep (using no other observers), and then upon awaking, make some temperature measurements with no moving about. It requires great discipline to set the frame of mind when going to sleep, effectively absolutely minimizing motion for about 48 hr, and avoiding any motion upon awaking. I did that experiment. It was clear that there was no jump in temperature, or only a very small one, if movement was avoided. On the other hand, upon repeating the test with more "normal" morning movement, it was clear that what appeared to be a "circadian" jump took place. In my interpretation, the temperature jump called circadian was just associated with activity.

In other work reported in *Ecological Psychology*, we've touched on the social physics that we also have been developing dynamically within homeokinetics (Iberall, Wilkinson, & White, 1993, summarizes that effort in the evolution of both social and biological systems). A portion of that homeokinetic flavor for social physics can be found independently introduced in Iberall and Wilkinson (1987). Because we have been involved in contract work in social physics since 1972, readers may find it interesting to compare our efforts with the work of others. For such a report, see Iberall and Cardon (1979).

SUMMARY

Notice, therefore, that we get at our fundamental homeokinetic ideas from a straightforward mathematical–physical theory that produces the Navier-Stokes approximation as a thermodynamic model for all real systems in their widely isolated form. We have shown that the small amplitude of descriptive equations of motion and action (namely, simple or complex system forms) are stable and dissipative. They do not go into oscillations unless they are so driven. Further, they are not chaotic. Nor are they extendedly fractal. For example, for the latter case, we have shown in our modeling of the vascular system and of rivers networks, you cannot "see" or find the dissipative terms in the networks until you have gotten down to the terminal arterioles. Or in rivers, you don't understand what is going on until you get down to the fine bedload of erosion that the flowing water forces out of the land with which it interacts. That bedload defines the effective "viscosity" of the fluid stream, the process that defines the thermodynamic dissipation. Or, a subtlety that is not appreciated in biomedical engineering, you find that the standard systems (e.g., organ subsystems, models that they produce) do not exhibit oscillations. Nor do the dynamical topologists who have attempted to get into the game produce more valid models. Thus, the various attempts at an irreversible thermodynamic model are just as lacking in this field as in other fields to which we have alluded. The proper way to add nonlinearity to the low amplitude thermodynamic modeling is still quite mysterious. It is for that reason we advise what sounds like mathematical nihilism. We do not intend it. We advise very careful work to determine experimentally what might be salient temporal processes, particularly of a limit cycle oscillating form. Then we recommend attempting to build a model around those physically constrained temporal processes. We offered Earth modeling on that ansatz in chapter 12, Iberall, Wilkinson and White (1993). So now here, we will recapitulate our results for human or mammalian command–control.

First, it pays to note what constitutes the thermodynamic program, particularly as we—Iberall and Soodak—have defined it. The continuum equations of Newtonian mechanics, as driven by pressure build up shock waves dispersively. (We neglect body forces here as adding considerable complexity to wave phenomena like in the ocean; it is of great hydrodynamic interest but doesn't add tremendously to some of the fundamentals we need.) To prevent them from destroying the causality that we attribute to the Newtonian laws of physics, there must be smoothing terms connected with the atomistic components of the medium. What these two ideas really imply is that both the shock wave fluctuating causality and the atomistic transport phenomena that looks so smooth (applying as it does from zero frequency to some high Navier-Stokes limit of a few relaxation times, just like the fluctuating shock wave) are both nonlinear. They confront each other and shave the process of fluctuations down to a very minimal size (e.g., the quantum mechanical and thermodynamical levels of persistent fluctuations). But there is another externally exposed nonlinearity, the convective type (e.g., quadratic in mechanical variables) that counterposes the more minute fluctuation–dissipation and maintains the macroscopic turbulent fluctuations. When that convection disappears, the macroscopic form of fluctuations disappears. That is how thermodynamics, per Onsager's linear law and how it introduces chemistry, and quantum mechanics, per Schrödinger–Heisenberg quantum mechanics, as well as Feynman–Schwinger Q.E.D. hold, hopefully now can be joined by a theory of the brain.

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