
INTRODUCTION

The present volume contains lecture notes and a number of my papers (often written in collaboration) on chaos and its applications. The emphasis is on conceptual questions; purely mathematical developments (on dynamical systems theory) have mostly been omitted. The volume is thus subject oriented rather than author oriented (even if my interests in statistical mechanics or the mathematics of dynamical systems are occasionally evident).

The ideas of chaos have been applied, and misapplied, in many areas of the hard sciences, and also in economics, finance, politics and philosophy. This raises all kinds of questions: has there been a change of paradigm (in the sense of Kuhn)? What should one think of the success of “chaos” in the media? What about the use of the notion of “chaos” by the layman? Before presenting my opinion on such questions, I want to discuss briefly the history of the subject and the contents of this volume.

Poincaré and Lorenz.

Let us consider the (deterministic) time evolution of a physical system, as described by a differential equation $\dot{x} = X(x)$. Trajectories starting near an unstable equilibrium x_0 (i.e. $X(x_0) = 0$) diverge exponentially. A small cause (small change of initial condition) therefore has a large effect (on the subsequent behavior of the system). It would seem however that this exponential separation of trajectories starting near x_0 is exceptional, occurring only if x_0 is an equilibrium (fixed point) or a periodic point for the system. In fact, at the end of the 19-th century, Jacques Hadamard [1] gave an example of a system with exponential separation of trajectories starting near *any* x_0 . This *sensitivity to initial condition* is (roughly) what we now call *chaos*. Such very unstable behavior appears at first counterintuitive, but is to be expected when there are many unstable periodic points (a dense set). At the beginning of the 20-th century, Henri Poincaré [2] clearly understood the implications of sensitivity to initial condition: limitations to predictability of the future of a system, for instance in meteorology, and philosophical justification of *randomness*.

The mathematical ideas of Poincaré played an essential role in the subsequent development of the theory of dynamical systems. But the impact of these ideas in the natural sciences was minimal. When chaos was rediscovered later, there had been much mathematical progress (ergodic theory for instance was now available) and the appearance of electronic computers made possible an experimental study of dynamical systems.

Based on a computer study, Edward Lorenz [3] identified in 1963 a time evolution with sensitive dependence on initial condition (the Lorenz attractor). He also rediscovered Poincaré’s idea that weather predictions are limited by sensitivity to initial conditions. Just as Lorenz was unaware of Poincaré, physicists and mathematicians were largely unaware of the findings of Lorenz (a meteorologist) until the mid 1970’s.

The onset of turbulence.

My own involvement in what would later be called chaos dates from the late 1960's when I read the book of Landau and Lifschitz [4] on fluid mechanics, and was dissatisfied with their description of turbulence as *quasiperiodic*, i.e., as a superposition of periodic oscillations (so-called "modes"). I knew (through René Thom) the work of Anosov, Smale and other followers of Poincaré, which showed that quasiperiodic behavior was very special, and that there were more general types of time evolution, more likely in my opinion to describe turbulence. In collaboration with Floris Takens, these ideas were put in mathematical form in the paper "On the nature of turbulence", which is item *n° 2* of the present collection; here we proposed that turbulence is described by *strange attractors*. These strange attractors were not given a formal definition at the time; the phrase just expressed that the long time behavior of some systems is much more complicated and interesting than quasiperiodic behavior (it can in particular be *mizing*).

The publication of "On the Nature of Turbulence" was delayed until 1971, but already in the spring of 1970 I had given a series of lectures in Lausanne, based on this paper. Handwritten notes, in French, by Jean-Pierre Eckmann were circulated at that time, but otherwise the lectures remained unpublished. A retyped (but for the rest unmodified) version of Eckmann's notes constitutes item *n° 1* of the present volume. This is basically a detailed version of paper *n° 2*: following Landau's idea, one tries to obtain quasiperiodic behavior by successive Hopf bifurcations, then one shows that small perturbations lead to strange attractors. (Originally we needed a quasiperiodic system with four different periods, but paper *n° 3* later extended the result to the case of three periods). The interest of the Lausanne lecture notes is that they show in detailed manner the use of arguments and mathematical techniques (center manifold, normal forms, etc) that would later be used repeatedly by various authors in related problems. A similar remark may be made about paper *n° 4* on bifurcations in the presence of a symmetry group; the point there is that symmetries (often present in experiments) alter the generic behavior of a system, and in particular its bifurcations.

The papers discussed thus far deal with the physical problem of the onset of hydrodynamic turbulence, and the mathematically related strange attractors. For a presentation of the latter see paper *n° 11*. The early history of chaos is also discussed in my book *Chance and Chaos* [5]. Paper *n° 14* gives an idea of the state of the subject at the end of the 1970's.

Chemical chaos and turbulent crystals.

If one believes that strange attractors and sensitive dependence on initial condition are present at the onset of hydrodynamic turbulence, it is natural to look for chaotic behavior in other nonlinear systems. In paper *n° 5* it is proposed to look at oscillating chemical reactions. (Chemical chaos has indeed later been observed experimentally, and the corresponding chaotic dynamics has been reconstructed and analyzed in great detail; see for instance [6]).

Crystals have a periodic structure under space translations. Actually, at finite temperature the positions of the atoms do not form a periodic lattice, but a precise periodicity statement can be made in terms of a decomposition of thermal equilibrium states (invariant under the Euclidean group) into Gibbs states (not invariant in general). Quasi-crystals have a quasiperiodic structure under space translations. It is thus natural (as discussed in paper *n° 16*) to ask if “turbulent crystals” exist, behaving chaotically under time translations. Experimental evidence for such structures is missing at this time.

Measures and attractors describing chaos.

One can hope that the time averages for a chaotic system are equal to “ensemble” averages, i.e., averages with respect to a suitable natural measure ρ invariant under time evolution, and carried by a (strange) attractor. Chaotic dynamical systems have (uncountably) many ergodic measures, so that a suitable definition of natural measure is nontrivial. Such a definition is important not only for the theory of turbulence but also for nonequilibrium statistical mechanics.

A satisfactory “natural measure” describing time averages for Lebesgue almost every initial condition was defined by Sinai [7] in the special case of Anosov systems. This definition was extended by myself and Rufus Bowen (papers *n° 6, 7*) to uniformly hyperbolic (i.e. Axiom A) systems, and it was shown that the natural measures satisfy a *variational principle*.

Based on the uniformly hyperbolic case I proposed a general philosophy for natural measures in physics (see paper *n° 8*; the idea is basically to do as if the system were uniformly hyperbolic). For an application of this philosophy see Gallavotti and Cohen [8]. Mathematically, Pesin’s theory [9], [10] allows to attack the problem of nonuniformly hyperbolic systems, and to implement in many cases the idea to “do as if the system were uniformly hyperbolic”. The main work in this direction was done by Ledrappier and Young [11]: the natural measures (called SRB measures, for Sinai, Ruelle, Bowen) describe time averages for initial conditions in a set of positive Lebesgue measure, are smooth in unstable directions, and satisfy a variational principle.

The physical problems and mathematical ideas just mentioned are discussed in papers *n° 8, 9, 13, 25*.

If the definition of natural measures is not obvious, neither is that of attractors. One would like nearby orbits to tend to an attractor but one would also like the attractor to be indecomposable into smaller attractors. Paper *n° 12* proposes a definition of attractors which is stable under small random perturbations of the dynamical system, and is topologically invariant. (It differs in this respect from the later definition of Milnor [12]).

More on hydrodynamics and turbulence.

Paper *n° 10* shows (quantitatively) that thermal fluctuations in a turbulent fluid can quickly be amplified by the sensitive dependence on initial condition to have macroscopic effects.

In paper *n° 17*, in collaboration with Jürg Fröhlich, we examine Onsager's idea [13] of negative temperature states for a gas of vortices in two-dimensional hydrodynamics. We prove that, at least in the standard thermodynamic limit, such negative temperature states do not exist. The problem is in fact equivalent to the statistical mechanics of electric charges in two dimensions, where *screening* is present at some temperatures. The question of possible screening in turbulence is discussed in paper *n° 33*.

As shown by Foias, Ternam, and others one can, using the Navier–Stokes equation, give *a priori* estimates for some ergodic parameters of (possibly) turbulent hydrodynamic flows (notably give upper bounds for the dimension of attractors). My contribution was to show that these ergodic estimates could be sharpened and simplified by using estimates about the bound states of Schrödinger operators due to Barry Simon, Elliott Lieb, and others. This is described in paper *n° 18*, and the related papers *n° 19*, paper *n° 20* with Nancy Kopell, and paper *n° 21* with Jean-Pierre Eckmann. The idea was successful, and later used by other authors, but without proper credit. (It is one of those occasional cases where one feels that one has lost a year's work for the benefit of other people.)

More on dynamical systems.

A few papers on the mathematical theory of dynamical systems have been included in this volume because of their relevance to physics.

Paper *n° 15* (with Massimo Campanino and Henri Epstein) is on the Cvitanović–Feigenbaum equation, and describes an approach to finding a solution (this was carried out in more detail in [14]).

Paper *n° 21* extends a formula due to Rufus Bowen which gives the Hausdorff dimension of self-similar sets in terms of the thermodynamic formalism. An application of this formula to Julia sets is given in paper *n° 22*. (For a later, more detailed calculation, see Widom, Bensimon, Kadanoff and Shenker [15]).

A physically important (and in general unsolved) question in dynamical systems theory is whether they are exponentially mixing (i.e., whether the time correlation functions of smooth observables decays exponentially). Paper *n° 24* gives a simple counterexample to the most obvious conjecture in this direction. (See also Pollicott [16]). Instead of time correlation functions, it is often easier to study their Fourier transforms. These turn out in some cases to be meromorphic and the poles, called *resonances*, are reminiscent of the resonances in quantum mechanics. Paper *n° 26* and paper *n° 27* (with Viviane Baladi and

Jean-Pierre Eckmann) are devoted to the physical aspects of these resonances, and give references to the corresponding mathematical work.

Diagnosis of chaos, and critical assessments.

The important problem of interpreting experimental time series is reviewed in paper *n° 25* (with Jean-Pierre Eckmann) and taken up again in papers *n° 28, 30* (both with Jean-Pierre Eckmann and Sylvie Oliffson Kamphorst) and paper *n° 29*. Paper *n° 28* discusses the numerical determination of Liapunov exponents. Paper *n° 30* shows how the global correlations of a time series can be visualized.

The early papers on chaos were received with suspicion and even disbelief, forcing the authors to be prudent. With success a more nonchalant attitude developed, leading to some questionable applications of the ideas of chaos. The remarkable Grassberger–Procaccia algorithm for instance (for the estimation of dimensions of attractors) was misapplied, and papers *n° 31* and *n° 32* discuss limits to its applicability. Finally paper *n° 34* is a critical discussion of the applications of chaos.

A change of paradigm?

A paradigm, according to Thomas Kuhn [17], is roughly speaking a model for the scientific thinking of a certain period. I would like to argue that we have witnessed (around the decade 1970–1980) a change of paradigm from *modes* to *chaos*. Complex nonperiodic oscillations of a physical system may be viewed (by Fourier theory) as a superposition of periodic oscillations. White light may thus be decomposed in monochromatic components, and complex sound into pure sounds. It is tempting to ascribe the periodic components to oscillators or *modes*. For a mechanical system with quadratic Hamiltonian, the decomposition into modes corresponds to the diagonalization of the Hamiltonian. Furthermore, KAM theory (Kolmogorov, Arnold and Moser) shows that the description by modes is in some sense stable under perturbations. The idea of modes has been extremely useful in physics. (The ptolemaic system of epicycles is an early example. A later example occurs in Planck’s theory of the black body radiation).

The limitations of the “modes” paradigm become clear if one looks at a chaotic dynamical system in finite dimension (say a chaotic mechanical system with a finite number of degrees of freedom). In this case the chaotic oscillations contain infinitely many periodic components, and this shows that there is no physically meaningful decomposition into modes. One is thus forced to abandon the paradigm of modes for that of chaos.

The above discussion may appear simplistic, and one may object for instance that the Russian school of dynamical systems (Kolmogorov and his students) understood quite well what is now called chaotic motion. The point is that paradigms concern what people do with currently available techniques, rather than what they know in principle. So it is that one would in the 1960’s prove the “stability of the solar system” (Arnold [18], using KAM

techniques) while in the 1990's one would prove the opposite result (Wisdom [19], Laskar [20], using computer methods).

Chaos and the media.

The popular success of chaos is due to several factors: the simplicity and power of the ideas involved, the striking terminology (*strange attractors* [paper n° 2], *chaos* [21]), and of course Gleick's bestseller [22]. I don't have the ambition to analyze this success here, but rather to formulate some opinions on its effects outside of the scientific community.

First it should be understood that the scientists' messages are carried by the media with a certain twist. So it is that books and articles written for the general public are often rewritten by the staff of the publisher. (I have myself had good and bad experiences in this respect). In particular tomorrow's historians of science will have to understand that an article they read, for instance, in the *Scientific American* reflects usually neither the style nor the choice of words of the author.

In my opinion, however, the most serious problems in the relations between scientists and the media originate with scientists: specifically with those willing to make irresponsible statements to catch attention at all cost.

When addressing a general audience, the best a scientist can offer is an improved understanding of the world we live in. Thus we know that the earth circles the sun and not the opposite, that many diseases are caused by bacteria and viruses. We now can also assess more accurately the predictability of our own future, and talk about it using the terminology of chaos, attractors, and sensitivity to initial conditions. It is true that these concepts are very often misused (and one can read rather appalling gibberish involving chaos). But more and more people are scientifically literate, can understand the new concepts, and are enriched by them.

Concluding considerations.

Philosophizing about contemporary science is dangerous: hindsight is lacking and one tends to take militant attitudes in current quarrels. Yet the evolution of the ideas on chaos is fascinating and provokes reflection: let me thus indulge in some final remarks.

A basic idea of physics (and other sciences) is to study simple situations, elementary systems. We have now reached a time when some simple situations are reasonably well understood, and there is a growing interest in *complex systems*. The use of modern computers has made these complex systems much more accessible, and has had for example a considerable impact in the studies of hydrodynamic turbulence. But scientific progress requires specific ideas and methods like those constituting the "theory of chaos" (which is really just one aspect of the theory of dynamical systems). It is a specific bag of tools containing powerful methods for the analysis of a specific type of problems, namely deterministic time

evolutions with relatively few excited degrees of freedom. There have been attempts to apply this bag of tools to a variety of irregular-looking time evolutions. Typically these attempts at first cause astonishment, or incredulity. Then detailed investigations either show that the ideas of chaos apply, or do not apply. In the favorable cases, the new ideas are integrated and accepted (so that we know for instance that turbulence is chaotic, and we believe that there is fundamental unpredictability in meteorology or the dynamics of the solar system). Then each domain of research resumes its own individuality, and can no longer be called "a branch of the theory of chaos". The whole process takes a few years. Very roughly I would say from 1963 to 1983 for meteorology, from 1971 to 1986 for turbulence, from 1973 to 1987 for chemical oscillations, from 1982 to the present for chaos in astronomy of the solar system, mostly still in the future for biology. In other words we see localized bursts of scientific activity rather than steady progress on all fronts. Amusingly, such a bursting type of activity is experimentally observed in hydrodynamic turbulence, and called *intermittency*. We might thus say that scientific progress in chaos has been intermittent. But as far as I can judge from personal experience, intermittency is not at all limited to chaos: it is a characteristic of scientific research in general. And the most exciting times are at the beginning of bursts of activity, when the shock of ideas of different sources originates new ways of understanding reality. The bursts of activity give rise to fashionable areas of research, but with some delay, and often the fun is over when the fashion comes.

To me this suggests the conclusion that present day scientific life is overorganized. In the theoretical part of fundamental research at least, good science is not obtained by setting up programs along currently fashionable lines of research, but by giving good people the means to do what they think is most interesting.

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