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DEFENDING CHAOS:

AN EXAMINATION AND
DEFENSE OF THE MODELS
USED IN CHAOS THEORY

DISSEPTION

Presented in Partial Fulfillment of the
Requirements for the Degree of
Doctor of Philosophy in the Graduate
School of The Ohio State University

by

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1997
ABSTRACT

The indispensable role of models in science has long been recognized by philosophers. In contemporary dynamics, the models are often simply sets of equations. Bridging the gap between pure mathematics and real-world phenomenon is especially difficult when the model is chaotic. I address the charge that this bridge has not, in fact, been built and that chaos remains "just math." Although the problems discussed have become acute with the rise of modern chaos theory, their roots were recognized nearly a century ago by Pierre Duhem. The skeptical attacks are both foundational and epistemic.

Targeting the foundations of chaos theory, the skeptic claims that the (fractal) geometry instantiated by some chaotic models cannot be representative of real-world processes. Specifically, mathematical fractals are said to have more complexity than can be captured in a material world of discrete atoms. Furthermore, there is what physicists call "the problem of quantum chaos:" quantum mechanics appears to forbid full-blown chaotic evolutions. I argue that the counterfactual nature of chaotic models can be used to resolve these tensions. Moreover, I show that the skeptic cannot attack the foundations of chaos theory without calling into question unproblematic areas of mathematical science.

The epistemological problems are driven by an essential property of chaotic dynamics, viz., sensitive dependence on initial conditions. This sensitivity confounds the confirmation schemes familiar to philosophers by precluding future predictions of the state of a system. The skeptic argues that without such predictions, the models
cannot be tested. I show how researchers in chaos theory can overcome these skeptical objections via a recently developed method of model construction. This new method not only yields a decisive answer to the skeptic, but reveals a lacuna in the philosophical literature on models in science. I present a new taxonomy of models that incorporates these recent discoveries and conforms more closely to contemporary scientific practice than do previous classifications.
To my wife, Marie
ACKNOWLEDGMENTS

My primary indebtedness is to my teachers, starting with J.P. Moreland and ending with those who directed me through my doctoral studies: Robert Batterman, Mark Wilson, and Ronald Laymon. Special thanks to Robert for his guidance and support. Thanks also to Diana Raffman, Robert Kraut, Stuart Shapiro, and Philip West. Thanks to Peter Smith for being a gracious target and for his many helpful suggestions.

I am also deeply grateful to my family. My great grandmother, Dorothy Miller, did not live to see this monograph, but helped make graduate school possible. My mother, Karen, was also an important source of support and encouragement. To my wife, Marie, thank you for your love and endurance. Your presence and patience have been far more important to this work than anyone will ever know. To my little boy, Andrew, thanks for letting me off the hook so often. Go find your ball, son, your papa's ready to play.
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CHAPTER 1

INTRODUCTION

Chaos theory has been called the third great theory of the twentieth-century, along with quantum mechanics and the general theory of relativity. Such claims are not restricted to the popular press. Even in the usually staid realm of the physics text, one may find proclamations such as this: "Arguably the most broad based revolution in the worldview of science in the twentieth century will be associated with chaotic dynamics."¹ This assessment is fueled by the hundreds of publications that apply chaos theory in such diverse disciplines as astrophysics, chemistry, biology, economics, and the humanities.

Recently both philosophers and physicists have raised warning flags, however: what, if anything, do the mathematical models studied by the chaos theorist tell us about the real world? As we shall see, claims about the ubiquity of chaos have often been made on the basis of computer simulations and highly idealized mathematics rather than empirical investigation. The unique properties of chaotic models are drawn together from a variety of (sometimes obscure) mathematical subdisciplines: topology, symbolic dynamics, measure theory, differential equations, ergodic theory, fractal geometry, etc. Chaos is therefore intrinsically interesting to many mathematicians independent of its applicability in the sciences. However, the realm of mathematical possibility far

outstrips that of physical possibility. How much does chaos theory tell us about the latter and on what grounds do researchers draw the line where it is?

After a brief overview of the philosophical interest that has been generated recently by chaos, we will begin to explore why these questions are starting to arise.

1. Characteristics of Chaos

Given the variety of phenomena in which chaos is supposedly found, it should be apparent that chaos theory isn’t purported to be a first-order theory of anything in particular. High energy particle physics, in contrast, derives its name from its subject matter. Chaos theory is instead part of a large area of mathematical science known as “dynamical systems theory.” A “system” in this context is some object or process that a scientist has deemed worthy of study. A simple pendulum is a system in this sense and so are chemicals in a mixing chamber and the moons of Jupiter. When the system can be described by means of a mathematical equation, the “state” of the system is given by the values of the variables in the equation at a given time. For example, in the familiar ideal gas law PV=kT (with k as a constant), the state of an ideal gas is given by an ordered triple \( (P, V, T) \) with values that satisfy the equation. Dynamical systems have states that vary over time. The mathematical model that describes a dynamical system specifies both its possible states and their evolution.

Most of the work done in dynamical systems theory in this century has dealt with so-called “linear” models. Such models are extremely useful, relatively well-behaved, and mathematically tractable. Nonlinear models, on the other hand, pose a

\[ f: \Gamma \rightarrow \Lambda \]

\[ f(ax + by) = af(x) + bf(y) \] for all \( x, y \in \Gamma \), where \( a, b \) are constants.

2
number of daunting technical challenges. Until recently, most researchers have simply chosen to steer clear of this difficult terrain. This isolationist policy quickly changed with the dawning of the computer age. Nonlinear mathematical models whose solutions were analytically out of reach could now be simulated on a digital computer. Since chaotic models are intrinsically nonlinear (the field is sometimes called "nonlinear dynamics"), the birth of modern chaos theory relied heavily on advances in computer science.3

Chaos has captured the interest of philosophers in a number of ways. At the top of this list is the clarification it has forced on the notion of scientific determinism. At least since Laplace, determinism has been linked to the possibility of prediction.

We ought to regard the present state of the universe as the effect of its antecedent state and as the cause of the state that is to follow. An intelligence knowing all the forces acting in nature at a given instant, as well as the momentary positions of all things in the universe, would be able to comprehend in one single formula the motions of the largest bodies as well as the lightest atoms in the world, provided that its intellect were sufficiently powerful to subject all data to analysis; to it nothing would be uncertain, the future as well as the past would be present to its eyes.4

Laplace's view dominated until quite recently. As late at 1982, Karl Popper invoked a lesser intelligence than the omniscient demon, but still maintained an analytic link between predictability and determinism.5 Chaos theory has made such a view untenable. It is now clear that simple mathematical models can be used, according to physicist E. Atlee Jackson, "to show that one cannot predict some observable results,

3Stephen Kellert has argued that there is far more to the story of the dynamicist's ignorance of chaos than the need for computers. See chapter 5 of Kellert's In the Wake of Chaos (Chicago: University Chicago Press, 1993).


5Earman, ibid., 8.
even when the results are deterministically related to the variables of the model."\footnote{E. Atlee Jackson, \textit{Perspectives of Nonlinear Dynamics}, vol. 1 (New York: Cambridge University Press, 1990), 11.} In contemporary dynamics, a sufficient condition for determinism can be stated roughly as follows: given the state of the system at some initial time $t_1$, there is one possible future state for each time $t_{1+i}$ (i.e., the future is uniquely determined by the present state). The future state both exists and is unique given a single set of initial conditions. A common type of deterministic dynamics is periodic motion: the state of the system at some $t_i$ repeats itself in regular intervals—after one full period. A clock pendulum is an example of periodic motion. If periodicity is taken as a paradigm case of deterministic evolutions, it is easy to see how predictability could be closely associated with determinism. Given the present state $x(t)$ and period $T$, one can predict a countable number of future states, $x(t+nT)$.

In contrast, chaotic evolutions are \textit{aperiodic}—the system never takes on the same state twice. Chaotic aperiodicity is in fact quite complex and in many ways is indistinguishable from a completely random evolution. This explains in part why chaos thwarts precise predictions: the future state of a randomly evolving system can only be predicted probabilistically. Nonetheless, these random-looking evolutions are still deterministic under the existence-uniqueness condition mentioned above. (Oddly enough, many philosophers I have talked with who know little about chaos believe that it presents counterexamples to all forms of scientific determinism, not simply the Laplacean variety. The fact of the matter is that if chaotic motion were not deterministic, it wouldn’t be attracting the attention that it has.)

A second way in which chaos has captured the attention of analytic philosophers is, not surprisingly, in the search for its precise definition. Philosopher Mark Stone and
physicist Joseph Ford have based their proposed definitions on the unpredictability of chaos. Robert Batterman has criticized both views, arguing that statistical and epistemological considerations are parasitic on more fundamental dynamical properties. The most important of these properties is *sensitive dependence on initial conditions* (SDIC). Initial conditions are simply the state of the system at some time $t_i$. If a system displays SDIC, then the future state of the system changes dramatically given an arbitrarily small change in initial conditions. If one were to plot the evolution of the system starting with initial conditions $x(t_i)$ and compare it to the evolution started from a slightly different $x(t_i) + \Delta x$, the curves would diverge from one another exponentially. This notion of small changes having large consequences sometimes goes by the more colorful name "the butterfly effect." The idea is that if the earth's atmosphere is subject to SDIC, then a butterfly flapping its wings in Japan today might be sufficient to change the weather in Miami sometime next year from what would have been a sunny day into a hurricane.

Chaos theory is also thought to have important implications for the notion of lawlikeness and for accounts of scientific explanation. Philosophers have recently shown interest in proposed applications of chaos in economics, literature, and the social

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sciences as well as debunking some of the hype surrounding the field in the popular press.

Misunderstandings fueled by the gush of interest in chaos provides the context for this monograph. The recent work of philosopher Peter Smith in particular corrects some of the infelicities. There is sometimes a fine line, however, between the friendly critic and the skeptic. The same arguments used to quiet unrestrained enthusiasm for a new theory are also part of the arsenal for those who are more antagonistic. A nice example is the field of artificial intelligence which, over the last thirty years, has seen its share of wide-eyed champions, careful critics, and aggressive opponents. As we shall see, there is plenty to be worried about in the application of chaos theory. My goal is to diagnose the philosophically interesting problems and show how they may be overcome—in short, to reply to the skeptic.

2. The Skeptic's Case: Foundational and Epistemological

Given the mathematical sophistication of the natural sciences, it is sometimes difficult to build bridges between the theoretician and the experimentalist. The problem is especially acute for the mathematical models of chaos theory. Jackson puts it this way:

The question can be reasonably raised as to what, if anything, these mathematical results mean in any particular physical context. It certainly means, at the very least, that we need to be careful in applying these concepts (dimensions, measures, and dense


sets), and perhaps we need to be even more careful about drawing conclusions or inferences which may be prejudiced by our unfounded 'understanding' of the concepts. Many of these mathematical concepts, which are based on limiting concepts, need to be applied with care to empirical sciences.\textsuperscript{12}

Most of what is captured under the chaos rubric is complex and interesting mathematics. However, nothing guarantees that what the mathematician finds interesting will have anything to do with the real world. Applying the mathematics is an often difficult and sometimes impossible task.

Consider an analogy. The Navier-Stokes equations are thought to govern the behavior of fluids from very gentle (laminar) flows to the complexities of turbulence. Unfortunately these same equations seem to imply that the density of turbulent fluids changes on very small scales. In order for turbulence to arise, it appears that the fluid density must change from one area to another, but in a space smaller than the constituent molecules of the fluid! This, of course, conflicts with our understanding of molecular physics: the internal density of a molecule is uniform and stable. The exact nature of this conflict is still an open question in continuum mechanics. Ultimately we wish to know how far the governing laws can be pushed before breaking down.

The chaos skeptic argues that the mathematics governing chaotic systems likewise conflicts with the microphysical structure of the systems being described. The details of this tension are somewhat perplexing and will have to wait until chapter 3 to be fleshed out. Suffice to say that physicists like Jackson would like a better balance between observation and theory.

That the truth of chaos theory can be questioned at this point in time should be somewhat surprising. After all, chaos theory is routinely mentioned in the media from \textit{Nova} to \textit{Newsweek} as having been discovered in this or that phenomenon. The

\textsuperscript{12}Jackson, ibid., 66.
producers of such programs and articles are presumably relying on the experts. The charge that empirical support is lacking seems incongruous.

Some researchers are aware of this problem. Physicist David Ruelle, a father of modern chaos theory, is concerned about a misperception by those outside the circle of professional researchers.

Many published papers give the superficial impression that they deal with real physical, biological or economic systems, while in reality they present only computer studies of models. By “real system” I mean a system in, say, astronomy, mechanics, physics, geophysics, chemistry, biology or economics with a time evolution that one wants to investigate. Computer study of a model is an important method of investigation, but the results can only be as good as the model.  

So, of course, if a poor model predicts chaotic behavior, there is little reason to expect chaos in the subject of the model. The broader concern is this. When the casual reader sees a journal article such as “Chaotic Behaviour in the Solar System,” he might easily be misled into thinking that chaos has been detected in the motion of nearby celestial bodies. Instead an astrophysicist has proposed a set of equations as a mathematical model for one such body, viz. Hyperion, one of Saturn’s moons. When the equations are solved with the help a computer, the output is apparently chaotic. However, this does not seem to be a discovery in the ordinary sense of the word, but rather the prediction of a computer simulation. Exploring the behavior of the mathematics is quite different *prima facie* from discovering a new species of moth. The existence of chaotic behavior in the solar system still seems to be an open question.

Peter Smith sums up the problem nicely:

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Why should I accept that, because a mathematical model has a certain feature, then the physical world has a corresponding feature? Discussions of chaos typically blur over the issue here by sliding in an indisciplined way between talking of dynamical systems qua physical phenomena and talking of systems qua mathematical constructs.15

The chaos theorist must bridge the gap from the mathematics to the real world. If the skeptic is correct, the foundational problems discussed in chapter 3 show that this bridge cannot be built, at least with respect to certain key properties of chaotic models.

A somewhat different set of worries motivates chapter 4. Even if the foundational difficulties can be overcome and chaotic models might govern real-world processes, we still need confirmation that they do. Unfortunately, there are a number of serious obstacles to testing chaotic models, the most important of which is SDIC. In particular, SDIC confounds what some consider to be the scientific method, which physicist Robert Shaw describes this way:

A model of the aspect of reality under investigation is constructed. . . . The model is then operated, either physically or by computation, producing a string of numbers. Another string of numbers is obtained by actual observation of the physical system, and the two are compared. The degree of correspondence of these two sets of numbers is a measure of the accuracy of the model. The model can then be modified to attain a better correspondence, and inexorable progress is expected toward the goal of making the two number strings identical.16

To see how SDIC defeats this straightforward confirmation scheme, consider the following thought experiment. Assume that the experimentalist has a perfectly accurate and realistic mathematical model in hand for some phenomenon. A measurement is taken to provide the equations in the model with initial conditions. If the model is chaotic, then even a small change in initial conditions makes for a very large difference in the predicted future states. The problem is that our imaginary experimentalist never has epistemic access to absolutely precise initial conditions. All measuring devices are

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subject to some degree of measurement error, however small. Under SDIC, any
difference between the measured and actual initial conditions is sufficient to produce
radically different future states in the model. Thus since the precise, actual state of the
system is unknown, the predictions made by even a perfectly realistic model are always
false! As we shall see, a similar problem thwarts several of the confirmation schemes
advanced by philosophers of science.

Although chaos theory is a relative newcomer to science, the concerns of the
skeptic over SDIC are not. With the remaining chapters devoted to issues in chaos and
contemporary philosophy of science, let's briefly consider the theory's historical roots.

3. Some Historical Perspective: Poincaré and Duhem

Jules Henri Poincaré is often considered the “last universalist” in mathematics
for having made significant advances in each of its branches. One of the great
landmarks in mathematical physics occurred when Poincaré took up King Oscar II of
Sweden's challenge in 1887 to prove whether the solar system is stable.17 Would the
planets continue in their regular orbits, or would their small gravitational interaction
someday combine to either pull Mercury into the sun or perhaps send Saturn spinning
off in space? Poincaré's answer appeared in his 1892 treatise on celestial mechanics:
the king's question cannot be answered. Specifically he showed that any solution to the
equations of motion would have to be given in an infinite series and that such a series
did not uniformly converge. If an infinite series does not converge, its usefulness is
limited in representing the solution to the relevant equations.

17Ian Stewart, Does God Play Dice? (Cambridge: Blackwell, 1989), 60-72; Stephen H. Kellert,
In the Wake of Chaos (Chicago: University Chicago Press, 1993), 121-122.
The third volume of Poincaré’s *New Methods of Celestial Mechanics* contains one of the “footprints of chaos,” borrowing from Ian Stewart, known as a homoclinic tangle. The context of this discovery was a highly idealized system of three interacting bodies, two very large and one small. Among other things, Poincaré realized that the motion of such a system would be extremely convoluted: “One is struck with the complexity of this figure that I am not even attempting to draw.” For our present purposes, it is enough to realize that the presence of a homoclinic tangle implies SDIC, as Poincaré well-understood. This discovery was ignored for the most part until topologist Stephen Smale revived the qualitative theory of differential equations in the early 1960’s. Homoclinic tangles and SDIC were interesting on purely mathematical grounds, but their implication for the physicist is what makes Poincaré the father of modern chaos. Sometimes prediction works more or less the way Laplace imagined, but Poincaré realized that

it is not always so; it may happen that small differences in the initial conditions produce very great ones in the final phenomena. A small error in the former will produce an enormous error in the latter. Prediction becomes impossible.

For a recent and dramatic illustration of this point, consider that an error of 15 meters in the position of the Earth today makes it impossible to predict where it will be in 100 million years—a relatively short time in celestial mechanics.18

Pierre Duhem had the strongest words for the impact of SDIC on mathematical physics. Predictions made of such systems are

mathematical deductions [that] will always remain useless to the physicist; however precise and minute are the instruments by which the experimental conditions will be translated into numbers, this deduction will still correlate an infinity of different

practical results with practically determined experimental conditions, and will not permit us to predict what should happen in the given circumstances.¹⁹

An example of what Duhem has in mind is this. Assume that a particular device can measure the position of a projectile to within ±0.001 millimeters. This means that the actual initial position lies somewhere within a 0.002 mm span. There are, however, an infinite number of position coordinates along that short length just as there are uncountably many real numbers between 0 and 1, 0 and 0.5, 0 and 0.001, etc. The "infinity of practical results" refers to the divergent future states possible from this infinite number of proximate initial conditions. In order to obtain accurate predictions—what Duhem calls "mathematical deductions"—the experimentalist would need to be able to isolate the initial position down to a mathematical point. Duhem argues that the infinite precision of the mathematician is experimentally unavailable, consequently,

for the physicist, this deduction is forever unutilizable. When, indeed, the data are no longer known geometrically [i.e., with infinite precision], but are determined by physical procedures as precise as we may suppose, the question put remains and will always remain unanswered.²⁰

Although the designation "chaos theory" was not used at the time, Duhem understood SDIC and its implications for the experimentalist. Straightforward prediction-and-test procedures (like the one sketched by Shaw above) are blocked.

4. Overview

Having invoked the notion of "the skeptic" once again, I would like to remark on its use throughout this monograph. I do not claim that any one person has all or most of


²⁰Ibid., 141.
the skeptic's worries. It is more accurate to say that many philosophically interesting reasons to be nervous about the application of chaos theory have been expressed by philosophers, physicists, engineers, and mathematicians from time to time. All of these have been bundled into my composite skeptic. I will use the skeptic first to make the reader nervous—i.e., to feel the bite of the problems—and then as an enemy-at-the-gate to be answered. The goal is not to destroy a straw man, but to make the concerns as strong and concrete as possible.

Briefly then, let's consider the structure of what follows. In the chaos theory literature, both scientific and philosophical, the term 'model' is tossed about without definition or clear example. As one begins to unpack the skeptic's concerns, this practice leads to some unfortunate confusions and equivocations. Philosophers and scientists use the term in rather different ways. In chapter 2 I present a new, albeit abridged, taxonomy of models in science and discuss some of their properties. This provides both uniform terminology for the remaining chapters and a link to an important thread in the philosophy of science literature in the work of Mary Hesse, Peter Achinstein, Ernan McMullin, and William Wimsatt. Among other things, we will find that the extant taxonomy contains a significant lacunae which is brought to light by developments in chaos theory.

Chapter 3 moves into the foundational problems of chaotic models. Chapter 4 analyzes the difficulties in testing chaotic models. Both of these were introduced in section 2 above. Chapter 5 reiterates key points and conclusions.

Skepticism has no doubt been used to motivate the advance of science and accelerate the demise of false theories and pseudoscience. Karl Popper's attack on Freudian psychoanalysis, for example, is well-known to philosophers. More recently, we have seen the rise and fall of "faddish" theories such as "cold fusion" and "hyperforce"
(supposedly the fifth fundamental force). As for chaos theory, I will argue that science has advanced beyond the skeptic's objections rather than succumbing to them.
CHAPTER 2

MODELING CHAOS

Interest in models and modeling peaked some thirty years ago in the philosophy of science. Recently, however, the subject has received little attention. One reflection of this change is the Encyclopedia of Philosophy. In 1968 Mary Hesse wrote an article for the Encyclopedia called "Models and Analogy in Science." The soon to be published Routledge Encyclopedia of Philosophy has no comparable piece.

Since chaos research includes contributions by applied mathematicians, experimentalists, and engineers, it should not be surprising that a wide variety of models are used. Keeping track of the kinds of models, their properties, and relations discussed in the more technical literature is no easy task. In the first ten pages of a recent monograph, the author mentions the following:

- theoretical models
- mathematical models
- dynamical models
- dynamical equations

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21 For Nancy Cartwright models are somewhat of a side issue in a larger irrealist picture. Michael Redhead, 1980, contrasts his views with those of Achinstein in the 1960's. William Wimsatt, 1987, is the only recent work that approaches anything like a systematic account. His concerns center on the use of models, rather the kinds of models, however.


23 Peter Smith, Chaos: Explanation, Prediction, & Randomness, a series of lectures presented to the Philosophy Faculty at Cambridge University in the Easter Term 1993.

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I don’t know whether these can be harmonized, but I suspect not. In any case, given the many classes of closely related models used by chaos theorists, a map of the terrain will prove to be most useful. A complete taxonomy of the models in dynamics, let alone science in general, with their various properties and inter-relations would require a book rather than a chapter. The following discussion focuses on those topics relevant to the problems found in later chapters.

1. Physical Models, Mathematical Models, and State Spaces

Sections A and B fit some of the distinctions made in the philosophical literature on models into a more modern framework. The terminology has never been uniform, so I will be imposing some of my own from time to time. The latter sections add important categories that are either missing or neglected in philosophical accounts of models in science, yet are essential in contemporary dynamics.

A. Physical models

Perhaps the most familiar class of models is the physical model. Hesse splits a large subset of these models into replicas and analogue machines. Paradigm cases of

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the former are scale models used in wind tunnel experiments. There is what Hesse calls a "material analogy" between the model and its subject: an isomorphism of physical structure. Replicas are often used when the laws governing the subject of the model are either unknown or computationally too complex to derive predictions. When a material analogy is present, it is assumed that a "formal analogy" also exists between the subject and the model. In a formal analogy, both the subject and model are governed, in part, by the same equations.

![Analogue Machine](image)

Figure 1: Analogue Machine

Analogue machines, in contrast, have a formal analogy with the subject of the model but no material analogy. They are governed by the same equations as the subject of the model, but are structurally quite different. For example, simple electric circuits obey the same differential equations as some mechanical systems (Figure 1). A mass $M$ on a frictionless plane that is subject to a time varying force $f(t)$ can be simulated by a circuit with a capacitor $C$ and a time varying voltage source $v(t)$. The voltage across $C$ at time $t$ corresponds to the velocity of $M$. Similarly, a lab rat used in nutrition experiments serves as an analogue machine for humans. The formal analogy between rats and humans might not be as strong as in the mass-circuit example, but the relevant biological laws are taken to be sufficiently alike to produce useful information about
reactions in humans. Stronger formal analogies allow an analogue machine to yield more information about the subject.

According to Hesse, material analogies exist only between a model and its subject. If two or more physical models are governed by the same laws, however, the models themselves are related by formal analogy. These relations are summarized in Figure 2.25

![Diagram of formal and material analogies]

Figure 2: Hessean Analogies

Much of the model literature uses Hesse’s other three analogy relations: positive, negative, and neutral. Positive analogies are the ways in which the subject and model are alike—the properties and relations they share. Negative analogies occur when there are properties that the model has but the subject does not. In a scale-model

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airplane (a replica), the length of the wing relative to the length of the tail is a positive analogy since the ratio is the same in the subject and the model. The wood in the model is a negative analogy since the real airplane would be, say, aluminum.

Neutral analogies are properties that are, in fact, either positive or negative analogies, but it is not yet known which. Part of the reason one uses a physical model is to see what properties the model itself has, make predictions based on these, and then test whether these predicted properties are found in the subject. Unforeseen characteristics displayed by the model which are later confirmed shows that physical models are more than mere summaries of what we already know. McMullin uses the term "surplus content" to describe those sometimes surprising properties by which a model extends our knowledge of the world.

Hesse's analogy relations are best suited to physical models like replicas and analogue machines. The models have a set of properties, some of which correspond to the subject, some of which do not. Since these models are themselves actual objects/systems, all of their properties are, of course, physically possible. For most of the models to be discussed in this chapter, this is not the case.

A more typical subclass of physical models correspond roughly to Hesse's "simplifying models." Such models are at least in part either nonactual or nonphysical since they are constructed by abstracting away properties and relations that exist in the subject. Here we find the usual zoo of physical idealizations: frictionless planes, lossless transmission lines, point masses, etc. A textbook mass-spring system with only one degree of freedom, for example, is an idealized representation of bodies and springs we see everyday. This particular system is physically possible, but nonactual. Real springs always wobble just a bit. If by chance a spring did oscillate in

one dimension for a period of time, this event would be highly unlikely but would not violate any physical laws. Frictionless planes, on the other hand, are nonphysical rather than merely nonactual. There are no physically possible worlds in which there are surfaces without friction.

Including simplifying models under the broader rubric "physical model" is not meant to suggest that they are material systems that exist in the actual world as replicas do. Idealizations are nonphysical/nonactual by definition, although simply calling them "false" is a bit crude. A given idealization might be more or less radical than another. Treating the moon as a perfect sphere, for example, is less severe and more realistic than treating it as a point mass. In neither case is the model "physical" in the same way that a billiard ball as a model of an atom is physical. All of the billiard ball's properties are both physically possible and actual.

The primary motivation for using idealizations is to simplify the mathematics that governs the physical model. In elementary circuit theory, e.g., one finds that the resistance in a wire is proportional to its length. Since this resistance is small, stipulating up front that the wires are resistance-free simplifies the governing differential equations. It will be important later to keep purely mathematical simplifications, like the elimination of a term, distinct from idealizations in the physical model. The two operations are closely related, but they operate within separate branches of the taxonomy of models. Simplifications belong to the next category.

Before moving on, there is one more important subclass of physical model, viz. Achinstein's "theoretical models."²⁷ Theoretical models, such as the Bohr atom and the billiard ball model of gases, are used to explain the properties and behavior of some object or system. Unlike replicas and analogue machines, these models are not concrete

²⁷Achinstein, 102-107.
objects distinct from the subject of the model. One cannot pick out a theoretical model by ostension. These abstract models are often part of a conjecture about the structure of the subject (e.g., the quark model of fundamental particles). If so, the model might one day serve as the cornerstone for a broader theory. Conversely, as Michael Redhead points out, theories can sometimes be demoted to theoretical model, e.g., "the primitive kinetic theory of the early Maxwell would now be disparagingly referred to as the billiard ball model of a gas."  

B. Mathematical Models

Unlike philosophers, scientists have no qualms about calling sets of equations "models" or "mathematical models." In chaos theory there is the famous Lorenz model (1), a set of three, coupled, ordinary differential equations (ODE's). Philosophers tend to treat equations such as (1) as analogous to axioms found in mathematical logic. In the logician's realm, axioms are distinct from the models that satisfy them. For this reason, referring to a set of equations itself as a model sounds to the philosopher like a category mistake.

\[
\begin{align*}
\dot{x} &= \sigma(y - x) \\
\dot{y} &= r x - y - xz \\
\dot{z} &= -bz + xy
\end{align*}
\]  

(1)

Indeed, the relation between a mathematical model and the physical model that it governs is very much like the satisfaction relation in logic. This, I believe, was the initial insight for the so-called Semantic View of theories, a model-theoretic approach.

\[\footnote{Michael Redhead, "Models in Physics," \textit{The British Journal for the Philosophy of Science} 31 (1980): 147.} \]

\[\footnote{The relation between a set of equations and a state space can be made perfectly precise. See for example, chapter 5 of V.I. Arnold, \textit{Ordinary Differential Equations}, Richard A. Silverman, trans. (Cambridge: MIT Press, 1973).} \]
Van Fraassen says explicitly that the use of ‘model’ in science and in logic is not all that different. But as this chapter shows, the term ‘model’ has far too many uses in science to make this claim plausible. For our purposes, the Semantic View complicates the terminology with no apparent pay-off. (In fact, French and da Costa call model-theoretic models “mathematical models,” an unfortunate choice from our present point of view.) I tend to agree with the verdict rendered before the Semantic View co-opted the term in philosophy of science:

[M]ost uses of ‘model’ in science do carry over from logic the idea of interpretation of a deductive system. Most writers on models in the sciences agree that there is little else in common between the scientist’s and the logician’s use of the term, either in the nature of the entities referred to or in the purpose for which they are used.

More recently, philosophers have begun to warm up to the notion of mathematical models. Adam Morton’s “mediating models,” for example, carve out an important subclass. The idea is this. There are many elegant and, as far as we know, perfectly correct sets of equations for complex phenomena that outstrip present analytical techniques. Turbulence is a prominent example. The Navier-Stokes equations in fluid mechanics are believed to correctly describe fluid behavior from smooth (laminar) to highly agitated flow, but few analytic solutions for these equations are presently known. Numerical methods and computer simulations are essential tools in the study of intractable equations such as these, but even so, our computational capacity is finite. The resources required tax even the most powerful supercomputer.

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Instead of trying to solve the governing equations directly, a host of simplifications are invoked in order to extract the dominant characteristics of the mathematics. A simplified mathematical model "mediates," in Morton's sense, between the intractable governing equations and the phenomenon they describe.

One common simplification is to expand an equation in the form of an infinite series and truncate it. (Such a maneuver is used in the derivation of the Lorenz model—a case study in simplifications.) For example, in the old quantum theory, it was common to run up against a Hamiltonian—a function expressing the energy of a system—that blocked the usual mathematical techniques. (Specifically, separation of variables could not be used to solve Hamilton's equations.) Instead, a perturbation parameter, $\lambda$, was used to convert the problematic Hamiltonian $H(I, \theta)$ into a power series such as (2). If such a representation for $H(I, \theta)$ could be found, an approximate solution could be generated to an arbitrary degree of precision by keeping a finite number of terms and discarding the rest. In the terminology to be adopted here, the set of equations incorporating the truncated Hamiltonian is a mediating mathematical model.

\begin{equation}
H(I, \theta) = H_0(I) + \lambda H_1(I, \theta) + \lambda^2 H_2(I, \theta) + \ldots.
\end{equation}

Scientists classify mathematical models in a number of ways, first and foremost by the kind of equations used to construct the model. For the most part, chaos theory is

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concerned with nonlinear, nonstochastic ordinary differential equations. Unpacking this jargon a bit, a differential equation involves an unknown function and its derivative(s). An "ordinary" as opposed to "partial" differential equation means the function depends on only one independent variable. "Nonstochastic" implies that there are no terms in the equation with random variables or distributions over random variables. "Nonlinear" is a bit more difficult. Technically it means that the ODE cannot be written in the form

$$a_n(t)y^{(n)} + a_{n-1}(t)y^{(n-1)} + \ldots + a_1(t)y' + a_0(t)y = h(t)$$

The practical consequence of nonlinearity is that the battery of techniques developed over the last two centuries that exploit the special properties of linear equations are for the most part not applicable.

Very often a mathematical model is used in tandem with a physical model. If so, then the physical idealization and mathematical simplification (usually) go hand-in-hand. Stipulations made when constructing the physical model (e.g., the spinning wheel does not wobble when it rotates) are motivated by the desire to make the mathematics easier to solve (e.g., since a wobbling wheel moves in three dimensions, the idealization reduces the number of equations and state variables required). Another common simplification occurs when the numerical contribution of a term in an equation is negligible compared to the others. The term is removed, sometimes in the middle of a derivation, because it makes no detectable difference to the application at hand. In these cases, the symmetry between changes in the mathematics and changes in the

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38 Difference equations also play an important role, but it is somewhat parasitic on the nonlinear ODE's, at least from the dynamicist's point of view.
physical model may break down. That is, the eliminated term might have an obvious physical interpretation, but it need not have.

In some areas of modern dynamics, physical models have a somewhat diminished role. Researchers, especially in chaos theory, have become increasingly interested in the properties of families of solutions. (Some of the reasons for this will become clearer when we discuss the confirmation problems for chaotic models in chapter 4.) These properties are often captured in another important class of models: state spaces.

C. State Spaces and Phase Portraits

This category receives scant attention in the philosophical literature on models. (State spaces are mentioned in passing by Redhead, but he does not consider them to be a type of model in themselves.39 Van Fraassen also mentions state spaces, but only as a way of approaching the Semantic View of theories.40) State spaces are closely related to mathematical models. State spaces are used in dynamics to represent the system state and its evolution. The “system” is usually a physical model, but it might also be a real-world phenomenon essentially free of idealizations.41 State spaces are found in a variety of forms. Quantum mechanics uses a Hilbert space to represent the state governed by the Schrödinger equation. The space itself might have an infinite number of dimensions with an individual state being represented by a vector. The ODE’s used in chaos theory

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39Redhead, 154-163. This is a helpful transition article from the 1960’s literature to this chapter; however, Redhead does not completely break free of thinking of models in the logician/philosopher’s sense. I.e., equations are not models, according to Redhead. Rather, they “express” a theory $T$ whose solutions “refer” to structures in a state space, p. 150.

40Fred Suppe, Modeling Nature: The Scientific Data Revolution, forthcoming, will also, it appears, take state spaces to be a legitimate member of the family of models.

41One might say, on the other hand, that as soon as state variables are identified and taken to be responsible for the dominant behavior of the real-world system, some idealizations have already been employed. For our purposes it will suffice to take “real-world system” in a naively realist sense.
require many-dimensional phase spaces. System states in these (usually Euclidean) spaces are represented by points. As the state evolves over time, a trajectory is carved through the space. Every point belongs to some possible trajectory which represents the system's actual or possible evolution. A phase space together with a set of trajectories is called a phase portrait. Since the full phase portrait cannot be captured in a diagram, only a handful of possible trajectories is shown in textbook illustrations like Figure 3.

Figure 3: Non-chaotic Attractors

If the system (physical model or real-world phenomenon) allows for dissipation, then attractors can develop in the associated phase portrait. As the name implies, an attractor is a set of points toward which neighboring trajectories flow. Once a trajectory encounters an attractor, it remains there unless the system is perturbed. A mass-spring apparatus illustrates this nicely. If the spring is moved from equilibrium, it will tend to return there over time. The point in phase space that represents the spring-mass at equilibrium is called a point attractor (Figure 3a). In other systems there are attractors known as limit cycles: sets forming closed curves in phase space toward which neighboring trajectories flow (Figure 3b). Sets of points that trajectories flow away from are known as repellors. Like attractors, there are a number of topologically distinct repellors possible in phase space.
Part of the intense interest in chaos theory is due to a particular type of attractor: the so-called *strange attractor* (e.g., Figure 4, the Lorenz mask). One of the features that sets this attractor apart from the others is that it has a noninteger, or fractal, dimension. Point attractors have dimension 0; limit cycles have dimension 1. To be precise, the fractal dimension of a strange attractor is a geometrical property that is related to but distinct from "strangeness," which is a property of the dynamics. Strangeness implies sensitive dependence on initial conditions (SDIC). In terms of the system's phase space, SDIC means that neighboring trajectories, which represent possible evolutions of a system from similar initial states, diverge exponentially over time.

\[ \text{\textsuperscript{42}A limit cycle is more generally called a 1-torus. For higher order } n \text{-tori, the dimension of the attractor is } n. \]
As we have seen, a phase portrait is a geometrical representation of the system state and dynamics governed by a mathematical model. In a given phase space, the parameters of the equations in the mathematical model are fixed. Sometimes researchers are interested in the qualitative changes a physical process might display as parameter values are modified. To represent a changing parameter, a new dimension can be added to the phase space. Such a space is, not surprisingly, called a parameter space. Figure 5 is a common illustration in introductory chaos texts. It represents a two-dimensional slice of a parameter space representing the evolution of a system from a limit cycle to its chaotic regime. Such diagrams are especially useful when discussing the so-called routes to chaos.

In short, there are four main categories of interest: real-world phenomena (i.e., the subjects of the models), physical models, state spaces, and mathematical models. (One can be overly optimistic about our ability to naturally isolate a “real-world system”
from its environment, as Wimsatt points out. Such decisions may contain biases like those in other stages in the modeling process.)

Let's consider an example that puts the four categories together. A device that has proven to be quite fruitful for dynamicists over several centuries now is the simple pendulum. The real-world process to be modeled could be a glob of clay on a piece of string swinging gently back and forth. For the physical model, we will stipulate that (i) there is no friction, (ii) the clay is to be treated as a point mass, (iii) the string is rigid but massless, and (iv) the pendulum oscillates in only two dimensions. The mathematical model corresponding to these conditions is well known:

\[ \ddot{\theta} + \left( \frac{g}{l} \right) \sin \theta = 0 \]

\[ \theta \text{ is the angle of the string from vertical, } l \text{ is the length of the string, and } g \text{ is the acceleration due to gravity. For a relatively small initial displacement, Figure 6 represents the evolution through phase space of solutions to (4). Each point on the closed curve completely specifies the state of the system at a given time. The conclusions one might draw from the phase portrait depends on the idealizations involved. Since every real pendulum experiences some sort of friction, this particular}

\[ ^{43}\text{Wimsatt and Schank, 7.} \]
physical model's usefulness is limited. Removing condition (i) requires that a damping term be added to (4), which in turn changes the evolution of the system. The resulting phase portrait would show a family of trajectories spiraling down toward a point attractor. The state of the system at the attractor represents the pendulum hanging at rest.

As the pendulum example shows, a given phase space, physical model, and mathematical model can each describe one real-world process. An alternate approach would be to say that there is only one abstract model for the pendulum and (at least) three closely related ways of expressing it. I take this to be a stylistic rather than a substantive choice. Physical models are well-known to philosophers. Sets of equations are considered models by researchers. Phase portraits are sometimes said “to model” the dynamics of a system and hence seem to be legitimate members of the taxonomy as well.

2. Models of Data

The goal of model-building in the physical sciences is typically an idealized but useful physical model of the phenomenon under study along with the equations that govern it. The method follows a “top down” approach: start with very general laws and first principles, then work toward the specifics of the phenomenon of interest. This procedure is made possible through a combination of the modeler's experience and a background theory. Graduate-level textbooks are filled with models that can serve as the foundation for a more detailed mathematical treatment (e.g., an ideal damped pendulum or a point particle moving in a central field). If the equations in the mathematical

44Something like this was suggested to me by Ronald Laymon.
model are nonlinear, then a phase portrait will often be generated by solving the
equations numerically (i.e., by computer—analytic methods tend to exploit the special
properties of linear equations). Experimental observations can then be used to test the
models.

A model of data, in contrast, is constructed from the bottom up. As the name
suggests, one starts with the data in order to devise the model. (Henceforth, I will use
the terms ‘model of data’ and ‘bottom up model’ interchangeably.) Although the notion
was introduced by Patrick Suppes in the early 1960’s, it has been ignored in the standard
philosophical literature on models. This category is somewhat orthogonal to the
others. A state space or a mathematical model might be a model of data depending on
its construction. The phase spaces discussed so far are parasitic on simplified
mathematical models, which are often derived in top down fashion from a correct but
intractable law. In a model of data, the order is reversed: the state space is generated
from the data and a mathematical model is (sometimes) found to match it.

What little attention models of data have received historically is negative. Such
models are given the lesser status of “phenomenological” or considered “mere curve-
fitting.” Hesse calls these “mathematical hypotheses designed to fit experimental data”
as opposed to models. According to McMullin, sometimes physicists—and other
scientists presumably—simply want a function that summarizes the data. Curve-fitting
and phenomenological laws do just that. What sets curve-fitting apart from true

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45It appears, however, that Frederick Suppe might soon be giving the notion new life. His
Scientific Data Revolution,” National Science Foundation Proposal #94-22233 (August, 1994).

46Hesse, Models and Analogies in Science, 38.

47E. McMullin, “What do Physical Models Tell Us?” in Logic, Methodology, and Philosophy of
391.

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modeling is that models can explain the data, summaries cannot. Models are said to have "surplus content" (McMullin) or properties that are "neutral analogies" (Hesse). As we have already seen, these features are the sometimes surprising properties in the model that are only later found in the subject. Since curve-fitting merely summarizes what the experimentalist has already observed, they claim, there can be no surplus content to exploit. (Trends and one-step-ahead predictions apparently do not count.) McMullin puts it this way:

The presence of this surplus content is our assurance that the model-structure has some sort of basis in the "real world". For what is "reality" if not the reservoir from which such a surplus is drawn? The fact that the Bohr model worked out so remarkably indicates that the structure it postulated for the H atom had some sort of approximate basis in the real.  

Clearly there is something right about this assessment. The physical and mathematical models used in the old quantum theory did produce some astounding results, the theoretical prediction of the Rydberg constant being perhaps the most dramatic. As McMullin sees it, the models were meant to explain the phenomena that was anomalous from a purely classical point of view. Phenomenological laws, like the Balmer series, were a convenient way of describing the problematic observations, but they were incapable of explaining them. To have explanatory power, a model requires some sort of surplus content with implications for the physical substructure responsible for the phenomena.

This intuitive demarcation between models and curve-fits begins to crumble, however, when one looks more closely at how modern systems analysis treat time-series data (i.e., observations of some quantity that varies with time). Contemporary

48 Ibid., 395-396.

researchers use a much finer grain. Specific examples appear at the bottom of the following chart:

![Flowchart of Time-Series Analysis Techniques]

- **Linear**
  - No Memory
    - Simple Regression
  - Memory
    - Autoregression
    - Moving Average
    - ARMA(n,m)

- **Nonlinear**
  - Memory
    - Systems Approach
    - ARMA(n,n-1)
    - Phase Space Reconstruction

**Figure 7: Time-Series Analysis Techniques**

Hesse and McMullin seem to have in mind the techniques mentioned on the far left: regression. This paradigm case of curve-fitting tries match a simple (linear stochastic difference) equation of the form $X_t = \beta a_t + \varepsilon$ to a set of data points taken over a finite time. (Specifically, this equation represents a generic first order simple regression.) The “best fit” minimizes the error term $\varepsilon$. Simple regressions are said to be without memory in the sense that an input at time $t$ affects the output $X_t$ at $t$, but not $X_{t+1}$.

Autoregressive (AR), moving average (MA), and the hybrid ARMA techniques, in contrast, do have memory. The time series at $X_t$ is dependent on previous values of some input signal or noise (MA), on previous values of the output itself (AR), or some combination (ARMA(n,m)). This dependence is explicit in the (first order) difference equations for each:

1. **MA**: $X_t = -\theta a_{t-1} + a_t$
2. **AR**: $X_t = \phi X_{t-1} + a_t$
3. **ARMA**: $X_t = \phi X_{t-1} - \theta a_{t-1} + a_t$
The subscript $t-1$ signifies one step in time prior to time $t$. From the modeler's point of view, these delay terms capture information about the dynamics of the system that produced the time-series and describe how the system continues to be affected by previous system states.

ARMA($n, n-I$) models are syntactically the same as (7), but differ somewhat in their implementation. The technical details are unimportant for our purposes. What matters here is that the technique captures still more detail about the underlying system dynamics, so much in fact that a clear break is made from approaches further to the left of our chart. One text puts it this way:

[0]ur approach to modeling is not merely of regression or curve fitting... We treat modeling as finding a representation of a stochastic dynamic system, in the form of difference equations, derived from and dependent on the time series data. Such an approach may be called a system approach to modeling as compared to the regression or correlation approach. If the data gathering procedure is such that the data truly represents the behavior of the underlying physical system, then the system approach provides its true representation and can be used for analyzing the physical system.50

The point is that at a certain level of sophistication, system analysts believe that (bottom up) models can emerge from the data, not merely curve-fits. McMullin and Hesse put all bottom up constructions into the latter category, failing to capture legitimate models of data in their taxonomies. This oversight can be excused to some degree because of the relative obscurity of models like AR and MA. However, the recent surge of interest in chaos has brought another kind of bottom up model to the forefront.

The bottom up model used to study chaotic systems goes by several names including time-series embedding and phase space reconstruction. Few philosophers

50This so-called system's approach takes the relation between the input and output signal as a Green's function or what engineers call a transfer function. The relation between the output signal and its time delays is interpreted as an autocovariance function. Both pieces are then used to construct the mathematical model. See Sudhakar M. Pandit and Shien-Ming Wu, *Time Series and System Analysis with Applications* (New York: John Wiley and Sons, 1983), 78f., 142f.

51Ibid., 142.
appear to be aware of it.\textsuperscript{52} Some chaos theorists, on the other hand, see the method as something other than business as usual in experimental dynamics. A full explanation of phase space reconstruction will have to wait until more mathematical machinery is in place. It is the most technically complex topic to be discussed in this monograph. Briefly, embeddings capture the so-called "geometric invariants" of a dynamic system evolving in a phase space, such as the frequency with which a trajectory tends to visits a neighborhood on a strange attractor. Note that the linear models of data just discussed are mathematical models, specifically stochastic difference equations. Embeddings, in contrast, are phase portraits.

In order to head off a potential confusion, let's consider one more distinction among phase portraits. We have already discussed how a phase space is generated from a mathematical model of ODE's. In fact, there is a hidden ambiguity. Usually sets of nonlinear ODE's are numerically integrated by computer algorithm and the results plotted on a simulated phase space. Numerically integrated solutions are not identical, however, to those found by analytic methods, i.e., the strategies found in textbooks on differential equations. Some researchers refer to computer simulated phase portraits as "experimental" to distinguish them from the "real" phase portraits that would have been generated if one had the analytic solution to the equations in hand.\textsuperscript{53} (Note that both kinds of phase portraits might be plotted on a computer monitor. The difference between the two phase portraits is the source of solutions, numerical or analytic, not the fact that a computer is involved in some way or other.) To distinguish these, the latter


"real" phase portraits will be referred to as "theoretical" and those generated by numerical methods as "simulated." In chaos theory, theoretical phase portraits are hard to come by given the complexity of nonlinear equations.

Neither theoretical nor simulated phase portraits are identical with embeddings: they follow a top down approach, starting from a mathematical model in order to produce a phase portrait. Neither counts as a model of data. Although computers are typically involved in time-series reconstruction, they are not solving equations. There are, recall, no equations to be solved in a model of data. We must therefore make a third category of phase portrait alongside theoretical and simulated: *reconstructed* phase portraits (or "reconstruction space," the terminology is not uniform). Of the three categories, only this one counts as a model of data.

3. Artefacts

'Artefact,' unlike 'idealization' and 'simplification,' is a technical term that cannot be dealt into a single category of models. Like an idealization, it is a kind of Hessian negative analogy—a property or relation in the model but not in the subject. However, idealizations are found in the front-end of the model-building process. The modeler recognizes the false properties for what they are and uses them for a specific purpose—usually, once again, to simplify the mathematics. An artefact, on the other hand, is a (typically\(^\text{54}\)) false consequence of an idealization, simplification, or method.

Perhaps the most elementary examples are the wooden models of molecules used in high school chemistry classes. Three balls held together by sticks can, to some

\(^{54}\)As Robert Batterman has pointed out to me, it is possible for an artefact to be realistic. Just as unsound arguments can have true conclusions, an artefact can by chance have a positive analogy with the subject.
degree, represent a water molecule, but the color of the balls is an artefact. (As the early moderns were fond of pointing out, atoms are colorless.) In population studies carried out by the Census Bureau, noninteger values for persons is an artefact of the mathematics, e.g., the average American family with 2.14 children. Artefacts are often benign and obvious, as in the examples just mentioned. Other times things aren’t so clear.

Artefacts in models of data can occur due to the limitations of the measurement and signal-processing equipment used. It is impossible, e.g., to fully shield an oscilloscope from the periodic signal produced by its AC current source. Moreover, an instrument can sometimes seem to provide information that it cannot, in fact, measure. For example, a digital voltmeter might display six decimal places when only the first three are meaningful. The precision of the digital display is an artefact. Like other artefacts, those just mentioned can be recognized for what they are by the experienced experimentalist.

Hesse’s neutral analogy is in reality either a positive or negative analogy, recall, but the experimentalist does not yet know which. One way of understanding the following chapter is this: there are models in chaos with neutral analogies to real-world systems. Most researchers believe they are realistic, positive analogies providing both useful and true information about the subject of the models. A small number of skeptics challenge this view. They argue that some of the most striking properties of chaos models must be artefactual. I will show that although the issues raised by the skeptic are philosophically interesting, the majority view is correct. The neutral analogies are shown to be positive.

In this chapter, we have identified three kinds of falsehoods that make their way into models and model-building: idealizations, simplifications, and artefacts. The first is a characteristic of physical models, the second of mathematical models, and the third
cuts across all categories. Mathematical models and state spaces were shown to be full members of the taxonomy rather than merely ways of expressing the information already found in a physical model. Finally, models of data were introduced, filling an important void in the philosophical literature about models.

The value of this chapter is twofold. First, it stands alone as an up-to-date contribution to a topic in philosophy of science that deserves a renaissance. Works in progress by Suppe and others suggest that this might in fact be happening. Second, it provides the tools with which to address the chaos skeptic’s concerns.

In order to gain some perspective on the problems in the next two chapters, I would like to first consider an analogous case from the recent history of science in which the questionable application of models plays the central role.

4. Catastrophe Theory

Whatever the specific issues involved, the chaos skeptic is clearly motivated by the explosion of popular interest in the field. It is both interesting and profitable to note that a similar phenomenon occurred only twenty years ago to a close cousin to chaos: catastrophe theory. Consider some of the claims:\(^5^5\)

- The most important development since calculus. (Newsweek)
- Affords novel and deep insights into the world in which we live. (New Scientist)
- The Promised Land may be in sight at last. (Science)

Mathematicians Héctor Sussmann and Raphael Zahler played the role of skeptic whose attacks were aimed for the most part at E. Christopher Zeeman.

A. Applied Catastrophe Theory (ACT)\\n
Catastrophe theory *qua* mathematics is not controversial. The groundwork provided by René Thom in the early 1970's is presented in an awkward style in the opinion of many mathematicians, but no one disputes the validity of the relevant theorems. Questionable application of the mathematics is what brought out the critics. What makes ACT so interesting is that it provides mathematical models and state spaces for discontinuous behavior—a notoriously difficult area to capture with differential equations. The models often seen in articles on the subject show, among other things, a manifold representing the parameter space for some system ($M$ in Figure 8). Recall that a parameter space is roughly a phase space (in which the state variables are represented) with an additional dimension for each parameter (also called *control variables* since, intuitively, these are the variables the experimentalist can manipulate). For our purposes, it will do to focus on one of the seven so-called elementary catastrophes: the cusp.

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\[56\text{There are a number of nontechnical introductions available. See Sussmann and Zahler, *ibid.*, and chapter two of John L. Casti, *Complexification* (New York: HarperCollins, 1994).}\]
The cusp catastrophe involves two parameters, $x$ and $y$, and a single state variable $z$. Each point on the manifold $M$ is uniquely determined by $(x, y, z)$. For each point in the $x$-$y$ plane outside of region $B$, there is a unique point on $M$. Inside $B$, the region of bimodal behavior, a point on the plane corresponds to three points on $M$. The curve $CS$, for “catastrophe set,” bounds $B$ on two sides. Both branches of $CS$ start at the cusp point $C$ and are tangent to one another at $C$. In ACT one assumes that as $x$ and $y$ vary continuously, the corresponding point in $M$ will obey the Delay Rule: if possible, $z$ will also vary continuously; otherwise, $z$ will make a discontinuous jump. The only motivation for this rule appears to be that it “approximates the facts,” i.e., physical models and real-world systems only “jump” when they have to and not before.\footnote{Tim Poston and Ian Stewart, \textit{Catastrophe Theory and its Applications} (London: Pitman, 1978), 84.}

To see how all this is a model of discontinuous behavior, consider a smooth change in parameters from $a = (x_1, y_1)$ to $b = (x_2, y_2)$. Each point on the parameter plane determines a unique point on $M$ until the nearest branch of $CS$ is
encountered. A choice must be made as to which fold of $M$ will be followed. According to the Delay Rule, the trajectory carved out on $M$ (determined by the change in the parameters) must remain continuous as long as possible. Hence the lower fold is chosen. As the parameters continue toward $b$, the next critical boundary is the second CS. The $z$ values have simply run out of manifold and so must jump to the upper fold once $(x, y)$ reaches this branch of CS. This event is called the catastrophe (a term which apparently is less apocalyptic in French than English). Since $z$ is the state variable, one will observe a qualitative change in the system at the catastrophe.

Let's consider an oft used example of Zeeman's: animal aggression. The relevant state of a given animal is taken to lie on a one-dimensional spectrum ranging from flight to attack. Two conflicting drives, fear and rage, are taken as parameters. This two-parameter-input, one-state-variable-output relation fits the cusp geometry. In order to invoke Thom's theorems, it must be assumed that the state of the animal is a function of fear and rage and that this function is smooth and generic (i.e., if the function governs a manifold $M$, then this manifold is structurally stable—small perturbations will not change its topology). Thom's Classification Theorem says that for all functions $f(x, y)$ meeting the smoothness and genericity conditions, a cusp catastrophe will be found in the parameter space with probability 1 (in the measure theoretic sense). Assuming the requisite conditions are met for fear, rage, and behavior, Figure 9 can be used to model the attack-flight reaction.

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58See Sussmann and Zahler, Part II for more details on the Classification Theorem and its application. For the case at hand, the theorem shows that, generically, a $f$ function with two control parameters and one state variable will have a parameter space in which every point is ordinary, a fold singularity, or a cusp singularity. In measure theoretic terms, genericity here means that in the space of all $f$ functions, the theorem holds except for a set of measure 0.
The model shows that if rage is present without fear, the animal will attack. If there is fear but no rage, the animal will flee. Things are more complex when both drives are present. Consider path $P$ in the parameter plane. The animal starts with a given amount of fear and rage, but becomes increasingly angry. Tracing the animal's state across $M$, and of course being careful to obey the Delay Rule, one should observe a discontinuity in behavior as $P$ crosses the second catastrophe set. This discontinuity represents an attack, which is consistent with the notion that an animal generally can be provoked only so much until a violent strike or bite is made. Had $P$ gone in the opposite direction, the discontinuity associated with sudden flight would be traced. The cusp catastrophe provides a good model for observed animal behavior, so its advocates claim, whereas rival models have great difficulty accommodating the discontinuity. As Zeeman points out, a naive model might predict that the conflicting values of rage and fear cancel each other out.

The models in ACT range from physics and biology to the social sciences. The more controversial applications include prison riots ($x=$ tension, $y=$ alienation,
z=disorderly behavior, catastrophe: riot) and censorship (x=aesthetic value, y=erotic content, z=public access, catastrophe: censorship).59

B. The Critics

The further one progresses from the hard sciences, the greater the number of assumptions that are needed to invoke the Classification Theorem (without which the mathematical foundation of the models disappears). The more assumptions that are required, the greater the controversy over a proposed application. Let’s consider two examples of the critics’ complaints.

(1) The mathematical assumptions required for applications outside of physics cannot generally be justified. One assumption is that the system has a fixed steady state (equilibrium) for each set of parameter values. This rules out periodic states (represented by a limit cycle in the relevant phase space) and aperiodicity (e.g., a state evolution governed by a strange attractor).60 Given the supposed ubiquity of chaos, this is a somewhat naive assumption.

(2) Sussmann and Zahler repeatedly point out that Zeeman’s models yield bad predictions.61 For example, the behavior variable z in the animal aggression model, according to Zeeman, is supposed to range through a smooth continuum: flee, cower, avoid, ignore, growl, attack. But this is simply false; the behavior of an attacking animal is clearly discontinuous. There is no smooth transition before and after the point at which the dog jumps or the snake strikes. They either jump (strike) or they do not.

59See for example Poston and Stewart, ibid., 409-423.

60Casti, 81.

61Sussmann and Zahler, 130f.
In fact, it is this very discontinuity that the model was supposed to capture via the catastrophe.

C. Ad hocness

Perhaps the best way of capturing the bulk of the charges leveled against ACT is that the models are often *ad hoc*. Although Zeeman explicitly denies this, it does appear that many of his most controversial examples lack any theoretical grounding. The assumption that there is some unknown smooth function relating the parameters and state variables looks very optimistic when applied to, e.g., rage and fear. Perhaps it would have been better not to call them applications in the first place, as Michael Berry suggests,

> The applications should . . . be distinguished from what I shall call invocations of the theory, where it is employed because of the suggestiveness of its images in the hope that its axioms might eventually be shown to apply. . . .

It appears that any observable discontinuity in biology, psychology, and the social science can be fitted to one of the seven elementary catastrophes given enough assumptions and creativity. It should not be assumed that ACT plays any sort of explanatory role when this is done, however. Importing the machinery of ACT might well be suggestive, as Berry notes, but unless the assumptions used can be supported by an underlying theory, the charge of ad hocness cannot easily be rebutted.

So where has this caveat landed us vis-à-vis the chaos skeptic? Ad hocness surely provides some motivation for the more extreme “invocations” of chaos in ethics and literature. Now, there is little doubt that the generalizations about order and

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63Michael Berry, “Correspondence: In support of catastrophe theory,” *Nature* 270 (December 1, 1977): 382-383.
randomness found in chaos theory do have metaphorical applications. Theoretical physics is often used this way. (The quantum mechanical notion of measurement influencing the outcome of the experiment can be applied to every activity in which persons are monitored and evaluated by others. The principle is taught in some management courses, for example.) Nonetheless, whatever chaos-in-literature is, it isn’t what nonlinear dynamicists are working on and *prima facie* has no more than an analogical connection to the sciences. The requisite mathematical relations simply aren’t there. (Similarly, someone might claim that some relations in cultural relativism are analogous to those in special relativity. For example, a person’s judgments depend on his “frame of reference.” However, *pace* the occasional student in introductory ethics, this weak analogy does not provide cultural relativism with any kind of support from physics.)

The skeptic is also concerned with appraisals of chaos theory as the third great theory of the twentieth century. The claim is especially misleading to those who know little about the field. Chaos theory isn’t a theory in the sense that one might have a coherent set of facts and hypotheses about particle mechanics or gray whale migrations. Chaos isn’t a theory of *anything*, rather it’s a theory in the sense that mathematicians talk about number theory, set theory, or even the theory of differential equations. Each of these transcends the first-order theories of the sciences and, like calculus, might bear on many fields of study. That’s why mathematicians, engineers, biologists, astronomers, meteorologists, and physicists can all work on chaos. Researchers in many areas are interested in nonlinear dynamics. Like the overblown praise of catastrophe theory in the

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64 Stephen Kellert has a forthcoming article on this very issue. What I call “analogical connection,” Kellert calls “metaphorical extension.”
citations at the beginning of this section, the advances in chaos theory are subject to exaggeration. Skeptics hope to curtail the overly enthusiastic.

What about the broad problem of ad hocness? Is this a worry for the chaos skeptic? To some degree, yes, but the main problem is deeper than this. If we were to consider only, say, chaos in the humanities, then ad hocness would be an issue. It is highly unlikely that a "theory of literature" will develop in which a legitimate dynamical model can be constructed. But without such a theoretical base, any application of chaos must be ad hoc. Most of the skeptics' attention, however, is on applications within the sciences. In these cases, the charge is not merely that the model is free-floating and unjustified, but that certain properties of the model in fact conflict with theory. As we will see, Smith's skeptic questions the realism of the fractal structure of strange attractors given how the state variables are interpreted.

There is one interesting parallel between ACT and present day chaos theory that will likely be discussed decades from now by students in the history and philosophy of science. The similarities are found in the dialectic between the theory's advocates and critics. Consider the correlations: both theories make it into the popular press and have overly optimistic supporters (Zeeman; Joseph Ford). This in turn draws the attention of critics (Sussmann and Zahler; Smith). The critics have some things right, the experts agree, especially in cases where ad hoc applications go well beyond the hard sciences. Finally, both sets of critics go too far in their deflationary zeal.

Sussmann and Zahler, to complete the analogy, came under intense fire in 1977 primarily in the correspondence section of Nature. Zeeman, Berry, Ian Stewart (co-author of a text on catastrophe theory), and John Guckenheimer (a founding member of the "Chaos Cabal" at UC Santa Cruz) were among the respondents. The reported flaws

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in the Sussmann-Zahler attack cover a wide range: (i) A basic misunderstanding of the theory, specifically the role and importance of structural stability. (ii) Failure to appreciate the stimulation that even a faulty approach can give to experimentation and model-building. (iii) Exaggeration, e.g., when Sussmann and Zahler say “catastrophe theory is a blind alley” at a time when there are successful applications in hand. Note that “successful” here means both the explanation and prediction of phenomena (e.g., short wave optics in Berry’s case). (iv) Finally, misquotes, misleading exposition which “[oversteps] the bounds of decency,” deception, and even “character assassination!” Such an exchange certainly challenges the stereotype of the methodical and dispassionate mathematician. Had Sussmann and Zahler been careful to restrict their criticisms of Zeeman to his applications in the social sciences, they would have had the support of other researchers. Instead they overshot the mark and were rebuked by the experts.66

66In passing we might note that a similar but lesser known dispute among mathematicians has been underway more recently regarding the status of fractal geometry. The primary antagonists are mathematicians Stephen Kratz and Benoit Mandelbrot. For a brief review, see William Brown, “Twisting the Fractal Knife,” The New Scientist (September 29, 1990): 63.
CHAPTER 3

FRACTALS, ATTRACTORS, AND ARTEFACTS

Some of the most interesting science and philosophy of science of this century has been driven by the desire for theories to cohere with one another. Whatever exactly the "unity of science" amounts to, we clearly want our best theories to mesh together in a tidy fashion or, at the very least, not to conflict with one another. Science has frequently fallen short of this ideal; tension rather than coherence marks its history. The conflict between thermodynamics and atomic theory in the pioneering work of Maxwell, Boltzmann, and Gibbs is a prominent example.

In this chapter, I examine a more recent foundational clash, this time between the dynamic models of chaos theory and well-established theories within the natural sciences. The conflict can often be sketched as follows: there is a chaotic model of some phenomenon $P$; but, given our best theory of the nature of $P$, $P$ cannot possibly have the properties of the model. Peter Smith has done the most to bring this problem to the attention of philosophers. I will repeatedly draw from his critique to give a voice to the skeptic in this chapter.

One problem that can occur in model building is that too many properties of some phenomenon are abstracted away for the model to be useful. The gap between model and reality becomes too great. This, however, is not one of Smith's driving concerns. For him, the problem with chaotic models is that they contain more properties and relations than can physically exist. Instead of simplifying a complex
phenomenon, the chaos theorist imputes characteristics that cannot be instantiated in the real-world.

Smith’s argument is focused on strange attractors which, somewhat idiosyncratically, he takes to be the mathematical structure at the heart of studies in chaos:

(i) The characteristic feature of chaotic models—the combination of sensitive dependence and confinement in virtue of which they count as chaotic—depends on their being infinite [sic] intricate.67

Combined SDIC and phase space confinement indicate that “the characteristic feature” here is a strange attractor. All attractors “confine” nearby phase space trajectories by drawing them in. Trajectories near a strange attractor exhibit SDIC. As we noted in the previous chapter, the phrase “chaotic model” used here is somewhat unfortunate in that most physicists use the term to refer to sets of equations. Smith often uses the term to denote the idealized physical models (e.g., a damped, driven pendulum) that are governed by such sets of equations, but not in this case. Here the referent is related to phase portraits and their constituents.

‘Intricacy’ could be used to gesture toward a number of properties. One possibility is that it refers to a feature of fractal geometry: self-similarity at all scales. Fractal structures (often) reproduce themselves on smaller scales or, as Stephen Kellert puts it, higher degrees of “magnification.” We will examine in what sense a strange attractor is a kind of fractal in the next section.

Unfortunately ‘chaotic model’ is subject to a different interpretation which significantly changes the meaning of (i). (I believe, in fact, that Smith conflates the

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two. One could rightly say, given the ambiguity in the term, that the phase space/portrait in which a strange attractor is embedded is the dynamical model, therefore it has the property of infinite intricacy that a strange attractor requires. In that case, intricacy might be related to the \( \mathbb{R}^n \)-structure of phase space.

Depending on whether infinite intricacy is a property of attractors or their state spaces, the next claim will either be controversial or commonplace.

(ii) The physical processes that chaotic dynamical models are typically intended to represent can not themselves exhibit true infinite intricacy.

Before discussing (ii), Smith's conclusion is

(iii) The characteristic (infinitely intricate) features of chaotic mathematical models cannot truly represent features of the modelled physical process.

If infinite intricacy is a property of entities within the phase space, then (iii) concludes that the fractal structure of a strange attractor cannot possibly represent real-world processes. The fractal geometry of attractors in the model is a negative analogy. Smith claims that it is this very feature, however, that allows for both SDIC and "confinement" in a finite phase space volume.

If, on the other hand, infinite intricacy is to be interpreted as a property of phase space itself, then the nonrepresentative feature mentioned in (iii) must somehow be tied to the use of real-valued \( n \)-tuples as states of a system. On this latter reading, (ii) would then be alluding to Smith's claim that some variables cannot take on an irrational value without conflicting with well-established theory. That is, the theory might predict that a given property can realistically only take on rational values. For an simple example,

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68 In *Chaos: Explanation, Prediction, & Randomness*, for example, between pages 16 and 39, fractals are said to be infinitely intricate, but occasionally fractal structure is said to depend on or presuppose infinite intricacy in the models to which the fractal belongs. That is, the fractal set might itself be a strange attractor that exists in a given phase space. Sometimes it is the attractor that is said to be infinitely intricate, sometimes it is the phase space. The consequences of this will be discussed at length in section 3d of this chapter. This conflation is much less evident in Smith's most recent work, *Explaining Chaos*, forthcoming.
consider a predator-prey population model that allows, say, the existence of 2.7 foxes in a given area \( A \). At any given time, \( A \) might have \( n \) foxes, but clearly \( n \in \mathbb{Z} \). Hence, a model that relies on or presupposes state variables with values that range across the reals allows for system states that are not physically possible.

Consider Smith's first illustration of this point. Let temperature in a gas be defined as (roughly) "the limit (as the volume of the sphere shrinks to zero) of the average kinetic energy of the gas molecules in a sphere centred at the point \( P \)."\(^{69}\) Although temperature is often represented by a real number in mathematical models, Smith claims that it makes no physical sense to suppose, given this definition, that temperature can have an infinitely precise value at a point in space. There is no fact of the matter, e.g., about whether the temperature at time \( t \) is 9.00000001°C or 9.00000002°C. Why not? To have an average kinetic energy of some finite number of molecules within a sphere, the volume of the sphere must actually capture some molecules over which an average can be taken (Figure 10a). The notion of temperature at a point, however, requires the radius of this imaginary sphere to shrink toward zero. But since molecules have finite size, Smith argues, eventually the shrunken sphere will no longer contain any molecules over which to average (Figure 10b). If gases were continuous fluids rather than aggregates of molecules, temperature-at-a-point would make perfect physical sense. In the possible world we live, however, such an infinitely precise notion is physically impossible to cash out.

[I]f locations along dimensions of the phase space are supposed to represent physical quantities like (say) temperature and fluid velocity, then it makes no physical sense to

\(^{69}\)Peter Smith, "The Butterfly Effect," *Proceedings of the Aristotelian Society* 91 (1991): 256-257. The illustration is essentially the same in *Chaos: Explanation, Prediction, & Randomness*, 9, with respect to fluid mechanics: "the circulation velocity at a point is (roughly) the limit of the average velocity of the molecules in a small sphere around that point as the diameter shrinks to zero." The failure to capture any molecules in the sphere drives both examples.
suppose that there is really infinitely sensitive dependence . . . in nature. What there is can only be something coarser.\textsuperscript{70}

![Figure 10: Smith's Temperature-at-a-Point](image-url)

Two additional facts support the notion that temperature cannot be a quantitatively precise state variable which is mapped to a real number. As we have seen, the size of the sphere in which the average is taken must have some finite size, but need not have any particular size. The radius could take on an infinite number of values within some bounded range. This arbitrariness supports the argument that there is no fact of the matter about the precise value of the temperature at some time. Similarly, one may arbitrarily choose how to treat molecules crossing the border of the sphere at $t$: as if they were captured within, as if they were completely outside, or split the difference in some way. In any case, since the choice is by convention, there is no fact of the matter to be discovered regarding the precise temperature at $t$. (Perhaps a somewhat more intuitive take on this situation, suggested by Robert Batterman, is that temperature-at-a-point isn't well-defined. This is quite different from saying the property can only be quantified with finite precision. Smith believes that temperature in a gas is perfectly well defined, but given the definition and the molecular nature of

\textsuperscript{70}Smith, "The Butterfly Effect," 266.
gases, there is no fact of the matter about its value to an arbitrary number of decimal places.)

There are a number of issues that stem from this notion of infinite intricacy. Making the term precise will be the greatest challenge. Ultimately the question is whether there is anything special about chaos that may serve as a foundation for skepticism. I will argue there is not. To that end, the remainder of this chapter is structured as follows. First, I will give a more rigorous description of strange attractors, fractal structure, and the nature of phase space. Second, we will try to gain some perspective on the purported tension between chaotic models and physical reality by drawing specific analogies with continuum mechanics. Third, we will consider how strange attractors are thought to exploit the properties of phase space in a manner that purportedly leads to an artefactual fractal geometry.

Although Smith will have the most prominent role qua skeptic in this chapter, I wish to reemphasize that his questions and concerns are not his alone. Physicist E. Atlee Jackson reinforces the claim that chaotic models demand that dynamicists face certain issues that otherwise can be happily ignored:

[C]haotic motion . . . brings THE INFINITE and THE FINITE face to face. It does so because the various forms of 'chaos' which we have defined mathematically were naturally based on concepts from THE INFINITE, whereas our computations and observations fall within the realm of THE FINITE. In most dynamics, which involves regular forms of motion, this distinction is not so clearly apparent because the dynamics never explores the 'fine structures' contained in THE INFINITE, particularly when only finite times are considered. . . . What the chaotic motion of THE INFINITE does is to force us to consider whether, within a very finite time, our actions in THE FINITE have any relationship to these theoretical ideas from THE INFINITE. It is clearly a very basic and important issue, about which there is presently only a very limited understanding.71

71E. Atlee Jackson, Perspectives of Nonlinear Dynamics, vol. 1 (New York: Cambridge University Press, 1990), 210-211.
1. Chaos, Fractals, and Fine Structure

Smith's skeptic is primarily concerned with what he calls the "infinite intricacy" of "chaotic models." In short, whatever these chaotic models are, they have a property that is not merely unconfirmed in real-world systems but rather nonactual. Unpacking the bundle of worries 'infinite intricacy' gestures towards is difficult but worthwhile. As is often the case, we will find some wheat and some chaff. First, recall the argument:

(i) The characteristic feature of chaotic models—the combination of sensitive dependence and confinement in virtue of which they count as chaotic—depends on their being infinite[ly] intricate.

(ii) The physical processes that chaotic dynamical models are typically intended to represent can not themselves exhibit true infinite intricacy.

(iii) [Therefore] the characteristic (infinitely intricate) features of chaotic mathematical models cannot truly represent features of the modelled physical process.

Infinite intricacy is a property of chaotic models. Unfortunately, chaotic models are found within each of the three broad categories presented in chapter 2: physical models, mathematical models, and their state spaces. Out of context, the term is ambiguous. However, infinite intricacy does not cut across all three types of model. Smith (typically) associates the term with fractal structure and the notion of scale invariance. In dynamics, these geometric notions are captured in phase spaces.

A Euclidean line can provide an intuitive understanding of scale invariance. No matter what scale is used to describe the line, e.g., rounded to the nearest whole micron, mile, or light-year, one still "sees," as it were, a line. Looking at the fine detail of a line only reveals more line. Geometrically speaking, there is no scale at which "line atoms" appear—constituent parts used to construct the large-scale object. Likewise, one never runs out of line, no matter how much of its length is traversed. Technically these conditions are sufficient for a line to be both scale and translation invariant, two properties that together make an object self-similar. Planes are also self-similar. A line segment, on the other hand, is self-similar to a degree, but if the scale
becomes too large or the translation too great, the segment fails to be recovered. For example, a translation of 5 meters from a point on a 1 meter line segment maps to a point in space that is obviously not on the segment.

As a first approximation, we may take a fractal to be infinitely intricate in that it reproduces itself at all scales. Magnifying a fractal to any power generates a similar structure. In some cases, without knowing the degree of magnification in advance, one would not be able to tell that the fractal object was being magnified at all, much like a line or plane. For example, consider a version of the Sierpinski triangle (Figure 11). Notice the box marked off in 11a. When magnified, this box produces 11b and then again from 11b to 11c. In each case, the structure reproduces itself at a finer scale. Given the governing equations and the right software, one could go on with this box-and-zoom process indefinitely.

^72^To be more precise, some structure can be found at all scales in a fractal and this structure has a non-integer dimension. Self-similarity is often found at all scales, but is not a necessary condition for being a fractal set. Fractals that exhibit self-similarity are called scaling.
This is close to the sense in which scale invariance leads to infinite intricacy. But unlike this computer fractal, in which a new figure is generated at each step, a (scaling) fractal contains a complete self-similar organization. The scale invariant structure is there to be explored.

The study of fractals meets nonlinear dynamics (primarily) in strange attractors.\textsuperscript{73} Let's consider these fascinating creatures of phase space in more detail.

\textsuperscript{73}We should acknowledge that fractal structure crops up in other places besides strange attractors. I ignore for now fractal basin boundaries.
A. Fractals and Strange Attractors

The initial description of the phase space trajectories approaching a strange attractor seems contradictory. Like their linear counterparts, chaotic attractors are contained in a finite volume of phase space toward which nearby trajectories converge. On the other hand, chaotic models are subject to SDIC which requires exponential divergence of trajectories. The tension is clear: convergence and divergence at the same time. How can this be?

Figure 12: Hyperbolicity

Part of the answer lies in the notion of hyperbolicity. Keeping the technical details to a minimum, if an orbit $O$ through some point (e.g., the middle of the cross-section $S$ in Figure 12) is hyperbolic, then all of the nearby trajectories either converge on that orbit or diverge from it. Converging orbits approach $O$ as $t \to +\infty$; diverging orbits approach $O$ as $t \to -\infty$ (i.e., diverging orbits would approach $O$ if time were reversed). In Figure 12, trajectories in the vertical plane running perpendicular to the
square cross-section $S$ are compressed toward $O$. Those in the horizontal plane radiate away from $O$.

This can’t be the whole story, however. Trajectories in the horizontal plane appear to move off to infinity, whereas attractors are limited to finite volumes of phase space. Once “on” an attractor, trajectories cannot leave unless more energy is added to the system. (The reason for the scare quotes is that technically, trajectories only reach an attractor asymptotically—in the limit as time goes to infinity.) For example, consider a motionless yo-yo dangling at the end of its string. Having wound down to equilibrium, the state of this system represented in the proper phase space is found on a point attractor. An orbit zipping off toward the far reaches of the space is equivalent to, e.g., the yo-yo spontaneously climbing back toward my finger.

Apparently stretching and compression are not sufficient to characterize system evolution on a strange attractor. Another operation must be added: folding. Consider the Rössler band embedded in a three dimensional phase space (Figure 13).\textsuperscript{74} The “band,” like all chaotic attractors, is actually a large family of trajectories that have been pulled in from the surrounding basin of attraction. Recall that the evolution pictured is aperiodic: a single state point is never revisited.

\textsuperscript{74}Graphics from Ralph H. Abraham and Christopher D. Shaw, \textit{Dynamics: The Geometry of Behavior}, 2d ed. (Redwood City, California: Addison-Wesley, 1992), 317f.
Let's cut away part of the attractor and isolate a single "layer" of trajectories rising out of plane P (technically a Poincaré section). In Figure 14, orbits are traced from the line A-B which lies on P. (As we'll see, the line is actually a so-called thick line segment.)
As the line makes it way back toward the plane, it stretches and folds over on itself. The line is, in effect, being dragged along by a family of nearby trajectories. The stretching of the segment is the result of diverging orbits. By the time the cycle reaches P again, A-B has doubled its width. As the segment folds over, orbits are compressed without crossing. Starting with this folded, compressed segment, let's repeat the process starting again from P.

![Figure 15: Second Cycle on the Rössler Band](image)

Again, we see the characteristic stretch-and-fold operation. Repeating the process a final time should make the procedure obvious if it isn't already.

![Figure 16: Third Cycle on the Rössler Band](image)

We have found that A-B is only one small segment in an infinite number of layers that have been stretched, folded, and compressed.
Exponential divergence is captured by the stretching operation in which each line segment of a given fold doubles in width every cycle. Trajectories cannot leave the attractor, so the finite volume requirement is met. Compression is more difficult to see, still the mathematics demands a convergence of trajectories. Convergence cannot be accomplished by squeezing orbits onto one another, since crossing trajectories in phase space violates determinism. Another well-placed cut will make the evolution a bit clearer. After an indefinite number of stretch-and-fold cycles, we slice through the Poincaré section P with another plane (a Lorenz section).

![Lorenz Cut](image)

Figure 17: Lorenz Cut (Abraham and Shaw 122-123, used by permission)

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\(^{75}\)Specifically some of the Lyapunov exponents, which provide a measure of the convergence and divergence of orbits, are negative.

\(^{76}\)Technically this restriction is a consequence of the standard existence and uniqueness proofs for ODE's.
Each fold on the attractor produces a horseshoe curve on plane P. Each curve then pierces the Lorenz section like tissue paper, leaving two holes for each horseshoe. Now look at the holes (Figure 18). This gives us some idea how the folds are arranged, although the precision of the figure is limited: curves that are too close together go through the same hole. If the process is allowed to continue indefinitely, the number of layers marches to infinity. If the Lorenz section had unlimited precision, then the number of holes would likewise approach infinity.

Figure 18: Cantor Dust from the Lorenz Cut

Notice the order of the dots. There are, as Abraham and Shaw put it, gaps within gaps within gaps. The construction of this pattern is one example of a Cantor process. The most famous such operation begins with a line segment and then removes a third of its length from the middle (Figure 19). Do the same for the two new segments. If this process continues to infinity, removing the middle third from each new segment, a Cantor set is produced. Such a set has the cardinality of the continuum although, like the rational numbers between 0 and 1, the set has measure zero. (Intuitively, this means that the probability of picking a rational number "at random" from the reals between [0,1] is zero.)
The visible dots in the Lorenz section (Figure 18) are part of a Cantor set. The corresponding line segments in the Poincaré section are Cantor 1-manifolds or "thick lines." The lines are "thick" in that through each hole runs an infinite number of lines. The folds on the attractor corresponding to each thick line segment are Cantor 2-manifolds or "thick surfaces." The manifolds, it should now be apparent, are stacked and compressed like pastry dough with infinite detail—infinte intricacy.

Thick lines and surfaces are ways in which a figure can have fractal microstructure; hence, the Rössler band is a fractal. The characteristic stretch-and-fold operation found in all strange attractors allows for exponential divergence of trajectories (SDIC) within a bounded volume of phase space. The compressed structure of a thick manifold shows how convergence can be accommodated at the same time.

B. Fractal or Fractal-like?

We now have one way of understanding how strange attractors are fractal. To discuss the nature of fractal geometry itself, one can initially be tripped up by an unfortunate quirk in our language. 'Circle' is a noun; 'circular' is an adjective. Likewise for 'rectangle' and 'rectangular,' a difference in spelling reflects a difference in grammar. This is not the case for 'square' which can be used as a noun or adjective. (One generally doesn't describe a box as squarish or square-like.) We are also comfortable with some degree of sloppiness when such adjectives are used. An ellipse
with foci that aren't too far apart counts a circle, for example, even though the figure is not a circle.

What about 'fractal'? When dynamicists use the term to describe a strange attractor, is this phase space entity, strictly speaking, a fractal or is it merely fractal-like? To this point we've been somewhat loose with the notion of fractal structure and to some degree this can be excused. Fractal geometry is only a small part of nonlinear dynamics. Fractals might be of intrinsic interest to mathematicians, but they are tools in the hands of scientists and engineers. As such, one seldom sees more than an operational definition in the chaos literature. To say whether chaotic attractors are fractal or merely fractal-like, necessary conditions are needed.

A more common but somewhat less intuitive understanding of fractal structure than that in the previous section is in terms of dimension. A point attractor, as one would expect with a mathematical point, has dimension 0. A limit cycle is a closed curve in phase space and so has dimension 1. Strange attractors, like other fractal objects, have dimensions with non-integer values—a consequence of their Cantor set structure. There are several nonequivalent methods for calculating fractal dimension; the differences are not important here. One result of their fractal dimension is that strange attractors occupy zero volume in phase space. Consider an $\mathbb{R}^3$ space. Thick lines and thick manifolds do not have the dimensions of typical lines and surfaces, as we have seen, but neither do they have volume in this 3-space. Only objects whose dimension equals that of the space in which they are found have volume in that space. Hence the stretch-and-fold operation is unlike stacking cards. There are never so many folds that eventually a thick manifold becomes a thin parallelepiped in 3-space.

Of the two properties closely associated with fractals, self-similarity (scale invariance) and non-integer dimension, which is the cart and which is the horse? Different texts give different answers. According to Mandelbrot, who, if anyone, should
count as the author on the subject, a fractal is a set with non-integer (Hausdorff-Besicovitch) dimension. The lack of uniformity is due to the fact that a) some fractals are not self-similar at all scales, but b) self-similarity is often used to calculate the dimension.

For a simple example of (b), consider the first six iterations of a simplified Sierpinski triangle (Figure 20). The process begins by removing an inverted triangular

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77Benoît B. Mandelbrot, *The Fractal Geometry of Nature* (New York: W.H. Freeman, 1983), 15. The precise definition requires the Hausdorff-Besicovitch dimension to be greater than or equal to the topological dimension: \( D_H \geq D_T \). \( D_H \) is not much used in nonlinear dynamics; more familiar is the box-counting dimension or capacity \( D_0 \). Since \( D_0 \geq D_H \), if the capacity is greater than or equal to the topological dimension, the set is a fractal. A further complication is that Mandelbrot has recently changed his definition, ignoring dimension in favor of self-similarity of some kind (see Jens Feder, *Fractals* (New York: Plenum Press, 1988), 11). I will use the original definition since the newer version has not yet been made precise.

78Henceforth, I will use ‘scale invariant’ and ‘self-similar’ interchangeably, but this is not quite correct. Self-similarity means, roughly, that the figure is reproduced for some scaling ratio \( r_1 \). Scale invariance means the figure is reproduced for all \( r_i \). Except where noted, I will also use both of these terms in place of self-affine, which is actually a slightly weaker relation. In self-affine figures, the subimages found at smaller scales can be oriented in odd ways which allows for less symmetry than in the regular, self-similar fractals typically used in elementary texts. See R.J. Creswick, H.A. Farach, and C.P. Poole, Jr., *Introduction to Renormalization Group Methods in Physics* (New York: Wiley-Interscience, 1992), 1; Harold M. Hastings and George Sugihara, *Fractals: A User’s Guide for the Natural Sciences* (New York: Oxford University Press, 1993).
section from the middle. Three triangles remain, each with a side \( \frac{1}{2} \) the length of the
original. The process is repeated for each of these three triangles, and so on. Reducing
the sides by \( \frac{1}{2} \) in each iteration is equivalent to decreasing the scale by a factor of 2.
Each time the scale is increased by this factor, three subimages of the original triangle
are formed. This reproduction of subimages generates self-similarity at successively
finer scales. The Hausdorff-Besicovitch dimension for the complete Sierpinski triangle
is given by

\[
D_H = \frac{\ln 3}{\ln 2} \approx 1.58
\]

For the moment, let self-similarity be considered the key to fractal structure.

There are some important details about real-world applications that need to be
mentioned. First is that scale invariance always breaks down at some level when fractal
geometry is used to characterize material objects like crystals and ferns. Mandelbrot
puts it this way:

Here as in standard geometry of nature, no one believes that the world is strictly
homogeneous or scaling. . . . One should not be surprised that scaling fractals should
be limited to providing first approximations of the natural shapes to be tackled. One
must rather marvel that these first approximations are so strikingly reasonable.\textsuperscript{79}

Often a fractal is bounded from above and below. That is, if the set is
considered with too coarse a scale, the intricate structure gets washed out. The
coastline of Britain—a common example in the literature—looks smooth from far out in
space. In such cases, the dimension of the set approaches an integer value and hence is
no longer a fractal. There is nothing peculiar about this effect: two dots placed close
together look like one big dot from a distance.

More importantly perhaps is that fractal structure runs out when the scale is too
fine. Although computer simulations like the one that produced the fractal fern (Figure

21) give the illusion of fine structure all-the-way-down, this is clearly not the case for any physical object. The fractal geometry that describes a mountainside does not extend to the molecules that make up its rocks, grass, and trees. Intricate fractal geometry eventually gives way to bodies better described in Euclidean terms. In short, fractal structure is descriptive of physical objects as a "first approximation" only in the midrange of scales. If, following Mandelbrot, $D_0 \notin \mathbb{Z}$ (the dimension of the object is not a member of the set of integers) is sufficient for a fractal, calling these physical applications fractal-like is too crude. Rather, they are fractal-in-a-given-range or bounded fractals.

Figure 21: Fractal Fern

Returning now to dynamics, let's try to add some precision to the skeptic's worries. He understands the dynamicist's claim that strange attractors are fractals with thick manifolds and all the rest. However, popular applications of fractal geometry are bounded at some scale, as discussed in the last paragraph. At some level of description the fine structure of a coastline must run out (e.g., the pebbles on a beach have smooth surfaces worn down over time, not irregular fractal surfaces that reproduce themselves at finer scales). The skeptic might argue then that dynamicists are playing fast and loose
with fractal structure. In application, fine structure always breaks down, hence it must break down at some scale on a strange attractor as well. Pushing chaos theorists to be more precise, we should expect that strange attractors do not have infinite intricacy any more than mountainsides or coastlines.

Off-hand comments in some chaos texts seem to support this line of thought.

A fractal object may have some largest scale, but, in principle, there should be no smallest scale at which the basic structure is not reproduced. (In practice, there may well be a lower limit too.)

In the case of fractal sets arising in typical dynamical systems, such as the forced damped pendulum, self-similarity rarely holds.

If strange attractors in application do not, in fact, have fractal structure all-the-way-down, then the skeptic has been successful. The demand for rigorous definitions shows that his foundational worries are justified. These passages suggest that infinite intricacy is an artefact of the mathematics that experimental strange attractors do not possess. Fractal structure is merely a “first approximation,” as Mandelbrot puts it, and should not be imputed to real-world processes.

Given the number of pages remaining, the reader has probably guessed that things aren’t, in fact, quite so neat. The passages just cited are talking about self-similarity having a lower limit, but we have already seen that this is not a necessary condition for fractal structure. Let’s consider the rest of the second passage (from Ott’s excellent text):

In such cases the fractal nature reveals itself upon successive magnifications as structure on all scales. That is, as successive magnifications are made about any point in the set, we do not arrive at a situation where, at some sufficiently large magnification and all

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magnification beyond that, we see only a single point, a line, or a flat surface [i.e., a set with an integer dimension].

The fractal sets produced by dynamical processes—usually strange attractors—do have structure at all scales, says Ott, but the set is not self-similar all-the-way-down. As Strogatz puts it, at each level one sees

a complex pattern of points separated by gaps of various sizes. The structure is neverending, like worlds within worlds. In contrast, when we look at a smooth curve or surface under repeated magnification, the picture becomes more and more featureless.\(^2\)

We might also try to picture this contrast from an ant's-eye-view. To an ant on a plane, the terrain looks exactly the same in every direction—forward and backward if the ant is on a line. On a fractal, the terrain is noticeably different in all directions from the ant's perspective. Having fine structure all-the-way-down means that the figure never blends into a simple curve or surface. The ant's-eye-view always yields complex structure, no matter how small one makes the ant.

In fact, there must be some structure at all scales on the strange attractor if not the self-similar structure of a regular, scaling fractal. Understanding why this is so is essential for understanding the properties of the models involved. First, the phase space trajectories on a chaotic attractor never cross and never join up with themselves. Crossing entails a violation of determinism. If it were possible for trajectories to cross, then more than one state at time \(t_0 + \Delta t\) would develop from a single state at \(t_0\). Furthermore, chaotic orbits are aperiodic.\(^3\) This is important since a chaotic attractor is supposed to be different in kind from point attractors and limit cycles.

Aperiodicity, exponential divergence of nearby trajectories, and phase space confinement collectively require fine structure all-the-way-down in a strange attractor.

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\(^3\) Since a strange attractor displays SDIC, quasiperiodic motion is ruled out even though phase trajectories do not meet under that kind of evolution either.
Intuitively, trajectories need space to explore so as not to intersect. But since they lie on an attractor, their wanderings must stay within a fixed volume of phase space. Since strange attractors are structurally stable (roughly, they do not spontaneously decay and can endure slight perturbations), a single trajectory can “keep going” indefinitely; hence carving out infinitely fine structure. The structure will not typically be self-similar, as Ott and Tabor point out, yet it must exist at all scales.

The bottom line is that unlike the bounded fractals used to describe coastlines and snowflakes, the fractal structure of a strange attractor has no lower limit. Dynamicists aren’t being fast and loose with their descriptions as the skeptic might have claimed. They don’t qualify their use of ‘fractal’ (e.g., fractal-within-a-range-R) and they ought not, given the nature of the objects involved.

C. A More Careful Skeptic

The sophisticated skeptic still has reason to be concerned it seems (chaos theorists do use models that contain infinite intricacy it turns out), but that concern is still a bit nebulous. What, after all, is wrong with a strange attractor having an unbounded fractal structure? It isn’t difficult to see how clouds and mountainsides cannot have fine structure all-the-way-down, but attractors aren’t made up of pebbles or discrete molecules. They are, as has been repeatedly pointed out, creatures of phase space.

Let’s reiterate the problem by giving an example closely related to the one using average temperature. An important concept in fluid mechanics is the velocity of a point in a fluid:

[Fluid mechanics] supposes that the velocity of the fluid at each point \( x \) can be given by a vector \( \mathbf{v}(x) \), usually varying smoothly with \( x \). Strictly, this is nonsense. On a small
enough scale we would see molecules bouncing off each other with highly varying velocities, and empty spaces between them where no velocity could be assigned.\textsuperscript{84}

Rutherford Aris makes a similar point in the introduction to his fluid mechanics text.\textsuperscript{85}

If one has a molecular view of fluids, then the velocity at a point can be defined as an average of velocities in a neighborhood around that point. But if the neighborhood is too large, then its connection to a specific point becomes questionable. If the neighborhood is too small, then the average breaks down: the "neighborhood" might at one time be filled with a single molecule and at another time capture only empty space. The problem is essentially the same in Smith's temperature-at-a-point example.

Now consider the thick manifolds on a strange attractor. These manifolds consist of families of phase space trajectories. Since the attractor has a non-integer dimension, it is a fractal and, according to dynamicists, one that does not have a lower bound. Let one of the dimensions in the phase space represent the fluid velocity at some location in the system being modeled, then trace the representative state point along a single trajectory. The problem is now quite simple: the infinitely fine structure of the attractor guarantees that the representative point will take on velocity values that the actual system cannot have. In other words, the model will predict values that are physically impossible.

To show this, Smith argues that most of the points on a chaotic attractor correspond to real-number values for properties like temperature-at-a-point.\textsuperscript{86} Real numbers are infinitely precise; they require an infinite number of decimal places to fully specify their value. But this infinite precision cannot be had, he argues, by averaging


\textsuperscript{86}Smith, "The Butterfly Effect," 263.
over discrete molecules in a small but finite sphere in the system. At best, an infinitely precise value for temperature could be constructed by a limit procedure on the size of the sphere (e.g., the value for temperature-at-a-point approaches a real number in the limit as the radius of the sphere shrinks to zero). But if the sphere shrinks to zero volume, it cannot capture any molecules over which to average. Hence, temperature cannot have an infinitely precise value. (Note that the immediate goal is to show how these property-at-a-point problems motivate the skeptic, not to present alternative interpretations of their physical significance.) Since the fractal structure is responsible for driving the model into physically impossible states, this structure has a negative analogy with the subject of the model; the world is not that way.

The base intuition in all this has a well-known Quinean analog: the indeterminacy of translation. The indeterminacy doctrine is developed within the thought experiment Quine calls "radical translation." The data for a linguist trying to derive a translation manual for a hitherto unknown language community, Quine argues, amounts to only "native utterances and their concurrent observable circumstances." Given this sparse data, various manuals of translation using different analytic hypotheses may be derived. Analytic hypotheses are tentative equations of native utterances to English sentences (or some other natural language). We may expect these various manuals to be internally coherent but logically inconsistent with one another. To this point, semantics faces the ever present problem of underdetermination, except that instead of, say, studying the stars to derive a theory within astrophysics, the data are the totality of native speech dispositions and the theory is a manual of translation.

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Mere underdetermination of translation manuals becomes full-blown indeterminacy since the facts one is allowed to draw on to construct the correct translation (i.e., what the natives "really" mean) are fixed by science as a whole. According to Quine, the scientist tells the linguist what counts as a fact—a piece of data with which the linguist may build a theory/manual. If the sum total of all possible data (i.e., all the truths of science) is not sufficient to determine which translation manual is correct, then there is no such thing as the correct manual. Quine concludes that the behavioral facts cannot determine a single correct manual, therefore there is no fact-of-the-matter regarding what the natives mean.

Smith's view parallels Quine. The facts in the temperature example are governed by the atomic theory of gases. Facts about temperature that are found within the models must be constructed from more fundamental physical truths about gases. That is, the physicist tells the modeler what counts as a fact. If the sum total of all possible facts (i.e., all the truths of molecular physics) is not sufficient to determine a single, precise value for the temperature of a gas at a point, then there is no such thing as the temperature-at-a-point. Smith argues that the molecular facts cannot determine a single correct value for temperature, therefore there is no fact-of-the-matter. However, if fractal structure is to be taken realistically, the models express just such a fact. Given the unquestionable strength of molecular physics, he concludes, the models must be wrong.

To briefly recap, we have discussed where the problem of infinite intricacy crops up, what properties found in chaotic models might correspond to this intricacy, and why these properties are peculiar to chaotic models. We have seen what it is to have fractal structure and why strange attractors are fractals. Finally, we see how fractal structure is purported to drive the relevant models into physically impossible states and hence why the skeptic claims this structure should not be taken realistically.
D. Problems with the Paradigm Cases

Before attempting to unravel some of the skeptic’s legitimate concerns, the concrete examples presented thus far bear closer inspection. It’s true that state points on a strange attractor take on real, infinitely precise values, but one need not appeal to shrinking spheres and limits to make sense of this. After all, kinetic energy can, at least classically, have an infinitely precise value and change by infinitesimal amounts. Assessing the temperature-at-a-point example as a problem about precision is misleading since the average is said to be taken over discrete molecules rather than kinetic energy.

Consider a simple example from a freshman physics text. When a ball drops from a height $h = 1\text{m}$ with an initial potential energy of $mgh$, its kinetic energy $\frac{1}{2}mv^2$ smoothly increases as $h$ decreases. $h$ takes on all values in the interval $[1,0]$ meters as the ball falls. Most of these values, as we know, are irrational and so infinitely precise. Given their relation, kinetic energy will also take on irrational values at times as the potential energy decreases. If ten balls are dropped from $1\text{m}$ within a few microseconds of one another, one could take the average kinetic energy of the ten balls at some time $t$. The average kinetic energy of this ten-ball system will have an irrational value with probability 1. Hence the average kinetic energy of a system with discrete bodies not only can but most likely will be infinitely precise. Temperature, therefore, can have a real number value in a (classical) world of discrete molecules.

(Of course, some rationals require an infinite number of decimal places to be fully specified—consider repeating decimals such as $1/3=0.333\ldots$. Smith’s intuition, I believe, is that repeating rationals can, at least, be written in a finite form, either as a simple fraction or using devices like the ellipsis or repeating bar. Other than that, I don’t wish to defend this notion of infinitely precise numerical values and/or numbers that require infinite decimal places to be uniquely specified. Showing how it helps motivate the skeptic’s case is enough.)
As for the fluid velocity case, velocity-at-a-point $\bar{u}(x, y, z)$ is an important notion in fluid mechanics. Again, a common strategy is to identify this quantity with an average of the velocities of the molecules in some neighborhood of the point. In contrast to Smith's use of this example, however, the problem according to Aris is that the value for fluid velocity is supposed to change smoothly in continuum mechanics. In reality, the value must change discontinuously as molecules enter and leave the neighborhood over which the average is taken. There is no problem here about assigning real-number values to $\bar{u}(x, y, z)$, at time $t$. Nonetheless, this example is still grist for the skeptic's mill. The smooth orbits in the phase portraits entail that the state variables do change continuously, which is Aris's concern. Hence, there is still conflict here between the models and the world—the skeptic finds a foothold.

(A bit of foreshadowing: note that the smoothness of trajectories is not unique to strange attractors nor is it driven by fractal structure. The same argument could be made against a phase space with the same state variables containing a limit cycle or point attractor. This suggests that if there is some special foundational problem presented by chaos theory, we have yet to see an argument that nails it down.)

The skeptic's charge, once again, is that models containing strange attractors have a nonphysical fine structure. That this aspect of the models is not realistic has either not been appreciated by the experts or, it is suggested, has been brushed under the rug in their more popular publications. Probing more carefully, we find this terrain littered with conceptual landmines. In particular, the relation between continuity in idealized models and the real-world of discrete particles—not to mention quantum uncertainties—is subtle. Instead of reinventing the wheel, we will look a bit deeper into another area of physics in which such tensions have been wrestled with for two hundred years: continuum mechanics.
2. Lessons from Continuum Mechanics

In the last section, we saw how fractal structure seems to clash with reality at very fine scales. In this section, we will try to glean some insight from another well-known case in which macroscopic properties have refused to sit comfortably on their microscopic foundations—specifically, the conceptual tension between the real-world of discrete particles and models that presuppose a continuum. Our primary guide will be Clifford Truesdell with the unique and useful perspective of Hilary Putnam mixed in.

Field theory texts begin innocently enough by applying Coulomb’s law to simple systems of point charges. Very soon, however, the student is told that such systems are overly cumbersome. Since the engineer is principally interested in macroscopic phenomena, it is simpler to consider charged bodies as having a smooth, continuous distribution of charge or volume charge density, \( p \). The charge due to individual electrons is thereafter ignored. The total charge within some volume is captured by integrating the charge density throughout the volume \( (Q_{\text{tot}} = \int_{V} p \, dv) \) rather than adding up the number of electrons.

The move from discrete charges to continuous charge density is an idealization. The negative charge on a real iron ball is, of course, concentrated in individual electrons not distributed on a smooth sheet of charge density. What, if any, artefacts are generated by this initial simplifications?

The question is often nontrivial. Nancy Cartwright illustrates this point with the discovery of the Lamb shift.\(^{89}\) In the Weisskopf-Wigner derivation of the quantum mechanical decay law, the discrete modes of the field available to an atom in the ground state are close enough and numerous enough to be considered a continuum.

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Mathematically, a summation is replaced by an integration (cf. \( \int \), above). After another simplification is made, the derivation produces a term that at the time appeared to have no physical significance. This term, uninterpreted for seventeen years, was eventually shown by Lamb and Retherford to represent the phenomenon now known as the Lamb shift. The point is that for some time the uninterpreted term was considered an artefact of (in part) the continuum idealization. Cartwright shows that if the derivation is carried out in a slightly different order, the term is eliminated—\textit{prima facie} evidence that this "mathematical debris" is an artefact of a single procedure rather than a realistic property of the world.

In order to separate artefacts from realistic properties, we should close off an easy exit from the task: instrumentalism. The instrumentalist, or some more sophisticated anti-realist like van Fraassen, might want to dismiss the foundational questions by citing a simple truth: continuum theories work. Electrical engineers design high voltage capacitors using the idealized notion of charge density rather than point charges. Aeronautical engineers produce efficient wing designs by treating air as a fluid rather than a swarm of molecules. What more could we want?

Typically we want two things. First, we want what the scientific realist usually asks of the instrumentalist: an \textit{explanation} for why these engineering practices work. Second and more importantly, the skeptic wants to know how it is that imputing an infinite amount of non-empirical structure to the relatively lean and simple reality of particles can possibly be useful. It appears that a very high degree of complexity is used in chaotic models to describe a comparatively simple world (e.g., in the use of unbounded fractal structure). Since the modeler is usually in the business of simplifying a complicated world, how can all this added complexity help? The question cannot be evaded by the scientific realist or instrumentalist.
Furthermore, accepting such questions as legitimate puts one in good company. The conceptual tensions between theories has provided some of the most compelling lore found in the history of science. The foundations of statistical mechanics pioneered by Boltzmann and others is a good example. For a more contemporary case, physicists such as Michael Berry are currently wrestling with the problem of quantum chaos. As we will eventually see in greater detail, the question there is whether the underlying physics of quantum mechanics can accommodate classical SDIC. As an answer to the instrumentalist, I, and the skeptic for that matter, simply claim to be working in the tradition of Berry and Boltzmann: isolating and addressing conflicts between theories or between theory and model.

One more point needs to be emphasized before we begin. Continuum mechanics (CM) applies to entities in physical space, not phase space. The tensions are between the macroscopic continuum and microscopic particles, rather than the fractal structure of attractors and the physical interpretation of that structure. Therefore, whatever gold there is to be mined in CM will have to be applied by analogy to chaotic systems. The fact that both sides of the analogy fall within the broad realm of dynamics does not mean the issues are precisely the same. Inferences must be drawn carefully.

Let’s now turn to Truesdell and Putnam for some answers.

A. Underlying physics and conflict

The physical models in continuum mechanics treat the mass in a body as a continuous distribution without gaps. Discrete\(^90\) models concentrate mass in particles, often in point masses. The mathematics corresponding to these descriptions only

\(^{90}\)Although imprecise, for our purposes we may lump atomic, structural, corpuscular, and particle models in a single group as distinct from continuum and field models.
reinforces the appearance that these are very different, even conflicting, approaches to modeling. *Prima facie*, Truesdell agrees:

The corpuscular theories and the field theories are mutually contradictory as direct models of nature. The field is indefinitely divisible; the corpuscle is not.\(^{91}\)

Atomism was still in dispute in the late nineteenth century. It wasn't clear whether the nature of matter would turn out to be corpuscular or field-like. Given such uncertainty, a modeler might be in a position to simply choose which approach—particle or field based—was most useful in a given context. Such blissful ignorance is unavailable now, of course. Since (i) our best theory of matter is atomic, (ii) our theory of charge is discrete, and (iii) particle models are inconsistent with the continuum, doesn't nature force the physicist's hand?

Apparently not, given the ongoing research in electromagnetic field theory and fluid mechanics. If modeling choices were that constrained, entire engineering departments and a large number of scientific journals would have been eliminated some time ago. Let's consider a bit more of the Truesdell passage:

To mingle the terms and concepts appropriate to these two distinct representations of nature [particle and continuum], while unfortunately a common practice, leads to confusion if not error. For example, to speak of an element of volume in a gas as 'a region large enough to contain many molecules but small enough to be used as an element of integration' is not only loose but also needless and bootless.

The contradiction mentioned in the previous passage, we now see, comes about if one tries to somehow apply both modeling strategies at the same time. Trying to preserve our commitment to a molecular world *while* invoking a continuum-based mathematics is what generates the confusion. Not only is one tidy package lacking for both approaches, Truesdell seems to see little value in the search for one. (Why this is will become clearer as we go.) The example in this passage is especially interesting:

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\(^{91}\) Clifford Truesdell, *An Idiot's Fugitive Essays on Science* (New York: Springer-Verlag, 1984), 22-23
volume in a gas, a case not very different from the temperature-at-a-point example sketched earlier. If Truesdell is correct, then finding conceptual tensions in such examples is not all that difficult nor surprising. Whether these conflicts should count in favor of some sort of skepticism or anti-realism toward one model or another is a completely separate question.

The first application to chaos can be found here. Let's grant that the skeptic has discovered some tension between chaotic models, whose state variables apply to macro-level properties, and the governing micro-theory of gases. Specifically, the variables in such models take on values that the molecular theory of gases shows are physically impossible. Analogously, mass density variables in CM take on values that are physically impossible given an atomic theory of matter. Following Truesdell, our initial reaction might well be, "So what? Tensions between macro-properties and micro-physics are commonplace." One ought not be disturbed by every poor fit between theory and model. Doing so rests on the somewhat naïve view of science as a seamless and perfectly coherent web—a view that does not respect the often messy interaction between theorist and experimentalist.

Another target for Truesdell is the view that a particle or structural model is fundamentally better than a continuum one. Both approaches use idealizations to describe aspects of a complex world. But the existence of atoms (or electrons in electromagnetism) *prima facie* gives structural models the advantage, if for no other reason than that they are more realistic. Being more like the real-world is just what it is, on some accounts, for one model to be better than another. This would suggest that starting with a more realistic model (particles rather than a continuum) should yield better results.

In fact, such a broad generalization is far too heavy-handed. In practice, the experimentalist finds that the "goodness" of a model does not depend wholly on its
ontological assumptions. Often the more realistic model is simply the wrong tool for the job:

It should not be thought that the results of the continuum approach are necessarily either less or more accurate than those from a structural approach. The two approaches are different, and they have different uses.\(^{92}\)

Some of these differences will be discussed in the next subsection. For now, let's consider an important consequence of seeing both approaches as justifiable modeling strategies: structural models and continuum models are not in conflict with one another. One ought not condemn the latter for imputing spatial continuity on a material that is "gappy" at some microscopic level. Doing so, in essence, faults the continuum model for not taking a structural approach. Furthermore, Truesdell explicitly denies the need to derive a macroscopic continuum model from more fundamental physics.\(^{93}\) On the contrary, the continuum approach is fully legitimate in its own right.

In sum, Truesdell shows a profound disinterest in proposed clashes between modeling strategies. The modeler is portrayed as a pragmatic investigator. Success in the lab provides the only defense needed in order to use one model rather than another.

So, one might ask, why isn't this simple instrumentalism? It might be. Truesdell does not always employ the same subtlety of thought in describing the work of engineers and technicians as he does to rational mechanics. Nonetheless, even if Truesdell sounds unduly pragmatic here, the underlying motivation is correct. That is, I believe the thrust of the argument is designed to carve out a legitimate niche for bottom-up modeling. Truesdell is fighting the notion that all parts of a model must be justified.

\(^{92}\)Truesdell, *An Idiot's Fugitive Essays on Science*, 57.

in a top-down manner—from one, accepted governing theory working down to a specific application. In fact, continuum models start with materials—stone, steel, glass, rubber—rather than types of atoms from the periodic table. (“Start” in the sense that the models require so-call constitutive equations that are based on macroscopic, real-world materials.\textsuperscript{94})

The lesson derived from continuum theories is that even if a \textit{prima facie} conflict between models holds up under closer scrutiny, it should not by itself be cause for concern. The modeler ought not be overly constrained by micro-physical theory. As Truesdell sums up, “It is as ridiculous to deride continuum physics because it is not obtained from nuclear physics as it would be to reproach it with lack of foundation in the Bible.” We might grant that (i) continuum and field models presuppose structure where there is none, (ii) molecular models are more realistic than their continuum cousins, and (iii) the two modeling approaches cannot be harmonized (in fact, that’s too strong, but we can let it go for now). What follows from this? Are continuum models suspect since they fail to capture the existence of atoms? The answer that sums up this subsection is “No, continuum models are rendered neither false nor dubious by the existence of atoms.” There is no conflict if the continuum approach is taken as a legitimate modeling strategy independent of theoretical particle physics.

Turning back to chaos, attacks on chaotic models are driven by the view that real-world systems cannot support the kind of complexity displayed by unbounded fractal structure. The models conflict with reality by imputing fine structure where there is none, or so the argument goes. I urge that the same attitude adopted in the

\textsuperscript{94}Ibid., 2-3. The field equations themselves are typically derived in a top-down fashion, according to Truesdell. But the constitutive equations for materials are grounded in actual substances. That is, one starts with steel or rubber and then imposes specific idealizations which, as usual, have mathematical counterparts.
previous paragraph is reasonable vis-à-vis chaotic models. Perhaps fractal structure cannot be neatly harmonized with the underlying physics. This is not a sufficient reason for removing it from the modeler's toolbox. Attractors, repellors, and all the other creatures in the phase space zoo are used to describe the evolution of real-world systems at some scale. The models simply have nothing to say about the microscopic behavior of these systems. Their use can be defended from the bottom-up, regardless of whether any top-down, theoretical justification is forthcoming.

B. Surplus structure added vs. detail ignored

A related set of claims about modeling describes the limited ontological commitment required for success.

Widespread is the misconception that those who formulate continuum theories believe matter 'really is' continuous, denying the existence of molecules. That is not so. Continuum physics presumes nothing regarding the structure of matter. It confines itself to relations among gross phenomena, neglecting the structure of the material on a smaller scales. It seems odd to think that, even thirty years ago, such a misconception about the use of continuum theories could really be widespread, hence the enery at Truesdell's gate isn't clear.

What is clear is the final point: continuum models neglect the small scale structure of a material. The engineer using CM is not concerned with the microscopic makeup of the material or item (e.g., a steel beam, a rubber ball, or water in a pipe) that is the subject of his model. Although he believes that the air flowing over a wing is composed mostly of nitrogen molecules, the mathematics governing air flow ignores this fine structure. For the application at hand (a qualification often invoked by engineers), the microstructure simply doesn't matter. At bottom there might just as well be

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95Truesdell, An Idiot's Fugitive Essays on Science, 54.
Newtonian corpuscles, Boscovichian point masses, or Leibnizian monads, so long as they can behave in large numbers like a fluid.

This benign neglect is not at all like the picture painted by the skeptic. Chaotic models, we are told, impose surplus structure on the discrete world. An infinite amount of fine structure is added to a relatively simple reality. And intuitively it is difficult to see how adding complexity can facilitate the modeler’s task (especially when the added structure is fractal). Idealizations are supposed to simplify complex phenomena. For example, the frictionless planes used in introductory dynamics allows one to drop a force vector from the physical model and a term from the equations of motion. Why would we complicate things by adding more structure than physical reality can bear?

First we should say a bit more about the notion of “structure” used here. The term has a geometric or pictorial connotation that dynamicists seem to like, especially chaos researchers who are comfortable with state space properties. There is no precise definition available, I believe, because the term is used to gesture toward a large number of loosely connected technical terms. If dynamic model $A$ is said to have more structure than model $B$, several specific propositions might be true. $A$ might have more degrees of freedom. Perhaps $A$ evolves by taking time in infinitesimal steps rather than discrete chunks. $A$ might have a more complex state space (e.g., a complex, infinite-dimensional Hilbert space rather than a real, finite phase space) or, if both models can be captured by a phase portrait, the evolution of $A$ might simply look more complicated than $B$ (e.g., a strange attractor compared to a limit cycle). Like the term “complexity” in (so-called) Complexity Theory, there are no necessary nor sufficient conditions for structure.

The puzzle remains then, why would the modeler add structure, regardless of what this means in a specific application? Although the skeptic’s intuition about adding structure seems quite reasonable, we should not be chased so quickly from the Truesdellian picture. In the CM realm, it seems as if, instead of abstracting away

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properties like friction, the models appear to add structure that isn’t there: gaps between atoms are smoothed over; particle models with a large but finite number of degrees of freedom are replaced by CM models with an infinite number. But instead of explaining how adding properties can be useful, the very notion of “adding to” needs to be corrected. The continuum idealization does not add structure to a sparse reality. One or more features of the world are ignored or, as Truesdell says, “neglected” in continuum models, in particular an object’s composition. At a macroscopic level of description, the material behaves like a continuous body. Capturing what the material is like at some deeper level is an irrelevant complication. As far as doing work for the modeler, the continuum idealization is essentially like the better known examples of frictionless planes, point charges, and rigid rods, all of which neglect some feature of reality.

Putnam makes roughly the same point but with a much simpler example.\(^{96}\) Consider a board with two holes, one round and one square. Given the cubic peg in my hand, a very simple request for an explanation comes to mind: why does the peg go through one hole but not the other? One could, it seems, answer this request with a long-winded story about molecular chains, the strength of atomic forces in close quarters, etc. Such a discourse, Putnam rightly claims, might well be true, but it wouldn’t be a good explanation. A much better answer could be given in geometric terms with a naïve concept of rigidity mixed in. One lesson drawn from this example is that the constituents of the board don’t matter when it comes to giving a good explanation of peg-and-board interaction. Taking the board as a rigid body, rather than

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a collection of molecules, ignores the irrelevant substructure and brings out the relevant properties.

Putnam's board is a simple example of Truesdell's "gross phenomena." In both cases the point is that the underlying structure can often be ignored when constructing explanations and mathematical models. If microscopic configurations were relevant in Putnam's example, one would need a new explanation for each peg-and-board of a different material: one for hard plastic, one for wood, one steel, etc. In fact, given the variety of trees and the structural differences grain makes in a board, a single explanation for wood would not suffice. But since these structural differences don't matter for the problem at hand, a single geometrical story can be told.

Moving back to chaotic models, is the infinite intricacy of chaotic models something added to real-world phenomena? Following CM and Putnam's board, this should not be our first choice. Smith's temperature-at-a-point example is supposed to show that there is more structure in chaotic models than can be cashed out in the world. Instead of taking this structure as an addition, however, we should first try to see it as the result of ignoring discontinuities. At the macroscopic level, temperature is field-like, smoothly changing values over time. The microscopic discontinuities that Smith builds his case around are irrelevant detail from Putnam's perspective.

Most of this section has dealt with physical models and their idealizations. Shifting our attention to their mathematical counterparts will provide another perspective. Part of the skeptic's case may stem from a tacit failure to appreciate the simplification gained by moving from the very large to the infinite. As Truesdell says, we have no choice:

However discrete may be nature itself, the mathematics of a very numerous discrete system remains even today beyond anyone's capacity. To analyse the large, we replace it by the infinite, because the properties of the infinite are simpler and easier to manage. The mathematics of large systems is the infinitesimal calculus, the analysis of functions which are defined on infinite sets and whose values range over infinite sets. We need to
differentiate and integrate functions. Otherwise we are hamstrung if we wish to deal effectively, precisely, with more than a few dozen objects able to interact with each other. Thus, somehow, we must introduce the continuum.\textsuperscript{97}

Truesdell has in mind principally the move from discrete particles governed by ODE's to a field governed by PDE's (partial differential equations). The first approach collapses under the computational requirements needed to keep track of a large number of particles. A more obvious difficulty, perhaps, is the need to measure the initial conditions of those particles to feed into the solutions of the ODE's—a manageable problem in celestial mechanics but not in electric field theory (the number of electrons in the smallest capacitor is quite large). The move to PDE's escapes the problem by transcending individual particles.

Since models in chaos theory are (typically) governed by ODE's rather than PDE's, some bridges need to be built in order to complete the analogy. Let's grant for the moment that a continuum \textit{is} imposed in fluid mechanics (\textit{contra} Truesdell) by smearing matter throughout the volume of a body instead of matter being isolated in point masses or tiny rigid bodies. In chaotic models, the state trajectories that constitute a phase portrait are taken to be continuous when, in reality, the represented properties change discontinuously. Continuity is imposed on the state variables in order, once again, "to differentiate and integrate."

The payoff in both CM and chaos is the same: computational convenience. In order to gain mathematical tractability in a complex world, idealizations are required. Continuity can be used as one such idealization. So, the answer to "how can a model that imposes structure on reality be useful in understanding an already complex world?" is this. In both CM and chaos, the idealizations (a continuum and smooth change of state, respectively) are not only useful but often required in order to use differential (and

difference) equations. Even if we grant that structure is imposed rather than ignored in mechanics (contra Truesdell), this move could still be justified on computational grounds.

So then, on one hand I have argued against the view that the move from discrete reality to continuum/continuous models imposes structure. Instead, this transition should be viewed as simplifying by ignoring fine structure. On the other hand, even if one were to grant the imposing rather than ignoring line, the associated mathematical models show how it is that such an imposition could possibly benefit the modeler: it allows one to bring the power of differential equations to bear.

C. Straw man?

There is a curt reply to the last three subsections: straw man. The objection might go something like this: “No one is attacking the continuity idealizations in either CM or chaos. Agreed, smoothing over irrelevant discontinuities yields mathematical simplicity, but that is not the target. Truesdell is no enemy; CM is a red herring. The problem lies not in the continuum but in the use of boundless fractal structure. That is where the added complexity is to be found. How can it be that the lean and simple world of atoms (restricting ourselves to the classical realm) can best be modeled by adding the infinite intricacy of fractal structure all-the-way-down?”

To answer the straw man charge we must reemphasize a key distinction from the previous chapter: idealizations vs. artefacts. The former is a simplifying assumption made prior to the construction or application of a physical model. For instance, when an automotive engineer decides to ignore the friction in a new bearing design, he does so because, for the application at hand, friction is negligible. The engineer has idealized away the damping force and so the associated mathematics is simplified—likewise in considering the planets to be point masses, transmission lines to be
noiseless, and all the rest. Sometimes these simplifying assumptions are made for the specific purpose of allowing certain types of mathematics to be used à la Truesdell and CM. In short, idealizations are (strictly speaking false) assumptions made in order to apply certain kinds of mathematical machinery, principally differential equations, and to simplify the equations used.

Artefacts, in contrast, are the false properties or relations that can be consequences of idealizations. An artefact is not an abstraction built into the model; it is a (possible) result of simplifying assumptions. Artefacts are often benign. No one is confused when the Census Bureau refers to the 2.14 children in the average American home. At times, however, it is difficult to separate the artefacts from genuine physical properties, or in Hesse's terms, to figure out whether the neutral analogies are in fact positive or negative. This is especially so in the mathematical sciences where discovering the physical significance of certain terms is often a nontrivial task. Cartwright's discussion of the Lamb Shift gives one example.

Now let's address the charge. If fractal structure were uniformly treated by the skeptic as an artefactual consequence of some initial idealization(s), then the straw man accusation would be justified. But it has not been so treated. Conflating the idealization/artefact distinction is a surprisingly easy trap to fall into when it comes to the geometry of phase space and its constituents. The problem, which has been sitting in the background since the beginning of this chapter, hinges on the two possible interpretations for "infinite intricacy." The reading adopted for the most part in this chapter is intricacy = fine structure at all scales of a fractal set. An alternative reading takes intricacy to either be identical to or a consequence of the \( \mathbb{R}^a \) geometry of phase space.

Say the skeptic starts by arguing that "chaotic models," contra Truesdell, impose an infinite amount of surplus structure on a discrete world. The models impose
structure by forcing various properties to take on some (real) values that they cannot physically have. This idealization shows up in smoothly changing state variables and in the manifold of phase space. In short, chaotic models have infinite intricacy.

From here two subtle shifts in meaning can occur. One is that 'infinite intricacy' is a pictorial locution (like 'infinite complexity,' 'fine structure,' etc.) for fractals. Hence the term is susceptible to an equivocation: intricacy as the continuity idealization used to construct an $\mathbb{R}^n$ phase space vs. intricacy as fractal complexity—an artefact. I believe the skeptic's case appears stronger than it is because he is able to shift from one meaning to another when it is convenient.

Obfuscating this shift is the fact that 'chaotic model' is also ambiguous in a number of ways. Most importantly here, it can be used to describe the phase spaces that contain chaotic trajectories or it can refer to an attractor itself. After all, it is the strange attractor that is often said to model—a verb form that only adds to the confusion—the dynamics of the system, not the phase space as a whole.

The result of the equivocation is this. Examples are used to show that chaotic phase portraits require an idealization that nature cannot honor. Up to that point, infinite intricacy is associated with continuity across state variables. Once the discussion turns to fractal structure, however, intricacy naturally comes to be used to describe Cantor set-like complexity. Having already shown that infinite intricacy is strictly speaking false, the skeptic acts as if fractal structure has been shown to be false. But it has not. Pointing out that the continuity idealization is just that—an idealization—does not by itself entail that fractal structure is nonphysical.

In other words, a very simple version of the argument is:

1) Chaotic models have infinite intricacy.
2) Nature is not infinitely intricate.
3) The infinite intricacy of chaotic models is an artefact.
The argument is, of course, invalid if infinite intricacy refers to the state space manifold in (1) and (2), but refers to fractal structure in (3).

I believe the argument the skeptic wants to make is this. In order to construct an $\mathbb{R}^n$ phase space and to use ODE’s, the state variables must be capable of changing continuously, at least over some range of values. This requires structure to be imposed on nature by way of a continuity idealization: the properties captured by the state variables are taken to change smoothly over short intervals even if in nature the change is discontinuous. But having added structure to the models that is not found in real-world systems, there is a distinct possibility of further negative analogies as a consequence. That is, if pushed too far, these idealized models may yield artefacts that a more realistic model would not allow. The fine structure of fractal attractors is just such an artefact.

As it stands, however, we do not yet have an argument for this final claim. The bridge from continuity idealization to fractal artefact is merely alluded to; it has not been built. The skeptic is clearly nervous that fractal attractors might be an unrecognized artefact. Unfortunately, the possible equivocations involved tend to obscure the gap in the argument. In the final section of this chapter, we will see how difficult the task of closing this gap appears to be.

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98 Smith uses symbolic dynamics to stoke the skeptic’s suspicions. The idea is that analogues to chaotic evolutions can be developed by simple shift operations on real numbers (see chapter 3 of Coping with Chaos, Edward Ott, Tim Sauer, and James A. Yorke, eds. (New York: John Wiley & Sons, 1984). Since chaotic behavior seems to follow so naturally from the reals, the suspicion is that the $\mathbb{R}^n$ structure of phase space is responsible for chaos in dynamic models. For a critique of this view, see John A. Winnie, “Computable Chaos,” Philosophy of Science 59 (1992): 263-275.
D. Legitimate conflicts

I have invoked Truesdell and Putnam to help show that we need not yet embrace the skeptic's worries as our own. How then *does* a scientific skeptic earn the right to make his colleagues nervous about a conflict between macro-models and micro-theory?

The answer, according Putnam, is not whether there is a *prima facie* tension, but whether the underlying micro-physics can approximate the higher level phenomenon.

[A] discontinuous structure, a discrete structure, can approximate a continuous structure. The discontinuities may be irrelevant, just as in the case of the peg and the board. The fact that the peg and the board are not continuous solids is irrelevant. One can say that the peg and the board only approximate perfectly rigid continuous solids. But if the error in the approximation is irrelevant to the level of description, so what?

The question for evaluating chaotic models, following Putnam, is 'Can the underlying physics yield a fractal at the level of description that the modeler is interested?' As we saw in the last subsection, the fractal structure of a strange attractor depends on a logically prior continuity idealization. With this in mind, the answer to the question is "yes," the underlying physics can support a fractal in the phase space if this initial idealization is justified. That is, once the modeler determines whether he can properly smooth micro-discontinuities into continuous state variables, there is no further question about whether fractal structure is physically possible or not. If the trajectories in the phase portrait are pulled toward a fractal limit set, the model cannot be criticized on the grounds that individual trajectories take on nonphysical states. Trajectories do not determine physical possibility, they may only take advantage of what, in a sense, has already been declared possible by being in the phase space.\(^{100}\) If a set of states in the

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\(^{99}\)Putnam, 301.

\(^{100}\)In many cases the full phase space is "larger" than what is physically possible. E.g., a state variable for the temperature of a system might, in some spaces, take on values beyond the melting point of the objects in the system. The objects being modeled cannot exist at those temperatures. In these cases, what is physically possible is a subset of the phase space.
phase space turns out to be nonphysical, then the idealizations employed to construct the phase space are at fault, not the entities that develop in the phase portrait.

Note that I haven't argued that fractal structure cannot be an artefact. I have shown that attacking it via the physical impossibility of the state points that make up a strange attractor is a *non sequitur*.

As for the continuity idealization itself, whether there is underlying discreteness that has to be smoothed over has to be decided on a case-by-case basis. Shaw's crude model of a dripping faucet uses velocity and position to construct a phase space with a Rössler band.\(^{101}\) Both of these properties (classically) undergo smooth change through time, at least in a piecewise fashion. (Quantum discontinuities have yet to be considered.) Another well-known example of chaos is the behavior of Hyperion, one of Saturn's moons.\(^{102}\) One of the state variables in this mathematical model represents the attitude of the principle axis. Just as a pendulum continuously sweeps through its arc, the angle of the principle axis changes smoothly as the moon tumbles.

The point is, once again, that not all chaotic models require a continuity idealization to get off the ground. In those cases at least, there seems to be no justification for the artefact charge other than that the models fail to match observations, if they do in fact fail.

Let's briefly review what has been done in this section. First, if fractal models are in conflict with the underlying physics of the systems being modeled, this is not cause for great concern. Truesdell shows how easy it is to fabricate such tensions by

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\(^{101}\)Robert Shaw, *The Dripping Faucet as a Model Chaotic System* (Santa Cruz: Aerial Press, 1984), 17.

pushing the relevant models beyond the realm to which they properly apply. Second, we saw that macro-level idealizations should be understood as ignoring underlying structure rather than imposing more of it. Third, smoothing over discontinuities often yields a tremendous gain in mathematical simplicity by allowing the use of differential equations. Fourth, the important difference between the continuity idealizations used to construct a phase space and artefactual fractal structure was reiterated. Fifth and finally, we discussed what the skeptic has yet to show: that the microstructure of the subject of a chaotic model cannot support the continuity idealization and a fractal geometry. If there is a reason why the underlying physics prevents a family of trajectories from forming an attractor with fractal structure in phase space, that would be interesting and important.

3. Fine Structure without Chaos

The foundational problems discussed in the previous section were diffused by the use of analogies in CM. Here we will take the problems head on. In terms of organization, those points most closely associated with the continuity idealization are addressed first. The chapter concludes with the skeptic's supposed trump card: the limitations on fine structure imposed by quantum mechanics.

A. Continuity and ODE models

If the continuity idealization is what the skeptic is calling into question by the claim that infinite intricacy is non-physical, there is an obvious problem: chaotic models are not at all unique in this regard. The idealization, if one is required, must be in place in order to construct a phase space. The dynamics found within the phase portrait might display fractal structure, but it might just as well evolve into a limit cycle,
point attractors/repellors, or any other phase space entity. If the mathematical model
used lacks a dissipative term, the associated phase space cannot yield an attractor at all.

Since many phase spaces require this kind of fine structure to get off the ground, it is somewhat improper to call our antagonist a chaos skeptic. His target seems to be the construction of phase space itself. But phase space manifolds are simply representations of the solutions of coupled ODE's—a relatively tame area so far as mathematical physics is concerned. Unless there is something special about chaotic models, the skeptic's concerns no longer seem altogether interesting. "Infinite intricacy" must be interpreted as an artefactual fractal structure, rather than the smoothing of state variables, to make the claims worth pursuing.

B. Special exploitation?

As we have seen, there is an important gap between the idealizations used in chaotic models and the artefacts for which they are supposedly responsible. One way to bridge the gap would be to give a "special exploitation" argument: strange attractors uniquely exploit the continuity idealization whereas more pedestrian evolutions do not.

The reason dissipative chaos is suspect, we're told, is because of its fine structure all-the-way-down (i.e., on all scales in phase space) described by fractal geometry. (Nondissipative chaos displays a kind of fine structure too, but it isn't fractal.) A more technical way of describing certain kinds of fine structure is to say that a subset of phase space displays topological transitivity.103 An attracting set of phase space points is topologically transitive if a single trajectory visits every region in the limit set, no matter how small the region. This provides a precise way in which to describe how a dissipative chaotic evolution exploits the \( \mathbb{R}^d \) structure of phase space:

strange attractors are topologically transitive. A single trajectory on a strange attractor explores every region of the attracting set as \( \rightarrow \infty \). (More precisely, a map \( f : \mathbb{R}^n \rightarrow \mathbb{R}^n \) is topologically transitive if for any pair of open sets \( A, B \subseteq \mathbb{R}^n \) there is a \( k \) such that \( f^k(A) \cap B \neq \emptyset \).\(^{104}\) \( k \) represents forward iterations of the map.)

The introduction of another technical term might be confusing this late in the chapter. Why do we need topological transitivity when we have the notion of fractal structure all-the-way-down? For one, researchers typically use fractal geometry to characterize the dimension of an attractor and little else. Questions about fractal structure and its limits receive no attention in the nonlinear dynamics literature. Another reason for introducing this term is that the connection between trajectories and minute regions of phase space is concise and explicit. Contrast this with the short essay needed to relate fractal structure and strange attractors in section 2.

So then, chaotic evolutions "exploit" phase space by way of topological transitivity. The skeptic has tried to make the case that fine structured properties such as these are artefactual. However, non-chaotic evolutions with the same or similar properties seem to have been overlooked.

\(^{104}\)Robert L. Devaney, *An Introduction to Chaotic Dynamical Systems*, 2d ed. (Redwood City: Addison-Wesley, 1989), 49.
For example, the attractor in Figure 22 might be found in the phase space of a damped, driven, two-dimensional oscillator. The limit set forms a 2-torus. Trajectories on the attractor wrap around the two axes of the doughnut-shaped surface with characteristic frequencies $\omega_1, \omega_2$. If $\omega_1$ and $\omega_2$ are rationally incommensurable, no single trajectory on the torus ever meets itself. The evolution is said to be quasiperiodic. As $\rightarrow \infty$, each trajectory densely covers the torus. This entails that for any region on the torus, a quasiperiodic orbit will eventually visit that region no matter how small.

To see why this is so, let's cut through and unwrap the torus (Figure 22) into the rectangle in Figure 23. Let point 1 be the initial state point for a trajectory on the torus. Since the torus has been unwrapped, point 2 on both the left and right hand sides are actually the same point—likewise for 3. As the trajectory winds around the attractor, more of the rectangle will be explored. Since the ratio of characteristic frequencies $\frac{\omega_1}{\omega_2}$ is irrational, the trajectory never meets (nor crosses itself) and forms a

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dense cover over the surface. The trajectory will pass through every subsection of the rectangle regardless of scale as $\rightarrow \infty$.

![Unwrapping the 2-torus](image)

Figure 23: Unwrapping the 2-torus

In several important ways, the fine structure of our quasiperiodic attractor is on a par with a typical strange attractor. In fact, the 2-torus with incommensurable frequencies is topologically transitive.\(^{106}\) (Actually, this nonchaotic attractor meets three of the four (operational) criteria for being strange—only SDIC is lacking.\(^{107}\) It seems,

\[^{106}\text{Devaney, 42.}\]

\[^{107}\text{David Ruelle, Chaotic Evolution and Strange Attractors (Cambridge: Cambridge University Press, 1989), 24. The four characteristics are topological transitivity, SDIC, attraction of nearby trajectories, and time invariance.}\]
therefore, that any argument directed against the physical impossibility of artefactual chaotic fine structure must apply equally to quasiperiodic fine structure. No one, however, including our chaos skeptic, believes quasiperiodicity is suspicious.

In slightly different terms, the entire argument runs something like this. The skeptic wants to show that strange attractors cannot realistically govern the behavior of real-world systems. One way to do this is to attack its fractal properties as unrecognized artefacts: nature cannot honor fine structure all-the-way-down. In particular, a phase space fractal requires that no matter how small a neighborhood one considers on the attractor, there is a trajectory cutting through it. Unfortunately for the skeptic, this last statement holds for quasiperiodic evolutions as well.

A proper understanding of topological transitivity suggests that fractal structure has drawn undue attention. The Cantor set-like properties of a strange attractor cannot exploit the smallest regions of phase space to any greater degree than quasiperiodicity. Both have structure at all scales. Given the relative tameness of quasiperiodic motion, it is unlikely that the skeptic would want to attack it as well. Lacking a more focused argument to the contrary, chaotic attractors seem no more artefactual than the unproblematic 2-torus.

C. Quantum uncertainty

The final trump card yet to be played. Our chaos skeptic might grant in the end that nature has more fine structure than first thought. In some phase spaces, the state variables do not seem to require a continuity idealization to smooth over gaps. Those cases support a robust Euclidean space that the trajectories in and around a strange attractor might explore. Hence, the “underlying physics” of our dynamical models seems to allow for classical chaos after all, except for an undeniable fact: the underlying physics is not classical.
There is an important and interesting clash between chaos theory and quantum mechanics (QM) known simply as the problem of quantum chaos. Instead of merely being unable to predict the onset of classical chaos from within QM, the latter seems to disallow the former. There are a number of ways to motivate the problem, but one way in particular plays into the skeptic's argument. Small regions in phase space are subject to the uncertainty principle. Applied to a state space, the uncertainty principle in effect blurs all structure on too fine a scale. Fractal complexity all-the-way-down is therefore prohibited; when the scale becomes too small, the uncertainty relation smooths it away. Writers in diverse fields (mathematics, physics, and philosophy) have recognized the conflict:

Classical dynamical chaos is understood . . . in terms of the emergence of complexity at all levels of description in the classical phase space; that is, for chaotic systems, regions in phase space no matter how 'small' will contain microstates leading to completely distinct behavior—diverging trajectories. In QM, the uncertainty relation $\Delta Q \Delta P \approx \hbar$ 'smoothes over' regions in [phase]-space. Because quantum systems cannot display classical trajectories on a finer scale than that of Planck's constant, as soon as the chaotic trajectory evolves to level of complexity that has implications at that scale, quantum interference (i.e., wave) effects appear and the trajectory stabilizes . . .

Classical chaos involves fractal attractors, that is, structure on all scales. But in quantum mechanics . . . structure does not exist on a scale smaller than Planck's constant. So quantum effects smooth out the fine detail so necessary of true chaos.

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108 The smoothing effect is most readily seen in a Hamiltonian phase space in which an "area" has the same dimensions as $\hbar$.


Michael Berry refers to this smoothing effect on the scale of $\hbar$ as part of "the quantum suppression of classical chaos."\textsuperscript{112}

\textit{Prima facie}, this suppression is devastating to any realistic interpretation of chaos. The infinite complexity of a fractal attractor is physically impossible according to the current theory of fundamental physics. Most importantly, the smoothing appears in the state space itself. In many of the examples examined earlier, there was a tendency to carry over the limitations of particles in physical space (e.g., self-similarity in ferns and mountainscapes always runs out) to phase space. There is no such confusion here. Strange attractors are clearly among the entities that are eaten away by $\hbar$-smoothing.

But like so much else in mathematical physics, the devil is in the details. We can begin to see why the picture that has been painted thus far is incomplete by asking another question: if the smoothing away of fine structure is the heart of the quantum suppression of chaos, why doesn't QM suppress quasiperiodic motion? Why is there no "problem" of quantum quasiperiodicity alongside quantum chaos? Recall from the previous subsection that a single trajectory on the surface of the 2-torus (Figure 23) produces a dense blanket as $\to \infty$. No matter how small an area one picks on the torus, the trajectory will eventually visit that area multiple times. Classically, there is a fact of the matter as to how close two passes come to one another through some neighborhood (depending on the metric defined on the space), including those neighborhoods on a scale smaller than $\hbar$. Quantum uncertainty seems to eliminate that fact and true quasiperiodicity with it. (Actually once you see the problem, the same question can be raised for any classical topologically transitive evolution.)

Since QM does, in fact, allow for quasiperiodic motion, it should be clear that some important subtleties have been overlooked. Taking the passages above out-of-context creates a naïve and seriously misleading view of ℏ-smoothing, making it sound as if it were perfectly coherent to talk about a classical phase space subject to quantum uncertainty. Looking at the phase portrait in Figure 24, for example, one might think that uncertainty does not affect the integrity of the trajectory on relatively large scales. Only if one zooms in too close will ℏ-smoothing blur the fine structure. This description is simply false.

Figure 24: Nearby Trajectories with Uncertainty

A step in the right direction is to realize that quantum uncertainty does not merely wipe out nearby trajectories passing through ℏ-scale areas. Once uncertainty is invoked, the consequences are far more severe. In fact, the very notion of a phase space trajectory is destroyed. Why? One simply has to recall what a single trajectory in a phase space is: a smooth curve of state points. Consider the ℏ-scale area in Figure 25, a magnified piece of a single periodic trajectory. When the uncertainty relation is taken into account, all of the state points within a sufficiently small area are subject to quantum smoothing. (More precisely, one can treat the phase space Γ as having been
coarse-grained in boxes of "size" $\mathcal{N}$, $N = \text{dim}(\Gamma)$. The single, continuous curve gets washed out since there are no well-defined, individual state points to be traversed in the magnified section.

![Figure 25: Single Trajectory with Uncertainty](image)

A similar explanation of the problem can be given in terms of the manifold of phase space rather than orbits. Consider a space in which position and momentum (of one or more particles) are the state variables. Uncertainty disallows exact values of these quantities to be had at the same time and so they cannot be represented by points in an $\mathbb{R}^n$ state space. In short, uncertainty forces the switch to a nonclassical (non-pointlike) notion of system state, viz. vectors in a Hilbert space.

All of this is just to say that classical mechanics is, strictly speaking, false. The skeptic cannot selectively point to fractal complexity in strange attractors as the sole victim of uncertainty since, as we have seen, $\hbar$-smoothing destroys all classical structures.

What then are we to make of the passages cited at the beginning of this subsection? The answer is that once uncertainty is allowed on the scene, the subject has shifted away from classical mechanics. Technically, we have entered the realm of
semiclassical mechanics (SCM). Fine structure that is unique to chaos is an important topic in SCM. When researchers gesture toward $\hbar$-smoothing to motivate the problem of quantum chaos, the phenomenon is a semiclassical one.

Let's briefly consider why this is so. (A detailed treatment of quantum chaos and SCM can be found in recent papers by Batterman.) The first step is to associate a wavefunction with a classical Lagrangian manifold. (A Lagrangian manifold is a more general version of an energy surface. Both are found in conservative systems and are distant cousins to an attractor in dissipative systems. Orbits on a Lagrangian manifold cannot leave, however, nor are nearby trajectories are not drawn in. There are no attractors in a conservative (Hamiltonian) phase space.) This is easily done for classical quasiperiodic motions or periodic orbits, e.g. Figure 26, the 2-dimensional phase space of a simple harmonic oscillator with a 1-torus Lagrangian manifold. The details of the method need not concern us, only that the association begins to break down in regions with an area $\approx \hbar$, e.g. the shaded area $R$. (That is, the associated wavefunction blows up around one set of classical turning points. Whether the problematic points are at $p=0$ or $q=0$ depends on the specific construction.)

![Figure 26: Lagrangian Manifold](image)

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Fortunately there is a technique due to Russian mathematician V.P. Maslov for dealing with this breakdown. Maslov’s method works so long as the problematic $\hbar$-areas are few in number. When they proliferate, as is the case in chaotic evolutions, the method is overwhelmed. The constructed wavefunction cannot tolerate too much fine structure—in this case regions of the Lagrangian manifold smaller than $\hbar$. So long as the motion is periodic, multiply periodic, or quasiperiodic, the number of $\hbar$-regions remains constant and relatively small. When the motion is chaotic, the uncertainty principle smoothes over the classical fine structure causing the association between the Lagrangian manifold and semiclassical wavefunction to breakdown, even using Maslov’s technique.

We have briefly examined SCM in order to understand the nature of quantum chaos and $\hbar$-smoothing. With some of the technical background filled in, it is clear that care must be taken when playing the trump card of quantum chaos. Once $\hbar$-smoothing is recognized as a semiclassical development, classical chaos cannot be directly attacked using QM. The clearest indicator of this fact is that the uncertainty relation does not create nor motivate a “problem” of quantum quasiperiodicity. Loosely put, if the uncertainty principle simply makes phase portraits fuzzy when we zoom in too close—if that were the problem of quantum chaos—then uncertainty should prevent the dense orbits on a 2-torus. It does not principally because a torus is precisely the kind of structure on which Maslov’s method can be used to construct semiclassical wavefunctions.

When a modeler starts with ODE’s and a phase space (i.e., a fully classical model), he has already judged quantum effects to be negligible. (Technically, he takes $\hbar = 0$ rather than the semiclassical $\hbar \to 0$.) The skeptic is misled into thinking that uncertainty can be mixed into a classical phase space in order to show that fractal complexity is physically impossible. But the uncertainty relation does not even permit a
classical notion of state to be used, as we have seen. Invoking uncertainty destroys all classical state space structures, not just the chaotic ones.

In the end, the quantum trump card trumps too much to be useful. The problem is supposed to be chaos, not the whole of classical dynamics. The uncertainty principle is too blunt an instrument for the job.

Throughout section 4, we have seen a recurring theme. It is very difficult to attack the structures of chaos without taking on a much larger and unquestionably realistic chunk of classical mechanics. Some skeptical questions directed against fractal structure apply just as well to quasiperiodic motion, but no one thinks the latter is problematic. On atomic scales, the uncertainty principle does count against fractal complexity, but only because it counts against all classical phase space entities, not just strange attractors. If the point is simply that classical mechanics is strictly speaking false, then the point isn’t interesting. We knew that already.

4. Conclusion

The most general conclusion to be made at this stage is that the artefact problem in chaos theory is not much of a problem from a theoretical point of view (the experimentalist has yet to weigh in on the matter). The skeptic’s concerns admittedly pack an intuitive punch; nonetheless, the following points have been established.

First, unlike coastlines and snowflakes, it is physically possible for strange attractors to have fractal structure all-the-way-down (i.e., at all scales). An $\mathbb{R}^n$ phase space has more than enough room for Cantor set-like cross sections.

Second, suggestive terms like ‘infinite intricacy’ and ‘fine structure’ need to be made precise or replaced. There are a host of terms used in the chaos literature to describe the geometry of phase space, fractal dimension and self-similarity, etc. Terms
that can be used to gesture toward more than one of these, as we have seen, lead to the fallacy of equivocation.

Third, the skeptic cannot merely call attention to a possible clash between macro-level chaotic models and the underlying micro-physics. Such tensions are commonplace where idealizations are used—which is virtually everywhere in the mathematical sciences. What is needed is something stronger, specifically that the underlying physics disallows the properties in the model. One attempt to meet this demand is found in the problem of quantum chaos. Quantum uncertainty prevents fractal attractors. Unfortunately once the uncertainty principle is invoked, the entire manifold of phase space is destroyed along with the classical notion of system state. If the argument degenerates into a wholesale denial of classical mechanics, it is neither new nor particularly interesting.

Fourth, since quasiperiodic evolutions are topologically transitive, it is extremely difficult to make a case against the fractal complexity of a strange attractor without attacking quasiperiodicity as well. The latter, however, is unquestionably realistic.

A common thread in this chapter is that a tightly focused attack on chaos and strange attractors is nearly impossible to carry out. One ends up either questioning unproblematic cases such as quasiperiodicity or destroying the basic structures of phase space altogether. The theoretical challenges directed against chaotic models have been met. Their fate is ultimately in the hands of the experimentalist.
CHAPTER 4

CONFIRMING CHAOS

Overcoming the foundational issues in the last chapter warrants a rather weak conclusion: chaotic models might govern real-world systems. The more important question is, do they? The previous questions tend to be “philosopher’s problems”—questions that do not bother most working mathematicians and scientists in the field. Turning now to the experimental confirmation of chaos, we move much closer to mainstream concerns. As we shall see, it is unusually difficult for chaotic models to make the transition from mere physical possibility to descriptive science.

There are three broad challenges to chaotic models that are epistemic rather than foundational. The first has to do with how researchers in nonlinear dynamics have gone about their business in promoting their field. Given the often fierce competition for grant money as well as publishing pressures, it is clearly in the researcher’s interest to discover chaos in as many new and interesting real-world systems as possible. What is sometimes touted as a “discovery,” however, is very different from finding a new comet or species of moth. The second is related to Hesse’s neutral analogy. These, recall from chapter 2, are properties in the model that might prove to be realistic (positive analogy) or artefactual (negative analogy) but are not yet known to be one or the other. This separating of sheep from goats encounters special problems when the models are chaotic. One of the specific issues here is that experimentalists are forced to infer long-term, \( \rightarrow \infty \), properties of a system that has been observed for only a relatively short time. Why should we believe that asymptotic properties would materialize if the system
were allowed to evolve down through the eons? The third is a consequence of SDIC: prediction-and-test confirmation schemes that are well-known to philosophers are not applicable when studying chaotic models.

1. The Problems

Let's consider the three areas of concern just sketched in greater detail.

A. "Just math"

There has been a persistent question about the applicability of the mathematical models of chaos since its rise in the early 1960's. (Although there are many kinds of equations used in chaos theory, by default I will use 'mathematical model' to refer to sets of first-order ODE's.) By 1980, the most that even groundbreaking researchers like Shaw could claim was that

[i]the occurrence of chaotic behavior is well established in both dissipative and conservative model systems, but the extent to which simple chaotic models will be useful in describing the real world is an open question.

What then are we to make of the recent discoveries of chaos in all the natural sciences? Recall Ruelle's warning from chapter 1: the "discovery" of chaos isn't always what one might think.

Many published papers give the superficial impression that they deal with real physical, biological or economic systems, while in reality they present only computer studies of models. By "real system" I mean a system in, say, astronomy, mechanics, physics, geophysics, chemistry, biology or economics with a time evolution that one wants to investigate. Computer study of a model is an important method of investigation, but the results can only be as good as the model.114

If the models on which the computer simulations are based are themselves in question, then the conclusions derived from such studies are of course questionable. One cannot rely on these "discoveries" to answer the skeptic.

Consider the now classic Lorenz model, the governing equations for the Lorenz mask. Edward N. Lorenz is a meteorologist. At the time of his accidental discovery in the 1960's, he was attempting to construct a mathematical model for atmospheric convection currents. The three, coupled ODE's of the Lorenz model are derived from a (somewhat idealized) model of energy transport through a layer of fluid. The hydrodynamical laws that govern such a system (a set of 2-dimensional nonlinear PDE's) are formidable and require a variety of simplifications before getting down to Lorenz's three equations. In the terminology adopted in chapter 2, these equations constitute a mediating model, constructed from the top down. The simplifications are so extreme, in fact, that the "model" is practically useless for modeling anything, including the original subject. Furthermore, no turbulence is found in the numerical solutions to the original PDE convection equations. The equations from which this paradigm of chaos theory is derived are not chaotic! In other cases, mathematical models that do not meet the necessary conditions for chaos display chaotic behavior when solved by a numerical algorithm. If such cases are typical, then there seems to be some reason for thinking that the entire theory is "just math."

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In fact, the three-ODE model that bears his name was not the one first derived in the early 1960's, but rather a simplified version of the original. E. Atlee Jackson, Perspectives of Nonlinear Dynamics, 2 (New York: Cambridge University Press, 1990), 139f.; Pierre Bergé, Yves Pomeau, and Christian Vidal, Order within Chaos (New York: John Wiley & Sons, 1984), 301-312.

A.J. Lichtenberg and M.A. Lieberman, Regular and Stochastic Motion (New York: Springer-Verlag, 1983), 446.

Solving an ODE numerically produces a map, because time on the computer is necessarily discrete. Maps, as opposed to flows, can exhibit chaos in as little as one dimension, whereas three are needed for a flow. See S. Ushiki, "Central difference scheme and chaos," Physica D 4 (1982): 407-424.
B. Long time properties

Many of the properties of chaotic systems are only defined in the limit as time goes to infinity. Since, of course, the experimentalist can only observe a system for finite time, many of the characteristic properties of chaotic models evolution cannot be directly confirmed. The fractal dimension of a strange attractor is a prominent example. Batterman has pointed out this gap in the available evidence for chaos.

Classical chaos is a "long time" property of dynamical systems. Were one to observe a system for any finite period of time and notice apparently random and unpredictable behavior, it is impossible to infer with certainty that the system is genuinely chaotic. A necessary [but not sufficient] condition for a system to be fully chaotic is that is possess strongly statistical ergodic properties, the weakest of which is ergodicity. But even ergodicity is defined in terms of infinite times; that is, in the limit as $\Rightarrow \infty$.

![Figure 27: Hyperbolic Flow](image)

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118 Moon is explicit about this, but gives an example that is close. Francis C. Moon, *Chaotic and Fractal Dynamics* (New York: Wiley, 1992), 350, 371.

Ergodicity is itself a statistical rather than a dynamical property. (In fact, ergodicity is the weakest property in the literature’s “hierarchy of randomness.”)\footnote{Michael Tabor, \textit{Chaos and Integrability in Nonlinear Dynamics} (New York: John Wiley \\& Sons, 1989), 173-174.} For a strictly dynamical characteristic of a system, recall from chapter 3 the notion of hyperbolic flow. Consider the orbit $O$ running through the origin in Figure 27. Nearby orbits either converge toward $O$ or diverge away. By definition, converging orbits in a hyperbolic flow approach $O$ as $t \to +\infty$; diverging orbits approach $O$ as $t \to -\infty$. Both are defined asymptotically for a stable dynamical system, yet hyperbolicity is a necessary condition for chaos. Like ergodicity, a hyperbolic flow cannot be directly detected, only inferred from finite data.

A simpler example of this gap between theory and observation is the notion of chaotic aperiodicity. Motion on a strange attractor is first of all nonperiodic: trajectories on the phase portrait do not meet in closed curves. Neither is the motion quasiperiodic where trajectories are confined to the surface of an $n$-torus. But given the finite time that one can observe the evolution of a system or run a computer simulation, how does one know that the trajectory on a strange attractor isn’t really periodic motion with an exceptionally high period?
As the skeptic is quick to point out in the case of computer simulation, long time periodic behavior is inevitable. If the simulated phase portrait is plotted on a computer monitor, there are only a finite number of pixels on the screen. Eventually the trajectory must run over itself.\textsuperscript{21} For example, Figure 28 is the well-known Lorenz mask in which the simulation was run for minutes rather than seconds. Although only one trajectory appears on the phase portrait, it is indistinct. Furthermore, digital computers have a finite number of internal states. Once these are exhausted, the same sequence of states must appear again unless the program is changed in some way.

So, how does one detect long-time properties given finite data? Without such evidence, it seems, the experimentalist cannot separate the artefacts from the realistic properties of the models.

\textsuperscript{21}Numerical integrations start from a single set of initial conditions, so there is only one trajectory plotted per simulation. The results show up as individual points since the integrations are done in discrete time steps.
C. SDIC

Finally, there are some special problems that arise due to SDIC. As I mentioned in chapter 1, some of these were recognized by Poincaré, Hadamard, and Duhem a century ago. Their immediate significance lies in how they confound model confirmation strategies recognized by philosophers. An explanation of these problems will be given when we discuss these schemes in the next section. At this point, it should be no surprise that studying inherently unstable systems presents unusual challenges.

In sum, chaos presents a number of difficulties for the experimentalist each of which provide a foothold for the skeptic. In the next three sections, we will consider a battery of responses, some more successful than others.

2. Confirmation

Philosophers such as Glymour, Laymon, and Wimsatt have discussed the importance of *improvability* and its relation to the goodness of a model. The intuitive idea is that if a (mathematical or physical) model is idealized but basically correct, then it can be improved by making the idealizations or simplifications less ideal and more realistic. A model that cannot be improved in this way is disconfirmed. In a dynamical model, the usual strategy is to find a set of equations that match past data and predict the future evolution of the system state. Often the model is a simplified version of a less tractable law, following a top down approach (see chapter 2b). An improved model is, obviously, one whose output follows the observed data with less deviation than the previous model. The process is relatively straightforward if one knows in advance how to make the model more realistic. Physicist Robert Shaw goes so far as to call this procedure “the scientific method.”
A model of the aspect of reality under investigation is constructed. . . . The model is then operated, either physically or by computation, producing a string of numbers. Another string of numbers is obtained by actual observation of the physical system, and the two are compared. The degree of correspondence of these two sets of numbers is a measure of the accuracy of the model. The model can then be modified to attain a better correspondence, and inexorable progress is expected toward the goal of making the two number strings identical.122

This broad notion of improvability appears in three distinct confirmation schemes.

A. Piecemeal improvability 1

Ronald Laymon argues that the “piecemeal improvability of idealizations and approximations with resulting improvements in prediction is a significant feature of real science.”123 This version of piecemeal improvability (PI1) focuses on the results of increasing the accuracy of initial conditions. Laymon’s discussion can be difficult to follow since he applies the notion of improvability at one time or another not only to models but also to theories and laws. In adjoining paragraphs we find the following claims:

[T]he aim of science should be to construct more accurate models where the anticipation is that our theories, if true, will produce more accurate predictions when applied to these more accurate models.124

A set of fundamental laws receives confirmation if the use of more realistic specifications of initial or boundary conditions in fact leads to more accurate predictions.

The idea is that acceptable theories . . . be monotonic toward the truth in the sense that more accurate and less idealized initial condition descriptions lead to more accurate predictions.


124 Ibid.
The basic intuition is clear: a realistic theory, law, or model should yield more accurate predictions when it starts with more accurate initial conditions. Still, given the parts of Cartwright's work that Laymon agrees with, a slightly more detailed reconstruction is possible. According to Laymon, a theory is a set of fundamental laws.\textsuperscript{125} Since these laws cannot be directly applied to the real world (i.e., hard numbers typically cannot be extracted to compare against measurements), simplified mathematical and physical models must be derived in a top down fashion. The quantitative predictions made with the help of such a model can then be tested empirically. A theory/law is confirmed indirectly, i.e., through the success of its models. Under Laymon's scheme (my PI\(_1\)), models are not confirmed merely by making correct predictions within some experimental error. The predictions of a realistic model are expected to converge: better initial conditions must yield better predictions.

Although Laymon doesn't distinguish physical and mathematical models, initial conditions and quantitative predictions are usually associated with the latter. PI\(_1\) therefore takes the following form. The experimentalist begins with an idealized mathematical model \(M\) and a set of initial conditions which in turn yield a string of predictions \(\mathbf{p} = \langle p_1, p_2, \ldots, p_n \rangle\). This string is tested against measurements \(\mathbf{m} = \langle m_1, m_2, \ldots, m_n \rangle\) taken from the subject of the model. Several prediction sets are generated from \(M\) each time with more precise initial (and/or boundary) conditions. This procedure produces a sequence of prediction sets \(\mathbf{p}^1, \mathbf{p}^2, \mathbf{p}^3, \ldots, \mathbf{p}^i\) to be compared with the empirical data. The key principle of this scheme is that if \(M\) is realistic, then the \(\mathbf{p}\)-sequence should converge toward \(\mathbf{m}\). If prediction sets begin to diverge from measured results, the model is disconfirmed.

\textsuperscript{125}Ibid., 355.
This strategy is severely limited when applied to systems with SDIC. The problem is that on each successive trial, an improved set of initial conditions means a slightly different set of initial conditions. But no matter how close initially, different system states under SDIC must diverge exponentially. Hence one would expect each trial $p^t$ to produce very different results from the previous set $p^{t-1}$ even though the input to the model is only slightly changed. This is true even when the change in initial conditions is known to be an improvement. The desired monotonic convergence in time-series predictions is blocked by chaos.126

B. Piecemeal improvability 2

The distinctive characteristic of $\text{PI}_1$ is that the model is held fixed. The initial conditions are what one “improves” in a piecemeal fashion. In contrast, William Wimsatt’s version—what I will call $\text{PI}_2$—focuses on improvements made to a series of models.

The primary virtue a model must have if we are to learn from its failures is that it . . . [is] structured in such a way that we can localize its errors and attribute them to some parts, aspects, assumptions, or subcomponents of the model. If we can do this, then “piecemeal engineering” can improve the model by modifying its offending parts.127 Although a model $M_0$ might be “false,” as Wimsatt puts it, $M_0$ can often serve “as a starting point in a series of models of increasing complexity and realism.”128 Again, if

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126 As Smith points out (Chaos: Explanation, Prediction, & Randomness, 31f.), this confirmation scheme does have some very limited application for chaotic models. A good model is still expected to track the data for a longer time given more precise measurements. Even exponentially diverging trajectories will be similar over a small $\Delta t$. If $M$ is realistic, then each trial should produce slightly more accurate predictions for incrementally longer times. Still, the experimentalist must be able to measure this improvement in order to compare successive trials. Given the short time intervals and relatively small improvement from one trial to the next, the practical use of this scheme is severely restricted.


128 Wimsatt, ibid.
one starts with an idealized but somewhat accurate, realistic model, then making the idealizations less severe should improve its performance.

For mathematical models, the core of PI2 can be summed up as follows. Let $M_1, M_2, \ldots, M_n$ be a series of mathematical "model-stages" in which each stage is slightly less idealized than the previous one. The series is confirmed if each stage produces more accurate predictions than its predecessor.

There are a number of different changes that might be made from one stage to another. Parameter values might be improved, for example. Perhaps $M_i$ did not contain a small friction term that could be included in $M_{i+1}$. In some cases, the model-stages are expressed as truncated series expansions. Including more terms should improve the predictions of a model stage if the core of the model is realistic.129

Physicists Crutchfield and McNamara take this confirmation strategy to be characteristic of classical mechanics:

Traditionally, in a classical mechanic universe, there has been the tacit assumption of Baconian convergence of successively refined models to those which predict detailed behavior, such as the future evolution of a system's state. Once a model predicts this detailed behavior, it has been validated.130

However,

When investigating nonlinear processes, one concludes that the existence of chaotic, deterministic behavior precludes the detailed comparison of theoretical model to experimental data. The conventional picture of inexorable improvement of models only applies to non-chaotic behavior.

Why? As a thought experiment, let's say that the experimentalist has a perfectly realistic, chaotic model in hand for some phenomenon. A measurement is taken to

\[^{129}\text{Strictly speaking, the recipe "more terms entails greater precision" is only true for a regular perturbation series, not a singular one. See Tabor, 90-96, for a discussion of the mathematics and Batterman, "Theories Between Theories" for some of the philosophical implications.}\]

\[^{130}\text{James P. Crutchfield and Bruce S. McNamara, "Equations of Motion from a Data Series,"}

provide the model with initial conditions and the equations are solved either analytically or numerically. Since the model is chaotic, even a small change in initial conditions makes a very large difference in the predictions of the model. The problem is that measurement error insures that the initial conditions plugged into the model are slightly different than those in the real-world system. The only way our perfect model could yield good predictions is if it had perfectly precise initial conditions, but, as Duhem pointed out, these are never available. If a perfect model cannot track the data accurately, then neither will any of the model-stages. Measurement error will infect each stage.

To make matters worse, measurement error is not the only kind of “noise” the experimentalist is confronted with. Since mathematical models are usually solved by numerical methods, the truncations made by the computer’s internal calculations are another kind of error which is amplified by SDIC.\(^{131}\)

C. Bootstrapping

In an earlier paper on the nature of idealizations and testing, Laymon sketches another confirmation scheme which, he claims, is based on Clark Glymour’s method of “bootstrapping.”\(^{132}\) In both \(P_1\) and \(P_2\), it is assumed that the experimentalist knows in advance how to make the relevant improvements—either more precise initial conditions or improvements to the model itself. As Laymon points out, this is not always possible.

Consider a situation where the relative realism of idealizations \(I_1\) and \(I_2\) is unknown or indeterminate with respect to some existing background standard. On the assumption

\(^{131}\)Occasionally someone will suggest that roundoff error can be escaped by using analog computers. Although this is true, analog systems are subject to a “noise floor.” That is, instead of very exact values being truncated, they get washed out by thermal noise. One cannot simply turn up the gain to recover the signal.

that our theory $T$ is true, we would expect the more realistic idealization to produce a better predictions.\textsuperscript{133}

$I_1$ and $I_2$ correspond to models $M_1$ and $M_2$, respectively, both of which are derived from $T$. If $T$ is true, then the essential core of $M_1$, i.e., the part that remains unchanged from one model-stage to the next, should inherit some degree of realism. In cases where the experimentalist does not know in advance whether $I_1$ or $I_2$ is more severe, she may infer the answer by considering which model produces the more accurate predictions.

Say that, with respect to phenomenon $P$, idealization $I_2$ produces the better prediction [i.e., better than $I_1$]. Therefore, assuming the truth of $T$, our judgment is the $I_2$ is the more realistic idealization.

Such an inference is common in research-and-development: a small modification in a model or a method is made, perhaps with little theoretical backing, just to see what happens (assuming, of course, that the time and cost of doing so are not too great). If the change produces better results, it is assumed that there must be some sound theoretical explanation behind this improvement, regardless of whether one takes the time to look for it.

The final step is where bootstrapping gets its name. Having determined that $I_2$—and therefore $M_2$—is superior, the same model is tested against on a new set of observations. $M_{1,2}$ are first used to assess the relative realism of $I_{1,2}$ and then $M_2$ is used to generate a new set of predictions. Laymon argues that if the predictions of the improved model fail to be consistently better than its less realistic rival, the common core shared by both models is disconfirmed. On the other hand, if $M_2$'s predictions are correct, then the bootstrap method should be able to produce a series of model-stages in which the relative realism of $I_{i,i+1}$ is determined and the more realistic model, $M_i$ or $M_{i+1}$, is tested. Again, monotonic improvement of the model-stages is expected if in fact the governing theory $T$ from which they are derived is true.

\textsuperscript{133}Laymon, "Idealizations and the Testing of Theories by Experimentation," 166.
This approach is defeated in chaotic models for the same reasons as before: measurement and roundoff error prevent the convergence of prediction and measurement. Noise prevents chaotic models from ever producing a good degree of correspondence between a predicted time-series and observation.

Each of the three schemes considered requires some sort of systematic improvement between the output of the model(s) and observation. Since SDIC precludes the needed convergence, researchers in nonlinear dynamics have been forced to look for new approaches to testing.

D. Statistical confirmation

Why is it that chaos theorists like Crutchfield and Shaw emphasize the inability to confirm "precise" (i.e., non-statistical) predictions but then say nothing about statistical ones? Primarily, I believe, it is because of how SDIC forces the experimentalist away from precise time-series analysis. They wish to emphasize the uniqueness of chaos. Still, there is at least one important way in which statistical confirmation plays a role in chaos, viz., the use of probability measures.134

A probability measure can be defined on any finite set $S$ within a bounded region of a Euclidean space $R$.135 $S$ is sometimes called the support of the measure. Let $R$ be a phase space, $\subseteq R$, and $x_0$ be the starting point for some trajectory. One way to specify a probability measure $\mu(S, x_0)$ is the fraction of time that trajectory spends in $S$ in the limit as time goes to infinity. (If $S$ is a region in a strange attractor and $\mu(S, x_0)$

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134See Moon 130f., 256f. and Ott 51-54. Moon tends to use 'probability density function' interchangeably with 'probability measure'. Ott is careful to distinguish the two.

135$S$ cannot consist of discontinuous, isolated points in $R$, however. Technically, $S$ must have a positive Lesbegue measure, which is not to be confused with the probability measure discussed here.
is the same for almost all points \( x_0 \) in the basin of attraction,\(^{136}\) then \( \mu \) is called the *natural measure* of \( S \). If \( S \) is the entire attractor, then \( \mu(S, x_0) = 1 \).

Approximate probability measures can be found experimentally by dividing the system's state variables into “bins” (also known as “coarse graining” the phase space) and keeping track of how often a trajectory visits each bin. To illustrate, consider a one-dimensional harmonic oscillator, e.g., the mass-spring in Figure 29 moving on a frictionless surface (hardly an experimental system, but a convenient example).

Figure 30 shows the associated phase portrait and a histogram with thirty bins. The bins represent the (discretized) position of the mass. Since the mass slows to a stop when the spring is either fully compressed or fully extended, it spends most of its time around these two turning points. Hence there is a much higher probability of finding the position value at one of these extremes (\( x = x_{\text{min}} \) \( x_{\text{max}} \)) than in the center (\( x = 0 \)) if one were to take a single random sample. This information is captured in the histogram.

Figure 29: One-dimensional Oscillator


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Not surprisingly, the experimental probability measure changes when the system goes from a periodic (or quasiperiodic) to a chaotic regime. But like other well-known time-series analysis techniques—principally Fourier spectrum analysis and the autocorrelation function—the histogram method is primarily a diagnostic tool for chaos, not a confirmation technique for a mathematical model. That is, such methods are able to show that a real-world system displays aperiodic, random behavior, but not necessarily chaos. With only these tools, one cannot distinguish between motion dominated by a strange attractor and highly amplified noise.

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137 'Random' in this context is sometimes used to describe a time-series with noise, something all experimental signals encounter. 'Random noise' is used to distinguish the deterministic signal being studied from other corrupting data that cannot be filtered out.
E. Qualitative Predictions

Given these obstacles to quantitative prediction, researchers in nonlinear dynamics increasingly turned to the qualitative properties of the models, a move urged by Poincaré a century ago. As Ralph Abraham and Christopher Shaw sum up in their well-known text,

Although qualitative predictions are not as precise as quantitative ones, they are a whole lot better than no predictions at all. *And for most problems of applied dynamics, qualitative predictions are impossible.*

Qualitative predictions include the number and kind of attractors and/or repellers found in the model’s phase space and topological changes in the phase portrait when parameter values are varied.

There are serious limitations to this approach as well, however. Robert Shaw concedes that qualitative similarity provides only an extremely weak level of confirmation for a given model that “is so crude as to have little or no predictive power. . . .” The reason for this harsh assessment is that even when a mathematical model provides good qualitative predictions for some real-world system, there are many others that would do just as well. Hence qualitative predictions can be tested, but they (generally) provide little confirmation for individual models.

Things seem rather bleak at this point. The strategies that rely on piecemeal improvement are hobbled without precise quantitative predictions. Some statistical predictions are available, but the ubiquitous presence of noise limits their use as well. And although qualitative predictions are easier to come by, they are too weak to confirm specific mathematical models. Faced with these problems, chaos researchers have had to be creative. Let’s begin to consider some of their successes.

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3. Shadowing

As we discussed in the last section, a chaotic model that is simulated by numerical methods inevitably makes false predictions due to SDIC. Even if the model is a good one and starts with perfect initial conditions, roundoff error infects the calculations. We have also seen what effect this has on the testing of chaotic models. Following this line, physicist Edward Ott adds his voice to the chorus of skeptics.

Given the difficulty of accurate computation, . . . one might question the validity of a picture . . . which show thousands of iterates of the Hénon map. Is the figure real, or is it merely an artifact of chaos-amplified computer roundoff?139

He then adds this remarkable response to his own question:

A partial answer to this question comes from rigorous mathematical proofs of the shadowing property for certain chaotic systems. Although a numerical trajectory diverges exponentially from the true trajectory with the same initial condition, there exists a true (i.e., errorless) trajectory with a slightly different initial condition that stays near (shadows) the numerical trajectory. . . . Thus, there is good reason to believe that the apparent fractal structure seen in pictures is real.

Let's unpack this a bit. A “trajectory” here is simply the curve one would see if the state of a system were plotted over time. A true trajectory is produced by the actual (analytical) solutions to the equations of a mathematical model (i.e., the solutions one would find by using textbook techniques, like integration, and plugging in the initial conditions). Numerical trajectories, also known as pseudo-orbits, are produced by numerical solutions on a computer, and so are subject to roundoff error.

Ott's point about shadowing is this. Consider the three trajectories plotted in Figure 31. Let (a) be a true trajectory with initial conditions \( x(\tau_0) \) which is governed by some mathematical model \( M \) (i.e., a trajectory driven by analytic solutions to \( M \)). (c) is also a true trajectory but from slightly different initial conditions; (a) and (c) diverge exponentially. (b) represents a pseudo-orbit started from the same initial

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conditions as (a), i.e., (b) is the numerical solution to $M$ starting from $x(t_0)$. (b) is in essence what the computer simulation has predicted for the trajectory of (a). (a) and (b) also diverge.

![Figure 31: Shadowing](image)

Note how (b) and (c) run parallel to one another in Figure 31. This is no coincidence. (b) is said to be shadowed by (c). It has been proved that for some chaotic models, (b) will lose track of (a) but will be shadowed by a different true trajectory (c).

Before discussing the details, consider the consequences of this shadowing effect. If a shadow orbit exists for a numerical solution to $M$, then the noisy, simulated trajectory does, in fact, closely represent some true trajectory. These results effectively eliminate the problems associated with roundoff error, at least when the conditions for shadowing are met. Numerical solutions are trustworthy, although noise due to measurement error is still present and a problem.

As an answer to the skeptic, these results are an important, albeit limited, first step. At best they show that some trajectories are not merely artefacts of the numerical methods used on nonlinear equations. When shadowing is present, there is a strong link between the mathematical model and the computer simulation. Well and good, says

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See Hammel, Yorke, and Grebogi, ibid., for sufficient condition for shadowing to occur.
the skeptic, but the link between these and the world is also in question. Does either the mathematical model or the simulation capture the dynamics of any real-world phenomenon?

Here is the problem. Let mathematical model $M$ be a set of equations with numerical solutions that have been proven to be shadowed. We now know that some true, analytic solution for $M$ closely resembles the computer output. Nothing said thus far shows that $M$ is in any way representative of the subject of the model. That is, we still don’t know whether $M$ is a good model or not.

The problem is that the experimentalist is left with a conditional: if $M$ is a good model for some process, then a very good approximation to some true trajectory can be calculated numerically. The shadowing results offer no new means for satisfying the antecedent, however. A bridge has been built from certain chaotic mathematical models to their simulated solutions. However, we have not yet bridged the gap from these models to the world.

4. Wimsatt's Robustness

William Wimsatt has provided the most noteworthy contributions on the philosophical difficulties of mathematical modeling. The notion of robustness is especially important and is used primarily to describe the realistic consequences of a family of false models.\(^{142}\) The idea is not altogether new. Wimsatt was clearly motivated by the work of biologist Richard Levins, whom he quotes at length and for good reason:

Even the most flexible models have artificial assumptions. There is always room for doubt as to whether a result depends on the essentials of a model, or on the details of the simplifying assumptions. This problem does not arise in the more familiar models, such as the geographical map, where we all know that contiguity on the map implies contiguity in reality, relative distances on the map correspond to relative distances in reality, but color is arbitrary and a microscopic view of the map would only show the fibers of the paper on which it is printed. But in the mathematical models of population biology, it is not always obvious when we are using too high a magnification.

Therefore, we attempt to treat the same problem with several alternative models, each with different simplifications, but with a common biological assumption. Then, if these models, despite their different assumptions, lead to similar results we have what we call a robust theorem which is relatively free of the details of the model. Hence our truth is the intersection of independent lies.  

A robust result is a property that each—or at least most—of the members of a family of models predicts. Since this property is produced under a variety of false assumptions, it is unlikely that each of the idealizations independently leads to the same result. Hence, a robust property is (most probably) not an artefact; it is representative of the real-world subject. Let's consider how robustness applies to the models of chaos theory.

A kind of robustness can be found in the qualitative predictions known collectively as "the routes to chaos": quasiperiodicity, period doubling, and intermittency. Each describes topological changes in the phase portraits of families of mathematical models as parameter values are changed. These changes, or "bifurcations," are often represented in a parameter space in which the stable and chaotic regimes are easily demarcated.

The family of models found within a given scenario is not restricted to a single subject; therefore, their results are not robust in precisely the same way Wimsatt uses

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144 See Bergé et al, 159-259. Each route is given a chapter length explanation.
the term (in print).\textsuperscript{145} I am applying a slightly broader notion. The fact that these qualitative predictions are found in such a wide range of mathematical models and state spaces cannot be ignored. The modeler must either explain away these results as an artefact tied to a common simplification or accept their robustness as evidence of their realism.

In fact, all three routes have been observed. For example, the period doubling route is thought to be found in Rayleigh-Bénard convection.\textsuperscript{146} Under this scenario, a stable periodic orbit undergoes a bifurcation into a new orbit with a period twice that of the original. As the parameter value is increased, the succession of bifurcations continues until the period becomes infinite. At this so-called “accumulation point” the motion is aperiodic. Pushing the parameter past the accumulation point puts the system in the chaotic regime.\textsuperscript{147} One fascinating relation all systems on this route seem to obey is

\begin{equation}
\lim_{n \to \infty} \frac{\lambda_n - \lambda_{n-1}}{\lambda_{n+1} - \lambda_n} = \delta \approx 4.6692016
\end{equation}

where \( \lambda_n \) is the period of the \( n \)th bifurcation. \( \delta \) is known as the Feigenbaum number.

R.B. convection is produced when a layer of fluid, often liquid mercury, is confined between two plates and subject to some force (gravity or a magnetic field). The plates have a fixed difference in temperature which is used as the control parameter. As the name implies, convection currents arise whose frequency defines the period to be

\begin{flushright}
\textsuperscript{145}Nonetheless, Wimsatt has expressed in correspondence that he believes my use of robustness here is legitimate.
\textsuperscript{146}Bergé \textit{et al}, 83, and Tabor, 222.
\textsuperscript{147}Technically, at the accumulation point the motion is no longer periodic, but neither is there a positive Lyapunov exponent—a measure of SDIC. Past the accumulation point, there are bands in the parameter space with positive Lyapunov exponents separated by smaller bands of periodic flow.
\end{flushright}

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doubled. It is well documented that the observed periods obey the Feigenbaum relation (9) to within 5%. As one group of researchers puts it, "The R.B. convection experiment offers indisputable confirmation of the existence of a subharmonic cascade in a physical system."148

Universal constants like the Feigenbaum number are robust since all mathematical models that start on this route to chaos exhibit the property, regardless of their simplifications and background assumptions. Furthermore, physical systems that display period doubling likewise obey (9). There is now a consensus that these results preclude a global skepticism toward chaotic models. The match of mathematics to observation cannot be written off as coincidence.

The qualitative predictions associated with quasiperiodicity and intermittency have likewise been empirically confirmed. Two experiments that have been studied in detail are the Belousov-Zhabotinsky chemical reaction149 and Couette flow,150 respectively.

The quasiperiodic route deserves special attention. It is similar in superficial ways to period doubling, although the nature of the bifurcation is somewhat different.151 For low parameter values, the quasiperiodic route agrees with the older Landau-Hopf approach to turbulence up to a point. Periodic motion—a 1-torus in phase space—becomes quasiperiodic with each bifurcation adding a dimension to the n-torus. The quasiperiodic route breaks from the Landau-Hopf view by predicting that the transition

\[148\text{Bergé et al., 213.}\]
\[149\text{Ibid., 91.}\]
\[150\text{Tabor, 192.}\]

\[151\text{Technically, the period doubling route undergoes a series of supercritical subharmonic bifurcations. In the quasiperiodic approach they are Hopf bifurcations.}\]
from a 3-torus to a 4-torus is atypical. Instead, this final bifurcation is often from a
torus to a strange attractor.

As with period doubling, the quasiperiodic route has been observed in many
models and physical phenomena. Moreover, this scenario has some unique
mathematical underpinnings. Ruelle and Takens have proved that if the conditions that
allow a succession of four Hopf bifurcations are met, then the occurrence of a strange
attractor is possible with an arbitrarily small perturbation. Diagnostic tools such as a
power spectrum analysis have successfully detected quasiperiodicity, the predicted
bifurcations, and the aperiodic regime. They cannot, however, detect what is
responsible for the aperiodicity. Still the Ruelle-Takens proofs provide an hypothesis
that is consistent with observation: the aperiodic regime at the end of the quasiperiodic
bifurcations is governed by a strange attractor.

The holy grail is still missing, unfortunately. Nothing said thus far amounts to
direct evidence of a strange attractor. Without it, the Ruelle-Takens prediction lacks
definitive empirical confirmation. That confirmation is found, I want to urge, in
something called phase space reconstruction.

5. Phase Space Reconstruction

The final piece of evidence in favor of the realism of chaotic models, and
specifically strange attractors, is the most interesting and powerful, but also the most
complex. Less than twenty years ago, skepticism towards chaos was the majority view

\[152\text{I.e., the phase space 4-torus produced after the forth Hopf bifurcation is structurally unstable. Ruelle and Takens did not prove, however, that such a transition is likely (in a measure theoretic sense). Some texts are very misleading on this point. The nature of the perturbation is, in fact, a nontrivial consideration. How likely the 4-torus-to-strange-attractor transition is depends on the specific system. See Ott, 202-204.}\]
among physicists and engineers. The mathematics was interesting, no doubt, but few believed that it told us anything about the real world. The technique known as phase space reconstructions (or embeddings) was developed as an answer to that skepticism. One of its pioneers, Norman Packard, recalls it this way.

It is important to put things in the perspective of 1977, when “strange attractors” were widely suspected to be nothing but a mathematical fantasy. They could be constructed mathematically, à la [mathematician Stephen] Smale, and they appeared to be present in computer simulations, although even this was vigorously debated. . . . Talks on deterministic chaos in front of physics, mathematics, and engineering departments at the time typically encountered skepticism and even hostility.153

The models, or more precisely models of data (chapter 2d), presented in this section effectively eliminated that skepticism in the scientific community, but have yet to be appreciated by philosophers.

Before presenting the technical details, let’s begin with a crude and inexact, but intuitively helpful characterization of the reconstruction process. The first thing to note is that the experimentalist begins with the data and works “from the bottom up,” rather than from a law or mathematical model. One starts with the hypothesis that the system under study is deterministic and dissipative. The state of the system is therefore uniquely specifiable, at least in principle. That is, even if the correct set of state variables is known, the experimentalist may not have access to all or even most of them. Nonetheless, a “natural” phase space is posited—an abstract space with a single trajectory that represents the actual evolution of the real-world system. The goal of phase space reconstruction is to gain access to the natural phase space. This is done by finding a particular sort of mapping relation from this space to an experimentally reconstructed phase space. If such a map exists, the experimentalist can infer the dynamical properties of the natural phase space from the trajectory in the reconstructed

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space. In short, the reconstruction space is a model of the natural phase space—which, in essence, is just a way of thinking about the system itself. No governing laws or first principles are presupposed. Phase space reconstructions are bottom up models built directly from observations.

What does one expect to see in the reconstructed phase space if the system is chaotic? The answer is a single trajectory carving out a strange attractor. If the mathematical chaotic models are correct, there should be strange attractors in the natural phase spaces of real-world phenomena. Embeddings provide access to such spaces.

Let's consider how such models are constructed, including some of the mathematical machinery on which they are based.

A. Background from Topology\textsuperscript{154}

The key theorems for embedding have their roots in topology. Starting with some terminology, let Γ be a k-dimensional Euclidean space, sometimes written $\mathbb{R}^k$. A point in this space is denoted by a vector $x = (x_1, \ldots, x_k)$. Let $F$ be a continuous function from Γ to another space Λ which is $\mathbb{R}^m$. Points in Λ are denoted $y = (y_1, \ldots, y_m)$. In vector notation $y = F(x)$, which is a convenient shorthand for the set of equations

$$
\begin{align*}
y_1 &= F_1(x_1, x_2, \ldots, x_k) \\
y_2 &= F_2(x_1, x_2, \ldots, x_k) \\
\vdots \\
y_m &= F_m(x_1, x_2, \ldots, x_k)
\end{align*}
$$

If $A$ is a subset of Γ, then the set of points $F(A)$ in Λ is called the image of $A$ in Λ. If $F$ is continuous and one-to-one on $A$ and its inverse, $F^{-1}$, is continuous, then $F$ is called a topological embedding of $A$ onto $F(A)$. A trajectory in Γ is one particular set of

points that may be treated as the subset $A$. If $F$ also has continuous partial derivatives, then it becomes a slightly stronger differentiable embedding. Among other things, this guarantees that if $A$ is a smooth manifold, then its image is as well, and that the dimension of $A$ is the same as $F(A)$, denoted $\dim(A) = \dim(F(A))$.

B. Reconstruction Space

What if all one has is a set of points that is believed to be an image of another set, i.e., if the mapping relation and its domain are unknown? When is one justified in believing that the relation between the observed time-series and the evolution of the subject is in fact a topological embedding? This is the experimentalist’s situation. He has a set of data points that can be plotted in a phase space $A$ and wants to know, presumably, whether this image is an accurate representation of the evolution of the real-world system. Recall that the points in a natural phase space $\Gamma$ just are the states of the real-world system.

Although the dimension of a phase space can approach infinity, the dominant behavior of the system can at times be captured by a low-dimensional strange attractor—one of Ruelle’s key insights that launched chaos theory. To infer that a strange attractor is present in a natural phase space $\Gamma$, one must (i) find a strange attractor in the reconstruction space $A$ and (ii) have reason to believe that this attractor has been produced by an embedding. The needed justification is found as we move from topology into experimental dynamics.

Let $\mathbf{x} = [x_1, x_2, \ldots, x_n]$ denote the state of a real-world system (i.e., $\mathbf{x}$ represents a single point in $A \subset \Gamma$). Packard et al. first demonstrated a technique for generating an $m$-dimensional image vector $\mathbf{y}$ directly from measurements.$^{155}$ The evolution of $\mathbf{y}$ forms

a phase portrait in a reconstruction space \( \Lambda \). Again, \( y \) is treated as the image produced by a map \( F \) from some unknown phase space \( \Gamma \) into \( \Lambda \), or
\[
(11) \quad y(t) = F(x) = [x(t), \ldots, x_n(t)]
\]
Both \( F \) and \( x \) are unknown.

To illustrate, suppose the phenomenon of interest is Rayleigh-Bénard convection. The data might consist of velocity values from \( m \) probes placed in the convection currents. Typically the probes are sampled, as opposed to being monitored continuously, but the method can be carried out both ways. The data stream from Probe 1 produces the time-series \( f_1(x(t)) \), likewise through Probe \( m \), \( f_m(x(t)) \). Each probe contributes a dimension to the image vector and hence to \( \Lambda \). (The fact that each probe is measuring a velocity value doesn't matter so long as there are enough probes to uniquely specify the state of the system for each time.)

The following claim rests on both experimental evidence and the proofs of Floris Takens:\textsuperscript{156}

If \( m \) is large enough and if there is a low-dimensional attractor \( A \) in \( \Gamma \), then as \( x \) ranges over the subset \( A \), \( y = F(A) \) produces an image of \( A \) in \( \Lambda \) and (with probability 1) \( F \) is a topological embedding.

Interpreting this conditional can be more challenging than it first appears. After all, how does the experimentalist know that \( m \) is "large enough" and that there is an attractor in \( \Gamma \) before it has been detected in the reconstruction?

Putting the dimension question to the side for the moment, notice that the theorem requires the existence of an attractor, but not necessarily a strange attractor. This means that the procedure only works on dissipative systems. The requirement that the attractor to be low-dimensional should be understood as a practical rather than a

theoretical limitation. As we noted earlier, one of the motivations behind chaos theory is the ability to capture the dominant behavior of a highly complex system with a low-dimensional model that is simple enough to be studied experimentally. Phase space reconstruction is applicable in theory for high-dimensional systems, but the cost in experimental and computational resources is prohibitive.

A somewhat puzzling phrase in Takens's theorem is "with probability 1." To what exactly does this parenthetical addition attach? A slightly different version might help:

Let \( A \) be a compact smooth manifold of dimension \( d \) contained in \( \mathbb{R}^k \). Almost every smooth map \( \mathbb{R}^k \rightarrow \mathbb{R}^{2d+1} \) is an embedding of \( A \).\(^{157}\)

The force of Takens's "with probability 1" is captured here by "almost every," which Sauer et al. have rigorously defined in measure theoretic terms. The upshot is this. Say the experimentalist has formed a reconstruction space \( \Lambda \) in which he finds some orbit. How confident can he be that the orbit has been produced from an embedding of an attractor \( A \) in the natural phase space? So long as the dimension of \( \Lambda \) is just over twice that of \( A \), the map producing the figure in \( \Lambda \) is an embedding with probability 1.

In one sense, the theorem contains the answer to the required size of \( m \): \( m \) is "large enough" when it is twice the (box-counting) dimension of \( A \). But how does the experimentalist know when he has achieved this goal? In terms of the R-B convection apparatus, how many probes are needed for a sufficiently robust reconstruction space? First, the space is too lean if trajectories cross, which would imply a violation of determinism. The notion that nonintersecting curves can look as if they cross when projected onto a space of too few dimensions is actually quite familiar. If the paths of

all airplanes flying over the United States today were projected onto the surface of the
earth, they would obviously intersect. The nonintersecting flight paths in a three-
dimensional airspace cross when mapped to a lower dimensional space.

So then, a reconstruction space is sufficiently robust when there are enough
dimensions for an attractor to be fully unpacked. Given the difficulties of having to spot
crossing orbits by inspection, a better indicator is the calculated dimension of the
attractor. If the reconstruction space has too few dimensions, then the dimension of the
attractor will appear to be smaller than it is. Adding a dimension to the embedding
space tends to make a dramatic difference in the calculated dimension of the attractor
unless the space is as large or larger than the actual attractor. To take a simplified
example, a sphere projected onto a 2-dimensional Euclidean space appears to be an
ellipse or circle. The full 3-sphere is recovered in a 3-dimensional space. Adding still
more dimensions to the embedding space, however, has no effect on the apparent
dim(sphere). Hence, the minimum space required for a complete representation of the
object is found when the observed dimension of the reconstructed figure stops changing
as dimensions are added to the embedding space.

The same strategy can be applied to attractors in $\Lambda$. Let an initial "best guess"
of dim($\Lambda$) have some value $d_\Lambda$. Say the calculated dimension of the image of attractor $A$
is dim($F(A)$) = $d_A$. Adding a dimension to the reconstruction space makes
dim($\Lambda$) = $d_\Lambda + 1$. If dim($\Lambda$) had initially been too small, then the newly calculated value
for dim($F(A)$) should increase dramatically. When dim($F(A)$) stops changing as dim($\Lambda$)
is increased, $\Lambda$ is taken to be large enough to fully unpack the image $F(A)$.
A more rigorous technique has recently been developed based on the notion of "false neighbors." When an attractor is mapped onto a space of too few dimensions, points that would not be close together in a larger space can appear next to one another. Such points are called false neighbors; their proximity is an artefact of an inadequate mapping. Consider for example how false neighbors are produced by collapsing a circle down onto a line (Figure 32). Some points on opposite sides of the circle will be mapped near one another.

![Figure 32: False Neighbors](image)

The existence of false neighbors shows that the range of the map is too small. When the space is free of false neighbors, it is taken to be sufficiently robust for the full attractor. Sketching the procedure using a reconstruction space is relatively straightforward. Pick two neighboring points in A and calculate their distance. (Since phase spaces are often Euclidean, a typical "straight line" distance function can be used. In standard Cartesian coordinates on a 3-d space, the distance \( r \) is given by the familiar \( r^2 = x^2 + y^2 + z^2 \).) Next, add a dimension to A and recalculate the distance. Since increasing the dimension of the reconstruction space will not significantly affect the

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distance between true neighbors, a dramatic increase reveals that the points in the previous calculation were false neighbors.

So then, there are several tests for when the reconstruction space is sufficiently large, fulfilling the antecedent in Takens's theorem. When the conditions are met, the image in hand accurately captures the geometrical properties of the attractor in the natural phase space. If no attractor is found, the method simply fails and nothing may be inferred. If the procedure is successful and if \( \Lambda \) is a strange attractor, a topologically equivalent attractor will appear in \( \Lambda \).

In sum, the experimentalist begins with the methodological assumption that the physical process to be studied is deterministic and that the dynamics could, in principle, be captured by a phase portrait. Instead of deriving a mathematical model from some governing theory, the experimentalist samples a number of independent physical quantities over some finite time. These measurements are used, à la Packard et al., to build a reconstruction space. If the reconstruction space is large enough, then almost every map from the natural phase space is a topological embedding. The reconstruction theorems guarantee that the embedding space is a faithful representation of the natural phase space. Additional conditions can be added to change 'topological embedding' to the stronger 'differentiable embedding.'

C. Delay Coordinates

Another way of characterizing a natural phase space is the space one would get if one knew all the degrees of freedom of the system and could track each as a state variable. Unfortunately, even when an attractor reduces the number of significant dimensions, the experimentalist can seldom make enough measurements to uniquely specify the state of the system. At times only a single quantity can be sampled, reducing the image vector \( y(t) \).
to one dimension. This vector is not robust enough, however, to meet Takens’s conditions and so it is not possible to build a reconstruction space.

Remarkably, it has been found that the needed dimensions can be created by adding delays to a single time-series. Recall that usually the image vector has the form

\[ y(t) = P(x) = [\epsilon_1(x(t)), \ldots, \epsilon_n(x(t))] \]

where \( \epsilon_i(x(t)) \) is the data stream from one probe, at least in the R-B convection example. With only one data stream to work with, a time delay \( \tau \) is used to form a proxy for the missing dimensions:

\[
\begin{align*}
\mathbf{y} & = [\epsilon(x(t - \tau)), \epsilon(x(t - 2\tau)), \ldots, \epsilon(x(t - n\tau))] \\
& = [y(t - \tau), y(t - 2\tau), \ldots, y(t - n\tau)] \\
& = [h_1(x(t)), \ldots, h_n(x(t))] \\
& = \mathbf{H}(x)
\end{align*}
\]

Amazingly enough, this image vector captures all the qualitative properties of (12) and is an acceptable state vector in reconstruction space. The proof of this was provided, once again, by Takens.\textsuperscript{159} With the image vector in hand, one may proceed as usual in plotting a reconstruction space.

This method is a powerful tool with two primary virtues. First, no \textit{a priori} knowledge about the dynamics of the system is needed; no governing laws or first principles are required. Second, the model is generated with access to only one degree of freedom—a fact that seems too good to be true. Counterexamples immediately come to mind. The dynamics of a wobbly wheel, e.g., can’t be properly analyzed with access only to the angular velocity in one dimension. Physicist Neil Gershenfeld understands this response, but offers an analogy to bridge the intuitive gap:

One’s first reaction to this claim is that it must be incorrect, because surely the act of eliminating all but one observed degree of freedom will eliminate information about the

\textsuperscript{159}Takens, ibid.
system. A two-dimensional projection of a three-dimensional scene does not provide enough information to unambiguously recreate the scene. However, to push the analogy further, a two-dimensional hologram can contain information about three-dimensional relationships by recording interference patterns. In our case the extra information comes from the dynamics. The variable that we observe does indeed only measure one degree of freedom, but its evolution is intimately affected by the other degrees of freedom and so its time evolution contains information about them.160

Unlike the angular velocity of my wobbly wheel, many observable degrees of freedom contain information about unobserved ones. The justification for this follows from some special properties of underlying dynamical equations. If $F$ in (12) is a smooth function from one manifold to another, its Jacobian derivative $D_x F(x)$ (14) maps the tangent space at $x$ to the tangent space at $F(x)$. So long as $D_x F(x)$ maps nonzero tangent vectors to one another (i.e., the Jacobian is of full rank) for all $x$, the Inverse Function Theorem guarantees that $F$ is one-to-one.161

$$D_x F(x) = \begin{bmatrix} \frac{\partial F_1(x)}{\partial x_1} & \frac{\partial F_1(x)}{\partial x_2} & \cdots & \frac{\partial F_1(x)}{\partial x_k} \\ \frac{\partial F_2(x)}{\partial x_1} & \frac{\partial F_2(x)}{\partial x_2} & \cdots & \frac{\partial F_2(x)}{\partial x_k} \\ \vdots & \vdots & \ddots & \vdots \\ \frac{\partial F_m(x)}{\partial x_1} & \frac{\partial F_m(x)}{\partial x_2} & \cdots & \frac{\partial F_m(x)}{\partial x_k} \end{bmatrix}$$

(14)

In the time-delay case, Gershenfeld's claim is that since the dynamics of the vast majority of systems studied are "nontrivial," the $m$ functions in (13) are related in a nontrivial way (specifically, the states variables are coupled—functions of one another). This intricate relation between the components of the image vector implies that the Jacobian matrix derived from (13), $D_x H(x)$, is seldom less than full rank. Only for pathological (measure zero) choices of the measurement function $f$ will derivatives of the

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161 Ibid., 364.
iterates of the map be linearly dependent. Hence, by the argument given in the last paragraph, \( H \) will be one-to-one with probability 1.

But why think, as Gershenfeld says, that the vast majority of systems have the required nontrivial dynamics (i.e., there is coupling between the state variables)? Answers to this question appear to come in two varieties, neither of which is as satisfying as we might wish. The first kind of answer appeals to a general view of physical systems. Czech mathematician Pavel Pokorny supports this line:

> If you investigate a real dynamical system, then you can assume, that (physical) quantities describing one inter-related system (a chemical reactor, an electrical circuit) are inter-related. If nothing else, then at least diffusion causes the influence. Sure, the temperature in your system is almost perfectly independent of the pressure of my system. But strictly speaking, I believe that the entire universe is one inter-related dynamical system.\(^{162}\)

On this view, the perfect independence of physical quantities is an idealization. Real-world dynamical systems are always inter-connected to some degree.

On the other hand, one sometimes finds more mathematical arguments. David Elliot, a visiting research scientist at the University of Maryland’s Institute for Systems Research, believes that coupled state variables are “generic” in that “if you pick them at random the rank test is almost certainly satisfied.”\(^{163}\) His argument is analogous to the claim that if one were to pick a real number at random, it would almost certainly be irrational. In this case, if one were to pick a Jacobian at random, it will typically be of full rank, satisfying the Inverse Function Theorem.

A striking test of the reconstruction method was carried out recently on the Lorenz model. Solving its three ODE’s numerically produces three “data streams,” one for \( x(t) \), \( y(t) \), and \( z(t) \). The Lorenz mask is usually produced in a simulated phase

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\(^{162}\)Pavel Pokorny, personal correspondence.

\(^{163}\)David L. Elliot, personal correspondence.
space by plotting the values of \( (x, y, z) \). To test the method of time-delay embedding, a single time-series \( x(t) \) is kept and the other state variables discarded. Two time delays are added to \( x(t) \) in order to form a three-dimensional image vector. The reconstructed Lorenz mask shown in Figure 33 is produced by plotting values of \( (x(t), x(t - \tau), x(t - 2\tau)) \).\(^{164}\) Further study confirms what seems to be apparent on inspection: the original attractor (inset) and its image are related by a homeomorphism.

\[ \text{Figure 33: Reconstructed Lorenz Mask} \]

In sum, the ability to build a reconstruction space from sampled data has been demonstrated. That attractors in reconstruction space mirror those in the actual phase space of a system has been proved mathematically and empirically tested—the Lorenz mask is just one example.

D. General Remarks and Application

To see how innovative this technique is, one must recall the standard view of scientific methodology (see Shaw, page 114). The usual goal is to derive tractable models of some phenomenon by starting with fundamental laws—like Schrödinger's equation in quantum mechanics, make predictions based on these models, and then test the predictions. This "top down" approach is made possible through a combination of the modeler's experience and a background theory. However, the method breaks down when the model displays SDIC. When Duhem concluded nearly a century ago that such predictions are forever "unutilizable" by physicists, he assumed that physics was intrinsically tied to the top down approach to model building.

Reconstructed phase spaces—which, again, constitute a class of models—are built from the bottom up. The experimentalist gets around the confirmation problems by starting with the data rather than first principles or a governing law.

In the past thirteen years, experimental dynamicists have used this technique to reconstruct phase portraits from an impressive array of phenomena. What they find is a startling degree of qualitative confirmation with the purely mathematical models of chaos. Qualitative predictions include the number and dimension of attractors found in the model's phase space. Two typical uses of embeddings are (i) to estimate various geometrical properties of a strange attractors discovered in the reconstruction space, and (ii) to allow the calculation of characteristic (Lyapunov) exponents. Among other things, the latter serve as a quantitative measure of SDIC. These system-analysis tools take chaos theory from the realm of the mathematician and theoretical physicist to that of the experimentalist and engineer. More recent results start with a reconstruction space and work toward a mathematical model. Once such a model is found, it can be used to make short-term predictions of the system state.
Let’s finally apply all this to the skeptic’s challenge. The punch line, as it were, is that phase space embeddings give what seems to be direct confirmation of the qualitative predictions of chaotic models. From the birth of modern chaos theory in 1963 throughout the 1970’s, nonlinear dynamics was essentially “just math,” as Packard puts it. Interesting physical and mathematical models were developed and simulated, but few theorists believed that these simulations applied to the real world. At best, one could claim that if the chaotic models were correct, then there were strange attractors in the natural phase spaces of real-world phenomena. Intuitively, a reconstructed phase space is a mirror of this natural phase space. In the mirror, we see strange attractors.

So, have we finally overcome all of the skeptics objections? Not quite, there are a couple of final bunkers from which to fight. In chapter 3, I argued that strange attractors with fractal structure all-the-way-down are physically possible. But phase space reconstructions, Smith has argued, do not confirm the existence of fractal structure at all scales of a strange attractor. That particular property of these bottom up models is empirically empty, even if physically possible. Figure 33, for example, is not a smooth manifold, but rather a series of discrete points that have been joined together in a crude connect-the-dots fashion. The skeptic must concede that something like a strange attractor exists in the natural phase space of some systems, but not the precise and intricate fractal attractors produced by mathematical models, like the Lorenz model. One alternative, given the asymptotic nature of a strange attractor, is that in the distant future the trajectory closes, making the attractor multiply periodic with a very

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165Recently, my view of phase space reconstructions as an answer to skepticism among physicists was confirmed by one of its developers, Norman Packard, in an open letter on the Internet newsgroup sci.nonlinear, May 4, 1995.

166Peter Smith, personal correspondence.
high period. The "something like a strange attractor" would be an attractor restricted to
the surface of another highly complex phase space entity, viz., an extremely high
dimensional n-torus.

There are several answers to this objection. First, it is not unusual for an
experimentalist to only have access to parts of a continuous process. Well before the
advent of chaos theory, the study of linear signals by sampling was commonplace in
system analysis. The fact that the data set consists of discretized rather than analog
measurements does not by itself cast doubt on the existence of, say, periodic signals in
nature—*mutatis mutandis* for chaotic evolutions.

Second, the signal-processing equipment used to build a reconstruction space
need not be digital. Shaw and others have long advocated the use of analog computers
in chaos studies. An attractor reconstructed with an analog device is not a rough
looking connect-the-dots figure like Figure 33. The "dots" represent samples taken
from the input signal \([x(t)]\). But since the input in an analog device is processed in
real time, not sampled, the output/image vector will be continuous so long as \([x(t)]\)
is.

Last and most importantly, how can one know that an attractor is strange and
aperiodic rather than multiply periodic with an extremely high period? The
experimental evidence provides a definitive answer. The view that relies on strange
attractors is often called the Ruelle-Takens model and the high quasiperiodic view the
Landau-Hopf model. Based on the following experimental results, the latter hypothesis
is no longer viable. First consider such diagnostic tools as power spectrum analysis and

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Faucet," *Physics Letters* 110A, no. 7,8 (August, 1985): 401. Here the focus is on the use of analog
computers for studying mathematical models, rather than for embeddings. However Moon, 371f., gives
a nice example of the use of analog devices for the measurement of fractal dimension.

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autocorrelation function. Although neither of these can distinguish chaos from noise, their results clearly demarcate (moderate) quasiperiodic from chaotic evolutions.\textsuperscript{168} The frequency power spectrum (squared magnitudes of the Fourier transform) in Figure 34 represents quasiperiodic behavior. The two peaked values appear at the characteristic frequencies $\omega_1, \omega_2$ of a 2-torus. The remaining frequencies indicate low level noise.

\begin{figure}[h]
\centering
\includegraphics[width=0.5\textwidth]{quasiperiodic_power_spectrum}
\caption{Quasiperiodic Power Spectrum}
\end{figure}

Contrast this with the broad-band spectrum indicative of chaos in Figure 35. The lack of spikes is typically interpreted as aperiodic behavior, either a strange attractor or noise. Unfortunately, once the number of active degrees becomes very large, the method cannot rule out the possibility of high dimensional quasiperiodicity, which also yields a broad-band spectrum. The problem is a technological rather than a theoretical limitation. Equipment capable of isolating a finer grain of frequency values can distinguish chaos from more extreme quasiperiodicity.

\textsuperscript{168}Bergé et al, 145-146.
Other experimental results in favor of the Ruelle-Takens model start with phase space reconstructions. The skeptic cannot, at this point, deny the results of the reconstruction process; he merely questions the interpretation of these results as conclusive evidence for the existence of strange attractors. The skeptic's case is hurt by two characteristics of reconstructed attractors that cannot be accounted for on the Landau-Hopf view. The first is that strange attractors have a fractal dimension, a fact captured by phase space embedding. (Technically, the Poincaré sections of $n$-tori do not display the Cantor set-like cross-section found in dissipative chaos, no matter how large $n$ becomes.\(^{169}\))

Second, nearby quasiperiodic trajectories do not diverge exponentially. In more precise terms, the \textit{Lyapunov exponents} of orbits restricted to an $n$-torus are either 0 or negative. Positive Lyapunov exponents quantify the degree of SDIC for a given evolution. As Bergé \textit{et al.} put it, "[F]inding a positive Lyapunov exponent is an unambiguous signature of a chaotic regime." Not surprisingly, reconstructed strange

\begin{figure}
\centering
\includegraphics[width=0.5\textwidth]{chaotic_power_spectrum.png}
\caption{Chaotic Power Spectrum}
\end{figure}

\(^{169}\)See Tabor 199f. and Bergé \textit{et al.}, 67-70 for more on the differences in Poincaré sections. Also see Moon, \textit{ibid.}, for the detection of fractal dimension in a time series.
attractors have exponentially diverging trajectories and hence positive Lyapunov exponents. A number of sources are available that explain their detection and calculation.\textsuperscript{170}

The hostility faced by early chaos theorists shows that the prediction of strange attractors governing the evolution of certain processes was bold (and dubious). Prior to the discovery of Lorenz and the work of Ruelle, Takens, and Shaw \textit{et al.} at UC Santa Cruz, the conventional wisdom in physics was that complex behavior, like turbulence, required complex deterministic models with many dimensions or stochastic models (the latter being the only option in fields such as biology until 1975 when R.M. May demonstrated that deterministic chaos was sometimes a better choice). Chaotic models showed that it was at least possible that strange attractors represent the evolution of real-world phenomena. Phase space reconstructions decisively confirm this prediction.

6. Conclusion

The skeptic is right in pointing out that since the properties of an attractor are long-time properties, no time-series observed for a finite time can confirm them with certainty. Certainty, however, is unavailable in experimental physics. If that is the standard for confirmation, then no theory or model is confirmed by successful predictions. Unreasonable demands on the amount of evidence required for the confirmation of strange attractors and their properties should be rejected. This is especially true given the lack of any plausible rival account to all that chaos explains. In

short, we have every reason to understand the import of phase space reconstructions in
the same way as researchers in chaos theory, i.e., as direct confirmation of the existence
of strange attractors.
CHAPTER 5

CONCLUSION

As with any monograph of this size, the central thesis can be obscured behind a cloud of arguments used in its defense. In this final chapter, I will begin by reweaving my conclusions into a succinct whole (section 1). From time to time we have also happened upon topics that, although beyond the scope of this work, show promise for future research. These will be summed up in section 2. Section 3 contains some closing remarks.

As I mentioned in the first chapter, the "chaos skeptic" is a composite of worries and criticisms expressed by philosophers, mathematicians, and scientists. No individual is quite as aggressive as my skeptic, with the possible exception of Peter Smith. Nonetheless, I have used this device to expose the philosophically interesting problems lurking behind the overwhelming optimism toward chaos research. Articulating these challenges is an end in itself insofar as certain orthodoxies about model construction and confirmation have been challenged. In the process of developing answers to these problems, a more precise picture of chaos itself has been developed, incorporating strange attractors, fractal structure, SDIC, shadowing, natural and reconstructed phase spaces, etc. As is the case with philosophical treatments of quantum mechanics or general relativity, getting the science right is at least half the battle.
1. Thesis

I take this dissertation to have shown the following. The properties that make chaotic models so intriguing also raise a number of questions. Bridging the gap between theoretician and experimentalist is seldom easy in the mathematical sciences, but the gap is especially large in nonlinear dynamics. First, there is some reason to believe that chaotic models cannot possibly be realistic since they appear to conflict with established physical fact. Second, even if the foundational problems can be overcome, there are worries about whether chaotic models can be empirically confirmed. As we saw in chapter 1, some of these difficulties were recognized by Poincaré and Hadamard. The charge that the state predictions from models subject to SDIC have limited value in experimental physics was leveled a century ago by Duhem.

A clear exposition of the problems is hampered by (often undefined) technical terms. Foremost among these is the seemingly innocuous use of ‘model.’ The wide variety of models used in contemporary dynamics outstrips the philosophical literature. In order to precisely diagnose the issues mentioned in chapter 1, an abridged taxonomy of models in dynamics was presented in chapter 2, including the relations between mathematical models, physical models, and state spaces. The scant philosophical attention paid to scientific modeling in the last twenty years has focused almost exclusively on top down models: models derived from an often intractable law or first principles. Models built up from the data itself were derided as mere curve-fits. This heavy-handed dismissal fails to capture the most important models in experimental chaos, which are built from the bottom up. A new branch was therefore added to the philosophical taxonomy of models including both nonlinear and linear bottom up modeling techniques.

With this preliminary work done, the foundational issues were addressed in chapter 3. The primary skeptical argument, due to Peter Smith, is straightforward. The
stretching and folding found within the dynamics of a strange attractor require a kind of infinite complexity and intricacy. The real world subjects of chaotic models, Smith argues, cannot support this kind of infinite intricacy. Models that contain strange attractors, therefore, cannot be realistic.

The primary way in which strange attractors display infinite intricacy is in their fractal structure. In most of the literature on fractals, concrete examples are in fact merely fractal-like. They do not display unbounded self-similarity since the small scale geometry of matter is limited at the molecular level. In contrast, for strange attractors to be realistic, they must have true Cantor set-like cross-sections. The skeptic's charge is that this structure is physically impossible.

One easy way to resolve the tension is to note a disanalogy between the fractal structure of material objects and attractors: the latter are limit sets in a phase space. Hence the scale at which their self-affine structure is evident is not bounded from below. Still, Smith goes on to argue that unbounded fractal structure is not physically possible even within a phase space. The required intricacy of a strange attractor drives the model into physically impossible states.

We saw in section 3c that the argument appears stronger than it is due to the ambiguity of terms like "infinite intricacy." If the modeler deems that the use of ODE's is warranted, then an \( \mathbb{R}^n \) phase space is required. This (typically) Euclidean space contains a tremendous amount of "intricacy" in the sense that each point corresponds to an ordered \( n \)-tuple of real numbers. It is important to note that whether a given set of points in a phase space is physically possible is completely independent of the behavior of the system, which depends in part on the initial conditions. Attractors can in no way affect the range of possible states for a given system since the attractor itself is simply a special subset of points which is already present in the phase space. That is, physical possibility is captured within the phase space itself, rather than in the more robust phase
portrait. Hence, Smith's claim that a strange attractor is somehow responsible for driving the model into physically impossible states cannot be correct. If the model can take on impossible states, the problem lies in the phase space itself. Entities in the phase portrait cannot affect the underlying space.

It turns out that narrow attacks on the realism of chaotic models are extremely hard to come by. Criticisms that focus on the geometry of chaotic attractors inevitably call into question nonchaotic evolutions, e.g. quasiperiodicity. What is required is an argument to the effect that fractal attractors somehow exploit the structure of phase space in a way that quasiperiodic attractors do not. Since both quasiperiodic and chaotic attractors are topologically transitive—a more precise term than infinitely intricate—it is unlikely that such an argument is forthcoming.

The primary lesson of chapter 3 is this: it is difficult and perhaps impossible to call into question the foundations of chaos without attacking unquestionably realistic areas of contemporary dynamics.

Finally, I considered three broad obstacles to confirming chaotic models. The first is the somewhat unusual notion of discovery employed in chaos research. Chaos is routinely said to be found in the real world, but such claims are often made on the basis of computer simulations and mathematical models rather than observation. The second challenge lies in the fact that many of the essential characteristics of chaotic models are defined only as time approaches infinity. Since experimentalists must settle for a finite number of observations, the skeptic exploits the gap in the available evidence. The third family of problems share a common root, viz. SDIC combined with computer roundoff error and/or experimental noise. The combination blocks a number of model confirmation schemes.

The first step toward a solution is found in the so-called shadowing theorems. These results eliminate the concern that chaos and specifically strange attractors are
merely artefacts produced by roundoff error and numerical methods. Given a mathematical model that meets the conditions of the theorem, chaotic pseudo-orbits in a computer generated phase portrait will be shadowed by at least one true trajectory. Therefore, if a pseudo-orbit carves out a strange attractor in the simulation, shadowing guarantees that a qualitatively similar attractor would be produced if an analytic solution to the mathematical model were available.

With this link forged between the analytic and numerical results, another is needed between the models and real world phenomena. One can be found using William Wimsatt's notion of a robustness result. A robust property is produced when a family of models constructed under a variety of idealizations and assumptions all yield the same result. If a property is robust, it is most likely not an artefact of the idealizations.

A family of robust results, I argued, is found in the three routes to chaos: quasiperiodicity, period doubling, and intermittency. Many mathematical chaotic models undergo changes in their associated parameter spaces that fall into one of these three scenarios. These qualitative changes have also been observed in experimental systems such as the Belousov-Zhabotinsky reaction, Couette flow, and Rayleigh-Bénard convection. The fact that these scenarios are robust and are observed in real world systems supports the claim that chaotic models are realistic. It is unlikely that a given route is a consequence of one or some disparate idealizations.

Finally in section 5, we find direct confirmation of chaos in the form of phase space reconstruction. Although the mathematics that supports this method is somewhat obscure, there is little doubt about its success. Strange attractors, which at one time could be dismissed as nothing more than odd creatures in computer generated phase portraits, can now found in natural phase spaces of real world systems. No other currently available hypothesis captures the data.
2. Future Research

A number of loosely related topics have been touched on that are beyond the scope of this work. I will briefly recap them here as guideposts for future research.

It should seem odd that a chapter on models was needed in a dissertation on chaos theory. Unfortunately, most of the philosophical literature in this area is simply out of date. No philosopher of science other than William Wimsatt has done serious, systematic work on modeling in over two decades. In contrast, a great deal of attention has been given to the logician's notion of model as part of the so-called Semantic View of scientific theories. My proposals in chapter 2 for updating this area were limited to what would be relevant to the discussion in later chapters. At the very least, a more comprehensive account of mathematical models, state spaces, and computer simulations is needed.

A more important development in experimental science is the proliferation of bottom up models (or models of data). I introduced this category in order to make room for phase space reconstructions in chapter 4, but distant (linear time-invariant) cousins of this method have been known since the early 1960's.\(^1\) Calling all such models curve-fits, as the older literature on models does, is far too heavy-handed; an up-to-date treatment is needed. Frederick Suppe, for one, seems to agree.

Standard philosophical analyses . . . shed little insight on the increasingly central roles of models in today's science—developments which call into question traditional philosophical concepts, fundamental dichotomies (e.g., theory or model vs. observation; observation vs. experiment), and standard philosophical accounts of testing and confirmation.\(^2\)


Somewhat parasitic on this lacuna in the treatment of models is the need for a new account of model confirmation. In particular, greater attention needs be paid to nonlinear science: catastrophes, dissipative and conservative nonlinear dynamics, singular perturbations, etc. A single, coherent account of confirmation that covers the whole of linear and nonlinear research is probably not forthcoming, however. Still, giving up on the search for a one-size-fits-all approach would itself be progress.

A less ambitious question focuses on Duhem and Poincaré. To what degree were their philosophies of science motivated by worries about SDIC and the intractability of nonlinear models? At least in Duhem's case, there is no doubt that nonlinearities play a role, as we saw in chapter 1. As the issues become more familiar, their importance will become more evident in the history and philosophy of science.

Finally, chaos and catastrophe theory are thought by some to be part of larger beast known as complexity theory. No one can say what this is exactly, but it attracts a mixture of interest and support from a wide range of fields. Its advocates include biologist Stuart Kauffman, chaos pioneer James Crutchfield, and Nobel Prize winning physicist Murray Gell-Mann. There is now a journal called Complexity while Complex Systems is almost ten years old. The driving principle of this research is that simple, local rules for behavior can produce surprising global complexity. The hope is that this research will eventually provide insight into the nature of emergent properties.

Complexity theory has generated its share of wide-eyed enthusiasm (cf. the short tour of catastrophe theory in chapter 2). Biologist Brian Goodwin hopes to precipitate the overthrow of neo-Darwinism while Kauffman sees his work as the development of a "physics of biology."\textsuperscript{173} The need for an analytic philosopher of science in all this is straightforward: to help separate overzealous confidence form concrete results by

bringing greater precision to the discourse. Furthermore, philosophical progress on reduction and emergence being what it is, the possibility of finding a few gold nuggets in this area seems to be worth the effort.

3. Final Thoughts

When David Ruelle, one of the founding fathers of chaos theory, expresses concerns about the progress of the field he helped launch, the problems are worth looking into. In the end, I believe that what motivates him, Peter Smith, and others is the desire to keep the level of hype in check. Unfortunately the competition for grant money forces researchers to be good at marketing as well. The temptation to exaggerate one's success is great. Ruelle realizes that no one can control embellishments once an idea catches the public's fancy, but one can try to keep a handle on the difference between popular science and peer reviewed research.

The motivation is praiseworthy. Lessons should be learned from the plight of catastrophe theory, a case study of a field failing to keep up with its press releases. The fact is that chaos is not on a par with quantum mechanics and general relativity. Concrete results from nonlinear dynamics are filtering down into undergraduate texts, but only as part of the larger field of mechanics. No paradigm shift is immanent. No older, established theory is in danger of being overthrown.

It seems that chaos might produce a somewhat larger wake in philosophy than in physics. Orthodox doctrines rooted in linear science are being reexamined one by one. The author hopes this work is a step in the right direction.
Bibliography


