PHILOSOPHICAL PROBLEMS IN CHAOS THEORY

BY

EVAN KIRCHHOFF

A Thesis
Submitted to the Faculty of Graduate Studies
in Partial Fulfillment of the Requirements
for the Degree of

MASTER OF ARTS

Department of Philosophy
University of Manitoba
Winnipeg, Manitoba

ISBN 0-612-13252-8

Name ______________________________

Dissertation Abstracts International is arranged by broad, general subject categories. Please select the one subject which most nearly describes the content of your dissertation. Enter the corresponding four-digit code in the spaces provided.

PHILOSOPHY — SUBJECT TERM

0 4 2 2 — SUBJECT CODE

U·M·I

Subject Categories

THE HUMANITIES AND SOCIAL SCIENCES

COMMUNICATIONS AND THE ARTS
Architecture ... 0729
Art History ... 0377
Cinema ... 0900
Dance ... 0378
Fine Arts ... 0357
Information Science ... 0723
Journalism ... 0391
Library Science ... 0399
Mass Communications ... 0708
Music ... 0413
Speech Communication ... 0459
Theater ... 0465

EDUCATION
General ... 0515
Administration ... 0514
Adult and Continuing ... 0516
Agricultural ... 0517
Art ... 0273
Bilingual and Multicultural ... 0282
Business ... 0688
Community College ... 0275
Curriculum and Instruction ... 0727
Early Childhood ... 0518
Elementary ... 0524
Finance ... 0277
Guidance and Counseling ... 0519
Health ... 0680
Higher ... 0745
History of ... 0520
Home Economics ... 0278
Industrial ... 0521
Language and Literature ... 0279
Mathematics ... 0280
Music ... 0522
Philosophy of ... 0998
Physical ... 0523
Psychology ... 0525
Reading ... 0535
Religious ... 0527
Sciences ... 0714
Secondary ... 0533
Social Sciences ... 0534
Sociology of ... 0340
Special ... 0529
Teacher Training ... 0530
Technology ... 0710
Tests and Measurements ... 0288
Vocational ... 0747

LANGUAGE, LITERATURE AND LINGUISTICS
Language
- General ... 0679
- Ancient ... 0289
- Linguistics ... 0290
- Modern ... 0291

Literature
- General ... 0401
- Classical ... 0294
- Comparative ... 0295
- Medieval ... 0297
- Modern ... 0298
- African ... 0316
- American ... 0591
- Asian ... 0305
- Canadian (English) ... 0352
- Canadian (French) ... 0355
- English ... 0593
- Germanic ... 0311
- Latin American ... 0312
- Middle Eastern ... 0315
- Romance ... 0313
- Slavic and East European ... 0314

PHILOSOPHY, RELIGION AND THEOLOGY
Philosophy ... 0422
Religion
- General ... 0318
- Biblical Studies ... 0321
- Clergy ... 0319
- History of ... 0320
- Philosophy of ... 0322

Theology ... 0469

SOCIAL SCIENCES
American Studies ... 0323
Anthropology
- Archaeology ... 0324
- Cultural ... 0326
- Physical ... 0327

Business Administration
- General ... 0310
- Accounting ... 0272
- Banking ... 0770
- Management ... 0454
- Marketing ... 0338

Canadian Studies ... 0385
Economics
- General ... 0501
- Agricultural ... 0503
- Commerce-Business ... 0505
- Finance ... 0508
- History ... 0509
- Labor ... 0510
- Theory ... 0511

Folklore ... 0358
Geography ... 0366
Gerontology ... 0351
History
- General ... 0578
- Ancient ... 0579
- Medieval ... 0581
- Modern ... 0582
- Black ... 0328
- African ... 0331
- Asia, Australia and Oceania 0332
- Canadian ... 0334
- European ... 0335
- Latin American ... 0336
- Middle Eastern ... 0333
- United States ... 0337

History of Science ... 0585
Law ... 0398
Political Science
- General ... 0615
- International Law and Relations ... 0616
- Public Administration ... 0617

Recreation ... 0814
Social Work ... 0452
Sociology
- General ... 0626
- Criminology and Penology ... 0627
- Demography ... 0938
- Ethnic and Racial Studies ... 0631
- Individual and Family Studies ... 0628
- Industrial and Labor Relations ... 0629
- Public and Social Welfare ... 0630
- Social Structure and Development ... 0700
- Theory and Methods ... 0344

Transportation ... 0709
Urban and Regional Planning ... 0999
Women's Studies ... 0453

THE SCIENCES AND ENGINEERING

BIOLOGICAL SCIENCES
Agriculture
- General ... 0473
- Agronomy ... 0285
- Animal Culture and Nutrition ... 0475
- Animal Pathology ... 0476
- Food Science and Technology ... 0359
- Forestry and Wildlife ... 0478
- Plant Culture ... 0479
- Plant Pathology ... 0480
- Plant Physiology ... 0817
- Range Management ... 0777
- Wood Technology ... 0746

Biology
- General ... 0306
- Anatomy ... 0287
- Biostatistics ... 0308
- Botany ... 0309
- Cell ... 0379
- Ecology ... 0329
- Entomology ... 0353
- Genetics ... 0369
- Limnology ... 0793
- Microbiology ... 0410
- Molecular ... 0307
- Neuroscience ... 0317
- Oceanography ... 0416
- Physiology ... 0433
- Radiation ... 0821
- Veterinary Science ... 0778
- Zoology ... 0472

Biophysics
- General ... 0786
- Medical ... 0760

EARTH SCIENCES
Biogeochemistry ... 0425
Geochemistry ... 0996
Geodesy ... 0370
Geology ... 0372
Geophysics ... 0373
Hydrology ... 0388
Mineralogy ... 0411
Paleobotany ... 0345
Paleoecology ... 0426
Paleontology ... 0418
Paleozoology ... 0985
Palynology ... 0427
Physical Geography ... 0368
Physical Oceanography ... 0415

HEALTH AND ENVIRONMENTAL SCIENCES
Environmental Sciences ... 0768
Health Sciences
- General ... 0566
- Audiology ... 0300
- Chemotherapy ... 0992
- Dentistry ... 0567
- Education ... 0350
- Hospital Management ... 0769
- Human Development ... 0758
- Immunology ... 0982
- Medicine and Surgery ... 0564
- Mental Health ... 0347
- Nursing ... 0569
- Nutrition ... 0570
- Obstetrics and Gynecology ... 0380
- Occupational Health and Therapy ... 0354
- Ophthalmology ... 0381
- Pathology ... 0571
- Pharmacology ... 0419
- Pharmacy ... 0572
- Physical Therapy ... 0382
- Public Health ... 0573
- Radiology ... 0574
- Recreation ... 0575
- Speech Pathology ... 0460
- Toxicology ... 0383

Home Economics ... 0386

PHYSICAL SCIENCES

Pure Sciences
Chemistry
- General ... 0485
- Agricultural ... 0749
- Analytical ... 0486
- Biochemistry ... 0487
- Inorganic ... 0488
- Nuclear ... 0738
- Organic ... 0490
- Pharmaceutical ... 0491
- Physical ... 0494
- Polymer ... 0495
- Radiation ... 0754

Mathematics ... 0405
Physics
- General ... 0605
- Acoustics ... 0986
- Astronomy and Astrophysics ... 0606
- Atmospheric Science ... 0608
- Atomic ... 0748
- Electronics and Electricity ... 0607
- Elementary Particles and High Energy ... 0798
- Fluid and Plasma ... 0759
- Molecular ... 0609
- Nuclear ... 0610
- Optics ... 0752
- Radiation ... 0756
- Solid State ... 0611

Statistics ... 0463

Applied Sciences
Applied Mechanics ... 0346
Computer Science ... 0984
Engineering
- General ... 0537
- Aerospace ... 0538
- Agricultural ... 0539
- Automotive ... 0540
- Biomedical ... 0541
- Chemical ... 0542
- Civil ... 0543
- Electronics and Electrical ... 0544
- Heat and Thermodynamics ... 0348
- Hydraulic ... 0545
- Industrial ... 0546
- Marine ... 0547
- Materials Science ... 0794
- Mechanical ... 0548
- Metallurgy ... 0743
- Mining ... 0551
- Nuclear ... 0552
- Packaging ... 0549
- Petroleum ... 0765
- Sanitary and Municipal ... 0554
- System Science ... 0790

Geotechnology ... 0428
Operations Research ... 0796
Plastics Technology ... 0795
Textile Technology ... 0994

PSYCHOLOGY
General ... 0621
Behavioral ... 0384
Clinical ... 0622
Developmental ... 0620
Experimental ... 0623
Industrial ... 0624
Personality ... 0625
Physiological ... 0989
Psychobiology ... 0349
Psychometrics ... 0632
Social ... 0451

PHILOSOPHICAL PROBLEMS IN CHAOS THEORY

BY

EVAN KIRCHHOFF

A Thesis submitted to the Faculty of Graduate Studies of the University of Manitoba in partial fulfillment of the requirements of the degree of

MASTER OF ARTS

ABSTRACT

This thesis is an attempt to explore the philosophical issues within the mathematical sub-discipline popularly known as "chaos theory". I begin by detailing the problems in formulating a single set of sufficient and necessary conditions for a system to count as "chaotic". I argue that the available explicit and implicit definitions of "chaos" suggest that the term is used to denote two different (and not necessarily coextensive) concepts: a novel form of unpredictability within deterministic systems, and the surprising emergence of order within apparently complex systems. I devote Chapters 1 and 2 to disambiguating each of these concepts, using a variety of popular, technical, and philosophical texts, and then argue that each of these concepts presents further internal problems for any effort to formulate an adequate set of criteria for "chaos". However, I also assert that attempts to find a unified definition of "chaos", as a property that either does or does not inhere in a given system, are ultimately misguided, as such a definition appears both impossible and unhelpful.

In Chapter 3, I survey the remaining primary problems evident in the recent philosophical literature on "chaos theory". These include questions about the relationship between "chaos" and determinism, the question of whether "chaos theory" represents a new form of scientific methodology, and the question of how to reconcile the postulates of "chaos" with those of quantum mechanics.

Finally, Chapter 4 consists of a brief survey of some recent attempts to combine chaos theory with literary theory. I argue that attempts to draw causal connections between the concepts of chaos and "postmodern" critical theories involve highly dubious empirical and conceptual claims, and that attempts to apply chaos theory as a "methodology" by which literary texts can be criticized typically produce nothing more than forced and often superfluous metaphors.

Special thanks to Professor Carl Matheson, who has been the primary influence on my philosophical methodology to date, and whose advice during this thesis, and during the undergraduate and graduate coursework that preceded it, has been invaluable.

Also, thanks to Professor Robert Thomas, for his considerable help in empirical matters of mathematics, and for his careful reading of my thesis. Any remaining linguistic errors are mine.

Finally, thanks to Professor Michael Stack, for his reassuring comments on my thesis.

I received additional assistance on specific points from Professor James Joyce of the University of Michigan, and from Professor Yvon Gauthier of the University of Montreal.

I am also grateful to my parents for being supportive and tolerant during the final stages of this thesis, during which I was forced to abandon many of the responsibilities of everyday life.

LIST OF FIGURES

INTRODUCTION: WHAT ARE THE PROBLEMS OF CHAOS THEORY?

"Chaos" is supposed to be an important and unique phenomenon, and the arrival of "chaos theory" on the scientific landscape is supposed to have been a monumental event. Some writers, such as the philosopher Stephen Kellert (1993, 104) hold that chaos theory represents an important overhaul in dominant scientific methodology. Others, most notably literary theorist Katherine Hayles (1989; 1990; 1991), take even stronger positions and insist that it signifies a conceptual revolution in modern culture as a whole. And chaos theory has certainly received its share of hype from all sides; one recent introductory textbook on the subject calls it "the third great scientific revolution of the 20th century, along with relativity and quantum mechanics" (Devaney 1989, v). Chaotic behaviour has been famously invoked in explanations of why some physical systems (such as the weather, or even simple models of the weather) seem impenetrably random despite our apparent understanding of the underlying phenomena (Lorenz 1963). Conversely, chaos theory has been used to assert that other systems (such as urban epidemics, or dripping faucets) seem to demonstrate surprising and novel forms of order (Stuart 1989, 276; Crutchfield 1986, 56).

Many of the claims that chaos possesses either new conceptual implications or unique explanatory power seem to presuppose that there must be some adequate definition of "chaos"; however, the first two chapters of this thesis will be devoted to arguing that no such unified definition is possible, for a number of reasons. First of all, while both philosophers and scientists have formulated a variety of attempted definitions, either explicitly or implicitly, these attempts can generally be shown to fall into one of two camps. This disjunction seems to exist because "chaos" is intended to capture two distinct and opposite phenomena: on one hand, the ability of simple deterministic systems to produce apparently random and unpredictable behaviour, and on the other, the ability of apparently random behaviour to demonstrate emergent order and stability when observed in the appropriate way. Thus, the idea of "chaos" can apparently embody both the surprising production of disorder and the equally surprising production of a different sort of order.

However, despite the fact that both concepts may be discussed under the common heading of "chaos theory", they are conceptually separable, and these two senses of "chaos" seem to play quite different roles in both scientific explanations and philosophical arguments. Furthermore, although it is often proposed that any "chaotic" system must embody both types of phenomena -- or even that one is a physical consequence of the other -- the phenomena themselves have rather different criteria by which their presence can be established, and it often appears to be difficult to apply both sets of criteria to the same candidate system. Finally, as I will argue, internal problems exist even within each of these two senses of "chaos" that prevent the formulation of any rigourous definition that could be used to classify various systems objectively.

But the lack of a definition of "chaos" should not be considered a scandal within the field of mathematics itself. In fact, it cannot even be considered a secret; for example, the introductory textbook cited above later admits in passing that there are "many possible definitions". (Devaney 1989, 50) And it seems fairly obvious that nothing would be gained at this point from a single, rigourous definition, given that each of the disparate concepts currently involved are arguably useful in themselves. However, it would seem to be profitable for individuals outside the field (e.g., the philosopher Robert Batterman [1993] and the physicist Joseph Ford, cited by Batterman) to consider the idea that "chaos" arguably is not an objective metaphysical property inherent in some systems, but should instead be viewed as nothing more than a loose array of subjective criteria, coupled with some useful modelling techniques for complex natural phenomena.

This thesis is also intended as a survey of the primary chaos-related issues which have emerged In the philosophical literature. Some of the relevant papers fall within the definitional issues discussed above; in my third chapter, I will turn to the issues presented in the remaining philosophical literature. These include: the relationship between chaos and determinism; the question of whether chaos embodies an epistemological shift in the methodology of the scientists who employ it; the question of whether the "sensitive dependence" said to be a property of chaotic systems is conceptually incoherent; and the apparent contradictions between chaos theory and quantum mechanics.

In my last chapter, I will consider some of the primary conceptual problems that emerge from the accounts of literary theorist Katherine Hayles, and from other authors who invoke "chaos theory" in their study of literary texts. Although Hayles is arguably the single largest influence in the contemporary sub-genre defined by the intersection of chaos theory and literary theory, many of the positions she takes appear highly debatable upon close examination. Similarly, the problems exhibited by current attempts to apply "chaos theory" to works of literature raise substantial questions about whether such a project can ever be appropriate or useful.

1. CHAOS DEFINED AS A NOVEL FORM OF UNPREDICTABILITY

Many of the commonly invoked criteria for chaos appear to be centered around the idea that "chaotic" systems can be distinguished by the drastic predictive problems they create. For example, philosopher Mark Stone states that

> [c]haotic systems represent a distinctive subset of classical dynamical systems....The most natural way to bring out that distinction is in the context of predictability, and indeed distinguishing them on the basis of predictability is already the working assumption of most scientists. (Stone 1989, 129)

And indeed, this appears to reflect a common understanding of "chaos"; for example, a very recent article on chaos in *Physics Today* begins by noting that

> Scientists in many fields are recognizing that the systems they study often exhibit a type of time evolution known as chaos. Its hallmark is wild, unpredictable behavior, a state often perplexing and unwelcome to those who encounter it. (Ott and Spano 1995, 34)

It is important to note that if chaos is to signify anything new or unique on the scientific landscape, then the predictive failures of "chaos" must not arise merely because the candidate systems are extremely complicated. Our practical inability to deal quantitatively with vast sets of simultaneous variables is hardly news; for example, in the field of thermodynamics, statistical generalizations and other unrealistic simplifying assumptions have long been used in cases where detailed descriptions of individual particles would be an impossible computational task. What is supposed to be novel about chaos is that we have now encountered relatively simple systems -- systems with very few variable quantities -- whose guiding principles can be easily and completely specified, yet whose long-term behaviour remains impenetrable. This unpredictability is said to be momentous because its imposed limit on our knowledge represents a defeat of the dearly-held hope of modern science that every detail of the universe (with perhaps a few exceptions in special cases, such as the realm of quantum mechanics) would eventually yield to our predictive abilities. For example, literary theorist Katherine Hayles enthuses that chaos

> embodies assumptions that bring into question presuppositions that have underlain scientific conceptualizations for the last three hundred years....Changed are not the disciplinary procedures and criteria of normal science but the epistemic ground on which it -- and much else in contemporary culture -- rests. (1990, 16)

Similarly, in a book on chaos for general audiences, A. B. Cambel holds that

> [i]t was generally believed that classical Newtonian mechanics is inadequate only in the cases of both very small particles where quantum mechanics is appropriate and very large bodies where relativistic mechanics applies. The recent developments in the study of complexity introduce new considerations. (p.21)

In the following sections, I will consider the various criteria for chaos that have been proposed in previous attempts to more precisely define this novel unpredictability. As I will show, all of these criteria appear to be either too broad, in that they include at least some systems which fail to pose novel predictive problems; too narrow, in that they eliminate some systems which do pose these predictive problems (including systems widely considered to be paradigm cases of "chaos"); or simply too vague, in that they fail to provide an objective criterion for distinguishing chaotic systems from non-chaotic systems, even in principle. Furthermore, apart from these metaphysical problems, many of these criteria possess the troublesome epistemological property of being difficult or impossible to verify -- especially in the rigorous sense demanded by the standards of mathematical proof; I will also discuss such problems as they arise.

(A) The Criterion of Sensitive Dependence on Initial Conditions

(i) The Butterfly Effect

The "Butterfly Effect" is an often-repeated anecdote which is said to illustrate the uniquely problematic nature of ostensibly chaotic systems -- for example, the Earth's weather. Peter Smith offers a typical formulation:

> A small blue butterfly sits on a cherry tree in a remote province of China. As is the way of butterflues, while it sits it occasionally opens and closes its wings....and the miniscule difference in the resulting eddies of air around the butterfly makes the difference between whether, two months later, a hurricane sweeps across southern England or harmfully dies out over the Atlantic. (p.247)

Similary, any chaotic system is supposed to exhibit this "sensitive dependence on initial conditions". This attribute is supposed to be interesting because, when combined with our necessary lack ot complete and accurate knowledge about the present state of any natural system, it produces a radical failure in our ability to predict the detailed long-term behaviour of many systems -- including systems that we had dearly hoped to de-mystify, or even control; the weather is again the most obvious example. We can never obtain a perfectly complete and absolutely accurate description of the Earth's weather at any given time, and so if it truly possesses this sensitive dependence on our errors, our long-term predictions are effectively no better than random guesses. (The question of whether the hypothesis of the "Butterfly Effect" is actually internally incoherent will be discussed in chapter 3.)

(ii) Sensitive Dependence in the Lorenz Equations

Historical accounts of chaos (e.g., Gleick, Stewart, Hayles, Crutchfield, Kellert, etc.) typically offer the simple 3-equation "Lorenz system" as a concrete example of the above phenomenon. This mathematical model, which is arguably the system most commonly cited as a paradigm case of "chaos", was introduced by Edward Lorenz in 1963, in a now famous meteorology paper entitled "Deterministic Nonperiodic Flow". In this paper, Lorenz begins with a basic model of air currents in the Earth's atmosphere -- calling it "a simplification of one derived by Saltzman (1962) to study finite-amplitude convection", while admitting that his "present interest is in the...nature of its solutions, rather than in its contributions to the convection problem" (Lorenz 134). Lorenz's schematic model consists of three now famous differential equations containing three dependent variables (x, y, and z), an independent variable (t, representing time) and three constants (10, 28, and 8/3 are the constant values used by Lorenz (136)); these equations can be expressed as follows:

$$dx/dt = -10x + 10y$$

$dy/dt = 28x - y - xz$

$dz/dt = 8z/3 + xy$ (Lorenz, p. 135-6, simplified in Stewart, p.136)

(The notation "dx/dt" signifies the derivative of x with respect to time; the meaning of derivatives and differentiation will be explained shortly.)

Through an approximate process known as "iteration", the above system can be used to provide an evolving model of convection: first, some arbitrary initial values are chosen for the variables x, y and z, to represent the "initial conditions" of the system. These values are inserted wherever "x", "y" and "z" appear in the right-hand sides of the three equations, each of which is then evaluated. The three results are then taken as the changes in the values for x, y, and z, respectively[1], and the resulting values of these variables are once again substituted for "x", "y", and "z" on the right-hand side of each equation. ("Iteration" refers to any such repetitive process of using the previous values of variables to compute their succeeding values.) Over time, the evolving values of these three variables can be plotted on a graph, to trace out a 3-dimensional path that, in a rough and qualititive way, resembles the motion of certain air currents in the Earth's atmosphere (see Fig. 1) -- although it should be noted that the resulting graph (known as a "phase space" graph) is not a representation of paths in 3-dimensional space. Rather, Lorenz's three varables X, Y, and Z represent more abstract properties. As explained by Lorenz:

> In these equations X is proportional to the intensity of the convective motion, while Y is proportional to the temperature difference between the ascending and descending currents, similar signs of X and Y denoting that warm fluid is rising and cold fluid is descending. The variable Z is proportional to the distortion of the vertical temperature profile from linearity, a positive value indicating that the strongest gradients occur near

the boundaries. (Lorenz 135)

However, the properties actually represented by these variables will be unimportant to the discussion that follows; "sensitive dependence" can appear in quantitative data independently of whether the data represent values in space, temperature, or any other empirical property.

Lorenz apparently constructed this 3-equation system as a simpler way of reproducing some odd mathematical behaviour he had observed in a slightly more complicated model of the weather. While using a computer to calculate the sequence of evolving values in the previous system, Lorenz had, purely by accident (according to Gleick), made the surprising discovery that small differences in the initial values he fed into the system, before the repetitive process of iteration began, led to large-scale changes later in the system's output sequence. In other words, that system, as well as the 3-equation system above, exhibited a dramatic sensitivity to initial conditions (Gleick 14-24). In his paper, Lorenz deduces that his system (along with any other possible "finite system of ordinary differential equations representing forced dissipative flow") can have no stable solutions. A "solution" to this system would be a unique path of evolution determined by a set of initial values for x, y, and z. A "stable" solution would be a path determined by a set of initial values for which values that deviated slightly would determine a relatively similar path. In contrast, each solution for Lorenz's system apparently exhibits instability, which is defined by Lorenz as the property that "solutions temporarily approximating it do not continue to do so". (141) Lorenz also notes that "almost all of [the solutions] are nonperiodic" (136), where "periodic" would refer to a possible path for the system that would repeat itself endlessly and at regular intervals.

Lorenz inferred these general conclusions about his system by observing the patterns in the system's successive peaks for its Z-values. If a graph is constructed in which each data point consists of one of these maximum values of Z plotted against the previous maximum value of Z, a very regular and predictable pattern emerges (see Fig. 2), embodying the underlying structure in Lorenz's quantitatively unpredictable system (this underlying structure will be important to the question of whether or not chaos effectively equals randomness, discussed below). From the shape of this graph, Lorenz deduced that, while there appeared to be a certain number of periodic solutions (solutions for which the sequences of maximum Z-values would repeat themselves indefinitely), any solutions that were even slightly different would result in sequences that would diverge from these periodic sequences exponentially over time. He also noted that there were only a finite number of these periodic solutions, with every other possible set of initial conditions resulting in a nonperiodic solution -- in other words, that nonperiodic solutions infinitely outnumbered periodic solutions. And each of these nonperiodic solutions would also be unstable, for the same reason that the periodic solutions were unstable (Lorenz 138-40).

In other words, Lorenz's system appears to be plagued by two different causes of unpredictability -- instability plus nonperiodicity. Its instability means that nearly-identical conditions will produce radically different paths of evolution (making the system's particular path difficult to predict ahead of time), and its general nonperiodicity means that most possible paths of evolution will not repeat themselves (making the system's future behaviour difficult to predict through the alternative method of simply observing its previous behaviour along the same path). The combined effect is that the system's future behaviour can only be predicted by determining precisely which possible path the system is following -- information which can only be obtained by knowing the exact values that the system started with.

Thus, in any real-world system with this kind of sensitivity, correct long-term predictions could only be made if the initial conditions were known fully and precisely, and it is obvious that in real-world cases, utterly precise knowledge is essentially impossible. Lorenz concludes his paper by stating that "[i]n view of the inevitable inaccuracy and incompleteness of weather observations, precise very-long-range forecasting would seem to be non-existent", while cautioning that his qualitative model proposes nothing about what span of time would actually count as "very-long-range forecasting", which could "conceivably...be a few days or a few centuries." (Lorenz 141)

(iii) Sensitive Dependence in the Real World

It is important to remain conscious of the logical leap implicit in Lorenz's conclusion. Lorenz's iterated mathematical model has been plausibly shown to exhibit sensitive dependence; some evidence of this property can be obtained simply by running the iterative process over again with slightly differing initial conditions and watching the divergence grow progressively larger; also, Lorenz's observations of the patterns in the system's maximum Z-values offer a compelling argument that this property will be common to virtually all possible initial values of the model. And the equations of Lorenz's model were based on simplified forms of equations which he appears to consider relatively uncontroversial in themselves as a model of convection, so Lorenz concluded that the Earth's atmosphere must also share his model's property of sensitive dependence. But this is not a rigorous proof; as Smith notes:

> Admittedly, it is a big leap from the observation that the Lorenz model exhibits the butterfly effect to the claim that more realistic models of the atmosphere will show the same kind of sensitive dependence on initial conditions. For it is another truth of dynamical systems...that even small changes to the equations in a model can radically affect how the model works.... (Smith 255)

However, there is no way to verify the assertion of sensitive dependence in the atmosphere through a more direct test. The ideal method of verifying sensitive dependence in the weather would be to observe the evolution of the same weather system from slightly different initial states -- but since there is no way to manually rearrange the weather into the desired initial states (and, as previously noted, no way to tell precisely what state the weather is in anyway), such an experiment is impossible. It is theoretically possible that the atmosphere could perform this experiment for us by coincidentally entering a state similar to some previously observed state, so that we could then observe the differing paths of evolution that succeeded the two similar states, but as Lorenz notes, "the variety of possible atmospheric states is so immense that analogues need never occur." (141)

There are, however, less complex real-world systems for which such experiments can be performed. For example, the evolving motion of a "double pendulum" (a pendulum suspended from the bottom of another pendulum) is known to be sensitively dependent (Kellert 1993, 88-9), and it can be placed in slightly differing initial positions which can then be observed to evolve in quite different ways[2]. But one of the values of chaos theory is supposed to be that it can illuminate rather more complicated real-world systems, including many systems where sensitive dependence cannot be directly verified by such controlled experiments, such as the weather or the irregular orbit of the moon of Saturn known as Hyperion (e.g., Hobbs 1993, 120-1). In fact, there is another sense of "chaos" under which chaotic behaviour can be more directly verified in such systems, but this is the second form of chaos, and will be discussed in the next chapter.

The hypothesis that a complicated real-world system like the weather is sensitively dependent implies that no matter how well the dynamics of the system are understood, no humanly possible amount of data-gathering can ever produce successful long-term predictions. The primary observational prediction of this real-world sensitive dependence is that the accuracy of any possible theoretical predictions will degrade over time, eventually to the point of becoming wildly incorrect and useless. However, this could also be the primary observational result of a variety of other hypothetical things -- for example, a fundamentally defective theory, a computational mistake, or a failure in the experimental apparatus. It is true that scientists confronted with quantitative failures in their predictions have always had to choose among assumptions of theoretical inadequacy, equipment defects, and other possible options; what is unusual about the hypothesis of sensitive dependence in the real world is that it seems to predict nothing but predictive failure. Additionally, the standards for evaluating models of sensitively dependent systems seem necessarily weaker than the general standards by which scientific models are judged. Neils Bohr's model of the atom was judged to be successful when its quantitative predictions were repeatedly found to resemble values recorded in the real world (and later judged to be inadequate when its quantitative predictions failed in more complicated cases); but these standards are unavailable to models of sensitively dependent systems.

Of course, it is trivially true that the existence of many of the common entities and phenomena of modern science can be "verified" only indirectly; for example, it seems fair to say that electrons cannot be "observed" in the same sense that the larger material objects of everyday life can be (although the criteria for what should count as a "theoretical object" are actually somewhat contentious, and will not be examined further here). Instead, our belief in the existence of electrons is commonly said to be based on some "inference to the best explanation" -- meaning that, given certain empirical observations, the theory that postulates the existence of electrons seems to explain these observations better than do theories which postulate other things (again, the question of how one theory offers a "better" explanation than another has long been a contentious issue, involving debate about which metatheoretical virtues of rival theories are, or should be, invoked in theory choices; this is another topic on which much has been written elsewhere). But the hypothesis of sensitive dependence in the real world seems to involve another type of inference altogether. Sensitive dependence is not offered as the "best explanation" for observed empirical data, simply because the primary empirical consequences of sensitive dependence would be incorrect predictions. At best, the inference is rather more indirect: a given dynamical model is offered as the "best explanation" for some kind of observed behaviour (for example, the Saltzman equations on which Lorenz's model is based embody previous hypotheses about the effects that mixed hot and cold fluids have on each other (Lorenz 1963, 135-7)); the model is independently observed to exhibit sensitive dependence; finally, it is inferred that the real system must also possess sensitive dependence.

It is sometimes suggested that the sensitive dependence proposed by chaos theory can be more directly verified by watching for the characteristic way that chaotic systems multiply errors; for example, Stone claims that

> [i]t helps that the rate of error amplification in chaotic systems is essentially constant. Indeed this constancy is an important empirical tool in distinguishing noise due to chaos from other noise sources. (1989, 127)

However, this can only work for real-world systems (such as the double pendulum mentioned earlier) which are so simple that we can model them fully enough to expect some kind of quantitative correspondence between model and reality (at least initially; see endnote 2). In a complicated system like the Earth's atmosphere, in which our models are necessarily so simplified that we can expect only qualitative correspondence with reality, there is no "error" to observe -- the difference between the output of the model and our empirical data from the real world will be large, arbitrary, and perhaps impossible to obtain at all, due to the lack of real-world units in the model (for example, Lorenz's mathematical data lack any stated units of velocity or temperature; there is no sense of the scale at which his model is intended to operate, if indeed this matters to him at all).

Worse yet, it may even be impossible to verify the existence of sensitive dependence in mathematical models. In a short introductory article on chaos, mathematician Morris W. Hirsch discusses the frequently cited conclusions by Lorenz:

> By computer simulation Lorenz (1963) found that trajectories seem to wander back and forth...in a random, unpredictable way. Trajectories which start out very close together eventually diverge, with no relationship between long run behaviors.
>
> But this type of chaotic behavior has not been *proved.* As far as I am aware, practically nothing has been proved about this particular system. (Hirsch 1985, 191)

Hirsch notes that there are other systems for which such properties have been rigorously proven to exist, but considers it "something of a scandal" that nobody has been able to provide such proof in the case of the frequently cited Lorenz system, despite the fact that Lorenz himself stated that "[t]hese conclusions have been based upon a finite segment of a numerically determined solution. They cannot be regarded as mathematically proven." (1963, 140)

There appear to be two separate problems with conclusively demonstrating the behavioural properties of such a system, both of which Lorenz alludes to in the above quote. The first problem is the necessity, already discussed, of investigating such systems from behind layers of approximation: both the approximate numerical methods needed in cases where the system can't be integrated precisely, and the artificial truncation imposed by the computers involved in the exercise. The second problem is due to the fact that only a finite portion of the system's evolution can be observed in a finite amount of time -- and therefore, as mathematicians C. A. da Costa and F. A. Doria assert, "[t]here is always the chance that the system is undergoing a very long and complicated transient state, before it settles down to some nice and regular behavior." (1992, 73) Again, there are special systems whose complex behaviour has apparently been proven to be non-repeating no matter how far into the future the system evolves; for example, according to mathematician Ian Stewart, the system defined by the Henon map (see subsection (iv) below and sections (B) and (C) below) has been shown not to be merely "something with a very long period" (1989, 184). But in general, there appear to be sufficient grounds for a healthy scepticism about whether the behaviour of systems observed for a finite period of time can be generalized to all possible times.

(iv) The Lyapunov Exponent: An Objective Definition of Sensitive Dependence?

Sensitive dependence, even in cases where it could be directly and unproblematically observed, might seem like a mildly subjective criterion (who defines what counts as "sensitive"?) but there is at least one objective mathematical definition for it: the presence of a "positive Lyapunov exponent"[3]. The Lyapunov exponent for a given initial point in the system can be evaluated by observing how an infinitesimally small change in initial conditions (known as "$\delta x(0)$") expands over time t to a larger difference (known as "$\delta x(t)$"). The Lyapunov exponent ("L") can be roughly defined[4] as follows (the equality in the following formula is approximate):

$$|\delta x(t)| = e^{Lt} \, |\delta x(0)| \quad \text{(e.g., in Ruelle 1994, 25)}$$

As explained by mathematical physicist David Ruelle, if the value of L is greater than zero, "the [adjacent] orbits diverge exponentially, and we have sensitive dependence on initial conditions" (Ruelle 25); obviously, for this "sensitive dependence" to be a property of the system, this exponent should be positive for virtually any initial points, rather than merely a few atypical points. The Lyapunov exponent seems to be widely offered as a good indicator of chaos; for example, Cambel states that "[p]erhaps the most terse way of pronouncing a system to be chaotic is to determine that there are positive Lyapunov coefficients" (1993, 209; similarly approving declarations can be found in Stewart 1989, 106 and Gleick 1987, 253)

However, note that this criterion requires that we obtain precise quantitative data regarding both the change in the initial conditions, and the degree of subsequent change that this initial change creates. Such data can only be obtained from systems where the underlying equations are known -- in other words, from mathematical models. The Lyapunov exponent will be impossible to evaluate in complex real-world systems where it will be impossible to observe the differing results of similar initial conditions, because similar initial conditions need never occur.

One possible suggestion at this point is that scientists might hope to construct a "controllable" version of the weather in the laboratory, in which they could perform controlled experiments by running successive trials with slight differences in initial conditions. However, quantitative conclusions from such a hypothetical "toy" version of the weather will be every bit as suspect as quantitative conclusions from the simplified Lorenz model, due to the fact that the Lorenz system (and therefore any real system whose dynamics qualitatively resemble it) is not "structurally stable". A structurally stable system is one whose phase-space evolution would exhibit the same topological properties even if its dynamics were slightly altered -- for example, through the simplification inherent in a model or in a reduced-scale laboratory version of the real system. As Devaney explains:

> ...if the dynamical system in question is not structurally stable, then the small errors and approximations made in the model have a chance of dramatically changing the structure of the real solution to the system. That is, our 'solution' could be radically wrong or unstable. If, on the other hand, the dynamical system in question is structurally stable, then the small errors introduced by approximations and experimental erors may not matter at all: the solution to the model system may be equivalent or topologically conjugate to the actual solution. (53)

Devaney holds that there is good reason to suspect not only that the Lorenz system is unstable in this sense, but that many other systems are as well:

> ...most dynamical systems that arise in classical mechanics are not structurally stable. There are also simple examples of systems such as the Lorenz system from meterology that are 'far' from being structurally stable. These systems cannot even be approximated...by stable systems. (53)

Therefore, while it can be hypothesized that the weather possesses dynamics which are in some way qualitatively similar to the Lorenz model, this same hypothesis makes it impossible to quantitatively test for properties such as the Lyapunov exponent in the weather itself by constructing controllable versions of it, since nothing can count as a topologically valid model for an unstable system. Thus, it will often be the case that the most objective-sounding test for sensitive dependence can at best be applied to the real world only via an extra layer of inference from mathematical models.

And in fact, the Lyapunov exponent can also be difficult to evaluate even in mathematical models. As Ruelle states, investigating the divergence from nearby points in a single area is not enough; it must be demonstrated that this divergence exists across the entire system. For the proper sort of system (which Ruelle defines as one which "comes back close to the same points [in phase space] again and again" (1994,24); this further criterion for chaos is discussed in the next subsection), the Lyapunov exponent will tend to be "the same almost everywhere" -- but even so, finding its actual value may be difficult:

> ...it is rare that one can prove the existence of chaos analytically. The standard way to show that a dynamical system is chaotic is by computer study and numerical estimation of [the Lyapunov exponent] L. Finding that L > 0 numerically [i.e., through estimation] does not prove mathematically that the behavior of the dynamical system is chaotic. (1994, 26)

The Lyapunov exponent does have the advantage of providing an objective criterion for what it is for a mathematical system to be chaotic; it reflects properties that indisputably exist within a given mathematical system, whether or not they can actually be seen. However, as a tool for empirically verifying the existence of chaos according to strict mathematical criteria, it may possess no special virtues.

(v) The Inadequacy of Sensitive Dependence

Chaos, considered as the production of a new form of impenetrable disorder, must arguably be more than just sensitive dependence, however. It is true that sensitive dependence is often loosely equated with "chaos"; for example, in a lengthy paper on chaos theory in *Reviews of Modern Physics*, J. P. Eckmann and David Ruelle write that

> [s]ensitive dependence on initial conditions is also expressed by saying that the system is *chaotic* (this is now the accepted use of the word *chaos*, even though the original use by Li and Yorke (1975) was somewhat different). (624)

Similarly, a recent *Scientific American* article on complexity theory mentions "chaos" in passing, casually defining it as the study of "systems that display...sensitive dependence to initial conditions and other mathematically defined behavior" (Horgan 1995, 109). And philosopher Stephen Kellert defines "chaotic unprectability" as something which occurs in "systems with sensitive dependence on initial conditions", due to their tendency to "produce random or chancy behavior" (1993, xii)

However, as some authors point out, there must be more to this kind of chaos than sensitive dependence. For example, Peter Smith holds that sensitive dependence by itself will not necessarily pose sufficiently grave predictive problems. He takes issue with the claim that chaotic systems are just systems possessing positive Lyapunov exponents, arguing that not all such systems will have "the interesting features" of chaos. Smith's example is a system defined by the single equation:

$$x(t) = 2^t x_0$$

where x at any later time t is defined as 2 raised to the power of t, multiplied by the initial value of x ("x_0"). Any two different initial values of x will curve away from each other exponentially as time increases (this can be easily verified by the fact that the system has a positive Lyapunov exponent (of log 2) for every initial value), but they will curve away from each other in a regular and predictable way (see Fig. 3), which Smith considers to be insufficient:

> ...if the initial state is pinned down into interval (a, b), then at time t the system is somewhere in [the interval] ($2^t a$, $2^t b$); and we can indefinitely keep halving the interval locating the system at any later t by halving the initial margin of error.
>
> To get an interesting case, of course, you typically need not only exponential divergence of trajectories but at the same time ultimate confinement of trajectories to some finitely bounded region of phase space: this will force the tangling of posible trajectories which can yield serious sensitive dependence (where e.g. halving the error in initial conditions does not reduce predictive error at all). (1991, 252, note 3)

And indeed, many chaotic criteria introduce topological considerations that seem designed to weed out simple divergent systems like Smith's example; these additional criteria are discussed in the next section.

(B) The Criteria of Sensitive Dependence With Topological Qualifications

(i) Confined Sensitive Dependence

Smith's argument for the insufficiency of simple sensitive dependence in creating the predictive problems of chaos is plausible, but the "tangling of trajectories" Smith hopes to accomplish through his additional criterion of "confinement" can also take place in a variety of systems with extremely regular and predictable motion. For example, Stuart points out that an idealized Newtonian model of a simple tossed coin is a system in which paths ending in results of "heads" and "tails" have such closely mixed initial conditions that any degree of initial uncertainty will cause us to be unable to predict the final result of the coin toss; this property is responsible for our frequent use of real tossed coins as convenient generators of apparent "randomness" (Stuart 296-7). Of course, in this case, it is technically true that there will be some small level of uncertainty that will be insufficient to defy our predictive efforts -- but on the other hand, the "coin" can also be made arbitrarily small, to create whatever desired degree of "sensitive dependence" through "mixing" we might feel to be necessary in satisfying Smith's criteria.

(ii) Irregularity

At this point, we might also give in and declare the tossed coin of the previous section to be "chaotic", but it is certain that Stuart won't assent to this, for he declares that "[t]he model coin isn't a chaotic system. It's a perfectly regular one." This declaration is important, because while no author ever offers anything like a rigorous criterion for "irregularity", it seems to be at least an implicit requirement of many accounts of chaos -- over and above mere quantitative prediction problems. This is seen most clearly in the attempted "apparent randomness" criterion of Ford and Batterman discussed in section (E) below, but is also typical of more casual formulations, such as a recent *Physics Today* article that introduces chaos as being "wild", "perplexing", and "frequently mistaken for noise". (Ott and Spano 1995, 34)

However, not only is "irregularity" maddeningly vague as a criterion, it invalidates many other initially plausible criteria. For example, a more concrete mathematical illustration of why sensitive dependence and confinement are insufficient for the interesting (i.e., "irregular") predictive problems attributed to "chaos" can be obtained by considering a hypothetical system which seems to meet all of Smith's criteria, yet remains "regular":

$$x(t) = \sin(tx_0)$$

The sine function, taken in itself, is a function which cycles between +1 and -1 during every multiple of 2(pi) -- so even the sine of a very large number will produce some value between +1 and -1; the above system is thus "confined" between these two values[5]. And adjacent values will diverge nonlinearly -- the term ("tx_0") inside the sine-function brackets ensures that different initial values will produce curving paths that diverge due to their different degrees of curvature (see Fig. 4). And this system would seem to produce a quite adequate form of "tangling", given that, for large values of t, it meets Smith's requirement that "halving the error in initial conditions does not reduce predictive error at all". After enough time has passed, the only thing we will be able to conclude in the absence of precise knowledge of the initial value of x is that the system has some value between +1 and -1. The problem with this system is that all of its possible paths seem quite "regular"; for any initial value, the system will simply cycle between -1 and +1 forever, with an unvarying period that depends only on the initial value of the system (it will repeat once for every multiple of 2(pi) achieved by the terms in the brackets of the sine function). In other words, every possible path of evolution will be completely periodic, yet have a different period of repetition from any other possible path. Therefore, despite possessing both sensitive dependence and confined, mixed trajectories, this system poses no long-term predictive problems. It remains plausible that some form of "tangling" or "mixing" in adjacent evolutionary paths of a system is necessary for chaos, for the reasons that Smith cites, but such behaviour seems to require a stricter characterization than Smith gives, if the "irregularity" of chaos is to be preserved.

(iii) Sensitive Dependence and Mixing (Precisely Characterized)

Smith's requirement does seem to be more precisely captured by what one introductory mathematical textbook on chaos (Devaney 1989) calls "topological transitivity". As explained by philosopher John Winnie, who invokes this criterion in another context[6], topological transitivity means that within the domain of a given system, "any subinterval (however small) contains at least one point whose orbit enters any other subinterval (however small or distant from the first)" (Winnie, 266). In general, a system possessing the property of topological transitivity will also meet the weaker requirement of being "ergodic" (Winnie 267) -- a requirement that is more frequently invoked as a criterion for chaos. An ergodic system is defined by Winnie as any system in which "[s]ome point in the domain of [the system] has an orbit that intersects every subinterval of [the domain of the system]". (Winnie 1992, 266)

Cambel attempts to explain the strong epistemological consequences of ergodicity by stating that it is "indicative of irreversibility [because]...Unlike a deterministic system, an ergodic system is independent of its initial position." (Cambel 150) Of course, Cambel's opposition of ergodicity and determinism cannot be taken literally, given that we are presumably still discussing deterministic physical or mathematical systems, but there is a sense in which our ability to retrodict the original position of a badly "mixed" system becomes progressively threatened as the degree of mixing increases -- and in the hypothetical case of infinite mixing known as "ergodicity", we may require infinitely precise knowledge of the system's current conditions to distinguish between the adjacent results of two initially distant positions.

Also, as detailed by Eckmann and Ruelle in a lengthy physics paper, this property can have useful statistical consequences for the investigation of the systems which possess it:

> Ergodic theory says that a time average equals a space average...A basic virtue of the ergodic theory of dynamical systems is that it allows us to consider only the long-term behavior of a system and not to worry about transients. (Eckmann and Ruelle 619)

What they appear to mean is that because an "ergodic" system must possess a path of evolution that passes through every possible region of the system, the allowable phase space of the system will eventually become infinitely packed with this path -- making it possible to conclude, for example, that any arbitrarily chosen 1/4 of the allowable volume will contain 1/4 of this path, and thus the system will on average spend 1/4 of its total time in that region.

But even sensitive dependence combined with this stronger sense of "mixing" (whether the "mixing" is formalized as "topological transitivity" or the somewhat weaker requirement of "ergodicity") can be attained in a very regular and ordered system[7]. Obviously, there is a "right" and a "wrong" sort of mixing involved in chaos.

A typical illustration of the proper sort of mixing process apparently required for true "disorder" is given by Crutchfield:

> Chaos mixes the orbits in state space in precisely the same way as a baker mixes bread dough by kneading it. One can imagine what happens to nearby trajectories on a chaotic attractor by placing a drop of blue food coloring in the dough. The kneading is a combination of two actions: rolling out the dough, in which the food coloring is spread out, and folding the dough over....after considerable time the blob is stretched and refolded many times. On close inspection the dough consists of many layers of alternating blue and white...The blue dye is thoroughly mixed with the dough. (52; similar accounts can be found in Stewart 145, Gleick 51, etc.)

One mathematical system typically cited as having exactly this "right" sort of mixing is the Henon map, generated by the equations given in section C (ii) below (and which seems to be second only to the Lorenz system in terms of the frequency at which it is cited as a paradigm case of "chaos"). The astronomer Michel Henon is credited with

> ...[inventing] a much simpler system of equations than Lorenz's which incorporated their main feature: stretching and folding....If you run the equations on a computer then, no matter what values you start with, successive points rapidly home in on [a] delicate structure, never breaking the multi-layered pattern. But, on the other hand, you can never guess whereabouts within the layers the next point will fall....The interplay between regularity and randomness is baffling (Stewart 152-3).

The "delicate structure" produced by successive iterations of Henon's system is shown in Fig. 5. It is true that there seems to be less "regularity" in this system than in my sine-wave system, and that there appears to be a more drastic form of "mixing" at work. However, we would presumably like to be able to draw more than this vague distinction of degree between the two systems.

Additionally, as with virtually all the criteria presented so far, these topological criteria present certain verification problems. Da Costa and Doria offer a mathematical proof that there can be no possible "general algorithmic procedure" to tell whether or not a given system is ergodic, or whether or not a given system with four or more dimensions possesses a "Smale horseshoe" structure (another iterative topological procedure of "stretching and folding" (Stuart 146-9), simply by examining the equations of the system (1991, 1067). Of course, this does not mean that such questions cannot be settled for some systems (obviously, a system explicitly constructed according to a Smale horseshoe mapping will possess Smale's horseshoe structure). On the other hand, da Costa and Doria apparently demonstrate that there can be no universally applicable test for chaos, stating that "for any nontrivial characterization of chaos in a dynamical system there will always be systems where proving the existence of chaos is unatainable within reasonable standard axiomatizations."[8] (1992, 75)

(vi) Sensitive Dependence, With Mixing, and Dense Periodic Points

There is an additional criterion that will distinguish between my excessively "regular" ergodic and topologically transitive system and some of the paradigm cases of chaos. This criterion is also proposed in the chaos textbook mentioned above (Devaney 1989, 50); that textbook proposes a tripartite definition of "chaos" -- sensitive dependence, plus topological transitivity, plus the criterion of "dense periodic points". As explained by Winnie, this new criterion means that "every subinterval of [the system's domain] contains a periodic point" (266) -- in other words, for any arbitrary interval in the allowed domain of the system in phase space, there must be at least one possible periodic orbit passing through that interval. In fact, this property is exploited in the experiments described in the physics paper "Controlling Chaos" (Ott and Spano 1995; discussed in the next chapter), which details how chaotic systems can be useful due to the fact that they "typically have embedded within them an infinity of unstable periodic orbits" (ibid. 35). Also, recall the original finding of Lorenz that his system possessed a wide array of (unstable) periodic solutions (1963, 140-2).

In contrast to Lorenz's system, the ergodic system I proposed above will have no periodic solutions whatsoever -- all possible paths will have X values which oscillate between +1 and -1 at an ever increasing rate, and will thus never repeat themselves. Unfortunately, periodic solutions may also be absent in the Henon system discussed above, since there seems to be no particular reason why a system defined exclusively in terms of its property of taking an initial distribution of points and "stretching and folding" them into an infinitely deep fine structure should have any points leading to repeating orbits. Admittedly, this fear is based on nothing more than topological intuition, but it is far from unfounded, since even Devaney admits that his chosen criteria are somewhat arbitrary, and will exclude at least some "important examples":

> There are many possible definitions of chaos in a dynamical system, some stronger and some weaker than ours. We choose this particular definition because it applies to a large number of important examples and because, in many cases, it is easy to verify. (Devaney 50)

Given such an admission as a preface to what appears to be one of the most rigorously objective definitions of chaos available, there certainly seem to be grounds for *a priori* scepticism about any attempt to unify the concept of "chaos".

(C) The Criterion of Nonintegrability

(i) The Trouble With Iteration

Sensitive dependence is not the only problem that Lorenz's system confronts us with; there is an additional source of predictive error lurking in the mathematical techniques we must employ in using his model. The Lorenz equations can only be solved through a method of iteration such as the one discussed above[9], but any such method is only an approximate way of investigating the evolution of a system of differential equations. The three differential equations that describe his system are continuous expressions, yet the method of iteration produces only a discontinuous series of data points -- and in fact, each of these points falls only approximately on the "real" curve of the system's evolution. In some systems, this might provide a reasonable estimate of the system's actual path, but the Lorenz system is assumed to be wildly sensitive to variations in initial conditions. Therefore, since each data point produced with Euler's method of iteration is by definition slightly incorrect, we can expect that this error will multiply rapidly over time -- and there will be further errors introduced by the approximation inherent in every stage of the iterative process, each of which will also multiply over time in a compounding sequence of expanding error piled upon expanding error. Furthermore, there is still another source of error -- in the computing equipment used to investigate this mathematical model, which can only calculate values to a certain finite number of decimal places and must artificially truncate all further decimals -- and of course these computational inaccuracies can also be expected to multiply drastically over time in a sensitively-dependent system.

(ii) Integrability vs. Nonintegrability

These catastrophic side-effects of the method of iteration could be eliminated if there existed a mathematical shortcut for calculating long-term predictions without having to perform a long sequence of iterations to calculate all the intervening states. There are many equations for which such shortcuts are possible; they are known as "integrable", because they are amenable to the mathematical technique known as "integration". This technique can be simply defined as the opposite process to the technique known as "differentiation", which is most easily illustrated in a two-variable case such as the equation

$$y = 4x$$

in which y is defined as a "function" of x -- its value at any point depends on the corresponding value of x; in this case, for any x, y will have a value four times as large. If y is plotted on a graph against x, the result will be a straight, sloping line. "Differentiating y" consists in finding the expression for slope of this line, via a series of well-understood mathematical rules. In more complex expressions, this slope may vary at any point; in the present case it will be constant; the resulting expression is:

$$dy/dx = 4$$

which means that "the graphed line for y has a slope with a value of 4 at every point". In physical terms, if x is an expression of the position of an object along some x-axis, then dx/dt -- the slope of the position-function, or the rate of change ot the position -- will express the velocity of the object along that axis, since velocity is simply the rate at which a position changes. In turn, differentiating the function describing the object's velocity will produce a function describing its acceleration (since acceleration is the rate at which velocity changes), and so on.

Integration can be defined as the opposite process -- the process of finding an "anti-derivative" for a given expression; in other words, the process of obtaining an expression that, if it were differentiated, would result in the present expression. Thus, performing integration on both sides of "dy/dx = 4" would result in the original expression, "y = 4x". Unlike differentiation, integration can be problematic -- most functions can be easily differentiated, using a simple system of rules, but only certain functions can be integrated by following generalized rules; many others can only be integrated by labor-intensive trial and error methods, or by depending on the previous labour of others (there are published reference works consisting entirely of lists of functions and the integrals that have been found for them). More importantly, many expressions -- such as the Lorenz system -- appear to be impossible to integrate at all.

The relevant consequence of integrability for chaos is that integrating a given function or system will generally (although not always, as noted in Batterman 51) provide a single expression for each variable in the system, from which any desired future values of that variable can be obtained -- without any need for iteration. For example, if the Lorenz system could somehow be integrated, we might hope to produce three equations like the following:

$$x(t) = F(x_0, y_0, z_0, t)$$

$$y(t) = G(x_0, y_0, z_0, t)$$

$$z(t) = H(x_0, y_0, z_0, t)$$

where notation such as "$F(x_0, y_0, z_0, t)$" refers to an unknown function involving only time ("t") and the three initial values of x, y, and z -- and since these three values are already known, we could make predictions for later values of x, y, and z at any time in the future simply by substituting these known initial values and the desired time ("t") into the above three equations -- if only we knew what the equations were. We would no longer suffer the stage-by-stage multiplication of our errors that the method of iteration produces. This might lead to the conclusion that integrable systems, which need not be iterated, cannot pose the appropriate predictive problems required for chaos[10].

However, integrability is arguably a red herring in the search for a criterion for chaos. The integrability or nonintegrability of a system like the Lorenz system has nothing to do with the sensitive dependence of its dynamics, and being able to integrate a sensitively-dependent system would not necessarily be helpful in making accurate long-term predictions. It is true that being able to integrate the system would remove the explosive errors caused by the approximate method discussed above, but this would only allow us to make good predictions about future states of the model. This would not help us to make good predictions about the real-world system itself, since given a sensitively dependent real-world system, we must still confront the dual problems that the real system cannot be adequately modelled, and that even given an adequate model, we could never obtain adequate information about the initial conditions. It may be generally true that chaotic systems will be nonintegrable, but this is simply because chaotic systems must be nonlinear (see the following section for an explanation of nonlinearity), and in contrast to linear functions, nonlinear functions can be difficult, and often impossible, to integrate.

Furthermore, the question of integration is often completely irrelevant, because many systems commonly discussed in the literature on chaos theory are explicitly specified as a purely iterative procedure -- so that the idea of worrying that there is no way to integrate them becomes nonsensical. For example, many writers (e.g., Crutchfield 53, Stewart 152, Gleick 149, Cambel 71) use the "Henon map" as an example of chaos. The Henon map is specified by the following equations:

$$x_{i+1} = ax_i(1 - x_i) + y_i$$
$$y_{i+1} = bx_i \qquad \text{(Cambel, p.71)}$$

(The notation "x_i" refers to the ith value of x; "x_{i+1}" refers to the value following the ith value in the iterative process; a "map" is a term for a system explicitly defined as an iterative procedure.) Therefore, unlike the 3-equation Lorenz system discussed above, which is specified by continuous differential equations, a system defined in this way can be investigated through iteration without adding an additional layer of inaccuracy between the observed values of the model and the "real" values the model ought to possess.

(D) The Criterion of Nonlinearity

Another commonly repeated requirement for chaos is that chaotic systems must be "nonlinear" (Gleick 1987, 68; Kellert 1993, 2). Nonlinearity, in this sense, refers to equations which contain "nonlinear terms" -- that is, elements which vary in a way that cannot be described by a straight line (for example, because they are multiplied by other variables, by themselves, or by trigonometric functions like the "sine" function). For example, "y + 3x" is a linear expression, since it involves only isolated variables without exponents or other complicating factors; however, "x^2", "xy" and "sin x" are nonlinear terms. The difference between linear and nonlinear terms can be envisioned as follows: graphing any linear term (or any function involving only linear terms) will result in a straight line, but when nonlinear terms are present, no such straight-line relationship can exist.

And it does appear that nonlinearity is required for any sort of interesting or complicated behaviour, despite the rather negative tone of the word "nonlinear". This word is due to the early tendency in the physical sciences to use exclusively linear approximations for modelling physical phenomena, due to the fact that linear systems can be solved explicitly, using a set of general, well understood methods (Gleick 68); they can usually be integrated, for example. With nonlinear equations, no such guarantee exists -- there will be no general method by which to integrate them, it might be impossible to integrate them at all, and the only way to investigate such mathematical systems may be to resort to approximate numerical methods such as the methods of iteration discussed above. But nonlinearity is often held to be vastly more representative of natural systems than linearity; several writers (e.g., Gleick 68) repeat a famous quote of mathematician Stanislaw Ulam to the effect that "to call the study of chaos 'nonlinear science' was like calling zoology 'the study of nonelephant animals'" (ibid. 68).

However, while nonlinearity seems quite necessary for any of the behaviour ascribed to "chaos", it is also insufficient; the most that can be deduced is the negative criterion that linear systems are *a priori* non-chaotic, but since any model of a real phenomenon that is even marginally representational will generally include nonlinear terms, using linearity as a falsifying criterion for chaos will not eliminate enough candidate models to be helpful. Also, the previously discussed criterion of "sensitive dependence", defined as exponential (i.e., nonlinear) divergence between adjacent initial points, implicitly subsumes the criterion of nonlinearity, it seems redundant to bother going on to characterize chaotic systems as "nonlinear". Nonlinearity is thus both the most uncontroversially necessary criterion for chaos and the least illuminating.

(E) The Criterion of Randomness

The physicist Joseph Ford holds the "disordered" nature of chaotic systems to be of fundamental importance, and attempts to define chaos through a rigorous definition of appropriate randomness (Batterman 1993a, 57). Ford, at least according to Robert Batterman's reconstruction of his views, holds that a chaotic system is one whose output is "algorithmically random". Ford obtains this definition from "the algorithmic complexity theory of Andrei Kolmogorov, Gregory Chaitin, and Ray Solomonov", and Batterman summarizes their definition of randomness as follows:

> ...this definition asserts that a sequence is random just in case the shortest program which would compute it on [a] universal Turing machine...is essentially the program which says, Print X' where X is a

complete copy of [the output sequence] S. (56).

In other words, such a sequence is without redundancies or precise patterns of any kind. But there are at least two problems in holding that chaos exists whenever this type of randomness is observed.

The first problem is raised by Batterman himself, who argues that there are many examples of integrable systems which would meet Ford's criterion of "randomness", although Ford would agree that no integrable system can be chaotic. For example, the output of an ideal roulette wheel is algorithmically random, and yet the wheel is an integrable system -- future states of the wheel can be unproblematically predicted, given both a complete expression of the underlying dynamics, and a reasonably accurate knowledge of its initial conditions. Batterman concludes that algorithmic randomness in a system's output "is only a typical consequence of... chaos, [but] not a sufficient condition for it." (65)

However, Batterman is still giving the randomness criterion far too much credit; there are systems held to be paradigm cases of chaos which fail to satisfy Ford's account of randomness in at least two independent ways. First of all, as discussed in section A (ii) above, if the output of the Lorenz system is observed in the appropriate way, it no longer appears to be completely random; Lorenz's original paper demonstrates that a strikingly precise and regular structure emerges if each successive peak value in the Z axis of his system is plotted against the previous peak value (see Fig. 2). As Ian Stewart notes, "[i]t's not true to say that Lorenz found no pattern, that nothing was predictable. On the contrary, he found a very definite pattern." (139) In fact, the emergence of this pattern should not even be regarded as surprising; Lorenz himself states that he sought such a pattern in the Z values only as an attempt to more clearly illustrate the qualitatively predictable behaviour he noticed in his initial plot of the data (which resembled a more skeletal version of Fig. 1). If we closely examinine the double-loop form of this plot, Lorenz states,

> ...we find that the trajectory apparently leaves one spiral only after exceeding some critical distance from the center. Moreover, the extent to which this distance is exceeded appears to determine the point at which the next spiral is entered; this in turn seems to determine the number of circuits to be executed before changing spirals again.
>
> It therefore seems that some single feature of a given circuit should predict the same feature of the following curcuit. A suitable feaure of this sort is the maximum value of Z, which occurs when a circuit is nearly completed. (1963, 138)

And his graph of the successive maximum Z values can be used to generate quantitative predictions about future maiximum Z values. It remains true that such predictions can only be accurate to a finite degree, unless they are based on perfectly accurate initial data (this was the "instability" noted by Lorenz), and so long-term predictions will show an accumulating error -- and it also remains true that Lorenz's conclusions are inferences based on data acquired only through an imprecise method of numerical estimation. But Ford's criterion of pure randomness is falsified if any "shortcut" is found for representing the output of the system with less information than a complete list of the output itself, and this seems to indicate at least a minor shortcut. Of course, in the limiting case where infinitely precise data are demanded, it may remain true that no such representational shortcuts are possible, but for any finite degree of precision -- that is, any case that can be specified and computed in the real world

-- some minor "shortcut" should always be possible. Therefore, absolute randomness of output is neither sufficient nor necessary for chaos, since chaotic systems seem to be at best "quasi-random".

Secondly, chaotic systems seem to defy Ford's (and Batterman's) definition of randomness in another way. Recall that Ford's definition requires that for a sequence to be "algorithmically random", the shortest program required to reproduce the sequence must effectively contain a listing of the entire sequence. Yet the paradigm chaotic system known as the Henon map appears to consist of a perfectly adequate algorithm that can be expressed in just two lines:

$$x_{i+1} = ax_i(1 - x_i) + y_i$$
$$y_{i+1} = bx_i \qquad \text{(Cambel, p.71)}$$

This algorithm does not need to increase in complexity to generate longer sequences of output data, and it certainly does not contain anything analogous to a complete "copy" of its output sequence, so the motivation for such a claim is difficult to discern. (Even the Lorenz equations seem to constitute a short and perfectly valid "algorithm", in the sense that they consist of a finite sequence of rules that dictate all future evolution of the system, although in this case the issue is muddied by the fact that the Lorenz "algorithm" is not computable, except through approximate methods.)

One clue to Batterman's allegiance to this criterion is provided when he declares Ford's specification of it to be "a more precise formulation of some of [Mark] Stone's remarks" (44). In the paper to which Batterman is referring, philosopher Mark Stone draws a distinction between systems whose future states can be generated from "closed-form" algorithms, and those which require "open-form" algorithms. A standard definition of this terminology is given in Crutchfield's *Scientific American* article (1986), in which Crutchfield defines a "closed-form solution" to a system as one which "provides a short cut, a simpler algorithm that needs only the initial state and the final time to predict the future without stepping through intermediate states." (49) (as Batterman notes, this distinction sounds effectively similar to the integrable/nonintegrable distinction discussed above). Noting that such a solution is typically impossible for the systems studied in the "growing new area of research...known as *deterministic chaos*", Stone (1989) asserts that such systems, though predetermined, ought to be considered unpredictable by definition:

> We expect that a prediction is accomplished on less than complete information about the system; that is what makes it a prediction and not just an inspection. But an open form algorithm will just replicate all the relevant information about the system under consideration; there is no short cut. Thus it is not a prediction. (127)

But this distinction seems arbitrary. Surely the mathematical contortions required to produce the output of a given mathematical system are irrelevant to the question of whether the results count as "predictions" -- no matter what we demand from a "prediction" (for example, a description of what that system would do again if the same conditions were repeated, a description of what a second identical system would do under the same conditions, or a description of what might occur in the real-world system that the mathematical system might be modelling). Stone himself suggests the latter sense of "prediction" when he begins his argument by saying, "[s]uppose we have some physical system under study...[and] that we have actually discovered the (open form) algorithm that drives the system, and we attempt to use this in making our prediction", making his claims seem all the more unmotivated[11].

(F) Conclusion

Although "chaos" is commonly held to represent various novel forms of unpredictability in deterministic systems, the evident lack of generally agreed-upon criteria for this sense of "chaos" is no accident. It appears to be impossible to construct a single definition that can cover all of the systems commonly described as "chaotic", while excluding all of the systems which ought to fail -- especially if the widespread and implicit criterion of "irregularity" is taken at all seriously. On the other hand, it is difficult to discern how the vague demand for "irregularity" ought to be treated, given that the attempt to formalize it through a rigorous account of "randomness" is a failure, due to the fact that most common examples of chaos do not possess true randomness. Chaotic systems thus seem to be trapped on an awkward middle ground between excessive regularity and excessive irregularity, a region that may be impossible to demarcate rigorously. In the end, "chaos", in this sense of unpredictability, may represent less of a unified or isolable phenomenon than the simple recognition that systems defined by uncomplicated rules can exhibit surprisingly complicated and unpredictable behaviour -- where "complexity", "unpredictability", and "surprising" are every bit as vague as they sound, defined only in terms of certain systems' impenetrability to older mathematical tools that relied on the viability of linear and periodic estimates and the non-multiplication of small errors.

Furthermore, many of the proposed criteria and partial criteria for chaos pose additional verification problems. Therefore, over and above the question of what counts as chaos in principle, there is the additional question of distinguishing chaos from non-chaos in practice, and it appears that this empirical problem will remain intractable for many systems (especially real-world systems) even if general agreement on a unified definition of "chaos" can be attained.

2. CHAOS DEFINED AS EMERGENT ORDER FROM APPARENT DISORDER

While chaos is often described only as some form of quantitative unpredictability (of an ill-defined "irregular" nature), there is also a certain tendency to talk about chaos theory as providing a methodology that can reveal various forms of "order" in complex or apparently random systems, especially complex real-world systems. This view of "chaos" is commonly centered around accounts of the "strange attractors" which are said to be characteristic of chaotic systems; the majority of this section will be devoted to the various properties ascribed to these objects. However, as I will show, the traditional criteria for "strange attractors" are even more hopelessly vague -- and this problem may be insoluble, since the conflicting demands on these entities may be such that there is no way to formulate non-vague criteria for them. On the other hand, the mathematical theory involved in the investigation of "strange attractors" does seem to produce genuinely useful techniques for making predictions about complicated systems, even if it provides inadequate grounds for distinguishing between inherently "chaotic" and "non-chaotic" systems.

(A) Strange Attractors

It is commonly stated that chaotic systems should possess "strange attractors" (e.g., Crutchfield 50-1; Cambel 208) . This term is used to cover a variety of topological criteria for a system's behaviour in phase space. Some of these criteria are the same as those discussed in the previous major section --

except that in this case they are not being invoked as part of a description of how a system can be qualitatively unpredictable; rather, they are being invoked as part of a description of how a system can exist in a form of partial "order" that lies somewhere between pure randomness and true regularity. It is apparent that there are many cases in which the presence of "chaos" is being recognized not by a system's evident randomness, but by its evident failure to achieve randomness; this latter criterion often appears to be satisfied on the somewhat arbitrary basis of whether or not a given viewer perceives a "pattern" in the dynamics of the data emerging from an experiment. Strictly speaking, a "strange attractor" should satisfy both standard criteria for an "attractor" of a dynamical system, while possessing the additional qualities necessary for "strangeness"; however, the first set of criteria do not always appear to be rigorously applied in practice, and the second set of criteria seem difficult or impossible to formulate.

(i) Attraction

Invoking the concept of an "attractor" for a system places additional demands on its behaviour, over and above the topological criteria of the previous major section (this is one reason that "chaos as emergent order" is conceptually separable from "chaos as unpredictability", despite the congruence of some of the mathematical criteria invoked within each concept). As explained above, certain forms of "mixing" of initially adjacent paths of evolution may be quite sufficient to create the quantitative predictive problems to which "chaos as unpredictability" is supposed to refer. However, accounts of "strange attractors" not only require this sort of phase-space structure, but demand that the system be *compelled* towards producing this sort of structure; this demand imparts at least two new criteria, both of which must often be expressed as counterfactuals. First, it is required that the system *would have* tended toward the same structure in phase-space, even if it had been given different initial values -- and this must include values located outside of the phase-space region to which it is "attracted". As Gleick explains, an attractor

> ...is not just any trajectory of a dynamical system. It is the trajectory toward which all other trajectories converge. That is why the choice of starting conditions does not matter. As long as the starting point lies somewhere near the attractor, the next few points will converge to the attractor with great rapidity. (Gleick 150)

Therefore, a system that is simply confined (via external constraints -- say, a rigid box) to a certain region in phase space cannot automatically be said to possess an "attractor". The system itself must have internal dynamical properties such that at least some initial paths outside some "attracting" region of phase space will be drawn inexorably toward it. (Since every system in a finite universe is in principle bounded in some way, the lack of such properties would presumably mean that every system had an "attractor".)

The second counterfactual criterion is that a system producing values in this "attracting" region must continue to do so *forever*, in the absence of perturbation. Ruelle explains this as follows:

> ...the system of interest, after some transients, establishes itself in an asymptotic regime where it comes back close to the same points [in phase space] again and again. The set of points in phase space to which it returns is called an attractor of the dynamical system. (1994, 24)

In fact, the system should be able to remain within the "attracting" structure even if the fundamental dynamics of the system are slightly perturbed -- this property, mentioned in the previous chapter, is known as "structural stability" (Stewart 107-8), and can be more rigorously defined as follows:

> a map *f* is structurally stable if every "nearby" map is topologically conjugate to *f* and so has essentially the same dynamics...If, no matter how we perturb f or change *f* slightly, we get an equivalent dynamical system, then the dynamical structure of *f* is stable. (Devaney 53)

These criteria, of course, will lead to problems of induction in many real-world cases where chaos is "detected" by observing that some set of data form an "attractor". Or rather, these criteria would lead to problems of induction if they were taken seriously. However, in practice, the word "attractor" often seems to be invoked rather sloppily, and used to describe systems for which there are no evident grounds for additional hypotheses about the counterfactual behaviour of the same system under different starting conditions, or about the behaviour of a topologically "nearby" system with similar but not identical dynamics. For example, an article in the popular magazine *Omni* describes the belief of neuroscientist Paul Rapp that he has detected "strange attractors" in the EEG signals of subjects performing backwards-counting tasks of varying difficulty:

> When the EEG signal is fed into a computer and transformed into an abstract structure -- a strange attractor -- Rapp can immediately see a difference. When subjects are subtracting by seven, the strange attractor is rich and complex....In the much simpler task of subtracting by two, the strange attractor suddenly flattens out in one plane, resembling a Frisbee seen from the side. Mathematically the structure is in fact less complicated, or 'chaotic'. (McAuliffe 1990, 48)

(Also note that this particular article further muddies the conceptual waters by using "chaos" as a synonym for "complexity".) Similarly, Ian Stewart (1989) cites the results of two scientists seeing similar two-dimensional "attractors" in the sequences of measles cases per month in New York and Baltimore[12] (276), in which the designation of "attractor" seems to be based only on certain qualitative similarities between the apparent structure in the data of the two cases, rather than any suggestion that the event structure in each case would have emerged under different conditions; this is coupled with the hypothesis that a cross-section of the attractors has a general appearance that "strongly suggests the presence of chaos". (275)

An even looser account of chaos-as-order is given in a dripping-faucet experiment performed by Robert S. Shaw, one of the co-authors of the Crutchfield paper. In this experiment, the data resulting from the successive time intervals between the drips from a faucet were plotted against each other (using the method discussed in note 7), and the resulting plots were declared to be depictions of the "chaotic attractors" for the system. (Crutchfield 56) Crutchfield concludes that

> It could have been the case that the randomness of the drops was due to unseen influences, such as small vibrations or air currents. If that was so, there would be no particular relation between one interval and the next, and the plot of the data taken in pairs would have shown only a featureless blob. The fact that any structure at all appears in the plots shows the randomness has a deterministic underpinning (Crutchfield 56)

This correlation seems intuitively correct in one sense; for any system whose behaviour is determined by some set of underlying dynamical laws, it seems reasonable to expect that its output data should demonstrate a more evident "structure" than a lawless and "random" system would produce. But if we take Crutchfield's account seriously, it would seem that strange attractors have been reduced to nothing more than a flag for distinguishing determinism from randomness in complicated data sets. And if strange attractors are to be considered a sufficient indicator of chaos, this would in effect make every deterministic system count as "chaotic".

Crutchfield also makes inferences about the underlying "chaotic" (in the sense discussed in the previous chapter) dynamics of the system from nothing more than the shape of this "attractor", stating that it has "the horseshoelike shape that is the signature of the simple stretching and folding process discussed above". (56) And even these somewhat indirect inferences may only be available in special cases; Crutchfield adds that "[o]ther data sets seem more complicated; these may be cross sections of higher-dimensional attractors. The geometry of attractors above three dimensions is almost completely unknown at this time." (53)

(ii) Strangeness

The difficulty in formulating rigorous criteria to distinguish chaotic attractors from just any old "blob of data" lies in their property of "strangeness". Such attractors are called "strange" because it was once thought that the only possible long-term structurally stable states for dynamical systems were points and limit cycles[13] (cyclic sequences of output data, corresponding to the system repeating an identical "loop" in phase space forever) (Stuart 110; Devaney 211).

But while there are obvious non-vague criteria for the non-"strange" attractors that render systems too regular to count as "chaos", and there are at least somewhat plausible criteria for what ought to count as pure "randomness" (such as the algorithmic randomness account unsuccessfully advanced as a definition of chaos by Ford and Batterman, as detailed in the previous chapter), it appears difficult to formulate criteria for the correct sort of long-term behaviour that could distinguish only some of the remaining systems as "chaotic". On the other hand, letting "chaos" expand to occupy the entire middle ground between regularity and randomness reduces it to a profoundly unilluminating concept, as this would in principle allow it to cover most of the natural systems in the universe[14].

This may not be a particularly damaging criticism of chaos theory itself, however; in fact, the impression left by a book such as Ian Stewart's *Does God Play Dice?* is that the primary usefulness of "chaos theory" lies precisely in its ability to elicit some form of internal "order" from virtually any complicated natural system. Thus, the same considerations that appear to make chaos-as-order collapse into a hopelessly broad criterion may reveal it as a universally applicable methodology for extracting quantitative predictions from systems, even in the absence of knowledge about their underlying dynamics. There is, however, at least one more criterion that is sometimes invoked in an attempt to distinguish the attractors of "chaos" from emergent behavioural patterns in general.

(iii) Fractal Structure / Fractal Dimension

One source of confusion in discussions of "chaos" is the various ways in which the term "fractal" is invoked. The term itself is universally attributed to Benoit Mandelbrot, who, while employed as an IBM researcher, discovered that various forms of apparently irregular data (e.g., occurrences of noise in telephone lines, the changing levels of the river Nile) exhibited characteristic patterns that were scale-independent. He extended this idea to the problem of characterizing irregular forms such as coastlines, which had seemed intractable to normal geometry. For example, a coastline seems to possess no "length" in the standard sense, due to the fact that it possesses "roughness" at virtually every scale of measurement -- so that there is essentially no resolution of measurement fine enough to capture its entire length, but every attempted decrease in the scale of measurement will return a "longer" result, due to the tracking of new and smaller irregularities. Mandelbrot also showed that in many cases, this process will not even converge on a "limiting" value for the length, although of course in the real world there is presumably some limit on the ultimate "roughness" of any object. (Gleick 91-8)

Mandelbrot formulated the idea that such objects could be said to possess a "fractional dimension" -- a dimension said to lie somewhere between the standard dimensions into which objects are divided in Euclidean geometry (for example, "two-dimensional" planes versus "three-dimensional" cubes). According to Gleick, Mandelbrot intended to characterize

> the degree of roughness or brokenness or irregularity in an object. A twisting coastline, for example, despite its immeasurability in terms of length, nevertheless has a certain characteristic degree of roughness....{Mandelbrot's] claim was that the degree of irregularity remains constant over different scales. (98)

Mandelbrot also coined the word "fractals" to describe "his shapes, his dimensions, and his geometry" (Gleick 98). The most famous "fractal" object is the frequently-reproduced Mandelbrot set (see Fig. 6). This figure exhibits a surprising and counter-intuitive form of complexity: when some of its small features are examined at a high resolution, they are revealed to be essentially complete copies of the figure as a whole -- and in turn, these sub-structures also contain minute copies of the entire structure, and so on down to an infinite degree of miniaturization.

However, despite the fact that it is commonly used to illustrate publications on chaos, the Mandelbrot set is not itself "chaotic" in any sense of the term discussed so far. It does not depict the evolution in phase space of a dynamical system; rather, each point represents the result that occurs when a given number from the "complex number plane"[15], c, is evaluated to see whether its "Julia set"[16] is connected or discontinous. If the Julia set for the given number is continuous, it is assigned one colour; if discontinuous, it receives a different colour. The resulting graph is the "Mandelbrot set". In other words, the Mandelbrot set does not depict the progressive evolution of one system with one set of parameters, but simply tabulates the final results of a vast number of different systems. But the Mandelbrot set does provide a dramatic illustration of the two important properties attributed to fractals -- self-similarity at every scale of measurement, and an infinite deep structure.

These properties are also frequently attributed to the "strange attractors" of chaotic dynamical systems, although it is obvious that they cannot always apply. While the attractor resulting from the "Henon map" of Fig. 5 does display a vague self-similarity, the "Lorenz attractor" has no such small copies of itself concealed within its structure. The closest similarity it bears to this aspect of fractals is in Lorenz's observation that

> [i]t would seem, then, that the two surfaces merely appear to merge, and remain distinct surfaces. Following these surfaces along a path parallel to a trajectory, and circling [the center of each loop], we see that each surface is really a pair of surfaces, so that, where they appear to merge, there are really four surfaces. Continuing this process for another circuit, we see that there are really eight surfaces, etc., and we finally conclude that there is an infinite complex of surfaces, each extremely close to one or the other of two merging surfaces. (1963, 140)

But calling this "self-similarity" is stretching the analogy somewhat; in fact, even the Henon attractor can be seen to be self-similar only in an appoximate way (see Fig. 7), so this criterion cannot be taken particularly seriously as a strict demand for strange attractors.

On the other hand, it is true that strange attractors must exhibit "infinite deep structure" in some sense -- at the very least, the dual requirements that the paths of chaotic systems must remain in a bounded region of phase space while never repeating themselves mean that such systems will eventually pack some sub-region of phase space infinitely densely with any given path; this was the property of "ergodicity" discussed in the previous chapter. And it is in this sense (packing an infinite line into a finite volume) that such attractors can be said to be "fractal". (Gleick 139)

But for Lorenz's system of continuous differential equations that cannot be solved precisely, there are valid grounds for scepticism about the "Lorenz attractor" actually being a property of the Lorenz system. Lorenz himself notes that, because of the discontinuous, iterative method of numerical approximation he is using, the smooth orbits depicted in his "attractor" are actually formed only by connecting the dots between the successive positions of "a jumping particle rather than a continuously moving particle" (134). He also notes that, since the solutions are being computed to merely a finite degree of precision, there is only a finite number of possible points that can emerge as solutions, and therefore "only certain discrete points in phase space will ever be occupied" by his computed paths. The consequence of this is that his attractor in fact cannot have infinite deep structure, and will eventually repeat itself completely -- due to the finite precision permitted in the system's results, there will always be a moment when a point that should merely be very close to some long-previous point will be calculated to be the same value as that previous point. Because each stage of the system is completely defined by its previous stage, achieving the same value twice means that the system is locked into a permanent cycle. Lorenz holds that "in practice this consideration may be disregarded", since the system may not repeat itself until long after the "number of iterations ever likely to be performed". But cause for scepticism remains: if the Lorenz system has an attractor, it isn't the famous "Lorenz attractor", but something else -- something which might perhaps be qualitatively similar, but not something we can observe computationally.

More damagingly, in a sidebar to an interview in *Nonlinear Science Today*, the mathematician Stephen Smale lists "Ten Unsolved Problems in Dynamics", beginning with, "Are the dynamics of the Lorenz equations described by the geometric Lorenz attractor of Williams, Guckenheimer, and Yorke?", and adding that "the general problem of establishing and analyzing strange attractors of differential equations of physics and engineering is still wide open". (1991, 15) Finally, even the concept of "fractal dimension" itself is not entirely clear; Cambel isolates at least four forms of this concept (166), including some that apply to objects such as rocks, "capital markets", and "speech forms" which are presumably irregular only to a finite degree, and Stuart notes that "the appropriate concept of fractal dimension seems to vary from one application to another" (242).

(B) Period-Doubling Cascades

Many systems are observed to follow what is known as a "period-doubling route to chaos" as their parameters are altered. This can be easily observed in the "logistic mapping" function:

$$f(x) = kx[1 - x] \qquad \text{(e.g., Stewart 197)}$$

For small values of k, iterations of this function for any initial x (between 0 and 1) converge to just one stable output value; however, if the parameter k is increased sufficiently, the system will instead converge to a stable output cycle with 2 values -- and further increases produce a 4-value cycle, and so on for many more doublings, until k gets high enough that this apparent pattern vanishes and random-looking output takes over. Furthermore, each of the "critical" values of k for which the convergent values undergo this "split" lie in a series that decreases at a constant ratio of 4.669. In other words, when the convergent values of x are plotted against this k parameter, an image similar to a tree branch emerges, with successive branches becoming smaller in a consistent 1/4.669 ratio (see Fig. 8). One of the more interesting features of this mathematical phenomenon is that this period-doubling with a 4.669 ratio is observed in a variety of real-world systems (for example, in fluids and gases driven towards turbulence (Stewart 210-213)); this is said to demonstrate a property known as "universality" (Gleick 171-5, Stewart 197-201). And in general, when part of this period-doubling behaviour is observed in a system, accurate predictions can be made about successive behaviour (for example, when to expect further doublings, or the value of the point at which proper chaos will emerge).

However, this period-doubling is not an illustration of "chaos" in the way that the "attractors" of the previous section were. The graph in Fig. 8 does not depict the behaviour of a single dynamical system, but rather a tabulation of the final results of a continuous sequence of different dynamical systems (or, equivalently, the final results of different cases of "the same system" with different dynamical parameters). This process is related to "chaos" in two ways, though -- first of all, it illustrates a common form of pre-chaotic behaviour; many systems pass through such a period-doubling phase as their paramenters change from non-chaotic to chaotic values, and so such a cascade is "often a sign that chaos is present" (Ruelle 24). Also, the period-doubling phenomenon itself falls broadly under the heading of "chaos theory" in the sense that it provides a model for the behaviour of complex real-world systems that were previously incomprehensible -- for example, liquid helium heated towards turbulence (Stewart 211-2). This structure also possesses some of the properties attributed to the "attractors" of chaotic systems: its branching pattern scales progressively downwards at a constant ratio, and the structure as a whole exhibits self-similarity -- small areas that, when enlarged, reproduce the entire structure.

(C) Conclusion

It might be true that the emergence of "chaos theory" marks the discovery of some interesting mathematical artifacts -- for example, it is surprising and useful to know that the Feigenbaum period-doubling cascade occurs in such a wide variety of systems, and it was apparently also a surprise to learn that there are a range of relatively simple systems that exhibit long-term behaviour that is nearly periodic, without actually falling into periodicity. And chaos theory has also generated a variety of apparently useful qualitative prediction methods and modelling techniques for complex systems, including systems for which we do not know the underlying dynamics. But the broad array of concepts subsumed by "chaos" appear to be nonunifiable.

It is sometimes suggested that the attempted definitions that I have grouped into Chapters 1 and 2 should be combined in any properly rigorous definition of "chaos". For example, Cambel concludes that

> [t]here is no one measurement or calculation that can establish the existence or absence of chaos. For a system to be technically chaotic, certain specific conditions must prevail. These include
> a. The system must be nonlinear and its time series should be irregular
> b. Random components must exist[17]
> c. The behavior of the system must be sensitive to initial conditions
> d. The system should have strange attractors, which generally means that it will have fractal dimensions.

e. In dissipative systems the Kolmogorov entropy should be positive.
f. Perhaps the most terse way of pronouncing a system to be chaotic is to determine that there are positive Lyapunov exponents.[18] (208-9)

This definition encompasses most of the criteria discussed so far (with the addition of "Kolmorogov entropy", which is a measurement of the "information loss" due to the exponential separation of initially close paths in phase space; this is conceptually similar to the properties established by positive Lyapunov exponents [Cambel 154-6]), but this list cannot serve as the sole definition of "chaos" because it would invalidate many current uses of the term. As I have shown, there is a significant body of literature (using) the word "chaos" to describe only quantitative unpredictability in simple deterministic systems, without making any explicit or implicit demands for "strange attractors" or other forms of emergent order in the systems; furthermore, this quantitative unpredictability is an interesting concept in itself, and well worthy of designation with a term like "chaos" even if there can be no rigid boundaries determining the cases where this form of "chaos" should be properly said to exist.

On the other hand, other writers wish to use the term "chaos" to refer to recurrent behaviour and other forms of apparent order in complex systems, making only indirect inferences about the existence of sensitive dependence. Such accounts (such as Crutchfield's account of Shaw's faucet experiment) often make only indirect and tenuous inferences about the sensitive dependence of the underlying dynamics. Again, even though it may be difficult or impossible to formulate strict standards for what should count as this "emergent order", the techniques by which this "order" is recognized and measured have evident pragmatic value, and thus this second form of "chaos" is also worthy of conceptual demarcation. In attempting to subsume both of these broad senses of "chaos", Cambel's definition rules out the possibility of their individual use -- and more problematically, rules out the possibility of using "chaos" to apply to systems in which one of these two broad categories of "chaos" cannot be verified.

3. THE CONCEPTUAL IMPLICATIONS OF CHAOS

(A) Chaos and Determinism

Determinism, in the sense in which it is invoked in papers on chaos theory, can be broadly defined as any doctrine holding that the future, including all individual human acts, is in some sense "predetermined" by fundamental and inescapable laws of the universe. The doctrine of determinism is often attributed to the great empirical successes of science in correctly describing and predicting the behaviour of matter using sets of underlying laws -- for example, in the "universal" laws of mechanics derived by Isaac Newton -- combined with the assertion that human beings were completely material entities. An early and often cited (e.g., in Crutchfield 47) expression of determinism was given by Pierre Simon de Laplace in 1776, who defined it in terms of predictability:

> The present state of the system of nature is evidently a consequence of what it was in the preceding moment, and if we concieve of an intelligence which at a given instant comprehends all the relations of the entities of this universe, it could state the respective positions, motions, and general affects of all these entities at any time in the past or future.

Chaos is sometimes said (e.g., in Stone 1989) to have the interesting property of producing a split between determinism and predictability. When stated as simply as in the preceding sentence, this may seem unremarkable, since presumably any sufficiently complicated or even poorly understood mechanical system will be predetermined but unpredictable, but some writers argue that chaos offers a new and unique threat to determinism. For example, in an early *Scientific American* article on chaos, the scientist James P. Crutchfield declares that we have lately witnessed "the downfall of Laplacian determinism", due to both the appearance of quantum mechanics and, independently, to "the exponential amplification of errors due to chaotic dynamics", which ensure that "predictions are rapidly doomed to gross inaccuracy." (1986, 47-8). And Crutchfield enthusiastically concludes with the speculation that

> [i]nnate creativity may have an underlying chaotic process that selectively amplifies small fluctuations and molds them into macroscopic coherent mental states that are experienced as thoughts. In some cases, the thoughts may be decisions, or what are perceived to be the exercise of will. In this light, chaos provides a mechanism that allows for free will within a world governed by deterministic laws. (57)

However, Crutchfield is evidently relying on a nonstandard definition of determinism, because he seems to define determinism in terms of practical predictability. But the classical debate about determinism revolves not around whether humans can completely predict the future, but rather around whether future events and decisions could in theory be completely predicted from past states of affairs, and the particular abilities of humans are irrelevant to this question.

In fact, even Crutchfield's cited passage from Laplace makes this distinction clear. Laplace notes that, even though the future might be predetermined, due to the fact that it could be predicted by his imaginary ultimate "intelligence", there will be many predictions that remain forever inaccessible to human beings, except on a probabilistic basis:

> Physical astronomy, the branch of knowledge which does the greatest honor to the human mind, gives us an idea, albeit imperfect, of what such an intelligence would be....But ignorance of different causes involved in the production of events, as well as their complexity, taken together with the imperfection of analysis, prevents our reaching the same certainty

> about the vast majority of phenomena. Thus there are things that are uncertain for us, things more or less probable, and we seek to compensate for the impossibility of knowing them by determining their different degrees of likelihood. (57)

Also, it is clear from this passage that Laplace himself, long before the existence of "chaos theory", held that it would always be prohibitively difficult to predict certain things about the world, for basic and practical reasons. So not only do the prediction-foiling properties of chaos fail to be threatening to determinism; they also fail to be unique within the history of science.

A more sophisticated argument against determinism is advanced by Stephen Kellert (1993), who observes that it is relatively easy to specify chaotic systems for which there are "prediction tasks" that are impossible in a rigorous and technical sense, given the constraints of the physical universe. There are chaotic systems for which predictions of a given accuracy beyond a certain duration simply could not be made even with the fastest possible computing device one could construct from all the matter in the universe, and so on. Therefore, under the existent laws of nature, there may be no physically possible way to predict certain things about chaotic systems -- and Kellert concludes that it is meaningless to call these things "deterministic systems" at all.

Kellert is employing a definition of determinism that is similar to that of Crutchfield, but he justifies it in a much more subtle way. Kellert begins by distinguishing four possible senses of "determinism", each of which is "more robust" and involves "additional criteria that are not included in any previous level". Briefly, these are:

(i) The requirement that a system be describable by differential dynamics, so that "the future depends on the present in a mathematically specifiable way";

this is automatically true of all candidate systems for chaos, since they are always expressed in terms of differential equations[19] without probabilistic components.

(ii) "Unique Evolution", the requirement that "the evolution of the system", in either the past or the future, is "uniquely fixed once we specify the state of the system at any one moment"[20];

(iii) "Value Determinateness", the requirement that "physical quantities have exact values" rather than being "spread out or somewhat indistinct", which would cause the system to "seem insufficiently set, fixed, specified, and determined by the laws of physics";

(iv) "Total Predictability", the requirement that "the universe is predictable, in principle, by an all-powerful intelligence or computational scheme, given complete information of instantaneous conditions and the complete set of physical laws." (Kellert 50-60) (Note that this fourth sense of "determinism" sounds identical to the proposal in the Laplace passage quoted above).

Having drawn these distinctions, Kellert proceeds to argue that, although chaotic systems satisfy his first three senses of determinism, they remain "utterly unpredictable", and he concludes with the astonishing claim that that "by any definition of determinism that includes total predictability, determinism is false." (62)

This claim is based on two premises: the fact that chaotic systems "manifest sensitive dependence on initial conditions", and the fact that completely accurate measurements of any system are impossible to obtain. Therefore, since we can never know the current state of a system with complete accuracy, and since Kellert assumes that slightly different initial states must have significantly different future states in any chaotic system, the above requirement of "total predictability" is defeated.

At this point, Kellert produces yet another distinction: "physical determinism", which is constituted by the satisfaction of conditions (i) through (iii) above, versus "epistemic determinism", which Kellert insists is what is undermined by chaos and the failure of condition (iv) (he attributes this distinction to Hunt). Yet Kellert seems quite wrong in at least two ways.

First of all, one wonders precisely what Kellert means by "an all-powerful intelligence" in his earlier stipulation of what counts as "total predictability". Presumably, any "all-powerful intelligence" would not have to confront the human epistemic problems that result in our inability to make perfectly accurate measurements, but without these epistemic problems, "total predictability" is completely possible for chaotic systems. This is especially obvious given that Kellert is still discussing chaos within classical mechanics, which, he admits through a citation of Hunt, "remains a physically deterministic theory". By Kellert's own stipulation of "total predictability", any physically deterministic system (i.e., any system that satisfies (i) through (iii)), even if it is chaotic, is "totally predictable" by a hypothetical, ultimate intelligence and thus satisfies criterion (iv) quite well.

Secondly, even though chaotic systems may indeed fail to exhibit "epistemic determinism", this distinction seems to rest on an oddly unmotivated sense of determinism, given the history of the concept -- but Kellert attempts to press this argument even further, claiming that no competing sense of determinism is meaningful. Kellert holds that

> [i]f our theories, on paper, speak of a unique trajectory [for the future of the

> system] but that trajectory is not and cannot ever be observed or used, and cannot enter into useful practice, then we have run into the question of realism with regard to deterministic trajectories... (Kellert 65)

Kellert admits in passing that such a position "may appear somewhat verificationist" (64), but this scarcely hints at the magnitude of the problems underlying his argument. In fact, his re-definition of determinism cannot possibly be motivated except by adhering to a verificationist criterion for the meanings of words.

In general, a verificationist criterion of meaningfulness is any position holding that, for a term to possess meaning, there must be a set of observational criteria that can be used to verify whether or not the term is appropriate at any given moment (where "observation" is defined narrowly, as direct sensory experience). In particular, any references to external physical objects must be translated completely into descriptions of the sensory effects these objects ought to have on a human observer. For example, any discussion about tables must be reducible to talk about sets of brown visual sensations, hard and smooth tactile sensations, and so on. This philosophical program would indeed provide sufficient and appropriate motivation for Kellert's rejection of theoretical, "physical determinism" as a meaning for determinism, and his substitution of "epistemic determinism", which would fail to be the case whenever human observers cannot experientially verify that there is a predetermined path for the future.

However, verificationism is not without its own historical problems -- in fact, these problems are so severe that verificationism is now commonly considered to be untenable. Many mundane ordinary-language statements appear utterly untranslatable to this new format. Universal generalizations (like "all emeralds are green") cannot be converted to observational tests, since they would require an infinite amount of testing. Common metaphysical abstractions like "God exists" or "there are moral facts" have no conceivable sensory consequences. Even complex counterfactuals like "if you walk to any downtown store after six-o'clock, you'll find it closed" appear to require impossibly baroque catalogues of sense-data statements -- assuming that terms like "you" can even be subsumed by a verificationist criterion at all. Therefore, Kellert's proposed definition of determinism seems to lead to rather a large number of difficulties that Kellert not only fails to address, but presumably would wish to avoid.

Furthermore, Kellert ignores the fact that it is trivial to specify effectively impossible prediction tasks without invoking chaos; for example, any sufficiently complex nonintegrable system will be only finitely soluble, within the lifetime of the universe, by any physically possible computer. Therefore, any supposed change in the status of determinism can be traced back at least to some time around the initial invention of calculus, a date considerably before the appearance of chaos theory. Some of the systems that fall under the heading of "chaotic" may indeed present us with new types of effectively (although not literally) impossible prediction tasks, in that such effectively impossible tasks may emerge even from simple systems, but there seems to be no obvious epistemic distinction between the limits on our knowledge imposed by "chaotic" systems and the limits on our knowledge imposed by older forms of quantitative impenetrability. If determinism fails to hold now, it has been failing for a very long time.

But even apart from these questions about the meaning of determinism, Kellert's argument has further problems. For Kellert, the very existence of sensitive dependence in chaotic systems has new and uniquely fatal consequences for our predictive abilities. Yet this conclusion presumes that we are interested in making the predictions that chaos precludes in any given system. It is true that in a chaotic system, we will rapidly accumulate uncertainty about the system's future states -- but at the same time, we know that the system's long-term behaviour must be limited to states that fall close to its "strange attractor". In other words, we may not be able to predict where the system will fall in its allowed phase space, but we can predict with complete certainty that the system will be *somewhere* within the allowed phase space dictated by the attractor.

Thus, there will be numerous predictions we can make for which chaos will be irrelevant -- for example, even though we may not be able to predict the temperature of a given point in the Earth's atmosphere 90 days from now, we can predict that the temperature at that point will fall within 100 degrees of zero Celsius (a rather narrow temperature range relative to the possible range of temperatures in our universe). The predictive problems of chaos only occur within the range of the attractor itself; nothing in chaos theory prevents the attractor (and thus the "chaotic" dynamics of a given system) from being vanishingly small relative to the constant or predictable aspects of the system[21]. This point is made by David Ruelle, who cautions that

> a chaotic system is one for which a small uncertainty in the initial conditions leads to an exponentially growing uncertainty in the predictions of its future. Notice, however, that this exponential growth exists only for an infinitesimal change in the initial conditions and may saturate to small values for certain systems. In other words there are chaotic systems that have mostly predictable time evolutions with a litttle bit of superimposed chaotic noise...we should not be obsessed by chaos: The whole of the dynamics of a real system is interesting, and not just the occurrence of chaos. (1994, 26)

Thus, contrary to Kellert's conclusions, the predictive limits imposed by a chaotic system will not necessarily impose predictive problems for us, assuming that we take a sufficiently pragmatic view of "predictability", rather than Kellert's idealized view. So Kellert overstates the importance of chaos somewhat; also, the pragmatic view of predictability is arguably the one that most agrees with Kellert's own prejudices, inherent in his arguments about the appropriate meaning of determinism, towards verifiable properties rather than theoretical properties.

(B) The Effects of Chaos on Scientific Methodology

Another ostensibly unique aspect of chaos theory is that it marks some kind of shift in the locus of reality for scientists. This argument is advanced by Stephen Kellert, who claims that the mechanisms cited in chaotic predictions are distinguished by being "geometrical" rather than "causal" (Kellert 104).

One common example of these "geometrical mechanisms" is the "period-doubling route to chaos" mentioned in the previous chapter, which can be used to make certain forms of quantitative predictions in complex systems even when their fundamental dynamics are unknown. According to Kellert, these predictions are interesting because they are made on the basis of perceiving higher-level patterns in the behaviour of systems, rather than referring to the fundamental laws of nature that drive them. Yet, says Kellert, the useful predictive rules that come out of these chaotic models should not be considered "lawlike". For example, while successful predictions may be made on the basis of this observed repeating pattern of results, the system is not considered to be under the influence of any "law" holding that all patterns must be completed. The result is supposedly a division between the actual laws of the system, expressed in the underlying dynamical equations (which have no predictive value), and the observed large-scale rules of the system (which have no "lawlike" status).

Of course, it might be argued that a reliance on stochastic rules with no direct causal force is hardly new in the history of science; it has long been commonplace in biology and the social sciences, for example. But Kellert's argument has more fundamental problems. While this deep division between predictive rules and fundamental laws may exist in other domains, it arguably does not exist in chaos theory. First of all, despite Kellert's flat denial that such phenomena as period-doubling represent "laws governing qualitative features of systems" (Kellert 112), the 4.669 period-doubling seems to be a common and fundamental mathematical artifact, and not merely a happy coincidence. This is seen most clearly in Stewart's account of universality, when he explains that any "one-humped" function (for example, $f(x) = k[\sin x]$, or the previously mentioned $f(x) = kx[1 - x]$, or a vast array of other functions) will, when iterated, demonstrate convergent 4.669 period-doubling. Other large families of functions demonstrate period-doubling with different ratios. Therefore, period-doubling appears to be quite a good candidate for "lawlike" status.

Furthermore, its very universality leads to an extreme anti-realism about the underlying dynamical model that (for Kellert) is supposed to embody the "real laws" of the system. Since a wide range of possible systems will exhibit the same behaviour, conclusions can be drawn about the system without having to verify that any one particular model best exemplifies it. As Stewart notes,

> Feigenbaum's discovery of universality is a two-edged sword. It makes it relatively easy to test a particular class of chaotic models by experiment; but it doesn't distinguish between the different models in that class. (208)

Therefore, the underlying dynamical model, far from being considered the sole source of laws, is in fact completely irrelevant to (what I would call) the "laws" that actually permit the sorts of geometrical predictions Kellert is talking about. Thus, despite Kellert's argument, there is arguably no fundamental division between the "laws" of period-doubling systems and the "patterns" by which predictions can be made: they are both descriptions of precisely the same phenomena.

(C) Sensitive Dependence Reconsidered

(i) Questioning the Butterfly Effect

The centerpiece of Peter Smith's "The Butterfly Effect" is his initially plausible claim that all arguments from chaotic models to conclusions about "ultra-refined sensitivity" in the real world are internally incoherent. Smith begins by correctly observing that our models will always "impose surplus fine structure on the physical facts". For example, to represent a property like temperature, we will use a definite number--but in reality, the idea of the temperature at a given point having a definite value is a hopeless idealization:

> ...we will standardly represent temperature in a model by a real number. So the representing number will be determinate to an infinite number of decimal places. But it makes no physical sense at all to suppose that (say) the temperature at a particular point in a gas is itself infinitely precise, for at least two reasons. An idea of the precise temperature at a point P would have to be constructed from some notion as the limit (as the

> volume of the sphere shrinks to zero) of the average kinetic energy of the gas molecules in a sphere centered at the point P. But that limit is undefined since statistical fluctuations will ensure that as you shrink the sphere, the average kinetic energy of molecules within the sphere will...vary increasingly widely. And on top of that...the kinetic energy of each molecule is subject to a quantum uncertainty principle. (Smith 256-7)

Therefore, for most common physical properties, there will be many "equally acceptable" ways to obtain the artificially definite values needed for use in our mathematical models--and each calculation method will result in slightly different definite values, and thus slightly different statements of initial conditions. For non-chaotic systems, these minor variations would create no real problems, but if we insist that chaotic systems are systems with sensitive dependence, then we seem to have good reason to fear that our final results will vary widely depending on how we choose to idealize these real physical properties like temperature -- and these arbitrary variations might well be of equal or greater magnitude than the changes ostensibly introduced by the hypothetical butterfly that gives the "Butterfly Effect" its name. Smith concludes that the existence of the "Butterfly Effect" is an unwarranted inference, arguing that

> ...we are certainly not entitled to appeal to the model to say that the minuscule difference in the air currents [due to the actions of the butterfly] *would* have changed the course of the hurricane. In fact, grounds for buying a model with full-blooded sensitive dependence would be grounds for *denying* that butterfly-sized physical changes would have any determinate effect....*the applicability of a model with sensitive dependence to some phenomenon would actually be inconstent with the existence of a Butterfly Effect.* (Smith 257-8, emphasis in original)

However, Smith's argument that it is "inconsistent" simultaneously to believe in the existence of a Butterfly Effect in the real world and believe in a model proposing sensitive dependence is based on a category mistake. To disentangle Smith's implicit metaphysical claims from his implicit epistemological claims, it is useful to consider the following two questions:

> *(i) Could there be a fact of the matter about whether or not a "Butterfly Effect" would actually occur in a real-world system like the atmosphere?*
>
> *(ii) Can we distinguish a "Butterfly Effect" from the effects of falsely idealized physical quantities?*

Smith implicitly poses both of these questions, and appears to believe that the answer to question (ii) must be "no". He also seems to think that a negative answer to (ii) necessitates a negative answer to (i). However, both of these beliefs are incorrect, although for different reasons. Smith's negative answer to (ii) is based on his category mistake, and this can be illustrated by considering question (ii) in the way it should properly be posed -- as two separate questions, depending on whether one is referring to mathematical models or to the real world:

> *(ii a) Within a mathematical model, can we distinguish between a "Butterfly Effect" and the effects of falsely idealized physical quantities?*
>
> *(ii b) In the real world, can we distinguish between a "Butterfly Effect" and the effects of falsely idealized physical quantities?*

For the purposes of this discussion, a "Butterfly Effect" can be defined as any large-scale difference in the eventual state of a system which is (or would be) caused by a small-scale difference in the initial conditions of that system. Smith is correct that the answer to question (ii b) must be "no", but he is correct for the wrong reason. An occurrence of the "Butterfly Effect" in the real weather would be indetectable simply because, as previously discussed, it is impossible to run a controlled, manual test of the weather's behaviour from two slightly different initial conditions, and because the atmosphere is statistically unlikely to perform this experiment for us by coincidentally entering a state similar to a previously observed state. However, this problem has nothing to do with the arbitrary quantitative incorrectness in our mathematical model caused by our idealization of physical values. In fact, we would naturally expect that our model would be quantitatively incorrect at all times, not only for the reasons of artificially idealized properties that Smith cites, but for an array of other reasons -- the necessary oversimplification of its dynamics, for example.

In fact, any model of a complicated system will be drastically idealized in a host of different ways, and when modelling a system with infinite sensitive dependence, we would have no reason to expect this idealization not to cause the model's output data to completely fail to match the quantitative data observed in reality. But, as Lorenz's original paper makes clear, models such as Lorenz's are never expected to quantitatively resemble reality; Lorenz clearly draws only a general qualitative comparison between his model and the actual atmosphere, and then infers conclusions about the real world merely through the readily observed behaviour of his model (such as its easily demonstrated property that mildly varying initial conditions will cause great changes).

Furthermore, Smith is incorrect if he believes that question (ii a) must be answered in the negative. Again, concerns about the numerically imprecise nature of real-world physical values are irrelevant to the qualitative modelling upon which arguments such as Lorenz's are based. In Lorenz's model, there is no need for "realistically" inexact values to exist; their absence is not a "flaw".

And since the model does possess determinate physical quantities, there is no empirical problem in demonstrating that, in the model, the "Butterfly Effect" can occur. This observed effect can then be hypothesized as a property of the real world, on other inferential grounds (such as Lorenz's belief in the rough qualitative validity of the underlying equations). This inference may in itself be somewhat problematic, but there is no fundamental conceptual incoherence in simultaneously accepting artificial idealization in the model and conclusions about sensitive dependence in the real world.

Finally, even if Smith was not guilty of a category mistake, none of these epistemological concerns would influence the fact of the matter in question (i). The mere fact that we couldn't tell whether or not a butterfly had influenced a tornado -- or even tell whether or not such an event was theoretically possible -- would have nothing to do with the question of whether the butterfly had actually influenced a tornado, or the question of whether it was possible for it to do so.

(ii) Indeterminism Revisited

A few authors hold that the combination of the formally non-deterministic events at the atomic scale postulated by quantum mechanics[22] and the sensitive dependence postulated by many accounts of chaos are sufficient grounds for denying the existence of determinism -- not in the sense discussed above, in which empirically unverifiable determinism is merely equated with indeterminism, but in a literal sense, in which genuine macro-level determinism is said to be present in the world. For example, this thesis is found in Hobbs (1991), in which Jesse Hobbs states that

> chaotic systems provide for the reliable magnification of the slightest differences in initial conditions, such as might be provided by quantum fluctuations, into macro-level differences. So it seems likely that they magnify quantum-level indeterminism into macro-level indeterminism throughout their range, which, as mentioned, may dwarf the range of deterministic systems lacking sensitivity to initial conditions. (1991, 143)

And Stephen Kellert advances a similar argument, claiming that

> [c]haotic dynamics will take the tiny indeterminacies of quantum-mechanical systems and stretch them into huge variations, dilating the smallest patch until, at some sufficiently distant time in the future, almost anything is possible. (1993, 73)

However, there are at least two different problems with this position. As Batterman argues in another paper (1993b), it seems theoretically impossible to recover the infinitely fine-structured phase space (which infinite sensitive dependence would require) within the theoretical assumptions of quantum mechanics. A central assertion of this theory, at least in the way it is typically interpreted (e.g. in Brehm and Mullin 1989, 195), is that there are pairs of "conjugate variables" whose simultaneous indeterminacies, when multiplied, must be no smaller than a fixed tiny value known as "Planck's constant". Since one such pair of variables is the position and momentum of any given particle, and since the phase-space representations of dynamical systems will often involve axes for both position and momentum, there seems to be no quantum equivalent to chaos-through-sensitive-dependence. As Batterman explains:

> ...in a classically chaotic system the 'typical' trajectory eventually...explores the entire available phase space, becoming infinitely

> convoluted. There is complexity at all levels of description. But quantum mechanics involves Planck's constant which has the units of phase space area, and through the uncertainty relations places a limit on the level at which such structure can be resolved. Regions of phase space get "smoothed over" so that the concept of complexity at infinitely fine scales has no meaning in quantum mechanics. (1992, 53)

Batterman concludes that the best approach to the question of "quantum chaology" is suggested through Michael Berry's definition of the subject as "the study of semiclassical, but nonclassical, phenomena characteristic of systems whose classical counterparts exhibit chaos". Semiclassical mechanics is a body of theory that lies at the midpoint between classical theory and pure quantum theory; it consists of the study of systems under the mathematical assumption that Planck's constant (which determines the magnitide of the various strange properties of standard quantum physics, such as the "measurement problem" mentioned above) can be considered to approach zero arbitrarily closely -- without actually being assumed to be equal to zero (53). Although semiclassical mechanics is evidently not considered to be a "realistic" theory, since pure quantum mechanics is still proposed as the ultimate description of the world, it has demonstrated pragmatic value in furthering our understanding of certain quantum phenomena (1992, 63), and Batterman proposes that "it seems quite resonable to conclude that what is being studied and discussed at Quantum Chaos conferences is some third domain of behavior; namely, the semiclassical." (63)

The second problem with Kellert's argument is that if the small, random events at the quantum level truly could be magnified into large-scale effects through the action of chaotic systems, as he suggests, then intuitively we should expect to observe a tremendous and global failure of causality at every level of the world[23]. If the minuscule uncertainty of every atom and the tiny effects of every atomic decay could consistently effect massive changes, then we would expect Newton's laws of motion to fail to be even approximately accurate -- there would simply be so many large-scale events happening at random and without sufficient causes, that none of our familiar rules of physical causes and effects would ever be seen to apply. But since we do not observe any such thing, but rather a world in which virtually all macroscopic events have obvious and reproducible causes, this simplistic integration of chaos theory and quantum mechanics seems dubious indeed.

4. CHAOS AND CONTEMPORARY LITERARY THEORY

In 1990, N. Katherine Hayles (a professor of English with additional training in chemistry) published a book entitled *Chaos Bound: Orderly Disorder in Contemporary Literature and Science*, some portions of which had previously been summarized in the paper "Chaos as Orderly Disorder: Shifting Ground in Contemporary Literature and Science" (1989). *Chaos Bound* is worth considering in some detail, because it appears to lie at the center of a minor sub-genre of critical-theory papers produced in English departments which employ "chaos theory" (variously construed) in discussions of either literature or critical theories themselves; I will also consider the papers of several other authors. The general conclusion suggested by these works is that "chaos theory" seems to have been interpreted rather widely, to include a rather extensive array of questionably related topics. Also, even when interpreted more narrowly, "chaos theory" seems to present severe difficulties for anyone attempting to incorporate it into the domain of literary theory, difficulties of which many authors appear to be unaware.

(A) Chaos as Postmodernism: The Case of Katherine Hayles

The impact of Katherine Hayles's work in the field of literary theory may possibly be equalled, if by any work, only by James Gleick's bestselling popularization *Chaos* (1987); this can be readily observed by searching the Modern Language Association indices for publications citing "chaos theory" as a topic: the earliest entry returned is Hayles (1989); only one other such paper appears in 1989, and its subject listings link it to Gleick (1987). By 1994, however, 41 entries citing "chaos theory" accumulate -- including an anthology of papers by various authors edited by Hayles herself, entitled *Chaos and Order: Complex Dynamics in Literature and Science* (1991), and eight of the essays within this anthology (Hayles's anthology actually contains thirteen essays, but some are only tangentially related to chaos -- for example, via general discussions of "complexity"). Hayles therefore appears directly responsible for at least 25% of the published writing in this sub-genre, and examining the bibliographies of some of the remaining papers reveals that she is quite frequently cited by other authors.

Chaos Bound itself is a curious book that attempts to do a number of things simultaneously. Although its introduction bills it as a study of the way in which "various disciplines became interested in exploring the possibilities of disorder", as seen in various scientific developments and simultaneously in the shift to poststructuralism in literary theory, the book also pauses for lengthy considerations of several works of fiction that seem both somewhat unmotivated as selections and only tangentially related to the book's apparent argument, and by its final chapter Hayles is advancing bold theses about the "denaturing" of the human condition in the presence of contextually fragmented music videos, genetic engineering, and reconstructive medical technology. Therefore, some space must be devoted to determining what it is that Hayles is attempting to do in this book.

(i) Extracting the Argument

In *Chaos Bound*, Hayles appears to be primarily interested in tracing a progressive revaluation of the concept of "chaos" through the last two centuries, a process which culminates in the emergence of a "new paradigm" sometime in the mid-twentieth century that can be seen in both the scientific concepts of "chaos theory" (along with supposedly related ideas in information theory and thermodynamics) and in the methologies advanced by literary and cultural theorists such as Jacques Derrida and Michel Foucault. This argument dominates a majority of the book's chapters, and a reader of Hayles's preface could be forgiven for considering it to be the sole thesis of the work:

> ...Then the pendulum, having gone as far as it could in the direction of encompassing order, began to swing the other way as various disciplines became interested in exploring the possibilities of disorder....*Chaos Bound* traces these developments in literature and science and locates them within postmodern culture, particularly within the technologies and social landscapes created by the concept of information....The paradigm of orderly disorder may well prove to be as important to the second half of the century as the field concept was to the first half. (xiii)

But this is not the only argument threaded through the book; in fact, this argument essentially vanishes within chapter 8, "The Politics of Chaos: Local Knowledge versus Global Theory", and is also barely visible in the final chapter, despite the fact that it is apparently intended to be a "conclusion" to the book as a whole. However, for the purposes of the present section, only this argument for the "new paradigm of orderly disorder" will be considered, and even then only in terms of some of its especially problematic highlights.

In defense of Hayles, it might seem fair to raise the objection that attempting to extract and aggressively evaluate the "argument" of *Chaos Bound* is an unfair project, because it applies the specific normative standards presupposed by analytic philosophy to a work intended for reception within the field of postmodern literary theory, which possesses a rather different set of standards. And the text itself attempts to resist such projects; its final pages contain an extended apologia for all that has gone before, presumably intended to anticipate empirical objections to the book by suggesting that the book should not be conceived of as a rigorous, defensible, or unified argument:

> How can one write about postmodernism without being acutely aware that what one writes is itself a construction in the postmodern loop? For my part, I am conscious that out of the nearly infinite number of events that have happened in this century, I have chosen some few dozen to remark upon and weave together in a pattern. There can be no question that by choosing different locales, one could weave entirely different or contradictory patterns. Moreover, the assumptions informing my narrative are no doubt full of the very contradictions and ambivalences that I have described as characteristic of postmodernism. No, I have not described the social matrix....At best I have reenacted the cultural dominant in such a way as to make its dynamics clearer than they have been before. If my narrative is useful, it is because it self-consciously embraces what it cannot help being -- a denatured construction. (293-4)

But appending this disclaimer to the final pages of a book filled with very definite and generalized claims seems more than a little disingenuous. And even if the above paragraph can be accepted at face value, the book remains a valid target for close analysis because of its evident reception by other authors in the field. *Chaos Bound*, along with associated work by Hayles, seems to be at least partially responsible for a widespread impression that chaos theory and postructuralist literary theory are related in some nontrivial way -- either because they are conceptually isomorphic, or because they at least share underlying philosophical assumptions -- and that chaos theory can somehow be shown to mark a "new paradigm" (a term frequently invoked by Hayles) in mathematics and the natural sciences. Therefore, if these ideas can be shown to be resting on empirically dubious grounds, then the reception of the book may be largely unwarranted.

(ii) Claude Shannon: Red Herring of Ordered Chaos?

One of the primary problems of Hayles's book is the fact that her thesis depends on linking occurrences of vague synonyms for "disorder" in otherwise unrelated disciplines. For example, a pivotal early event in Hayles's proposed timeline for the emergence of the "new paradigm" is the 1948 publication of two papers by Bell engineer Claude Shannon, in a company journal under the common title "A Mathematical Theory of Communication". In these papers, which were reissued in 1949 in a volume containing additional commentary by Warren Weaver, Shannon proposes a formula for measuring the "information" present in any communications situation in which a number of events are possible, but there is no advance knowledge of which event will take place; this formula is similar to the physicist Ludwig Boltzmann's 1909 expression for statistical entropy, leading Shannon to characterize his quality as "information entropy":

$$H = -\sum p_i \log_2 p_i \quad \text{(Shannon and Weaver, 14)}$$

This formula expresses the "entropy" (in Shannon's sense) of a given communication source in terms of the probabilities p_i (e.g., p_1, p_2, p_3, ...) of the different choices that might have been made by the person originating the signal, for every one of the independent units of the signal. Using Shannon's entropy equation to quantify the available choice across a long series of related choices, it is possible to calculate the "redundancy" of a given communication system; As Shannon points out, this "redundancy" has practical applications: it can be used either as a means of compressing information through an encoding process, or as a means of reconstructing the correct form of a message in which some elements have been lost due to noise (he notes that the redundancy of English is not used [in 1948] to compress telegraph messages, but that the benefit of leaving this redundancy untouched is that "[a] sizable fraction of the letters can be received incorrectly and still reconstructed by the context" [75]).

Of course, the domain of information theory as discussed in Shannon's papers and Weaver's commentary, which concerns itself exclusively with the efficient and accurate communication of messages, has absolutely no direct overlap with the various problems and methodologies of "chaos theory" as discussed in chapters 1 and 2, which were concerned exclusively with classification and prediction problems in evolving dynamical systems. The link that Hayles attempts to draw between these apparently disparate disciplines is therefore one of conceptual implication, and this link seems dubious not only for the reasons discussed above, but because Hayles arguably misapplies the concepts involved.

"Entropy", as Hayles observes, is a word that carries echoes of Boltzmann's earlier application of the term. For Boltzmann, she says, entropy was a "measure of the randomness or disorder in a closed system", because it was a quantity obtained from the number of internal states available to a system (such as the energy levels permitted to molecules of a gas) at a given time, and because a greater number of potential states (a condition she equates with an increasingly "mixed up or randomized" system) resulted in higher entropy. Thus, the "universal tendency to the dissipation of mechanical energy" in a closed system, initially attribued to William Thompson (Lord Kelvin) and now known as the second law of thermodynamics, can be expressed in Boltzmann's terms simply as the statement that "the entropy increases with the probability of a given distribution, with the most dispersed being the most probable". (41)

This reconstruction is unproblematic, but Hayles then asserts that Shannon's theory performs a "reconceptualization of [entropy] as information", and contrasts this with "Kelvin's mid-nineteenth-century vision of entropy as universal dissipation". (61) She also holds that this new use of the term "entropy" by Shannon somehow prefigured a general reconceptualization of "chaos":

> Once randomness was understood as maximum information, it was possible to envision chaos...as the source of all that is new in the world....As we have seen, chaotic or complex systems are disordered in the sense that they possess recursive symmetries that almost, but not quite, replicate themselves over time. The metaphoric joining of entropy and information was instrumental in bringing about these developments, for it allowed complexity to be seen as rich in information rather than deficient in order. (51)

But Shannon's paper arguably accomplishes no such "metaphoric joining"; from a mathematical standpoint his entropy/information joining is in fact extremely literal, given that Shannon has chosen to re-define "information" in a way that makes it precisely parallel to Boltzmann's definition of statistical entropy (for an expression of Boltzmann's entropy formula that makes this parallel clear, see Cambel, 142). And this is not accidental; in one sense, both Boltzmann and Shannon are concerned with measuring precisely the same thing -- the amount of "choice" available within a system, given certain external constraints (e.g., the microstates available to a gas, given its energy; or the possible messages that can emerge from a source, given the rules and conventions of English spelling) -- and this makes Shannon's choice of the word "entropy" defensible on literal, rather than metaphorical, grounds. Weaver makes this clear in his commentary:

> That information can be measured by entropy is, after all, natural when we remember that information, in communication theory, is associated with the amount of freedom of choice we have in constructing messages. Thus for a communication source one can say, just as he would also say it of a thermodynamic ensemble, 'This situation is highly organized, it is not characterized by a large degree of randomness or choice -- that is to say, the information (or the entropy) is low.'" (13)

Hayles, however, maintains that these two applications of the word "entropy" are conceptually opposed, because the probabilistic aspects of Boltzmann's statistical entropy depict "our ignorance of the microstates", while the probabilities in Shannon's formula "derive from *choice* rather than ignorance; they reflect how probable it is that we would choose one message element rather than another, given a known ensemble". (54)

But Hayles's conclusion seems unwarranted; it is clear that Shannon's probabilities only emerge from "choice" in the indirect sense that they serve to quantify the problems faced by a given system's engineers as a result of their *ignorance of the choices* that will be made in attempts to communicate -- if the constraints on the source are lower, and there are more choices available in the construction of a message, it will be more difficult to distinguish noise from original content in the received signal, and there will be less "redundancy" that can be exploited in signal-compression. Therefore, both uses of "entropy" serve to measure some form of ignorance. In both cases, greater "entropy" signifies a greater lack of knowledge of specific future states, whether this is expressed as "which microstate will the gas be in at any given time?" or "which message will be transmitted by the source?" The fact that Shannon views his fundamental problem as being related to the ignorance of engineers about the specific messages that will be transmitted by the systems they build can be seen quite clearly in his introduction:

> The fundamental problem of communication is that of reproducing at one point either exactly or approximately a message selected at another point. Frequently the messages have meaning....[But] These semantic aspects of communication are irrelevant to the engineering problem....The system must be designed to operate for each possible selection, not just the one which will actually be chosen since this is unknown at the time of design. (31)

Similarly, the fact that Shannon intends to quantify the *problems* caused by choice rather than associating "entropy" with some metaphysical conception of "choice" itself can also be seen by the way in which he immediately removes any reference to actual human free will from his theory, in replacing the hypothetical human originator of a signal with an abstract "stochastic process" (40) considered to be operating purely at random, subject only to the probabilistic constraints of a chosen means of expression (such as the English language). Of course, one might arbitrarily choose to view Shannon's "entropy" as a measure of the choice available to the originator of a signal -- but then one could just as easily declare that Boltzmann's entropy is a measure of the "choice" available to a gas at a given temperature. It seems that the reasonable conclusion in any case is that there is no necessary conceptual opposition between Shannon's entropy and Boltzmann's entropy, and no evidence of any intention on Shannon's part to reverse the dominant connotations of the term "entropy".

Hayles also attempts to advance her case beyond questions about what Shannon actually intended. She is dismissive of arguments which seek to "suppress the metaphoric potential of Shannon's choice", on the grounds that this very metaphoric potential allowed a "new view of chaos to emerge", as seen in "decades of interpretive commentary that sought to explain why information should be identified with disorder rather than order." (50-51) However, a reading of Shannon makes it difficult to imagine how any account proposing a nontrivial explanation for this "identity" can be anything but misguided. Information is "identified" with disorder for Shannon for the simple reason that he began his work by defining "information" in a narrow technical sense that has nothing to do with meaning (as Hayles herself notes repeatedly), but simply expresses the array of potential states of a communication system -- and we have a certain colloquial tendency to refer to things that could be in a large number of possible but unknown states as more "disordered". But "disorder", of course, is an arbitrary value-judgment; there is nothing fundamentally "disordered" about a gas with a large array of available microstates, either, and therefore there is no necessary link between "disorder" and either version of "entropy". Therefore, the attempted linking of Claude Shannon to a "paradigm" that includes chaos theory fails -- not because the link is an attempt to demonstrate indirect relationships between the way that quite different disciplines consider forms of "disorder" (although, as previously asserted, this also seems highly debatable), but for the even stronger reason that Shannon's theory simply does not appear to provide the required "reconceptualization" of "disorder".

(iii) The Derrida/Feigenbaum Parallel

In chapter seven of *Chaos Bound*, Hayles attempts to "show that both scientific and literary discourses are being distinctively shaped by a reevaluation of chaos" (177), by giving examples of "interrelated propositions that appear in separate discourses concurrently because they are responses compatible with the cultural environment". (176) This assertion of a "cultural environment" might seem difficult enough to establish in itself, but she makes her program even more difficult by admitting that a fundamental opposition divides the roles of "chaos" in the two discourses: while "scientists see chaos as a source of order, poststructuralists appropriate it to subvert order". This opposition is attributed to differences in the "economic infrastructures" of the respective disciplines, and she seems to consider this difference to be less important than the way in which the disciplines "concur in assigning a positive role to chaos". (176-77)

The most obvious objection is, as always, the objection that there seems to be little reason to assume that every discipline that invokes one of the concepts that can be associated with the word "chaos" in some way (e.g., unpredictability, disorder, randomness, complexity, indeterminism, determinism with imposed ignorance, entropy in Boltzmann's sense, or entropy in Shannon's sense) is conceptually related to every other discipline that invokes something related to "chaos". This does not mean that any such account is automatically false, however; it seems plausible that at least some concepts appearing in science can have some kind of influence on concepts in other parts of the culture, and vice-versa. But Hayles's account has more specific problems -- she is arguably drawing unjustified implications from the scientific discourses she mentions, thus creating false parallels with poststructuralist literary theory, and apart from these conceptual difficulties, the particular examples that Hayles selects for her account result in grave temporal problems.

For example, one parallel that Hayles attempts to draw is between the postructuralist theory of deconstruction, as exemplified in the theories of Jacques Derrida, and the "mathematical techniques of chaos theory" that can be attributed to Mitchell Feigenbaum (previously discussed in this thesis). Among the points of similarity between Derrida and Feigenbaum that Hayles advances are the following:

> They agree that bounded, deterministic systems can nevertheless be chaotic; they both employ iteration and emphasize folds; and they concur that originary or initial conditions cannot be specified exactly. (184)

It is difficult to evaluate the first claim; it is true that chaos theory in general is the study of deterministic systems, and that the systems termed "chaotic" typically seem to be bounded in some way (either to satisfy demands for "chaos as unpredictability" through phase-space mixing, or to satisfy demands for "chaos as order" through the formation of bounded patterns in phase space). But in the case of deconstruction, this claim is apparently based only on the fact that a text is in some sense a "bounded system", which deconstruction shows to be "chaotic", presumably because it "opens writing to radical indeterminacy". (179) But this is "chaos" only in the loosest sense, construed as anything synonymous with some sense of "disorder"; also, it is unclear how a text (or a reading) is supposed to be in any way "deterministic" for Derrida, given that the repudiation of any single necessary reading of a given text is what separates post-structuralism from structuralism.

Hayles's comparison of the way in which both authors employ an "iterative methodology" and a "concept of the fold" is equally dubious:

> For both Derrida and Feigenbaum, iterative methodology is closely tied in with the concept of the fold. Feigenbaum showed that systems that make orderly transitions to chaos always have folds in their iterative paths....Since the iterative formulae and computer algorithms are perfectly deterministic, [this unpredictability] could come *only* from the initial conditions. Iteration produces chaos because it magnifies and brings into view these initial uncertainties. (183)

First of all, this proposed similarity between the "iteration" practised in both fields is rather strained. The term is apparently applied to Derrida only in the vague sense that Derrida believes that "[a]ny word...acquires a slightly different meaning each time it appears in a new context" (180) and in his "highly repetitive analysis" in which he "[repeats] Rousseau's language with incremental differences" (181-2) -- in other words, Hayles uses the term to denote any sort of repetition of similar phenomena, which is a significant hijacking of the word from its mathematical denotation of the repeated application of precisely the same function. Also, the claim that iteration is related in some important way to chaos is all but incomprehensible as it stands, given that iteration is neither sufficient for chaos (virtually any variety of dynamical behaviour can be defined through an iterative process, including phase-space motion in a straight line at a constant rate forever, or no motion at all), nor necessary for chaos (the Lorenz system, to name one particularly paradigmatic case, is defined as a set of continuous differential equations).

Secondly, in speaking of the "folds" in the paths of the functions observed by Feigenbaum to exhibit the period-doubling cascades previously mentioned, Hayles must either be referring to what Stuart calls a "one-humped mapping whose hump resembles a parabola" (one of the broad classes of functions for which Feigenbaum discovered "universality" in their limiting behaviour when iterated), or possibly to the "folded" structure typical of strange attractors. But no matter what Hayles means, the identification with Derrida's use of a "fold" concept seems inexplicable. Hayles herself explains Derrida's use of this concept as follows:

> In his view such a fold is necessarily present, because there must always be some means by which the text can constitute the differences that enable it to postulate meaning. The fold can be thought of as a way to create the illusion of origin. Once it is in place, all subsequent differences are declared to derive from the originary difference marked by the fold. When the text is 'unfolded', this stratagem is revealed and the regulated exchanges between the alleged origin and subsequent differences that enable the text to operate will appear. (182)

Hayles is essentially claiming that the methodologies of Derrida and Feigenbaum are importantly similar because they both, at some point, invite a metaphorical use of the word "fold" -- either as a description of a curved path on a graph, or as a description of the concealment of some artificially stabilizing assumption within a text. But in one case this word seems to connote bending, while in the other it apparently carries the completely unrelated connotations of marking and concealment -- and in neither case does the specific term "fold" appear to be at all necessary, making Hayles's comparison seem rather forced.

Finally, equating the work of Feigenbaum (or any aspect of chaos theory) with the theories of Derrida in terms of their respective views about "origins" seems extremely problematic. Hayles invites the following comparison:

> ...Derrida attributes textual indeterminacy to the inherent inability of linguistic systems to create an origin. In Derrida, 'always already' marks the absence of an origin, just as inability to specify initial conditions with infinite accuracy does for Feigenbaum. Thus nonlinear dynamics and deconstruction share not just a general attitude toward chaos, but specific methodologies and assumptions." (183)

But this equation seems to require a severe mis-characterization of Derrida. For example, one apparently typical formulation of Derrida's scepticism about the existence of any "origin" that could ultimately justify some objective structure of meaning is given in a version of his essay "Structure, Sign and Play in the Discourse of the Human Sciences" (1970):

> The event I called a rupture, the disruption I alluded to at the beginning of the paper, would presumably have come about when the structurality of structure had to begin to be thought, that is to say, repeated...From then on it was probably necessary to begin to think that there was no center...that the center had no natural locus, that it was not a fixed locus but a function, a sort of non-locus in which an infinite number of sign-substitution came into play...that is to say, when everything became a system where the central signified, the original or transcendental signified, is never absolutely present outside a system of differences. (961)

It is relatively clear that Derrida's scepticism consists in the belief that the "transcendental signified" which could serve as an "origin" for a system of meanings is not only empirically inaccessible, but utterly nonexistent. This point is made even more clearly by Christopher Norris (1991), who summarizes Derrida's commentary on Rousseau (one of the works of Derrida which Hayles also cites) as follows:

> [Rousseau's] texts are a constant, obsessive repetition of gestures which miss their rhetorical mark and display the insufficiency of language when it strives for an origin beyond all reach....The supplement is that which both signifies the lack of a 'presence', or state of plenitude for ever beyond recall, and *compensates* for that lack by setting in motion its own economy of difference....Philosophies that take no account of its activity are thereby condemned (Derrida argues) to a ceaseless repetition of the paradoxes brought to light in his reading of Rousseau. (37)

In contrast, the "sensitive dependence" often postulated within chaos theory is merely an expression of empirical difficulties -- either in specifying a current state accurately enough to determine corresponding future states, or in determining which initial conditions led to the current conditions. In either case, this might be plausibly restated as the conclusion that some "origin" is difficult or practically impossible to recover, but this is not the same as the conclusion that any origin is at best a necessary fabrication, a substitute for something that cannot exist.

There are also temporal and causal problems in Hayles's attempt to link Derrida and Feigenbaum. Derrida's *Of Grammatology*, which Hayles cites as an example of his "iterative methodology", was originally published (in French) in 1967, while Feigenbaum did not derive the "universality" results that Hayles cites until 1976 (Gleick, 175-83); the fact that Derrida's work precedes Feigenbaum's makes it difficult to envision a link between one and the other, especially given that Feigenbaum, in examining the properties of mathematical formulae, would presumably be working in a domain where at least some methodologies and properties were necessary, rather than contingent and open to external influence. Hayles attempts to address these problems by asserting that

> the two theories appear isomorphic not because they are derived from a common source or because they influenced each other, but because their central ideas form an interconnected network....because chaos, iteration, and an unreliable origin form an interconnected system of ideas, the correlative concepts were brought into play once the implications of the original premise were explored. (184-5)

But the claim that there is necessarily an "interconnected network" between these three ideas seems difficult to support once iteration is revealed as being both unnecessary and insufficient for the phenomena discussed in chaos theory (and only vaguely a property of deconstructive theory); "chaos" is shown to be involved in deconstruction only if the word is considered in the widest possible sense (to include any form of "complexity" or "disorder"); and the "origin" is demonstrated to possess a completely different existential status for Derrida than it does for Feigenbaum.

In fact, it rapidly becomes evident the following paragraph, located at the end of Hayles's previous chapter, is not merely another portion of Hayles's overall argument but the cornerstone of virtually her entire thesis:

> I do not believe that the scientists Gleick writes about acted in isolation. I think that they rather acted like lightning rods in a thunderstorm or seed crystals in a supersaturated solution. They gave a local habitation and a name to what was in the air. It was because the cultural atmosphere

> surrounding them was supercharged that these ideas seemed so pressing and important. (174)

The Feigenbaum/Derrida comparison is one example of why the above assertion is absolutely necessary if Hayles's thesis is to be plausible. In Gleick's account (1987), which is partially based on interviews with Feigenbaum (along with a large number of the other scientists he depicts), and which is the only account of the context of Feigenbaum's discovery that Hayles cites explicitly, Feigenbaum's universality results are presented as an "unexpected regularity" that he uncovers by himself and literally by accident (171-2). Gleick claims that Feigenbaum's results "met surprise, disbelief and excitement" from other researchers (180-3), and that "[a]mong mathematicians...a reserved attitude prevailed" for another three years, until his results could be proven "on mathematicians' terms" (183). And while these background details substantially contradict Hayles's assertion that the conceptual underpinnings of chaos theory were "in the air" before they were formalized by scientists, Hayles offers no new empirical evidence that could refute Gleick's account.

(iv) The Case for a "New Paradigm"

Apart from the fact that most of the parallels and connections that Hayles attempts to draw in Chaos Bound seem at least slightly debatable, the argument that chaos theory is part of a "new paradigm" in the sciences appears *a priori* rather difficult to support. It does seem arguable that there was a definite shift in the dominant literary theory of the mid-twentieth century, even if the term "paradigm" is a little strong. The emergence of post-structuralism marks the emergence of both new methodologies for the reading of texts, and new philosophical conclusions about the nature of texts -- for example, more liberal views about the array of possible readings that can be viewed as licensed by the text itself, or increased scepticism towards the subordination of text as a medium to the more "authentic" and unmediated presence of the writer in person.

On the other hand, while it is true that "chaos theory" produced new methodologies for analyzing and comprehending dynamical systems whose behaviour had seemed complicated and incomprehensible, as well as demonstrating that relatively simple systems could nonetheless be stubbornly unpredictable, it seems difficult to defend chaos theory as a "new paradigm" for science. Even if "chaos" is defined in a way that makes it potentially a property of virtually any dynamical system, there are only a limited number of systems where the methodologies of "chaos theory" may actually be helpful.

And as Batterman observes (1992), there are continuing difficulties in integrating the conclusions of chaos theory into older, more dominant scientific theories such as quantum mechanics. It is also instructive to note that quantum mechanics itself, while perhaps not fully defensible as a "new paradigm", possesses a much better claim to this distinction than does chaos theory, yet chaos seems to be in no danger of *replacing* quantum mechanics in its role as the presumed "fundamental theory" of the behaviour of matter -- it is difficult to know what to make of a "new paradigm" that fails to supplant previous "paradigms". Chaos also seems to lack any broad conceptual importance: for example, the uncertainties postulated by chaos theory (quite unlike those inherent in the conventional interpretation of quantum mechanics) are purely epistemological, and pose no new threat to determinism.

In opposition to Hayles's account, Kenneth J. Knoespel advances a more cautious comparison between deconstruction and chaos theory. He opens his paper by warning against the "totalizing vision" of Hayles, noting that it is possible to "compare Mitchell Feigenbaum and Jacques Derrida without transforming information theory into a meta-narrative that reveals 'underlying forces at work within culture'." (102) And unlike Hayles, he does not attempt to depict necessary parallels between chaos theory and deconstruction, but offers only observed and rather general similarities: for example, that both disciplines seem to provide an "invitation for pedagogical play", or that they both depend on the production of "surplus meaning":

> In the case of nonlinear equations, the erratic behavior of a dynamic system in one scale may urge the scientist to alter the parameters used to deploy and interpret the data. In the case of deconstruction, figurative language hitherto read into a traditionally enforced meaning of a text is allowed to play within a larger spectrum of meaning....Each project holds open the possibility of extending the strategies available for analysis. (115-6)

However, while Knoespel's non-causal claims may be more difficult to falsify, they also emerge as interesting but coincidental similarities, rather than necessary links. But his paper at least succeeds in its stated goal of "[limiting] the narrative grounds for comparing chaos theory and deconstruction" (102)

(B) Chaos as Literary Methodology

In contrast to Hayles's account of the necessary parallels between chaos theory and postmodern literary theories, there are a variety of other writers who attempt to apply some form of "chaos theory" to the discussion of texts that are not necessarily "postmodern" in themselves. Because this program removes the need for the cross-disciplinary causal claims of Hayles, it avoids the resultant empirical pitfalls -- however, it is apparent that this use of "chaos theory" has dramatic problems of its own.

For example, in a paper on the Tennessee Williams play *A Streetcar Named Desire*, Laura Morrow and Edward Morrow (1993a) attempt to "explore Williams's work in light of [chaos] theory", stating that it offers "a methodology that recognizes and accounts for the interpenetration of order and disorder" in the play. (60) But it is unclear whether invoking chaos theory adds anything at all to Morrow and Morrow's account. Their approach is typified by their claim that "[the] complex system called 'Stanley' followes simple rules that, like those in Chaos Theory, give rise to complex behavior", (62) and by the following comparison, which is worth quoting at length:

> We can understand the development of Blanche's character better if we can identify her 'attractors.' Stability within a Chaotic system arises from 'attractors,' which are 'nonperiodic' in that they do not repeat themselves (Gleick 138). When plotted mathematically, attractors form nonrepeating patterns....The Lorenz Butterfly is a stable, nonperiodic system, one that approximates but never replicates itself....
>
> '...The set of points going to the same attractor is called a basin of attraction' (Peterson 146), which, in a state cycle, is stable. What is modelled in the basin of attraction, then, is a sort of approximate psychological and social determinism. History is iterative: Blanche will always seduce whomever she can.
>
> Blanche's attractors are illusion and seduction, both emanating from words; her 'basin of attraction' consists of her seducing men with her body and herself with her dreams, repeating the same, would-be-ameliorative pattern in an endless cycle. Though the men's faces change, her devotion

to illusion and her commitment to rewriting history remain constant. (63-4)

However, in both of the above examples, it is difficult to see what the baggage of "chaos theory" adds to claims that could simply be expressed as: "*Stanley follows simple rules that give rise to complex behavior*" and "*Blanche seems locked into repetitive behaviour*". Morrow and Morrow do not argue that there is any necessary relationship between chaotic systems and Tennessee Williams's characters, so they presumably intend to make some kind of metaphorical comparison -- but even as a metaphor, their use of chaos-related terminology seems rather poorly motivated.

There is no general problem with the metaphorical use of scientific terms; if properly chosen, they can be a useful shorthand for connoting potentially illuminating concepts. But since Morrow and Morrow's audience is unfamiliar with the vocabulary of "chaos theory", the authors are forced to labouriously explain why they believe each term is an appropriate metaphor, at which point the term itself loses whatever concise illustrative power it might have had and becomes entirely superfluous to the ordinary-language explanation that follows it. Therefore, they could presumably demostrate equivalent conclusions about Williams's play in far less space, simply by removing every mention of "chaos theory" from the paper, except for the fact that such a paper would presumably cease to exemplify one of the "major critical approaches in vogue today" (in the words of Philip Kolin, the editor of the anthology in which the paper appers, who lists "chaos and antichaos[24] theory" among a catalogue of such "major critical approaches" [ix-x]). And there is the additional problem that the authors evidently feel that they are invoking a full-fledged "methodology" -- in fact, a "paradigm" (60) -- rather than merely a series of novel but inert metaphors, despite extensive textual evidence to the contrary.

And this paper is far from being a unique case; in fact, Morrow and Morrow themselves have written a strikingly similar paper on *The Glass Menagerie* (1993b), and their questionable critical approach is shared by a variety of other authors, such as Raylene O'Callaghan (1991):

> Robbe-Grillet's text moves beyond bi-polarity and neutral-ization [sic]; his symmetries are at once similar and different, repeating and distinguishing. The tiny initial differences often magnify through the levels of the system to provoke major disruptions in a movement that is remarkably similar to what Gleick (1987) has termed 'sensitive dependency on initial conditions' in Chaos theory, where microscopic fluctuations can spread imperceptibly to monstrously transform the macroscopic. (61)

A similar example is provided in a paper by Patrick Brady (1990). In a section entitled "Chaos Theory and the Arts and Humanities", he proposes that

> [t]he non-linear character of chaotic phenomena recalls the distinction between tragedy, which is linear, and comedy, which is non-linear. The 'non-linearity' of comedy is taken to be cyclical in essence, but after all, comedy, like history, never repeats itself exactly. Does this mean that the non-teleological quality of comedy justifies its assimilation to Edward Lorenz's Butterfly Attractor? I think not. The reason has to do with the difference between the Butterfly Attractor...and the Butterfly Effect. (70)

Again, it is unclear what his use of "chaos analysis" (70) provides here; presumably, the relevant properties differentiating comedy from tragedy can be discussed without having to decide whether comedy is best "assimilated" to the "Butterfly Attractor" or the "Butterfly Effect" (whatever "assimilation" means in this context).

Finally, a still more tortured application of "chaos theory" is provided in a paper by William Demastes (1994), who in part attempts to show that "chaos theory can help in comprehending several paths that the theatre has followed since the inception of postmodernism". (242) Discussing Henrik Ibsen's play *The Master Builder*, Demastes writes:

> ...At this point, [the character Solness's] linear, steady-state existence bifurcates -- between his Christian guilt springing from his betrayal of his past and the robustness of the Viking heritage he can't quite attain. The trolls are loosed on him only for him to resist. The result is a continuingly bifurcating bifurcation, until his life is pure randomness. (249-50)

Demastes then takes the alarming additional step of reproducing a diagram of the Feigenbaum cascade for the logistic mapping function (discussed in Chapters 2 and 3 of this thesis), with added labels to indicate how the diagram somehow corresponds to events in Ibsen's play (see Fig. 10). This diagram is apparently intended to bolster the claim that Ibsen "clearly anticipate[d] what chaos theoreticians have since verified: a breakdown of pre-determinable causality, leading to bifurcation, chaos, and finally windows of order within the chaos phase" (250), but "anticipation" in this context seems difficult to take seriously, since the word is presumably intended to connote more than merely coincidental similarities between different uses of vague and general concepts.

Demastes goes on to discuss Samuel Beckett's "chaos-informed rationalism", a phrase which evidently refers to the way that Beckett "aims to create a new hybrid, an integrating vision of order *and* randomness/chaos" (251), and also to Beckett's "recurring repetition of scale wherein windows reveal self-similar patterns of order and chaos". Demastes states that this "integrated vision" (or "paradigm" [251]) better explains features of Beckett's work than do previous accounts such as the "random absurdism" postulated by the critic Martin Esslin. However, if the advent of "chaos theory" was truly required before literary critics could propose that Ibsen's play exhibits a "breakdown of pre-determinable causality" with "windows of order", or that Beckett's apparently "random" plays possess "self-similar patterns", then there seems be far better evidence for a lack of imagination on the part of literary critics than for any claim that "chaos theory" is a important conceptual revolution in literary criticism.

(C) Conclusion

While it is difficult to draw general conclusions about interdisciplinary projects in general from this partial survey of Hayles and other literary theorists, their work does suggest a few cautionary heuristics for any future attempts to unite the concepts of literature or literary theory with those of remote disciplines such as science, engineering, or mathematics. First, it seems unwise to assume that all connotations or metaphorical implications are automatically significant. It may be true, as Hayles asserts, that the terms of science will typically invoke "'shared names' that cause scientific denotation to be interpenetrated by cultural connotations" (50), but denying that there is an important hierarchy in the strength and relevance of these connotations abandons any hope of obtaining normative standards that could prevent virtually any conceptual-unification account from being trivially true. The account of Hayles, for example, depends on the assertion that the word "chaos", as used in "chaos theory" must be understood in terms of its previous connotation of "disorder" -- a vague evaluative term that can in turn be used to link "chaos theory" to any other discipline that examines, celebrates, denies, creates, or suppresses "disorder" in any sense at all.

Also, projects that involve establishing a case for the emergence of some "new paradigm" across a given society should arguably be abandoned in favour of more modest projects that merely seek to uncover avenues of influence between specific disciplines in specific cases. It appears that there will always be necessary and perhaps insurmountable problems (to which Hayles eventually alludes, 293-4) in trying to unite an infinitude of temporally and intellectually disparate events under a single finite and defensible narrative.

Finally, attempts to "apply" the terms, concepts, or methodologies of a given scientific discipline in radically displaced contexts, such as the study of a work of literature or a literary theory, should probably be viewed with suspicion when the scientific discipline in question is relatively new and unfamiliar to individuals outside the field. It appears that such an application can only take the form of a metaphor, and while scientific vocabulary can be used in a metaphorical sense to more precisely illustrate some aspect of a text, it seems necessary that the audience already be familiar with some original, literal meaning for the vocabulary, if such a metaphor is to have any illustrative value. The alternative (as seen, for example, in Morrow and Morrow's essays on the plays of Tennessee Williams) is the metaphorical use of unfamiliar vocabulary which, because its denotations must be explained to readers of the work, runs the risk of being superfluous -- or, in cases where its denotations remain unexplained, completely opaque to any reader not acquainted with the meanings of the terms involved.

CONCLUSION: THE FUTURE OF CHAOS IN THE HUMANITIES

It is arguable that chaos was initially overrated, both by those outside the field and those within it. In a recent *Scientific American* article on complexity theory (the effort to "achieve a unified theory of complex systems" [104]), John Horgan proposes a long series of theories that, he asserts, have all turned out to be similarly overblown in their bold promises of explaining "well, almost everything" (108). This list includes chaos theory, and Horgan says that

> ...the French mathematician David Ruelle, a pioneer of the field, noted four years ago that 'in spite of frequent triumphant announcements of "novel" breakthroughs, [chaos] has had a declining output of interesting discoveries.' (109)

(Apart from complexity, Horgan's catalogue also includes cybernetics, catastrophe theory, and information theory -- all of which are invoked at some point by Katherine Hayles and other literary theorists.) Horgan also quotes mathematical biologist Jack D. Cowan's criticism of complexity:

> Too many simulators also suffer from what Cowan calls the reminiscence syndrome. 'They say, "Look, isn't this reminiscent of a biological or physical phenomenon!" They jump in right away as if it's a decent model for the phenomenon, and usually of course it's just got some accidental features that make it look like something.' (104)

Given the cautionary tone seen in Ruelle's above remarks, and also in his *Physics Today* article (1994), in which he warns that "the problems of relevance and usefulness should not be obliterated by the routine of 'contributing to the scientific literature'", it appears that Cowan's criticism would also be well-taken by advocates of the universal importance of chaos theory.

The future of "chaos theory" in philosophy also appears somewhat restricted. The existing literature suggests that the most fertile initial grounds for philosophical work were obtaining a sufficient and necessary definition of "chaos" and examining the question of whether chaos provided any new ammunition for the determinism/free will debate. However, both of these areas may be of only limited further interest. As I have argued, the formulation of a unified definition that will uncontroversially divide systems into "chaotic" and "nonchaotic" classes must not only confront a discouraging number of proposed properties, but may also be ultimately misguided, since such a definition appears neither possible nor even particularly useful, from the point of view of mathematicians or physicists. There is more work to be done on the question of how the phenomena of "chaos" can be integrated with the theories of quantum mechanics, but this may be a philosophical issue only to those philosophers who essentially pursue questions of theoretical science within the covers of philosophy journals. However, chaos may remain interesting as an especially acute illustration of some problems in scientific modelling (as briefly discussed in chapter 1).

In the area of literary theory, "chaos" seems destined, at best, for metaphorical applications, given the evident difficulties in establishing causal or necessary links between "chaos theory" and either literature or theories about literature. But even the current metaphorical applications of "chaos" often seem highly questionable or even semantically empty. On the other hand, it might be argued that there are many previous cases of scientific vocabulary eventually being transformed into perfectly acceptable metaphorical language, once some version of each corresponding concept has been absorbed by the popular culture. For example, it is obviously possible to speak of "the evolution of the modern novel" without being accused of misapplying the theories of Charles Darwin (similar examples might include the words "species", "Newtonian", and possibly "relativistic"), and so it seems at least initially plausible that "chaos" and some of the other terms discussed in this thesis might eventually achieve a similar status.

However, the present problem with using the vocabulary of chaos theory in literary contexts is twofold. First, any such application may be premature -- while many readers may have come to understand "chaotic" as a synonym for an especially extreme kind of sensitivity, the present uses of terms such as "bifurcation" and "nonlinear" seem to supply no connotations not already present in the ordinary language that is immediately required to explain the terms to noninitiates (and so, for example, "bifurcation" deflates to nothing more than a fashionable substitute for "dramatic change"). Secondly, chaos theory (as I have argued) is simply less important than the scientific theories of Darwin, Newton and Einstein, and so the concepts involved may never achieve the same degree of popularity. The most useful conclusion that general readers might draw from the ideas comprising "chaos theory" is that we now know that some things about the world are unpredictable for reasons that surprise us, and while this is an interesting finding, it is unlikely to threaten anyone's religious beliefs or philosophical convictions.

FIGURES

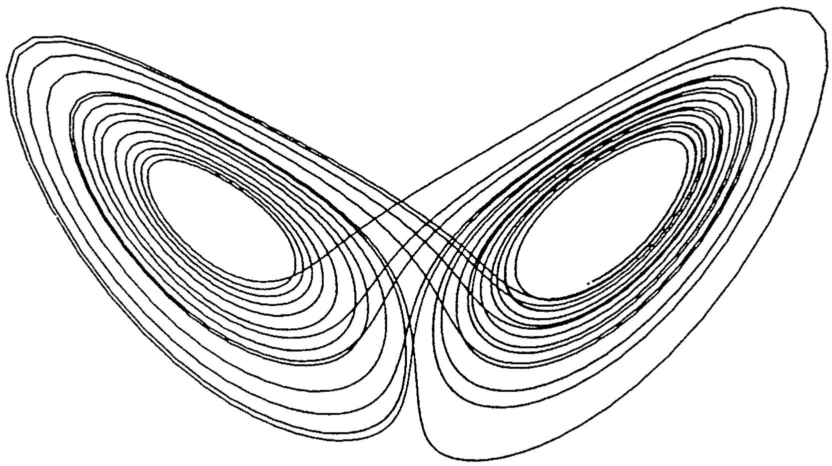

Figure 1. The evolution of the three dependent variables of Lorenz's system, plotted in an imaginary "phase space". (From Stewart 1989, 138)

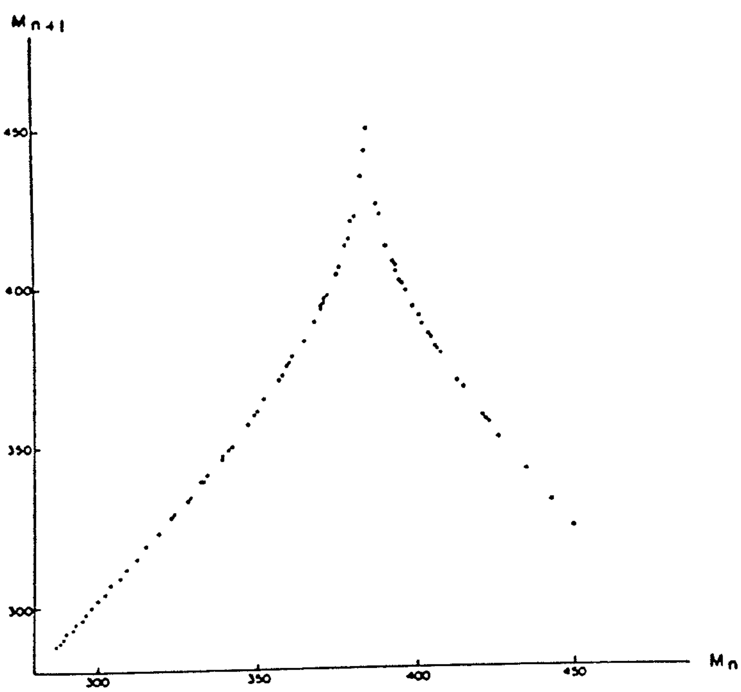

Figure 2. The series of successive maximum Z values for Lorenz's system (M_n); each value in the series is plotted against the following value (M_{n+1}). (From Stewart 1989, 140)

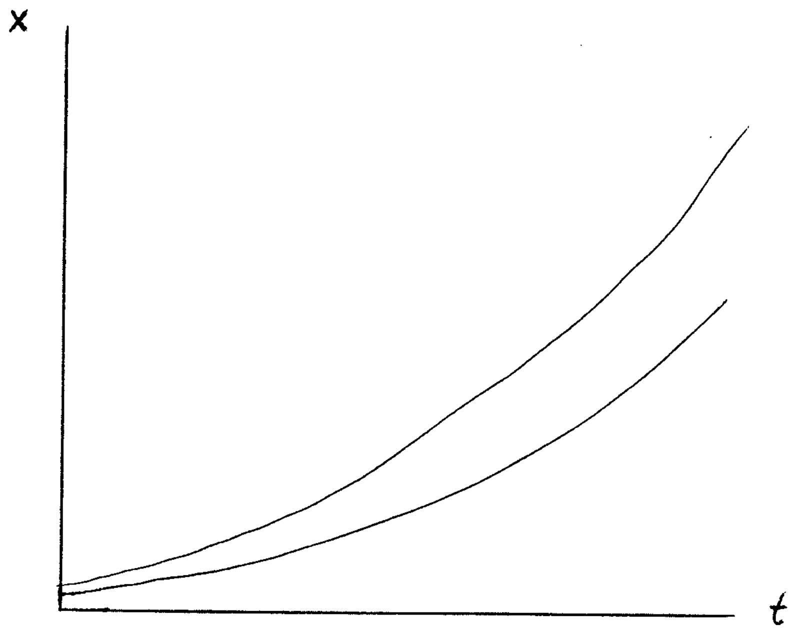

Figure 3. Smith's example of "sensitive dependence": the function $x(t) = 2^t x_0$, plotted for two slightly different initial values, x_0.

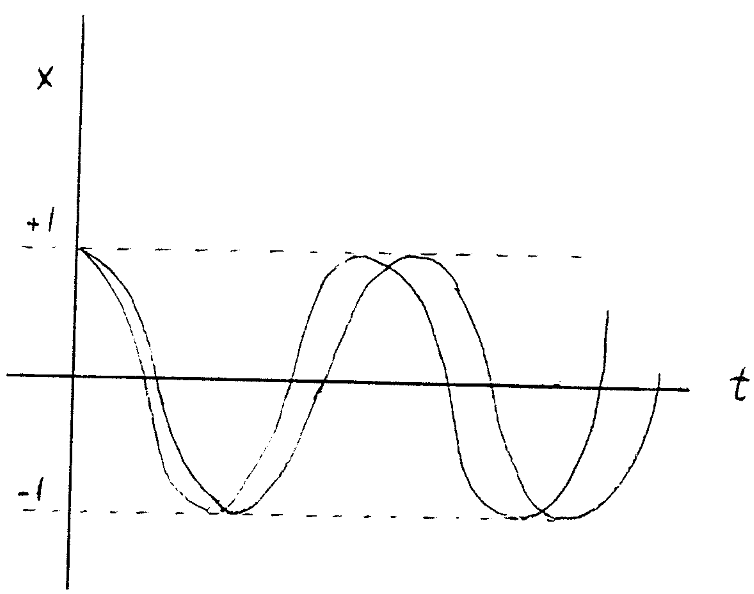

Figure 4. My example of "sensitive dependence with mixing": the function $x(t) = \sin(tx_0)$, plotted for two slightly different initial values, x_0.

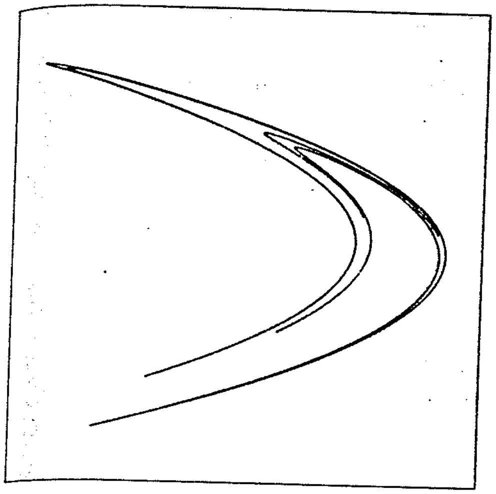

Figure 5. The two-dimensional output of the Henon system. (From Crutchfield 1986, 53)

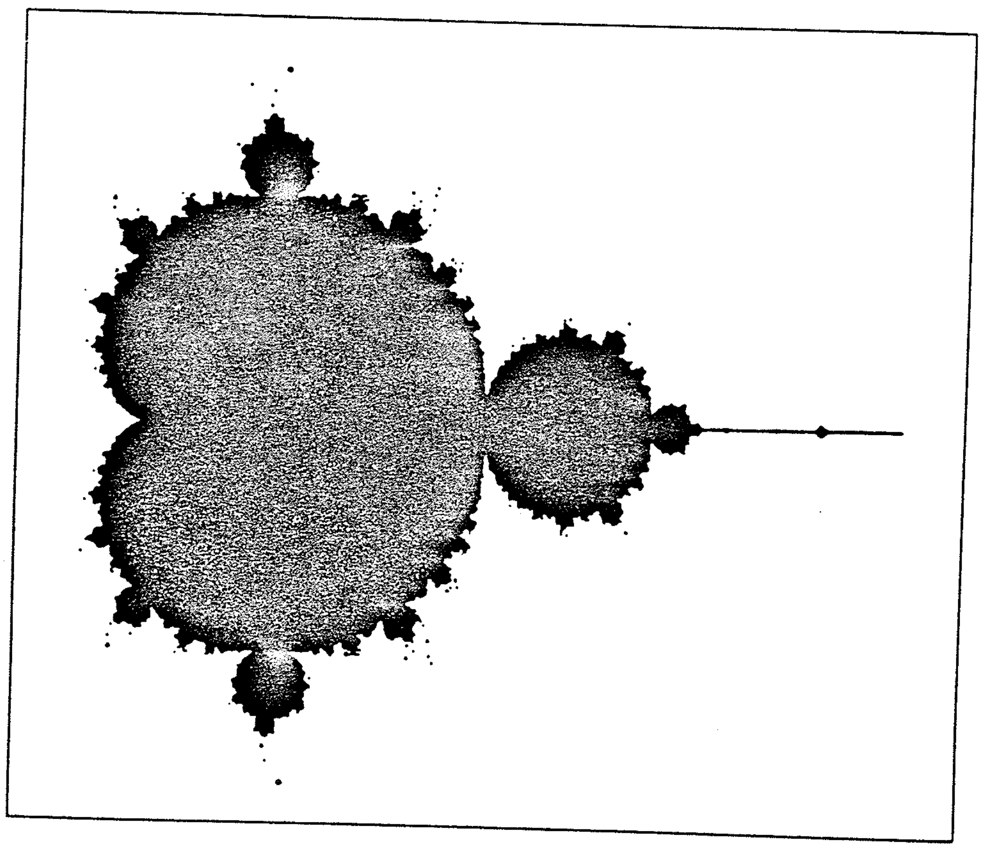

Figure 6. The Mandelbrot set. (From Stewart 1989, 237)

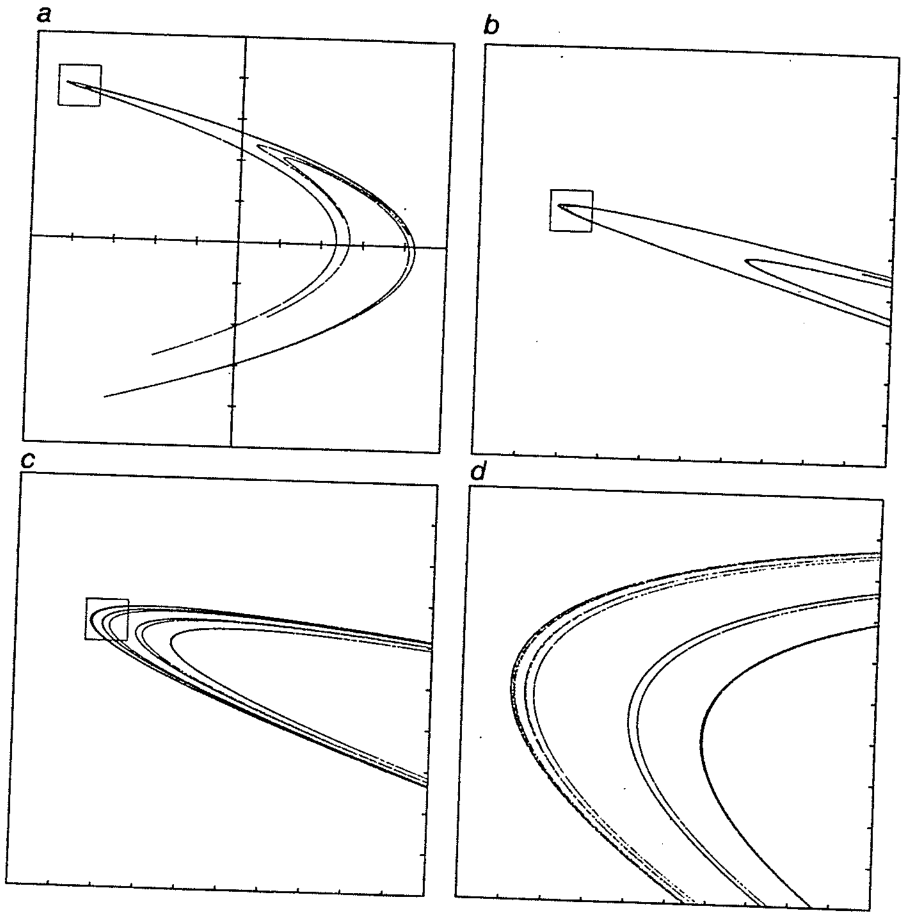

Figure 7. Successive enlargements of the Henon attractor. (From Crutchfield 1986, 53)

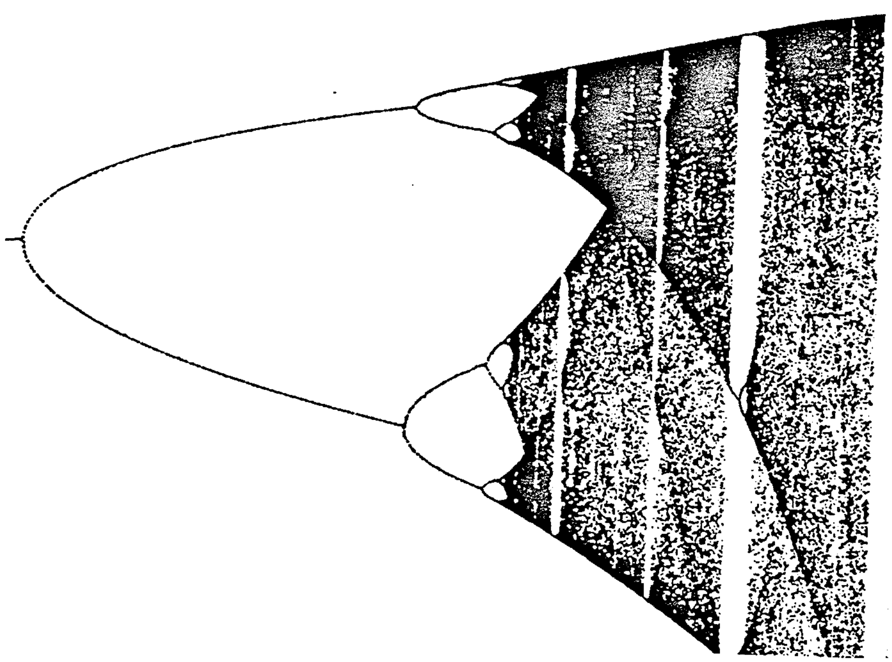

Figure 8. The Feigenbaum "period-doubling cascade", a graph of the values which result when the "logistic mapping" function f(x) = kx[1-x] is repeatedly iterated; the horizontal axis corresponds to different values of the parameter k (between 2 and 4). The horizontal distances between successive branching points on this graph decrease in a constant ratio of 1 / 4.669. (From Stewart 1989, 162)

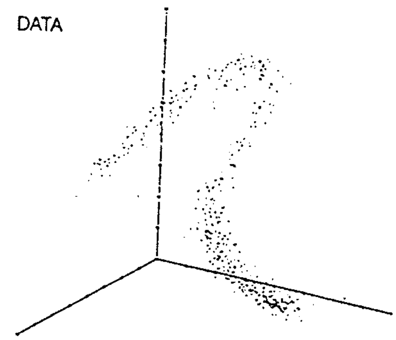

Figure 9. The output of Robert Shaw's dripping-faucet experiment. The 3-dimensional graph is constructed by measuring the intervals between successive drips, and then plotting each value in this data series against the two values which succeed it. (From Crutchfield 1986, 55)

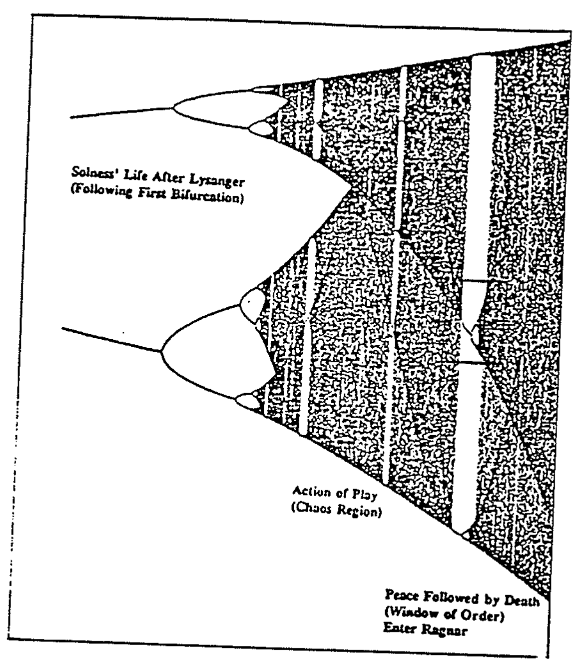

Figure 10. William Demastes's attempt to depict the way in which Ibsen's play *The Master Builder* corresponds to Feigenbaum's period-doubling cascade. (From Demastes 1994, 250)

BIBLIOGRAPHY

Batterman, Robert W. "Defining Chaos". *Philosophy of Science*, 60 (1993), pp. 43-66.

____. "Quantum Chaos and the Correspondence Principle". *Proceedings of the Philosophy of Science Association*, 2 (1993): 50-65.

Brady, Patrick. "Chaos Theory, Control Theory, and Literary Theory; or: A Story of Three Butterflies". *Modern Language Studies*, 20(4), (Fall 1990): 65-79.

Brehm, John J. and Mullin, William J. *Introduction to the Structure of Matter: A Course in Modern Physics*. New York: John Wiley & Sons, 1989.

Cambel, A. B. *Applied Chaos Theory: A Paradigm For Complexity*. Boston: Academic Press, Inc., 1993.

Crutchfield, James P.; Farmer, J. Doyne.; Packard, Norman H.; and Shaw, Robert S. "Chaos". *Scientific American* (Dec. 1986): 46-57.

Da Costa, N. C. A. and Doria, F. A. "Undecidability and Incompleteness in Classical Mechanics". *International Journal of Theoretical Physics*, 30, 8 (1991): 1041-1073.

____. "On the Incompleteness of Axiomatized Models for the Empirical Sciences." *Philosophica*, 50, 2 (1992): 73-100.

Demastes, William W. "Re-Inspecting the Crack in the Chimney: Chaos Theory from Ibsen to Stoppard". *New Theatre Quarterly*, 10(39), (August 1994): 242-54.

Derrida, Jacques. "Structure, Sign and Play in the Discourse of the Human Sciences". In *The Critical Tradition*, David H. Richter, ed. New York: St. Martin's Press (1989): 959-971.

Devaney, Robert L. *An Introduction to Chaotic Dynamical Systems (Second Edition)*. Redwood City: Addison-Wesley, 1989.

Gleick, James. *Chaos: Making a New Science*. New York: Penguin, 1987.

Hayles, N. Katherine. "Chaos as Orderly Disorder: Shifting Ground in Contemporary Literature and Science." *New Literary History: A Journal of Theory and Interpretation*, 1989 Winter, 20(2): 305-22.

____. *Chaos Bound: Orderly Disorder in Contemporary Literature and Science*. Ithaca:

Cornell University Press, 1990.

_____ (ed.) *Chaos and Order: Complex Dynamics in Literature and Science*. Chicago: University of Chicago Press, 1991.

Hirsch, Morris W. "The Chaos of Dynamical Systems". In *Lecture Notes in Pure and Applied Mathematics: Chaos, Fractals and Dynamics.* New York: Marcel Dekker, Inc., 1985. 189-96.

Hobbs, Jesse. "Chaos and Indeterminism." *Canadian Journal of Philosophy*, 21, 2 (1991): 141-164.

_____. "Ex Post Facto Explanations." *The Journal of Philosophy*, 90, 3, (March 1993): 117-136.

Horgan, John. "From Complexity to Perplexity". *Scientific American* (June 1995): 104-109.

Kellert, Stephen. *In the Wake of Chaos*. Chicago: University of Chicago Press, 1993.

Knoespel, Kenneth J. The Emplotment of Chaos: Instability and Narrative Order. In *Chaos and Order: Complex Dynamics in Literature and Science*. Chicago: University of Chicago Press, 1991. 101-122.

Kolin, Philip C. Preface. In *Contronting Tennessee Williams's* A Streetcar Named Desire: *Essays in Cultural Pluralism*. Westport, CT: Greenwood, 1993. ix-xi.

Lorenz, Edward N. "Deterministic Nonperiodic Flow." *Journal of the Atmospheric Sciences*, 20, (March 1963): 130-141.

McAuliffe, Kathleen. "Get Smart: Controlling Chaos." *Omni* (February 1990): 42-48, 86-92.

Morrow, Laura and Morrow, Edward. "The Ontological Potentialities of Antichaos and Adaptation in *A Streetcar Named Desire*". In *Contronting Tennessee Williams's* A Streetcar Named Desire: *Essays in Cultural Pluralism*. Westport, CT: Greenwood, 1993. 59-70.

_____. "Humpty-Dumpty Lives!: Complexity Theory as an Alternative to the Omlet Scenario in *The Glass Menagerie*". *Studies in American Drama, 1945-Present*, 8(2), (1993): 127-39.

Norris, Christoper. *Deconstruction: Theory and Practice (Revised Edition).* London: Routledge, 1991.

O'Callaghan, Raylene L. "Deconstructing Order, Constructing Orders out of Chaos: The Work of Alain Robe-Grillet, Chaos Theory, and the Female Body." *The Language Quarterly*, 1991 Summer-Fall, v 29(3-4): 57-69.

Ott, Edward and Spano, Mark. "Controlling Chaos." *Physics Today* (May 1995): 34-40.

Ruelle, David. "Where Can One Hope to Profitably Apply the Ideas of Chaos?" *Physics Today* (July 1994): 24-30.

Shannon, Claude and Weaver, Warren. *The Mathematical Theory of Communication.* Urbana: University of Chicago Press, 1949.

Smale, Stephen. "An Hour's Conversation With Stephen Smale." *Nonlinear Science Today*, 1, 1 (1991): 1, 3, 12-17.

Smith, Peter. "The Butterfly Effect". *Proceedings of the Aristotelian Society*, 91, (1991): 247-267.

Stewart, Ian. *Does God Play Dice? The Mathematics of Chaos.* Worcester: Billing & Sons, Ltd., 1989.

Stone, Mark A. "Chaos, Prediction, and Laplacean Determinism." *American Philosophical Quarterly*, 26, 2 (April 1989): 123-131.

Winnie, John A. "Computable Chaos." *Philosophy of Science*, 59 (1992): 263-275.

ENDNOTES

1. It is assumed that both sides of each equation are multiplied by some arbitrary time interval "dt"; thus, the left side of each equation expresses the change in a single dependent variable (e.g., "dx" represents the change in x), and the successive resulting right-side values are actually expressed as a multiple of the time interval ("dt") selected. The actual magnitude of this interval is irrelevant to the mathematical behaviour of the system, and serves only as an arbitrary constant of proportionality (see, for example, Lorenz [1963], who refers to this quantity as a "dimensionless time increment" [136]).

2. Of course, like any sensitively-dependent system, we would expect our necessary (if minor) imprecision in setting up the precise initial conditions of the pendulum to amplify into very large imprecision eventually. However, if we can make this initial imprecision sufficiently small in comparison to the known, deliberate differences in initial position that we are investigating, we can expect the multiplying effects of our deliberate changes to outweigh the multiplying effects of our unavoidable imprecision, at least for a certain amount of time (see later section on "Folding and Mixing" in chaotic data).

3. The Lyapunov exponent, as a technique for evaluating sensitive dependence, predates the appearance of "chaos theory"; its namesake is the "Russian mathematician A. M. Lyapunov (1857-1918)." (Cambel 1993, 76)

4. Ruelle qualifies this definition by adding: "[m]ore precisely, if $\|\delta(t)/\delta x(0)\|$ denotes the norm of the Jacobian matrix, the limit [as t approaches infinity] of $L = 1/t \log \|\delta x(t)/\delta x(0)\|$ exists and is called the largest Lyapunov exponent." (25) This more precise definition clarifies the apparent problem of how adjacent orbits can be said to diverge exponentially, forever, in a contained space. In the above formula, the "norm of the Jacobian matrix" quantifies the increasing area of the imaginary surface defined by the contemporaneous points of adjacent orbits. Since adjacent orbits will have loops of slightly different radii, successive loops will create a steadily accumulating linear difference between these contemporaneous points, even while the orbits themselves appear to remain roughly parallel. The "norm of the Jacobian matrix" thus expresses this cumulative error as a complex, expanding surface that may become increasingly perturbed, folded, or wrapped around itself a number of times, and so its area (and therefore the "error" of the system) can increase by an unlimited amount, even within a limited region of phase space. I am indebted to Professor Robert Thomas for explaining this point to me, and I hope that the above paraphrase sufficiently resembles the facts of the matter.

5. One obvious objection, after looking at the graph of the system in Fig. 4, is that the system is not confined along the "t"-axis. However, "confinement" in this case refers to confinement in the values attainable by the variables of the system (which in this case has only one variable, "x"), not to some kind of time-confinement, since of course the system should be permitted to evolve indefinitely into the future. This understandable confusion is due to the fact that the other dynamical systems under discussion (such as the Lorenz system) are not typically plotted with respect to time at all. However, it is very difficult to graphically depict the evolution of a one-variable system, such as the one presently under discussion, without showing its changes with respect to time itself.

6. In the paper "Computable Chaos" (1992), Winnie argues that the common definitions of chaos discussed in the present section can be adequately fulfilled even in systems whose domains are limited to the discontinuous realm of computable numbers.

7. Ergodicity itself can be attained in a vast number of systems, even in the absence of sensitive

dependence; as Cambel states, "the simple harmonic oscillator is ergodic" (150).

8. Despite the dramatic nature of these claims, da Costa and Doria's mathematical "incompleteness" results in the physical sciences may amount to a relatively unsurprising (to mathematicians) application of some generally known intractable problems of mathematics itself (my thanks to Yvon Gauthier for pointing this out). However, they may still come as a surprise to some physicists, or at least to physicists who would subscribe to "Penrose's thesis", which da Costa and Doria describe as the thesis that "classical physics offers no examples of noncomputable phenomena" (1992, 83). Also, whatever their ultimate mathematical importance may be, these results still reveal genuine problems in empirically verifying the existence of chaos in arbitrary systems.

9. Actually, Lorenz uses a more accurate (and slightly more complicated) method of iteration, known as the "double approximation" method (see Lorenz, 133-4, 136). However, it is similar in spirit to the simpler method discussed here, and both methods are merely approximations.

10. In fact, Robert Batterman (1993a) believes this very strongly – but for entirely different reasons. Batterman asserts that integrable systems have "trajectories [which] are confined to N-dimensional tori in the 2N-dimensional phase space" and that "[t]heir trajectories simply cannot diverge from one another very rapidly." (51) However, this claim appears mysterious for a number of reasons. First of all, as stated, it is trivially false; the system proposed by Smith in section (v) of this thesis is both integrable and unconfined, and so are an infinite number of other systems (for example, the system $dx/dt = dy/dt = 4$, which simply evolves along a straight line forever). Secondly, it can be readily observed that "integrable" systems have nothing in common besides the fact that someone has managed to integrate them; the wide array of functions that fall under this classification certainly appear to have no shared topological qualities. Finally, an especially strong rejection of Batterman's assertion can be found in da Costa and Doria (1991), in which they offer a mathematical proof that "the problem of finding a general integrability criterion for arbitrary Hamiltonian [e.g., energy-conserving] systems...is, in fact, algorithmically unsolvable." (1062) Since Batterman's criterion of "confined to N-dimensional tori" would appear to be a good general criterion, Batterman's assertion must presumably be false.

11. In fact, Stone retreats from his position to some extent when he admits that "[o]ne might respond that considerations such as short cuts and requiring less than complete information are not really important to predictability, that all we really care about is getting the desired result....I believe that this in in part a fair response. (127)

12. This work is based on the "Packard-Takens method" of constructing "attractors" for data that exists only as a series of observed values for a single quantity of a system. Floris Takens evidently formulated a general proof that an attractor with a topological resemblance to the "real" (but unknown) attractor of a system could be constructed using just one successively observed variable of the system, plotted against slightly offset versions of its own time-series. (Stewart, 183-4)

13. There are other, more complicated non-"strange" attractors, such as the "quasi-periodic" motion, common in energy-conserving systems (Stuart 105) that results when one or more periodic orbits are combined. If the ratio between the periods of these orbits is an irrational number, the resulting motion will be confined yet never quite repeat itself – but this does not count as a "strange" attractor because it can be expressed as a combination of simpler motions that are not themselves "strange". (Stuart 110)

14. For example, the slightest irregularity in an otherwise perfectly regular system (such as a bit of random noise imposed on a system that otherwise traces out an ideal sine wave in phase space) would render that system "chaotic".

15. The "complex number plane" is the hypothetical array of all possible complex numbers, each of

which consists of a "real" component (like "4.21") and an "imaginary" component (like 20.7 i, where "i" signifies the square root of -1). Each number's point on the plane is determined by using its real component as one coordinate, and its imaginary component as its other coordinate.

16. The "Julia set" for a given number can be produced by using it as the parameter "c" in the equation $z_{i+1} = z_i^2 + c$, which is then iterated a number ot times, beginning with some initial value of z, to see whether or not the result runs to infinity. Repeating this process for every different value of z on the complex number plane, and assigning different colours to the values of z that run to infinity and the values which don't, produces the Julia set for the chosen number, c – an image which may form a connected (if complicated) shape, or may be broken up into discontinous parts. (Stewart 1989, 235-6)

17. Since chaos is supposed to be a phenomenon of deterministic systems, Cambel can be assumed to be using "random" in a loose and empirical sense that is not opposed to a strict metaphysical interpretation of "determinism".

18. Cambel briefly mentions an alternative, computational method of evaluating the Lyapunov exponent of a real system without knowing the equations for its underlying dynamics, using only a time series of output data. However, it is evident that this is a somewhat tenuous test for chaos. Cambel offers an example in which data representing ventricular fibrillation of a human heart is evaluated: some of the calculated Lyapunov exponents for the data are found to be positive, "suggesting chaos", but Cambel then cites the results of a previous clinical study involving dogs which concluded that "ventricular fibrillation was not chaotic in the technical sense", and concludes that "[i]t is quite evident that much work remains to be done before chaos theory can be applied clinically with confidence." (213-4)

19. In a footnote, Kellert adds that "difference equations" such as the discontinuous iterative equations that define the Henon system (see chapter 1) must of course also satisfy this criterion; the important point is that the system "makes no reference to chance". Such a reference could be present either in the form of the equations themselves, or in the fact that the equations are merely stochastic and therefore represent some kind of "averaging" or "approximation of a huge number of complicated interacting subsystems". (56-7)

20. This criterion is necessary because there are a wide variety of mathematical systems which do not have unique solutions. In fact, there are a variety of systems which have no solutions at all, but these are presumably of no use in modelling physical processes.

21. For example, imagine the dynamics of the Lorenz system superimposed on a system following regular annual cycles, where the change due to these regular cycles is a thousand times greater than any change dictated by the "chaotic" components of the system.

22. For example, individual occurrences of atomic decay are said to be without cause under the standard interpretations of quantum physics, although the number of occurrences in a given time can be predicted statistically for large groups of atoms.

23. Even if most systems are not sensitively dependent, they will presumably be either directly or indirectly affected by at least one system which is. It is difficult to envision the effects of even a limited amount of true macroscopic indeterminism propagating through the world, but it seems intuitively clear that such a world would in no way resemble the world with which we are currently familiar.

24. Morrow and Morrow apparently acquire their views about "Antichaos Theory" from an article by Stuart A. Kauffman ("Antichaos and Adaptation", *Scientific American* 265, no. 2 [August 1991], pp. 78-84). This article apparently discusses emergent behaviour in computational networks of

Boolean logic functions, and thus has nothing to do with "chaos theory" as discussed in this thesis, except in the impossibly wide Haylesian sense in which every discussion of any form of order or disorder has something to do with "chaos theory".