MANIFESTATIONS OF CHAOS IN AN ECONOMIC THEORY OF THE ORGANIZATION

VOLUME I

by

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ABSTRACT

The social sciences have failed to develop a body of theory comparable to that attained in the physical sciences. One possible reason is that the Scientific Method is an improper paradigm for investigating the social sciences.

This dissertation argues that the social sciences are undergoing a broad "paradigm shift" battle between competing methodologies, triggered by the failure of the Scientific Method to develop social science knowledge. This failure is a broad psychological event with sociological ramifications.

Toward advancing this hypothesis, three central points are addressed:

1. Human behavior based on simple principles can result in complex or chaotic behavior. Such behavior may be more ubiquitous than supposed. To support these contentions, a simple computer simulation of a human organization is demonstrated to be chaotic. In addition, a simple equation representing growth in organizations, analogous to May's Logistic equation, is derived and shown to be chaotic. These could be representative of many other fundamentally chaotic human processes.

2. The Scientific Method is shown to be inappropriate for investigating chaotic systems.

3. If a paradigm shift in methodologies in the social sciences is underway, characteristic symptoms, taken from historical examples, should be present. In addition, another set of possible symptoms can be developed by examining human cognitive processes. The current state of the social sciences is surveyed, looking for these characteristic symptoms. All of the anticipated behaviors are discovered.

The failure of the Scientific Method to develop social science knowledge has resulted in a disruption of human cognitive processes allowing implantation of erroneous mental models into many social science communities. Many of the conflicts within the social sciences today, manifested in aspects of Postmodernism, Critical Theory, Cultural Marxism, the denial of self-evident truths, the unrestrained use of metaphor as a substitute for reasoned theory development, and other pathologies, can be traced in part to the disruption of mental models and cognitive processes.

CHAPTER I

INTRODUCTION AND OVERVIEW

Context of the Problem

The physical sciences and the social sciences are different—about as different as possible for two fields both called "science." The differences go far beyond the fact that they investigate different subjects. The dissimilarities are so significant that someone new to the sciences could easily conclude that the physical sciences and the social sciences had little in common at all. In these fields, the concepts of what science is and how science should be practiced are fundamentally different.

Consider the situation as might be seen through students' eyes.

In the physical sciences students are introduced to an orderly and systematic academic environment. They are taught a well-defined concept of a theory. They see how theory, working in concert with a methodological process called the Scientific Method, serves to expand knowledge in a way that is generally accepted throughout their community of scholars. They are trained in experimental methods designed to gather data to test theories or to provide a foundation for formulating initial hypotheses. They see that, when experimental conditions are properly and rigorously controlled, repetition of their experiments should—must—obtain similar results, regardless of whether the experiments were conducted today or tomorrow or in Asia or Europe or the Americas. They are presented with a large body of theory that is embraced as common, accepted knowledge. Most particularly, these theories can be used to successfully predict future behavior of physical systems. Some theories are so successful that they are referred to as "laws." And, while debate over new theories can become heated, students are taught that data and observations are the arbiter; new theories supplant older theories when they provide better prediction of observed phenomenon.

In contrast, students in the social sciences are faced with a turbulent and inconsistent environment. There are many competing and contradictory ideas on what constitutes an acceptable theory. To one faction theory might imply an ability to predict future behavior of a system, but to another theory promotes something called "understanding," that has nothing to do with predications at all. To others, theory has nothing to do with understanding or predicting but rather the promotion of certain desired conditions or end states.

Students of the social sciences are also presented with many competing and contradictory ideas of what constitutes acceptable scientific practice. In individual research areas there might be many fundamentally different approaches or basic conceptualizations of phenomenon. Data quality can vary widely, that is, where there is basic agreement on what constitutes acceptable data and agreement on how to measure the quality of data in the first place. Experiments are performed, but the unpleasant truth whispered behind the curtains is that experiments with human subjects or on human systems cannot be exactly reproduced; even when experiments are as similar and as controlled as possible, it is not unusual to obtain significantly different results.

Overall, the social sciences do not have a set of theories accepted in the community as a whole. There are few individual theories that might be nominated to the status of a "law." The introductory education of future social scientists generally consists of a narrative of past theories, when they were superceded, and the cast of characters that engaged in the debate. Acceptance—however temporary—of a social science theory is often more the result of good promotion and clever presentation. Reputations are made and lost, but somehow a knowledge of truth is never attained. Some social scientists have even given up any belief that truth exists.

If the physical sciences and the social sciences are both sciences, why should there be such differences?

Granted, the social sciences deal in studies of human behavior (in all its variability and richness), while the physical sciences are concerned with inanimate matter. The physical sciences have the indisputable advantage in that their experimental subjects are not constantly learning, changing, and transforming during the course of a study or experiment. But even so, variability should not in itself present an insuperable barrier to the regularization of concepts such as how to practice science, and what constitutes a theory. After all, the physical sciences have dealt with stochastic processes, turbulence, and chaos, without jettisoning their fundamental methods or generating widespread challenges to their terms of reference. The variability of human behavior should not prevent agreement as to what constitutes a theory, or what makes up proper experimental methods. Yet, in the social sciences, that agreement is lacking.

Why are there such differences between the social and physical sciences?

Background: Social Science as a Methodological Problem

In the physical sciences—taking physics as the archetypical example—a theory is expected to have the capability to predict the future behavior of a system. Physics has many theories that accomplish this objective.

The social sciences have tried, but failed, to replicate physics' success in creating a body of theory with the ability to predict future behavior of human systems.

Why has this effort failed?

I believe the reason involves two aspects of the nature of human behavior.

First, individual humans are motivated by a broad range of factors. These factors vary in their presence, influence, and strength, and are not constant from individual to individual. Sometimes people acknowledge their motivations and influences, but sometimes they are not even conscious of them. Simple experiments and studies conducted in accordance with the Scientific Method may not capture all the antecedent variables.

In addition, human behavior is subject to a phenomenon known as "extreme sensitivity to initial conditions." Very small changes in input values can result in very large changes in output behavior. The fact that small changes can make a great deal of difference in human behavior is a consequence of the existence of principles of human behavior.

Unlike many social scientists, I believe that there are principles of human behavior. I have seen too many places where such principles exist, and work, on a practical level. There are principles of leadership, principles of war, principles of civil behavior, principles of morality, principles of citizenship, principles of government and justice and equity and many other fundamental principles of human behavior and human relations. People practice them in daily life, from the council of a father to his child ("Son, you just don't treat people that way") to the cry of a small child ("That's not fair!"). Such statements on "how to treat people" and "fairness" are based on fundamental principles inherent in the human social condition, evident even to the smallest child.

Ask a common citizen whether such principles exist and you will most likely get a positive answer. Ask a citizen to *prove* the existence of such principles and you will get a shrug, and perhaps the comment that principles of human behavior are so obvious and intuitive that only academics could convince themselves that they do not exist.

That last sentence was not designed to be arch, clever, or cute. It is a statement for serious consideration, with significant ramifications. Many academics have convinced themselves that principles of human behavior and other self-evident truths simply do not exist.

How might an academic manage to disprove a truth that most people consider self-evident? They might create a hypothesis about the self-evident truth and test it with the Scientific Method. When the hypothesis fails, the rejection of the truth is considered justified.

That happened. Early efforts to identify principles of human behavior were deemed unsuccessful because experiments based on these principles did not give consistent results. Differing experimental results created counter-examples which (in accordance with the Scientific Method) disproved their existence. By that process, in accordance with their training, many thoughtful social scientists became convinced that principles of human behavior are just another bit of folk wisdom that has been proven, scientifically, to be false.

What happens when the Scientific Method disproves something that is blindingly obvious? In the physical sciences, should an experiment yield anomalous results, first the experimental conditions, data, and instruments are verified. If everything checks out, then the fundamental construction of the experiment is re-evaluated. The methodology might be wrong or inappropriate.

Social scientists have looked very carefully at their experimental conditions, data, and instruments, and have made substantial improvements. But when still confronted with results that were intuitively anomalous, they did not take the necessary next step. They re-ran their experiments and re-examined their data, but never questioned the applicability of the Scientific Method itself.

That is not surprising. After all, the Scientific Method has three centuries of success to recommend it, and continues to be a valid methodology for analyzing certain classes of physical science problems. Why question what has been demonstrated to work so well?

The social scientists pass over the fact that there are types of physical science problems that are not well served by the Scientific Method. These are problems dealing with stochastic systems or chaotic systems. Different assumptions and methodologies are used to address those problems, because it is recognized that the assumptions inherent to the Scientific Method are not valid for these "different" problems.

What if the majority of social science problems fell into the category of those "different" problems?

I contend that the social sciences have been using a methodology in their process of scientific inquiry that is inappropriate for developing knowledge about human social systems. While human behavior is generally governed by principles, these principles combine with circumstances in such a way as to result in behavior that is incredibly complex. In many cases the behavior results in deterministic chaos. And, because humans are so unique and dynamic, it is not possible to repeat experiments on social systems with the exact same initial conditions and the same treatments. Even if an experiment could be devised that could overcome this problem, it is impossible to avoid the fact that, as an additional characteristic of deterministic chaos, human systems exhibit extreme sensitivity to initial conditions. The smallest, undetectable differences can have a huge impact in the final results—or could, the next time, make no difference at all.

The result is that there is little regularity in human behavior of the type that conforms to the deterministic assumptions necessary for a system to be appropriately investigated by the Scientific Method. With social science systems, one could draw a straight line through a plot of today's experimental results, only to have tomorrow's results look totally different and unrelated to the previous results. Both experiments might be valid, but the methodology could not deal with the possibility that the system might be chaotic.

Moreover, the investigators were not trained to recognize and deal with such systems. Social scientists are not trained in the mathematics or characteristics of deterministic chaos as part of their fundamental education. They have not been given the background to recognize a data set that is chaotic. Most cannot differentiate between deterministic chaos and random noise. Most importantly, they do not understand the implications of deterministic chaos: that chaotic systems require an entirely new methodology of scientific inquiry.

Background: Social Science as a Behavioral Problem

This problem—the inappropriate analysis of complex or chaotic social systems—can be looked upon as a solely methodological issue. However, there are sociological and behavioral aspects to the situation as well. The social system that deserves examination is the community of social scientists themselves.

What happens in the intellectual life of a community of scholars when a deeply held belief is found to be false? This issue has been studied in the past. In 1970, Thomas Kuhn published *The Structure of Scientific Revolutions*, wherein he postulated that there have been many past examples where deeply held beliefs, which he called *paradigms*, were found to be wrong and were supplanted by new paradigms. Associated with these "paradigm shifts" were certain observable behaviors. So, if the Scientific Method can be characterized as a paradigm, and if that paradigm is wrong for social science problems and is failing to develop social science knowledge, the way is open for other paradigms to challenge the primacy of the established paradigm. It is likely that there is a battle underway between these paradigms. If the social scientific community is in some phase of the process of undergoing a paradigm shift away from the Scientific Method, then some or all of the behaviors that Kuhn found associated with past paradigm shifts should also be evident in the current academic scene.

Are those behaviors evident? Yes. Not only are they present, but the magnitude of the behaviors appear greatly magnified in scale and extent over what has been observed in the past. Why? I believe that there are two contributing reasons.

First, unlike past cases involving a single theory or belief, a paradigm shift involving the Scientific Method involves a process that serves as the foundation and pattern of practice for *all* fields within the social sciences. In my view, this is part of the reason that we see a paradigm shift on an unprecedented scale. If previous paradigm shifts could be likened to a tumble of snow off an overloaded tree branch, what we are seeing today is a massive avalanche crashing down the side of a mountain, enveloping the entire forest.

The second contributing reason involves the mechanics of paradigm shift behavior. One thing that Kuhn did not delve into was the "why" of paradigm shift behavior. Why does a paradigm shift involve certain behaviors? What process causes the behavior?

I believe that there is a linkage between paradigm shift behavior and the way the human mind functions. The Scientific Method nearly duplicates the brain's natural inductive process for developing mental models. A rejection of the Scientific Method thus places humans in a position where something that is natural and comfortable, that has a feeling of "rightness" about it, is being rejected. Thus, the failure of the Scientific Method has the same consequences as a situation where people are forced to deal with the collapse of a natural mental process. The result is a form of cognitive dissonance. Behavior that is common in cases of cognitive dissonance can be seen in past paradigm shift behavior, and also seen in the behaviors prevalent today in social science fields in general, on this greatly magnified scale.

Why has this paradigm shift not been recognized before? I believe that the phenomenon has been hidden by its magnitude: not that it has been too small to see, but it is too large to comprehend by academic specialists. Kuhn discussed paradigm shifts within a particular field or sub-specialty; here, we see a paradigm shift involving many fields simultaneously. Those interested in paradigm shifts have been conditioned to look at the trees, not at the forest. A generalist viewpoint is needed to connect all the individual phenomenon into a coherent pattern.

Purpose of the Dissertation

The purpose of this dissertation is to clearly state and substantiate the argument that the social sciences are undergoing a broad battle between scientific methodologies triggered by the failure of the Scientific Method to develop social science knowledge. This failure has become a psychological event with sociological ramifications. Many of the events and conditions in the social sciences today can be linked with this situation.

The scope of this hypothesis is broad, as it encompasses a holistic look at the social sciences. It would be impossible to address all the points and premises that would constitute a complete hypothesis over all the fields effected by the paradigm shift. However, central points can be identified and examined. Thus, the intent of this dissertation: to introduce the argument, and to substantiate a subset of the key arguments needed to forward the hypothesis.

Towards this end, the following four central points will be addressed in this dissertation:

1. Human behavior based on simple principles can result in complex or chaotic behavior.

2. Complex or chaotic behavior cannot be correctly analyzed with the Scientific Method. Consequently, the Scientific Method is an inappropriate methodology for investigating social science problems.

3. The social science community has tried to use the Scientific Method to develop social science theory, and, as would be expected if human behavior is complex and chaotic, has failed. This failure can be viewed in sociological terms. The inability of the Scientific Method to deal with complex or chaotic systems has resulted in predictable and identifiable behaviors in the social science community. Many of the observed behaviors (and pathologies) in the social sciences today are a direct or indirect result of this failure.

4. The Scientific Method can be likened to a mental model in human cognitive processes. The violation of a human mental model can cause certain behaviors. These behaviors can also be identified in the social sciences today.

Research Approach

This dissertation will concentrate on presenting evidence and support for these four key points. A number of different research methods will be used, ranging from computer simulations, mathematical derivations, causality modeling, to straightforward argumentation. The specific key points will be addressed as follows:

1. *Human behavior based on simple principles can result in complex or chaotic behavior.* To support this contention, a computer simulation of human organizations in a competitive environment was created, based on very simple economic and behavioral principles. This model exhibits chaotic behavior. In addition, a mathematical equation for the population of these organizations was derived from some simple initial assumptions. This function duplicates the behavior of May's Logistic Equation and its variant, the Lotka-Volterra equation, which are classic examples of deterministic chaos.

This raises the possibility that the interaction of other simple human principles of behavior, combined with circumstances, can be inherently chaotic. It does not "prove" that all human behavior in all human systems is chaotic; that is not the intent. The intent is to establish that human systems based on simple principles can be chaotic, and that human systems might be put together in such a way as to be inherently, structurally, and/or fundamentally chaotic. It is a feasibility argument that establishes the possibility that such behavior exists.

2. *Complex or chaotic behavior cannot be correctly analyzed with the Scientific Method.* This will be demonstrated using a few simple examples.

3a. *The social science community has tried to use the Scientific Method to develop social science theory, and has failed.* This will be shown by examining the history of the development of some of the fields within the social sciences, primarily organizational theory and organizational behavior, to coincide with the work shown in the simulation and mathematical modeling exercise. I believe that these individual cases are representative of the social sciences in general. To fully test this proposition would require testing in all the social sciences, which is beyond the scope of this dissertation.

3b. *This failure can be viewed in sociological terms. The inability of the Scientific Method to deal with complex or chaotic systems has resulted in predictable and identifiable behaviors in the social science community.*

If the Scientific Method is accepted as a paradigm, and it is accepted that there is a paradigm shift ongoing, then we can extract from Kuhn's earlier studies a list of expected behaviors.

4. *The Scientific Method can be likened to a mental model in human cognitive processes. The violation of a human mental model can cause certain behaviors. These behaviors can also be identified in the social sciences today.*

Given that the Scientific Method constitutes a mental model of how to do science, the similarity between the Scientific Method model for problem solving and the brain's natural inductive problem solving approach is shown. The fact that most scientific work does not follow a strictly inductive process but advances through the use of analogies (a natural mental problem-solving technique), reinforces the possible connection between the scientific work, the Scientific Method, and natural cognitive processes. Thus, if the Scientific Method fails to develop accurate social science knowledge, then the resulting behaviors could demonstrate similarities to a human's behavior when a mental model is in conflict with the world around it. This is a cognitive problem similar to cognitive dissonance. Thus, we can extract from the literature on human cognitive processes an additional list of behaviors that might be expected.

With these two lists of behaviors in hand, we can then survey the state of the social sciences in general and determine if these behaviors are evident.

The existence of these behaviors does not prove their cause. I hope to not fall into a *post hoc ergo prompter hoc* error. There might be other causes that have resulted in the observed behavior. However, the existence of these behaviors, coupled with the connection between these behaviors with paradigm shifts and collapsed mental models in the past, does establish the feasibility that such causality exists and is in operation today.

Limitations and Key Assumptions

The primary, and most significant, limitation to this dissertation is the very broad scope of the topic. Most dissertations concentrate on a limited topic and work with a hypothesis that can be proven or disproved by experimentation or research. Here, the sweep of the topic extends over all the social sciences, and the hypothesis is broad and has many individual supporting points. The scope is such as to potentially provide fruitful areas of research for a legion of future scholars (both in supporting the contentions offered here, and in fighting rear-guard actions against them). Consequently, this dissertation must approach the problem with limited aims relative to the scope of the hypothesis as a whole. It will concentrate on ensuring that the hypothesis is well and logically presented, and introducing and sufficiently supporting key points to establish the credibility of the central thesis.

There are a number of assumptions that are made in this work. In the computer simulation of the organization, there are assumptions made on the validity of such an approach, a large number of simplifying assumptions, and assumptions for initial values and parameters and a number of other details. Similarly, there are assumptions associated with the mathematical derivation of the Organization Logistic Equation, and in the discussion of paradigms, and in the discussion of cognitive modeling. To list them here would be confusing for those that are not specialists in those individual fields, since it requires context to understand the reason for some of the assumptions and their validity. Consequently, a detailed discussion of assumptions is left for the individual chapters dealing with those subjects.

There is one key assumption that must be mentioned here. This dissertation assumes the existence of truth and the reality of an external world where we can attempt to approximate the truth even if we cannot ultimately be sure if we are successful. It assumes that there are things that are right, correct, self-evident, and invariantly exist.

An additional point must be made regarding the proposed causality linkage. This dissertation proposes that: (1) human behavior can be complex and chaotic, and (2) the Scientific Method cannot properly deal with complex and chaotic behavior, which has resulted in (3) various behaviors in the social sciences, which have been observed in other paradigm shifts or can be extrapolated from human cognitive processes. This proposed causality chain is not meant to preclude other characteristics of human behavior that also can frustrate attempts of analysis by the Scientific Method. For example, in accepting the implications of arguments offered by Kosko (1993, pp. 3-33), it is also probable that human behavior governed by fuzzy logic processes rather than Aristotle's binary logic can also frustrate attempts to perform analysis by the Scientific Method, what Kosko identifies as "the paradigm shift from black and white to gray—from bivalence to multivalence."

In other words, there may be multiple mechanisms of human behavior that undercut the employment of the Scientific Method. While this is another fascinating possibility that deserves exploration, it is also the potential subject for an entirely new dissertation. To fully test this mechanism and explore its implications is beyond the scope of this dissertation.

And, while human behavior can be chaotic and/or fuzzy, I am not proposing that human behavior remains chaotic at all times. Indeed, as will be shown, one of the models of human behavior used in this dissertation has a strong propensity to erupt into chaotic behavior and then dampen it away.

It is possible to generate testable hypotheses about populations. As these populations get smaller, it becomes more difficult. In all cases, one must be alert to excursions into chaotic behavior, and not allow such excursions to disprove hypotheses about behavior in the non-chaotic regions.

Significance of this Work

This dissertation presents several new developments, both in the overall thesis of the work and in some of the detailed work done to support the thesis.

With regards to some of the detailed work, the derivation of the Organization Logistic equation, and the simulation modeling of the organization in a competitive economic environment, is both unique and valuable. The Organization Logistic equation provides a derivation from first principles of a basic human process that is shown to be chaotic. It allows a re-evaluation of the work of other economists and organization theorists, who have assumed that something like May's Logistic equation might apply to human organizations, and made guesses as to the pertinent parameters of the equation. This derivation places their work on a firmer foundation, and identifies explicitly possible variables in the equation.

The overall hypothesis of the dissertation, that there is an paradigm shift in scientific methodology ongoing, has many significant implications. First, it identifies some of the competing methodologies and ideologies (a list with such approaches as Postmodernism, feminist studies, and praxis prominent) as being dysfunctional artifacts of the disruption of mental models. This implies the need to re-evaluate the importance and place of many of these approaches in the ongoing social science dialog.

The implications of the modeling work, the possibility that human behavior can be manifested as complex and/or chaotic behavior, will require some widespread adjustments in the practice of the social sciences. Among the implications are:

1. The need for training students in the social sciences (and their professors) in chaos and complexity theory;

2. The requirement for the development of new methodologies to be employed in social science research and experimentation, including new mathematical tools adapted to the conditions of social science problems, able to deal with excursions into chaos and underlying fuzzy behavior;

3. The need to re-evaluate previous social science research under a new criteria for social science research and experimentation. It could be that the Scientific Method caused many correct hypotheses to be improperly rejected.

Different things might also be learned from past data and experiments that were previously misevaluated under the Scientific Method.

4. More specifically, the need to re-evaluate the place of principles of human behavior in the social sciences. It could be that many of the principles that were proposed by the early pioneers and later rejected deserve re-examination.

5. Broader use of computer modeling, computer simulation, and mathematical modeling as a fundamental tool in the social sciences; and

6. Re-examination of the basis for some of the non-traditional "new approaches" to social science (such as Postmodernism, critical theory, praxis, and the like). This dissertation raises the possibility that the acceptance of non-traditional approaches into the social science community was the result of psychological confusion resulting from the failure of the Scientific Method rather than any inherent truth associated with their approach. Other approaches (such as hermeneutics) will need re-evaluation with an eye towards applying their insights with a different objective, towards the possible development of principles of human behavior. Alternately, the re-evaluation of all the non-traditional methods of inquiry is suggested, towards separating out what is true and useful from what was a side effect of dysfunctional social science.

There are also implications to economics and other fields. Some of these more specialized implications will be better introduced in their individual chapters, to take advantage of an established context for the discussion.

Maintaining a Broad Perspective

This study takes a broad, interdisciplinary view of the current state of inquiry in the social sciences. It brings together fields such as mathematics, organizational theory, economics, computer programming, philosophy, history, architecture, colonial studies, a bit of Eighteenth Century French literature, geology, cognitive studies, and others, mostly from a generalist's perspective rather than from a specialist's concentration on detail and depth. As such, this study, perforce, walks a tightrope. Too little detail and the specialists might conclude that the analysis is superficial and lacks credibility; too much detail and the proverbial forest is lost in examining the bark of the trees.

Overall, the object is to understand the interrelationships that can be seen when a broader perspective is maintained. Mumford (1967, p. 16) refers to this as the special office of the generalist.

> . . . (T)he generalist has a special office, that of bringing together widely separated fields, prudently fenced in by specialists, into a larger common area, visible only from the air. Only by forfeiting the detail can the over-all pattern be seen, though once that pattern is visible new details, unseen even by the most thorough and competent field workers digging through the buried strata, may become visible. The generalist's competence lies not in unearthing new evidence but in putting together authentic fragments that are accidently, or sometimes arbitrarily, separated . . . ignor(ing) the fact that the phenomena studied do not hold to the same principles.

CHAPTER II

AN INTRODUCTION TO CHAOS

Introduction

This chapter provides a brief introduction to chaos theory. Its purpose is to present the specific concepts needed to support this study. Once chaos is better understood, and means of identifying chaotic systems have been established, examples of some of the research in chaotic systems will illustrate the prevalence of chaos in some of the social systems that have been investigated by other researchers.

There are a number of books that can provide a well-rounded introduction to chaos. *Chaos: Making a New Science* by Gleick (1987) and *Complexity: The Emerging Science at the Edge of Order and Chaos* by Waldrop (1992) provide introductions directed toward the non-specialist. An on-line document by Trump (2001), *What is Chaos?* offers a concise review. All three of these resources were used extensively in this chapter.

A note, first, on terminology. Throughout this section (and throughout this study), the terms "chaos and complexity" will be used as if they refer to a single unified theory. That is not accurate. Strictly, complexity theory is different from chaos theory. Some investigators define complexity as the domain that occupies the sliver of the problem space between causal behavior and chaotic behavior. Waldrop (1992) takes that approach. By his definition, systems can exhibit ranges of behavior in different parts of the problem space; in one area simple deterministic behavior, in another, complex behavior, in others, chaotic behavior. The most dramatic difference in behavior is the transition between causal behaviors to complex and/or chaotic behavior. As shorthand, the term "chaotic behavior" is used to represent both complex and chaotic behaviors.

Definitions of Chaos

There is no one globally-accepted definition of chaos. Kellert (1993, p. x) notes:

> Any attempt to define chaos theory must confront the fact that most scientists and mathematicians rarely use the expression "chaos theory" at all, preferring to speak of "the study of chaotic phenomenon" or "investigations of dynamical chaos." . . . There is no simple, powerful, and comprehensive theory of all chaotic phenomena, but rather a cluster of theoretical models, mathematical tools, and experimental techniques.

Given those caveats, Kellert believes that the focus of chaos theory is upon "unstable aperiodic behavior, that is intrinsically unpredictable."

Donahue (2001) has a similar approach:

> The acceptable definition of chaos theory states, chaos theory is the qualitative study of unstable aperiodic behavior in deterministic linear dynamical systems. A dynamical system may be defined to be a simplified model for the time-varying behavior of an actual system.

Kellert (1993, p. 2) states that a dynamical system

> . . . includes both a recipe for producing such a mathematical description of the instantaneous state of a physical system and a rule for transforming the current state description into a description for some future, or perhaps past, time. A dynamical system is thus a simplified model for the time varying behavior of an actual system.

Aperiodic behavior is simply the behavior that occurs when no variable describing the state of the system undergoes a regular repetition of values. Aperiodic behavior may show patterns (such as fractals), but the patterns are not repeated in a regular frequency. The function continues to manifest the effects of any small perturbation; hence, any prediction of a future state in a given system that is aperiodic is impossible.

Another more working definition is from Kellert's (2001) *The Qualitative Study of Unstable Aperiodic Behavior in Deterministic Nonlinear Dynamical Systems.*

> Chaos is qualitative in that it seeks to know the general character of a system's long-term behavior, rather than seeking numerical predictions about a future state. What characteristics will all solutions of a system exhibit? How does this system change from exhibiting one behavior to another? Chaotic systems are unstable since they tend not to resist any outside disturbances but instead react in significant ways. In other words, they do not shrug off external influences but are partly navigated by them. The variables describing the state of a system do not demonstrate a regular repetition of values and are therefore aperiodic. This unstable aperiodic behavior is highly complex since it never repeats and continues to show the effects of the disturbance(s). These systems are deterministic because they are made up of few, simple differential equations, and make no references to implicit chance mechanisms. This is not to be completely equated with

> the metaphysical or philosophical idea of determinism (that human choices could be predetermined as well). Finally, a dynamic system is a simplified model for the time-varying behavior of an actual system. These systems are described using differential equations specifying the rates of change for each variable.

Durham (1997, p. 32) provides a more applied approach to a definition of chaos by listing identifiable characteristics of chaotic systems:

1. Chaotic systems *are* deterministic, nonlinear, sensitive to initial conditions, bounded.
2. Chaotic Systems *are not* random, or periodic.
3. The trajectory of chaotic data mixes in phase-space.
4. A chaotic system usually possesses strange attractors, often with fractal dimensions.

While the mathematics of chaos can become quite complicated, the fundamental ideas are simple. Simple rules can generate complex behavior; small differences in initial conditions can result in wide variance in results; results can be bounded in areas, but not predicted with exactitude.

Several additional characteristics were identified by Durham (1997, p. 2):

- Chaos is not random behavior. It does not arise from stochastic processes. However, the apparent disorder in chaotic systems results in many situations where chaotic systems are mistaken for random systems.
- There is an underlying structure to chaotic systems that allow prediction of long-term trends, and predictions of very short-term behavior.

In addition, one of her points should be modified by ideas brought to my attention by Mr. A. Ihde of The Johns Hopkins University Applied Physics Laboratory:

- The potential for well-behaved systems to become chaotic is often not recognized. Some systems can be driven into and out of chaotic behavior. Thus, under special circumstances, potentially chaotic systems can occasionally be controlled by restricting them to regions where their behavior is orderly.

Dynamical Instability and Sensitivity to Initial Conditions

Dynamical instability refers to a special kind of behavior over time that is found in certain physical systems. Henri Poincaré first discovered it around 1900 when he was interested in Newton's equations describing the motion of planets around the sun. These equations are completely deterministic. Given a set of initial conditions for a planet, the equations predict the planet's position and velocity at any future time. As Gleick (1987, p. 12) put it for the laws of motion in general: "Those who made such [deterministic] models took for granted that, from present to future, the laws of motion provide a bridge of mathematical certainty. Understand the laws and you understand the universe."

However, it is impossible to measure the initial positions and speeds of the planets to infinite precision, both because measuring instruments are not infinitely precise, and because it is impossible to record measurements to infinite precision.

Before Poincaré, this lack of precision was ignored. It was assumed that any imprecision in initial conditions would result in an equivalently small error in prediction. In other words, it was believed that putting more precise information into Newton's laws would result in more precise prediction.

Poincaré noticed that certain astronomical systems did not obey that assumption. For systems of three or more interacting astronomical bodies, a very tiny imprecision in the initial conditions would grow at an enormous rate. Thus, two nearly identical sets of initial conditions for the same system would result in predictions that were vastly different.

Poincaré developed a mathematical proof that demonstrated that such systems could be predicted only if the initial conditions were measured to infinite precision. The smallest imprecision would result, after a short period of time, in such uncertainty that the deterministic prediction was little better than a random guess. The extreme "sensitivity to initial conditions" mathematically present in these systems has come to be called dynamical instability, or simply chaos.

Kellert (1993, p. xi) relates:

> Even in a simple system, chaos means that if you are off by one part in a million, the error will become tremendously magnified in a short time. Sensitive dependence on initial conditions makes chaotic systems unpredictable because even the smallest degree of vagueness in specifying the initial state of a system will grow to confront the researcher with enormous errors in calculations of the system's future state. . . . Chaotic systems also present us with a limitation—namely, an intransigent unpredictability—yet they can appear in the context of exceedingly simple and entirely Newtonian equations of motion.

Gleick (1987, pp. 15-18) and Waldrop (1992, p. 43) both relate an important discovery made in 1963 by the meteorologist Edward Lorenz. Lorenz wrote a computer program to study a simplified model of the weather. At one point he needed to replicate only a portion of a model run. Since computer code is deterministic, he expected that by inputting the values of the variables from the midpoint of the run as initial values he would be able to duplicate the run from that point on without having to spend the time regenerating the beginning of the run (computers then were much slower than those we enjoy now).

Lorenz was surprised to find that the results rapidly diverged from the previous run.

He realized that he was not exactly entering a duplicate of the numbers originally stored in the computer. He entered the numbers from a printout which recorded values only to the third decimal place, while the computer was internally working to the sixth decimal place. He had entered truncated values—entered "94.000" when the computer was actually using "94.000123." Such differences—less than one one-thousandth—would be considered microscopically insignificant by usual standards; it is generally smaller than the accuracy of meteorological instruments.

Thus, Lorenz discovered that even the smallest difference between two sets of initial conditions would result in a huge discrepancy after some period of time, for that set of equations (Figure 1).

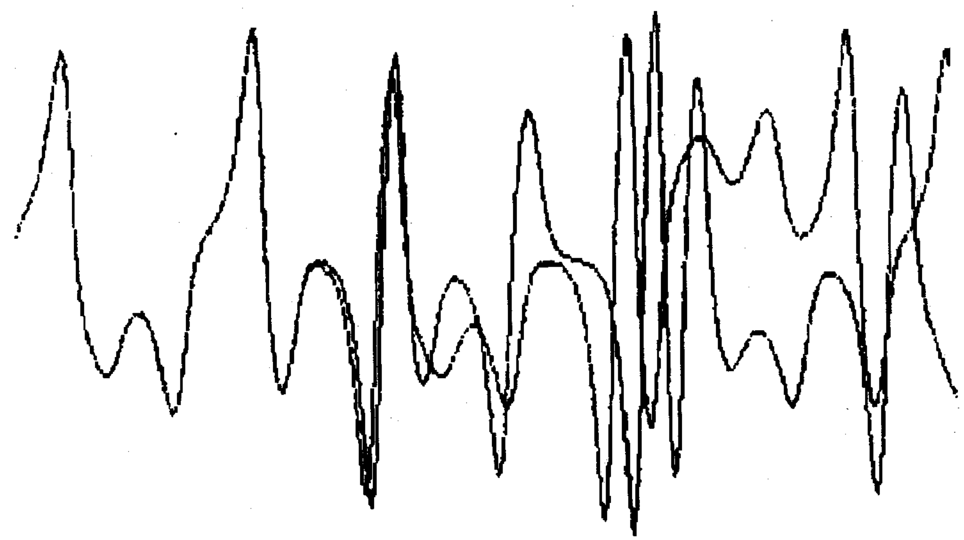

FIGURE 1

LORENZ'S EXPERIMENT*

*The difference between the start of these curves is only .000127.

Source: Stewart, Ian (1990). *Does God play dice: The mathematics of chaos.* New York: Bladewell Publishers, 141.

This principle came to be known as the "butterfly effect," after the suggestion that the disturbance caused by a butterfly flapping its wings can make the difference whether or not a storm arises later on the other side of the world.

The butterfly effect is certainly now part of the current popular consciousness, so much so that it was included in a recent advertising brochure sent out by the offices of E. M. O'Rorke (2001), Chief Operating Officer of *The Economist*. The letter says:

> In essence, the butterfly theory states that any event, no matter how seemingly insignificant or remote, can have far-reaching implications. . . . So imagine how much more powerful you would be if you were kept up to date on events that have <u>clear</u> implications. . . .

It goes on to assert that a subscription to *The Economist* would convey this power. Mr. O'Rorke's faulty understanding of the theory and his rather curious attempt at establishing a causality model (independent variable: reading *The Economist*; dependent variable: power) are not at issue; rather, what is delightful is the fact that at least one advertising bureau thought the concept of chaos theory had so penetrated popular society that it could be used to peddle magazines.

Non-Monotonicity

Non-monotonicity is a property that has been associated with chaotic computer simulations where non-intuitive results are obtained.

An example of this is shown in Dewar, Gillogly, and Juncosa (1996). A combat simulation based on simple Lanchester equations simulated a battle between two forces arbitrarily named "Red" and "Blue." With a given set of forces the Blue force defeats the Red force. Then, in the initial conditions for the next run, the Blue force is made very slightly larger. Intuitively, it would be expected that the Blue force would now defeat the Red force by an even larger margin. However, the result is reversed: Red actually defeats the larger Blue force. If you were to plot the size of the Blue force against the size of the Red force, in certain regions of the plot there were many reversals of outcome as one force or the other was increased in microscopically small increments. These regions are areas of chaotic behavior.

There has been considerable work examining such models. Some of the earliest work was done by Dewar, Gillogly, and Juncose (1991), Palmore (1992), and Louer (1993). Cooper (1994) indicated that non-monotonicity in combat models is not rare. Davis (1992) suggested that such non-monotonicity might be an underlying phenomenon in real life.

Helmbold (1993) found that adding stochastic decision points in place of fixed decision rules did not eliminate the non-monotonic effects. This is significant, since it is known that humans do not always consistently follow fixed decision rules. Deterministic chaos often is associated with systems that have elements of discontinuity. A "yes-no" decision scheme was often indicated as a possible source of deterministic chaos. Helmbold showed that deterministic chaos can also occur with stochastic decision making schemes.

Attractors

One of the properties of chaotic systems is that many are limited in their range of possible results. Under certain conditions, the variables will neither converge to a steady state nor diverge to infinity, but will stay in a bounded region. The system appears to move randomly within that region, and yet obeys a deeper order, since it never leaves the bounded area. The center or centroid of such an area has been called an "attractor."

According to Elliott and Kiel (1996, p. 26), Baumol and Benhabib define an attractor as "a set of points [to] which complicated time paths starting in its neighborhood are attracted." They go on to assert:

> More simply, the term attractor is used because the system's temporal evolution appears to be consistently "pulled" to identifiable mathematical points. The attractor functions as an abstract representation of the flow, or motion, of a system. In short, the attractor . . . is used as a means of examining the structure of the underlying order within a nonlinear system.

As related by Kellert (1993, p. 24), the basic method of reconstructing an attractor from a time series is to plot *x(t),* the value of x at some time t, versus *x(t+a),* where a is some suitable time lag. This creates a simulated state space out of the one-dimensional time-series record.

A famous example of this is the Lorenz Attractor. Lorenz modeled the location of a particle moving subject to atmospheric forces with a set of ordinary differential equations. When the system was solved for a numeric answer, he found that the particle moved wildly and apparently at random. Over time, however, a general pattern appeared. The particle appeared to move randomly yet obeyed a deeper order, since it never left the area of the attractors.

An example of the plot is shown in Figure 2. The pattern is in a butterfly shape. There are two attractors around which the particles appear to revolve.

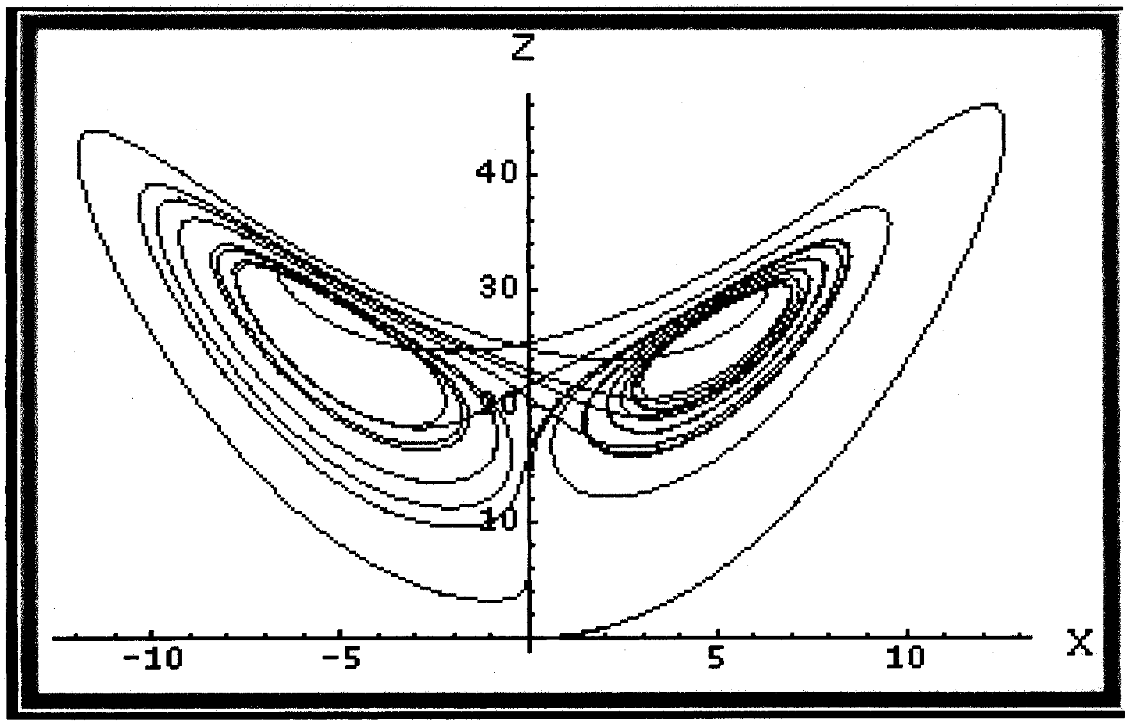

FIGURE 2

LORENZ ATTRACTOR

Lyapunov Exponents

The standard measure for determining whether a system is chaotic is the Lyapunov exponent. According to Banbrook (2001):

> The Lyapunov exponents of a system are a set of invariant geometric measures which describe, in an intuitive way, the dynamical content of the system. In particular, they serve as a measure of how easy it is to perform prediction on the system. When talking about a system here, it is easiest to think of it as a set of trajectories in phase space, i.e., an attractor. Lyapunov exponents quantify the average rate of convergence or divergence of nearby trajectories, in a global sense. A positive exponent implies divergence, a negative one convergence, and a zero exponent indicates the temporally continuous nature of a flow. Consequently a system with positive exponents has positive entropy, in that trajectories that are initially close together move apart over time. The more positive the exponent, the faster they move apart.

Bourke (2002) discussed the meaning of specific values of the exponent.

> If the Lyapunov exponent is positive then the system is chaotic and unstable. Nearby points will diverge irrespective of how close they are. Although there is no order the system is still deterministic! The magnitude of the Lyapunov exponent is a measure of the sensitivity to initial conditions, the primary characteristic of a chaotic system.

Lyapunov exponents are calculated from the time series history of the system. Marion (1999, p. 149) relates it to how much information is lost in the system. If a system is predictable, the history if its past determines its future. If information on the past is lost, the future becomes less predictable. A positive Lyapunov exponent is a measure of how much information is lost per time step.

If, for example, you have a time series of stock market quotations with a Lyapunov exponent of 0.024, then you can expect to lose 0.024 units of information per time step (say, per month), and you can expect to lose all information within about 1/0.024 = 42 months in the future.

As Wolf (1986, p. 273) relates: "Positive Lyapunov exponents indicate orbital divergence and chaos, and set the time scale on which state prediction is possible. Negative Lyapunov exponents set the time scale on which transients or perturbations of the system's state will decay."

In this study, the Lyapunov exponent will be calculated to confirm that the time series generated by the model runs are indeed chaotic. A Lyapunov exponent greater than zero indicates that the time series is chaotic. The calculations will be executed using the software included in Sprott and Rowlands (1992), *Chaos Data Analyzer*.

Summary: Characteristic Indicators of Chaotic Systems

This short introduction to chaos establishes several characteristic properties of chaotic systems, which shall be used later when examining the simulations and models of the organization:

1. Complex behavior arising from relatively simple, deterministic systems;
2. Aperiodicity;
3. Fractals;
4. Sensitivity to initial conditions (the butterfly effect);

5. Non-monotonicity;

6. The existence of attractors; and

7. Positive largest Lyapunov exponents.

Evidence of Chaotic Behavior in Social Systems

Are social systems really chaotic? It would appear that many are.

One indicator is the butterfly effect, or sensitivity to initial conditions. In practice this means that the social system can be very sensitive to events that would appear, when measured against a scale of populations that can number in the millions or billions, as relatively small.

There is a bit of verse that is often quoted as supporting the idea that social systems are sensitive to small perturbations:

> For want of a nail, the shoe was lost;
> For want of a shoe, the horse was lost;
> For want of a horse, the rider was lost;
> For want of a rider, a message was lost;
> For want of a message the battle was lost;
> For want of a battle, the kingdom was lost!

Certainly, history shows many examples of small events with widespread repercussions. For example:

- A single murder disrupted the Middle East peace process, on several occasions;
- A single fire fight at Mogadishu altered U.S. policy in Somalia;

- A single mortar round in the Sarajevo marketplace increased NATO involvement in Bosnia, and paved the way for the war in Kosovo;
- A single collateral damage event, the Al Firdos bunker bombing, changed the nature of the prosecution of the air war against Iraq during the Gulf War;
- A single terrorist attack against the U.S. Marine barracks in Lebanon caused U.S. disengagement from the region; and
- A single assassination is credited with triggering the onset of the First World War.

In contrast to historical and political events, one tends to think of economics and decision sciences as being more orderly and free from the random-appearing behavior that might characterize chaotic systems. However, the complex system nature of economies has been persuasively discussed (Anderson, Arrow, & Pines, 1997). Even simple economic systems, voting, statistics, and group decision procedures admit complex behavior rivaling anything from chaotic branches of the physical sciences. For example, Saari (1995a, b) has examined many common everyday social processes, from politics and economics, and demonstrated that they can behave as chaotic systems.

There is considerable additional evidence of manifestations of chaos in many other social systems. Barnett and Chen (1988) have found that some economic monetary aggregates are chaotic, and have attractors. Berry (1991) has discovered chaotic behavior in long waves of economic development cycles, and in certain aspects of political behavior. Several economic models that were previously thought to be models of equilibrium were discovered by Boldrin and Woodford (1990) to display endogenous fluctuations and other chaotic behavior. Brock and Sayers (1988) found elements of the business cycle displayed characteristics of deterministic chaos. De Grauwe and Vansanten (1990) found deterministic chaos in the foreign exchange markets. Population distributions were found to have quasi-periodicity and chaotic elements by Dendrinos (1991, 1992), along with other elements of urban life.

All this mirrors what physicists have come to recognize that chaotic behavior is widespread and, according to Trump (2001), "may even be the norm in the universe." Chaos and complexity have been found in so many social systems (and life in general) that Krasner (1990) titled his book *The Ubiquity of Chaos*.

Chaos and complexity theory have also been used to explore the potential behavior of social systems in a predictive context. In international relations, Saperstein (1996) used chaos theory and nonlinear dynamics to examine crisis stability and the propensity of war in a bipolar or tripolar political environment. Prector (1999) has effectively used principles of complexity theory to predict social moods, presidential elections, incidents of war, the lengths of hemlines in women's fashion, the popularity of solo singers vs. group bands vs. mixed gender group bands vs. female group bands in popular music, and through them predict movement in the equities markets.

Schuster (1989, p. vii) found that "a sensitive dependence on initial conditions, which result in a chaotic time behavior, is by no means exceptional but a typical property of many systems."

Econometrics, voting, social choice, decision making, sociological law, political economy, price fluctuations, ecosystems, animal populations, presidential campaigns, economic waves, and the outbreak of war have all been examined from a chaos and complexity perspective. There is a huge amount of work being conducted in these areas. One general reference listed over 500 books and articles associated with chaos and complexity themes.

The presence of chaos in (at least some) social systems appears to be an established fact.

A Next Step

This chapter has served to establish a common foundation for understanding what chaos is and how it can be identified. Some of the literature has been referenced that illustrates the many cases where social systems have demonstrated chaotic behavior.

However, chaos in social systems could still be an isolated phenomenon. Finding it somewhere does not mean that it is to be found everywhere. Finding examples of chaos on a case-by-case basis does nothing to suggest that the fundamental structure of social systems could be inherently chaotic.

The next two chapters present work conducted to investigate whether a particular human system might be put together in such a way as to result in chaotic behavior. The particular candidate system was the organization.

In the work on combat models discussed above in the section on non-monotonicity, in several cases the discontinuous nature of human decisions was cited as a cause of the chaos. Reinforcements were either committed or they were not committed; units would either decide to retreat or to stand their ground. When criteria for these decisions were established—"retreat if outnumbered by 3:1"—then there would be very fine dividing lines, where a unit would not retreat if outnumbered by 2.99999999:1. This suggests the possibility that human decision making that involves some criteria or "break points" for making decisions might be candidates for chaotic behavior.

Intuitively, human organizations in general appear to be good candidates for this investigation due to the bipolar nature of decisions. Organizational decisions are often made based on economic criteria that is expressed in numbers, just as the input to many military decisions are expressed in numbers. For example, a commercial organization might take one course of action if profitable, and another if taking losses; a public organization might hire additional employees based on the backlog of work, or the size of the budget; all these are criteria that are generally expressed in numbers, and decisions made based on levels that might trigger certain decisions. Even if the decision criteria were not a precise thresholds—if the criteria were fuzzy—the work discussed above by Helmbold (1993) with stochastic decision making would still suggest that even irregular or imprecise decision criteria would allow the appearance of deterministic chaos.

Consequently, the discussion above suggests that organizations have elements in their construction that may result in behavior characteristic of deterministic chaos. One way to test this conjecture is to build a computer simulation of an organization based on simple principles and see the resultant behavior.

The first step is to identify a suitable candidate organizational system upon which to base the simulation. There are several criteria for this selection.

The first criteria is simplicity. We know that complex simulations are more prone to complex results than simple simulations. Thus, a test for whether decision making in organizations might fundamentally be chaotic would be better served by testing a simple organization.

The second would be the amenability of the theory to computerization. There are organizational theories that assert that organizations are formed are based on human feelings and emotions or other criteria that are not easily quantifiable. This suggests that a theory of the organization that is based solely on economic criteria would be the best candidate. The model chosen must concentrate on rational explanations for organizational phenomenon.

One theory, in the school of the New Institutional Economics, meets both these criteria.

CHAPTER III

THE NEW INSTITUTIONAL ECONOMICS AND AN ECONOMIC THEORY OF THE ORGANIZATION

The Neoclassical Theory of the Firm

The neoclassical theory of the firm has roots in neoclassical economics. In it, most of the uncomfortable aspects of human behavior are assumed not to exist. Human decision making is optimal, rational, and goal directed, human behavior is invariant and working at a rate no more or no less than an idealized capacity, and access to information is perfect.

Moe (1984, p. 749) characterized the neoclassical school in the following way:

> The neoclassical theory of the firm is not in any meaningful sense a theory of economic organization. It centers around the entrepreneur, a hypothetical individual who, by assumption, makes all decisions for the firm and is endowed with a range of idealized properties defining his knowledge, goals, computational skills, and transaction costs. Virtually all aspects of business enterprise that organizational theorists find interesting and consequential—from formal structure to social context and worker psychology to bounded rationality, adaptive search, and goal conflict—are thereby assumed away. The model firm is simply a black box that produces optimal choices automatically as a function of any given environment.

The beauties of the neoclassical assumptions are that they are tractable to the approaches favored by mathematically oriented economists. Economists may calculate equilibrium levels based on these assumptions. That is not to say that equilibrium levels do not exist without the assumptions, only that without the assumptions the computations are generally beyond the abilities of existing mathematical tools.

However, consider what would happen if the assumptions were relaxed. The variability of human performance—and thus, by implication, the variability of the performance of any organization containing these humans—would be considerably greater. Add employee shirking, uncertainty of information, non-optimal search, uneven distribution of risk, rational expectations, voluntary overtime and other observed human propensities and the variability of organizational performance from one organization to another would be great.

The New Institutional Economics

The New Institutional Economics is the title given to a school of thought that addresses a wide range of social science studies from a predominantly economic viewpoint. The core concept, as explained by Williamson (1979, p. 233), centers on the costs (in time, money, and attention) of executing transactions. "The new institutional economics is preoccupied with the origins, incidence, and ramifications of transaction costs."

The origin of the idea of transaction cost economics is generally attributed to Coase (1937, p. 390). In "The Nature of the Firm," he asserted:

> The main reason why it is profitable to establish a firm would seem to be that there is a cost of using the price mechanism. The most obvious cost of "organizing" production through the price mechanism is that of discovering what the relevant prices are. This cost may be reduced but it will not be eliminated by emergence of specialists who will sell this information. The costs of negotiating and concluding a separate contract for each exchange transaction which takes place on a market must also be taken into account. Again, in certain markets, e.g., produce exchanges, a technique is devised for minimizing these contract costs; but they are not eliminated. It is true that the contracts are not eliminated when there is a firm but they are greatly reduced. A factor of production (or the owner thereof) does not have to make a series of contracts with the factors with whom he is cooperating within the firm, as would be necessary, of course, if this cooperation were as a direct result of the working of the price mechanism. For this series of contracts is substituted one.

Niehans (1987, p. 676), in *The New Palgrave*, discussed transaction costs in more detail.

> Transaction costs, like production costs, are a catch-all term for a heterogeneous assortment of inputs. The parties to a contract have to find each other, they have to communicate and exchange information. The goods must be described, inspected, weighed, and measured. Contracts are drawn up, lawyers may be consulted, title is transferred, and records have to be kept. In some cases, compliance needs to be enforced through legal action and breach of contract may lead to litigation. Transactions costs face the individual trader in two forms, namely (1) as inputs of his own resources, including time and (2) as margins between the buying and selling price he finds for the same commodity in the market.

According to Niehans (1987, p. 676): "In modern economies a substantial, and probably increasing, proportion of resources are allocated to transaction costs.

Nevertheless, up to World War II economic theory had virtually nothing to say about them." According to Williamson (1981, p. 3), Coase characterized his treatment of the firm as "much cited and little used." Williamson went on to say that "the reason why [Coase's article] is so widely cited, I submit, is that there is a general appreciation among economists that conventional treatments of firms and markets are not really derived from first principles but are instead arbitrarily imposed." Coase's article was tautological rather than mathematical, so that economists could not "assess the efficacy of completing transactions as between firms and markets in a systematic way."

In another article, Williamson (1981, p. 1538) expressed a possible reason for the long delay in recognizing the importance of transaction costs.

> The main reason is that the origins of transaction costs must often be sought in influences and motives that lie outside the normal domain of economics. Accordingly, a large gap separated an identification of transaction costs, as the main factor to which the study of the organization of economic activity must repair, and the efforts to give operational content to that insight.

Since Coase, and beginning in the 1970s, a number of scholars (Alchian and Demsetz, Arrow, Davis and North, Doeringer and Piore, Kornai, Nelson and Winter, and Ward, to name a few of the earlier proponents) have performed studies that directly or indirectly deal with the New Institutional Economics.

Williamson (1981, p. ix) gives a sense of the extent to which the proponents of the New Institutional Economics believe that understanding the impact of transactions costs is fundamental to a wide range of social issues.

> One can understand the powers and limits of market and internal modes of organization only by examining each in relation to the other. Rather than focusing in technology and production costs, attention is focused on transaction cost economizing. [Transaction cost economics] has bearing on the theory of the firm, the study of labor organization, and the evolution of the modern corporation, including vertical integration and conglomerate aspects.

In another publication, Williamson (1979, p. 233) stated transaction costs are *the* reason for organizations. "Indeed, if transaction costs are negligible, the organization of economic activity is irrelevant, since any advantages one mode of organization appears to hold over another will simply be eliminated by costless contracting." He went on to generalize this further, to say: "Transaction costs are central to the study of economics."

Proponents contend that the ideas embodied in the New Institutional Economics are applicable to many fields other than economics. In particular, in organization theory, attention has been directed to the rules, procedures, and staffing systems that make up the organization of bureaucracy. These properties are the crucial mechanisms of political control and the strategic means, therefore, by which political actors pursue and promote their own interest.

Moe (1990, pp. 215-216) includes the New Institutional Economics as a part of the more general Positive Theory of Institutions, which

> . . . has two basic concerns. One is to explain institutions—where they come from, why they take the forms that they do. The second is to understand their effects for political and social behavior. These are not, of course, really separate. Institutions arise from the choices of individuals, but individuals choose

> among structures in light of their known and presumed effects. A theory capable of explaining institutions, therefore, presupposes a theory of institutional effects.

He went on to say that positive theorists look to "the new economics of organization, especially transaction-cost economics and agency theory" which have found their way into political science, and have used the concepts when dealing with issues regarding "hierarchy, control, cooperation, and specialization, among others." For example, Moe cites Shepsle's (1989) use of the New Institutional Economics in understanding political institutions and the general problem of government commitment.

Moe (1994, p. 21) offered this opinion on the overall importance of the New Institutional Economics:

> In "The New Economics of Organization" (1984), I argued that the new economics was likely to prove the single most important development in the study of political institutions. Almost ten years later, I can say that it is more than living up to its promise. Through its component theories—transaction cost economics, agency theory, and the theory of repeated games—it brings the methodology of economics squarely to bear on organizational issues that until recently were not all that well explained. And because it is largely about bargaining and exchange, it easily extends to politics and, in particular, to political institutions that do *not* make decisions primarily though voting—most importantly, the bureaucracy.

Critics of the New Institutional Economics

Simon (1995) is one of the well-respected investigators of organizational behavior who is critical of the New Institutional Economics, which that he slightingly referred to as "a vigorous cottage industry." He discussed his objections in an article (pp. 274-275) where he indicated that the New Institutional Economics attempts to

> . . . explain when activities will be carried out through the market and when they will be carried out within the skins of firms, and tries to explain also how it is possible for firms to operate efficiently. In the literature of the new institutional economics, two ideas that play a major role in the explanations are "transaction costs" and "opportunism" (for example, Williamson, 1975 and 1985). Sometimes the explanations are couched in terms of 'information asymmetry' (Ross, 1973; Stiglitz, 1974). In other writings these topics are subsumed under agency theory, which treats the employment contract as an optimal contract between principal and agents, and studies how contractual arrangements can deal with shirking and other motivational problems.

Simon (1995, pp. 275, 293) summarized his thoughts in saying:

> Market driven capitalist economies need a theory of organizations as much as they need a theory of markets. The attempts of the new institutional economics to explain organizational behavior solely in terms of agency, asymmetric information, transaction costs, opportunism, and other concepts drawn from neoclassical economics ignore key organizational mechanisms like authority, identification, and coordination, and hence are seriously incomplete. . . . The foundation of these ideas is that a proper explanation of the economic phenomenon will reduce it to maximizing behavior of parties who are engaged in contracting, given the circumstances that surround the transaction. The terms of the contract will be influenced by the access of the parties to information, by the costs of negotiating, and by the opportunities for cheating are most often treated as exogenous variables that do not themselves need to be explained. It has been observed that they even introduce a sort of bounded rationality into the behavior, with the exogeneity of the limits of rationality allowing theory to remain within the magical domains of utility and profit maximization. A fundamental feature of the New Institutional Economics is that it retains the centrality of markets and

> exchanges. All phenomena are to be explained by translating them into (or deriving them from) market transactions based upon negotiated contracts, for example, in which employers become "principals" and employees become "agents." Although the New Institutional Economics is wholly compatible with and conservative of neoclassical theory, it does greatly multiply the number of auxiliary exogenous assumptions that are needed for the theory to work. For example, to explain the presence or absence of certain kinds of insurance contracts, moral risk is invoked; the incompleteness of contracts is assumed to derive from the fact that information is incomplete or distributed asymmetrically between parties to the contract. Since such constructs are typically introduced into the analysis in a casual way, with no empirical support except an appeal to introspection and common sense, mechanisms of these sorts have proliferated in the literature, giving it a very *ad hoc* flavor.

Hart (1995, p. 154) summarizes some of these points when he observes:

> An outsider to the field would probably take for granted that economists have a highly developed theory of the firm. After all, firms are the engines of growth if modern capitalist economies, and so economists must surely have fairly sophisticated views of how they behave. In fact, this is far from the truth. On the one hand, most formal models of the firm are extremely rudimentary, with the firm in question bearing little resemblance to the complex organizations we see in the world. In general, the New Institutional Economics has not drawn heavily from the empirical work in organizations and decision making for its auxiliary assumptions. . . . Nevertheless, it is appropriately subversive of neoclassical theory in that it suggests a whole agenda of microeconomic empirical work that must be performed to estimate the exogenous parameters and to test the theory empirically. Until the research has been carried out (and the existing literature on organizations and decision making taken into account), the new institutional economics and related approaches are acts of faith, or perhaps of piety.

However, March (1992, pp. 228-231) believes that economic theories have been successful in incorporating many of the behaviorists' theories into their approach.

> Limited rationality is now standard economics. Information is assumed to be costly, incomplete, and uncertain. The idea that organizations involve conflicts of interest is embraced fully. The theory considers how contracts can be constructed to permit cooperation among conflicting individuals. Rules, regulations, norms and institutions have been rediscovered and have been given rational interpretations. Ambiguous preferences and incoherence are rationalized and the problems of multiple equilibria are embraced. In short, contemporary microeconomics is a rhetoric of rationality surrounding a rich, behavioral interpretation attentive to limited rationality, conflict, ambiguity, history, institutions, and multiple points of equilibrium. It has adopted most of the substance of many of the early critiques of the theory and seems prepared to do the same with many of the later critiques. As a result, I come not to bury microeconomics but to praise it.

March concluded that:

> Recent works by students of microeconomics, decision making, organizations, and institutions provide a basis for constructing ideas of governance that reflect serious understandings of limited rationality, conflicts of interest, ambiguity, and inconsistency, and the role of norms, rules, and institutions.

Application to Public Organizations

One might ask if there is any application of these ideas to public organizations. After all, most public organizations are not motivated by profits. Frant (1996, pp. 365-367) indicates that, when compared to work done using agency theory, "the application of transaction-cost economics to the public sector has been less successful." Moe (1984, p. 739), in an article entitled *The New Economics of* Organization, indicated that:

> So far, positive political theory has not contributed much to our understanding of public bureaucracy. In part this is due to the unsympathetic treatment that rational modeling and most other modes of quantitative analysis have long received from students of public administration.

However, Frant (1996, pp. 365-367) went on to argue:

> The underlying ideas of transaction-cost economics can be quite powerful in increasing our understanding of public-sector organizations—shedding light on such important current issues as earmarking, public authorities, and the civil service personnel systems. . . . We must extend the framework by applying the logic of transaction-cost analysis to the very different institutions of the public sector.

In Frant's view, there exists a system of incentives, which he characterized, after Williamson (1985), as "High-Powered and Low-Powered." These incentives are the public sector equivalent to private sector transaction costs.

> High-powered incentives are those provided by market transactions, in which efficiency gains from a particular transaction flow directly to the parties transacting. In hierarchies (organizations), on the other hand, incentives are low powered. The particular individuals involved in a transaction may get a raise, a promotion, and so on, but they generally are not able to personally lay claim to the gains from trade.

The point here is not to pursue an argument on the validity of such characterizations of incentives; rather, it is simply to establish that, with a bit of transformation, the ideas of transaction-cost economics have been made applicable to models of public sector organizations, and that there are researchers now engaged in that effort. Any work on the theory of organizations formulated through transaction cost considerations will directly and indirectly apply to public organizations: directly, in that fact that public agencies make rules and laws that impact on for-profit organizations, and should thus fully understand these organizations; and indirectly, in that eventually the ideas considered in this work will likely be translated into terms applicable to the construct and principles of public organizations themselves.

Certainly a “public administration” model of an organization would look different from what is shown in this paper. Some “utility function” (in classical economics fashion) would have to be invented to take the place of costs and profits. But it could conceivably be accomplished. The model, most likely, would be more complex and more difficult to quantify than a simple model of a for-profit organization employing “economic man” with the capability of optimal decision making. A public administration model of a public or non-profit organization would thus, in this element at least, be more prone to complex, chaotic behavior. Given that the purpose of this experiment is to select a model of organizational/social behavior that is arguably as rational and deterministic as can be conceived by modern theories. Making our for-profit organization a public or non-profit organization, driven by a different incentives structure, would defeat the purpose of the experiment.

CHAPTER IV

MODELING AN ORGANIZATION UNDER AN ECONOMIC THEORY OF THE ORGANIZATION

Introduction

The following is a description of the computer model of an organization. The "Overview" section is in a narrative format in order to be most accessible to those not familiar with computer simulations. Subsequent sections contain more detailed review of specific components. Finally, the Appendix contains a listing of the computer code, in the Microsoft QuickBasic programming language.

Some aspects of the model come from practical experience. In the 1990s, I founded Conflict Analytics, an organization that created computer simulations of conflict situations. The organization was small (never more than 15 people), the employees worked out of their homes, and the company did not have any assets other than its product and the company's reputation (our main product was nominated for awards by the software industry press). Operations were suspended when an outside buyer purchased the rights to the firm's entire product line. Over the short time of Conflict Analytics' existence, I learned some things about creating a for-profit organization from scratch.

The firm modeled in this dissertation has many similarities to Conflict Analytics, which was a knowledge-based organization that essentially peddled processed information rather than a physical product. Thus, when the model of an organization in this dissertation ignores consideration of capital plant, physical property, depreciation of assets and similar aspects of traditional companies, it does so with some precedent.

One of the company's products was a series of computer simulations of historical naval combat, sold on the entertainment software market. This product had a business cycle exactly as depicted in the model: build the product, market it, see how successful it was, and based on the success or failure of that product make plans to hire or fire personnel in preparation for producing the next software product. This type of time-sequenced behavior inspired the decision to employ a relatively simple time-step simulation process rather than developing a continuous-time differential process simulation, which would have been more much more complex and thus presumably more prone to chaotic behavior.

So, while in many aspects the model of an organization shown here is idealized, it has roots that are firmly grounded in actual organizational experience.

Overview

Assume that there exists an entrepreneur who believes that there is a business opportunity in selling a product. In accord with the long and hoary traditions of neoclassical economics, the product shall be christened "widgets." The entrepreneur has an amount of initial capital to begin her venture.

The entrepreneur knows that there are two ways in which she can get her hands on widgets to sell. One way is that she could contract for them from independent workers who make widgets. She would have to contract for them on a worker-by-worker, widget-by-widget basis. The cost would be relatively high for such piecework; in addition, it would take time to negotiate these contracts, so she would only be able to contract for a limited number of widgets. If she wanted to expand sales, she could hire additional people to negotiate additional contracts for more widgets. These additional personnel would each have the capacity to negotiate a given number of contracts for widgets over a given amount of time.

There is another way to obtain widgets. The entrepreneur could start an organization that builds widgets. She could hire workers to make the widgets under a long-term employment contract. However, she will also eventually have to hire line managers to coordinate the workers, and support personnel to do additional tasks such as secretarial support, parts support, maintenance, janitorial, and similar tasks, or else the workers would be diverted from making widgets to doing those tasks and would not efficiently produce widgets. When the organization becomes large enough, she will need staff managers, to coordinate the line managers and the activities of the organization as a whole.

Fortunately, widget manufacturing does not require an investment in buildings or tools, and the materials are free, so all she has to be concerned with is paying the salaries of her employees. She also finds that it is most efficient to build and sell widgets in batches. So, her company would have a defined production cycle:

1. Make a batch of widgets;
2. Sell the widgets;
3. Compare costs and income to calculate profits (or losses);
4. Based on profits or losses, make decisions on whether to expand or cut production, i.e., hire or fire personnel.
5. Hire or fire personnel; and
6. Start again with step 1.

One thing that attracts her to building her own organization is that she would not have to constantly renegotiate contracts with the employees, because they would agree to work for an indefinite period under fixed terms of employment. However, the process of hiring personnel does involve the costs for the personnel search and for training the new personnel.

Another important decision the entrepreneur must address deals with the relative mix of skills. She must hire each type of personnel in the right balance with the other types. For example, if she did not have sufficient support personnel the efficiency of the workers would be reduced, and they would produce fewer widgets. If she hired too many support personnel, it would be a waste of money. There are similar balances to be struck in hiring the right number of line managers and staff managers. Too few managers and production would be impaired due to management tasks being undone or improperly done; too many managers would also reduce production, since managers with excess time on their hands would tend to micromanage the workers and pester them to distraction.

The good thing about our entrepreneur, though, is that she has perfect knowledge of all things going on in her organization. She knows exactly how many of each type of employee she needs to hire to get the maximum production from her organization as a whole. What helps immensely is that all her personnel have exactly the same characteristics and capacities, and always perform in the expected manner. And, to make the problem easier still, all of her personnel are paid the exact same wage, which, in her inflationless universe, stays fixed for the duration of their employment. She must pay a transaction cost to initially hire the personnel, but she does not have to pay that transaction cost each time a new set of widgets are to be manufactured.

With all this information our entrepreneur can then look at the transaction and widget costs associated with contracting for widgets individually, and compare it against the costs of making her own widgets in her own organization.

She calculates that it is cheaper to hire her own employees.

So, with her initial amount of capital, she allocates some of it to pay for the expenses associated with the growth of her organization and salaries for the time in which the personnel must be trained to their duties. She also decides that, when each batch of her widgets are sold, a certain percentage of the profits will be plowed back into the organization and allocated to further growth, if such growth would be expected to increase her profits.

So, she starts her organization with one line manager and two workers. She does not hire any support personnel to join the organization immediately, because there is as yet little support work to do—she could not employ a support person full time without wasting most of her money on slack capacity. Instead, she has discovered that there are support workers available that she can hire on a part time basis. These "temps" will work for only that amount of time that she needs them, so that she does not have to pay salary to workers in her organization that are not fully employed. However, she has to negotiate with these temps each time she needs them, so there are transaction costs involved, and this makes the temps rather more expensive on a per-hour basis. However, our entrepreneur is smart enough to know exactly when it is better to hire a temp and when it is better to hire a permanent support person. She always arranges to have sufficient permanent and temporary support personnel so that widget production is not impaired.

Unfortunately, managers are a more difficult problem. There are no management "temps," because management personnel are highly skilled, have to know the organization well, and be skilled widget-production coordinators before they can be effective managers. Thus, our entrepreneur recognizes that sometimes she will have too few managers and sometime she will have too many managers. Fortunately, she knows exactly when it is most cost-effective to hire on new managers. If market conditions require her to reduce the size of her organization, she will tend to retain her managers until a full work group of ten workers must be let go.

Production workers are only available on a full-time basis, which is acceptable, because she does not intend to produce any fractionalized widgets.

So, she looks upon her three initial employees, declares Western Widgets Worldwide (the original www.) to be good, and starts her organization to making widgets.

Initially, one line manager for only two workers leaves the line manager with a lot of time on his hands. It is really cheapest to make her own widgets, but until she builds up her work force she could use her line manager's extra time to contract for some widgets made outside of the organization, if they could be sold at a profit. So, she checks the current price of widgets, checks to see what it would cost her to contract for some production outside the organization (including all transaction costs), and calculates if she can make some profit by that means. Such production would be profitable. So, she tasks her manager to use his extra time (his slack capacity) to contract for extra widgets. She incorporates this aspect into her business, so that every time she begins a widget production cycle, she checks to see if there is slack management capacity and if she could make some extra profit by contracting for widget production outside the organization.

As a side note, if transaction costs were very low, the possibility exists that the entrepreneur could create an organization that contracted out for all of the widget production. All she would have to hire as permanent staff in her organization would be managers, and the support needed for her managers. This would be a purely contracting organization. Such organizations do exist in the world, as epitomized by the building contractors who have a minimal permanent staff and hire their workers nearly on a day-by-day basis by visits to the union hall. The simulation program was written to include the capability to simulate this type of organization also. However, it proved to be beyond the scope of this immediate effort, and so no analysis or model runs of pure contracting organizations were performed.

Our entrepreneur makes a very healthy sum from selling her first batch of widgets.

She pays the salaries of her organization and the fees for any production that was contracted external to the organization. If she had to borrow some capital to pay salaries awaiting the cash flow from the sale of the widgets, she deducts out the cost of borrowing. What remains is profit. The government takes a great interest in her profit, requiring her to pay half to them. Of what monies remain, she allocates a percentage of the after-tax profit to reinvest into growth, and the rest remains as working capital for the organization.

Her organization grows rapidly. As more widgets are produced and placed on the market, the price of widgets declines, but as long as profits are growing she continues to reinvest in her organization and grow her organization's capacity.

Eventually, there comes a time when her profits cease growing. When that occurs, she stops bringing on new workers, but since the organization is still making a profit, and her return on capital is greater than she would have achieved by bank deposits or bonds, there is no reason to fire any workers.

Possibly, her production is so great and the price so reduced that her organization must fire workers to cut production capacity until the organization regains profitability.

Detailed Modeling of the Organization: Characteristics

At this point it is important to review what we are trying to create and where we are going.

The object of this exercise is to create a model of the organization in accordance with the precepts of the New Institutional Economics, and see if it possibly will generate chaotic behavior. The point is to see if the fundamental processes inherent in creating and running an organization might result in deterministic chaos. Consequently, it is important to eliminate all other sources of variability in the model. It is suspected that variable, irrational decision processes could lead to variances in performance, so that will be eliminated; similarly, anything that contributes to other human variability in production, hiring, and other areas will be eliminated. Other marketplace realities, that Moe (1984, p. 740) characterizes as the "interactive, highly strategic process we ordinarily associate with competition," such as different organizational forms, and market-structuring devices such as vertical integration of product lines, tie-in sales, resale price maintenance, and others, would also tend to increase the variability of the results. Such variability would increase the possibility that the fundamental model would be driven to exhibit random or complex characteristics. Only a few basic core elements of the organization will be retained. The object is to build a model of the organization that would be expected, by any detached observer, to behave in a orderly, deterministic way. The objective is to see if such a model results in deterministic chaos.

Consequently, everything in the organization is going to be perfect—communications, information, and decision making—and regularized by the application of well-defined and reasonable heuristics. The "human-ness" of the organization—structure, social context, worker psychology, bounded rationality, adaptive search, goal conflict, and much more—will all be assumed away, so that any irregular, complex, or chaotic behavior could not be attributed to an irregular or complex behavior of any of its constituent parts.

As suggested by Moe (1984, p. 740), such a model "is simply a black box that produces optimal choices automatically as a function of any given environment." Moe goes on to assert that it is exactly from such a model that neoclassical economists perform their mathematics and derive stable conditions of equilibrium.

Rosser (1996, pp. 206-207) quotes Brock as dismissing some of those assumptions as a "nirvana fallacy." However, the model accepts nirvana in order to create what economists indicate should be an orderly model that converges to some point of equilibrium.

Consequently, the model begins with neoclassical assumptions, and adds in aspects of the New Institutional Economics (specifically, transaction costs) to build a model of an organization. The model is constructed with an eye to populating it with characteristics that are as rational and regular as possible.

Moe (1984, p. 741) says:

> These models can be easily criticized. But this is not new or even very disturbing to the mainstream supporters of neoclassical theory, since the theory was never intended to be realistic in its assumptions or to be accurate in its micro-level implications for individuals and organizations. Its development and use by economists have generally been grounded on its value in deriving formal implications for market prices and outputs, resource allocation, equilibria, and other aggregate properties of economic systems. Assumptions about the firm and perfect competition are simply vehicles by means of which these ends are pursued.

So, a model will be constructed with certain overall characteristics, such as:

Simplicity: We know that complex models have a greater tendency to become chaotic. We also know that complex or chaotic inputs can also drive a model into chaotic behavior. Thus, the model will be simple, while still trying to capture the organizational environment.

Frankly, that is a difficult requirement. A model can be so simplified as to be worthless. There is a story where the Defense Department requested a contractor to build a "simple" combat simulation for evaluating the outcome of battles. A comedian on the contractor's staff suggested that they just turn over a handful of dice, with the following simulation instructions:

"One, two, three, we win; four, five, six, we lose."

Obviously, taking simplicity to an extreme can be unproductive. There must be some richness to the model in order to meet a minimum level of credibility that the system under investigation is actually being simulated. Thus, the model assumes four categories of worker skills (line manager, staff manager, worker, and support), but pays them all the same and assumes that there is no worker turnover. Their performance on the job is deterministic, and a function of the proper balance of skills existing in the organization. Outside production can be arranged, but it is more expensive than internal production and has transaction costs.

Deterministic: We know that stochastic models are prone to complex behavior, and tend to reinforce complex or chaotic behavior. So, none of the decisions in the model will be left to chance. Every decision will be made in accordance with a fixed rule or heuristic. This also sets up an initial condition required for deterministic chaos. It is the demonstration of deterministic chaos under such assumptions that is fraught with implications to the social sciences in general.

Rational Decision Making: According to Rosser (1996, p. 199), "a central assumption in economic theory is that of rational behavior by economic agents, although this has been widely criticized by many noneconomists." This paper does not intend to engage in this debate, but rather to institute a reasonable set of deterministic decision heuristics for the key decisions in the model. The point is to institute a regularity in the decision process so that it is not possible to point to an irregular or irrational set of decision rules as the source of any resulting chaos.

This will require an additional set of assumptions that are enormous simplifications of real-world conditions. In general terms, the model will assume that the performance of personnel in the organization is fixed, invariant over time and circumstances, and is exactly the same as other personnel of the same skill category; that there is perfect communications between all personnel; that decision makers have perfect information of their environment and their organization. Clearly, these simplifying assumptions do not hold in real life.

The model will also assume that all decisions are made rationally, and that the decision maker makes optimal decisions. This is also an assumption of questionable validity. There is a considerable literature that documents how humans "satisfice" rather than optimize, and how their decisions can be biased, irrational, or even arational. However, the assumption of a rational "economic man" is common in the economics literature. Simon (1985, p. 301) has indicated his disagreement with the assumptions implicit in any model employing economic man:

> From the earliest times it has been seen that human behavior is not always the result of deliberate calculation, even of a bounded rational kind. Sometimes it must be attributed to passion, to the capture of the decision process by powerful impulses that do not permit the mediation of thought.

Consequently, any depiction of observed human behavior would tend to drive towards complex, chaotic results. In other words, "real" behavior will be more prone to chaotic results than that depicted in the model.

Decisions in the simulation are irreducibly nonlinear. Decisions are either "no" or "yes," 0 or 1, not continuous in the region between 0 and 1. One either buys a product or does not buy it; one hires an individual or does not hire him; one either starts the car or leaves it cold. Such non-linear aspects are a characteristic associated with chaotic performance in computer simulations, as documented by Saeger and Hinch (2001), and Davis (1992). Generally, the economic models that give such lovely conditions of equilibrium are of the form of differential equations, which do not have the discontinuities inherent in the human decision process.

Many economic models assume perfect prescience on the part of decision makers. Such assumptions are unrealistic. If there were actually perfect prescience, no one would ever have lost money on the stock market. Empirical evidence supports the critics of perfect prescience. In one example, the Urban Institute's Penner (2001) found that the track record of the Federal Government's budget predictions to be dubious at best. Prector (1997) believes that economist's predictions of the future of the economy are little better than straight-line extrapolations from past data. And, in the stock market, the opinions of experts are taken as a *contrary* indicator. When, for example, over 70% of stock market advisors are predicting a rising market, a decline is almost guaranteed.

Consequently, the simulation bases decision making on current data, without future forecasts. Any model of human's predictive powers would also have to include human predictive error, which would be a source of additional variability in the model, which is to be avoided in the experiment.

Single Goal Directed: The simulation will assume that decisions are made in order to maximize the profit of the organization. This is a common assumption in economic modeling, allowing simplification of computations and avoidance of messy human behavior. Nicholson (1978, p. 9) has objected to the unreality of such an assumption when he noted:

> The model of a *profit-maximizing firm,* obviously a simplification of reality, abstracts from the personal motives of a firm's managers and does not treat personal conflicts between the managers. It assumes that profits are the only relevant goal of a firm; other possible goals, such as obtaining power or prestige, are treated as unimportant. The simple model implies that a firm has sufficient information about its costs and the nature of the market to which it sells that it is able to discover what its profit-maximizing decisions actually are.

However, the assumption is often made in order to reduce the complexity of economic analysis, and usually yields classical, non-chaotic behavior. So again, the simulation does not include observed human behavior would tend to drive the results into chaotic behavior.

Time Step Simulation: Computer simulations can be characterized as either continuous or time step. In a continuous simulation, processes are considered to start and stop as they would tend to happen in reality. Processes can occur simultaneously or sequentially, can take short or long durations, and can be very complex. The simulation continuously calculates the state of the systems being examined.

A time step simulation is a simplification of such processes. In a time step simulation, the computer calculates all the processes that would occur in a single block of time. The state of the simulation is determined for a set time or a set condition.

The differences between the two could be likened to the difference between riding a horse and moving a knight in a game of chess. In riding the horse, things proceed continually and decisions on controlling the horse are made on the fly. In the game of chess, a move is made, and then everything stops in order to consider the best next move.

Which to choose for a simulation is not straightforward. Certainly a continuous simulation is more complex, has many processes being executed simultaneously, and is thus subject to a greater variety of results. Decisions in the model must be made continuously and are continually subject to reconsideration, which may lead to very complex decision making behavior. The time step simulation is simpler, and (when properly designed) the time step can correspond to those points in time where decisions are naturally made.

Under those terms of comparison, it would be expected that the continuous simulation might be more prone to chaotic behavior than a time step simulation. However, we also know that discontinuous functions are prone to chaotic behavior, while linear continuous functions are less prone to chaotic behavior. The time step simulation has a significant element of discontinuity.

So, there are two conflicting factors to consider in the selection of type of simulation, and there appears to be no obvious choice. On balance (and as a matter of judgment) the time step simulation was selected. If the time step model turns out to be chaotic, for the sake of completeness it might be reasonable to build a comparable continuous simulation to see if it is also chaotic. That effort is suggested for follow-on analysis.

Populating the Model

In deference to Adam Smith (1998, pp. 3-24), division of labor is included in the model. The model includes four types of employees:

1. "Workers" are those that physically produce the product of the organization. In concord with tradition going back before Al Capp, I have named the product "widgets." Workers can be internal to the organization (abbreviated by the variable WI), or outside (external) to the organization (abbreviated by the variable WE).
2. "Support" personnel are personnel, such as secretaries, machinery maintenance people, logistics specialists, and others that support the production process. Support personnel similarly can be either internal (SI) or external (SE) to the organization.

3. "Line Management" personnel provide leadership and management to the production process. Line management personnel can only be internal to the organization, and are abbreviated by the variable ML.
4. "Staff Management" personnel provide supervision and management of the line managers. They are the equivalent of the leadership that is centered in the headquarters of a large corporation. Staff management can only be internal to the organization, and is abbreviated by the variable MS.

The unit of resolution is one individual.

Figure 3 shows the model of the organization. It is an uncomplicated hierarchical pyramidal organization. The basic building block is one manager managing ten workers, with the requisite support staff. As more workers join the organization, more of these building blocks are formed (only three are shown in the diagram). These building blocks are not hired as a unit. Partial blocks are formed out of less than 10 workers, as the vicissitudes of hiring dictate. Similarly, when the organization becomes large enough to justify it, a layer of staff management is added.

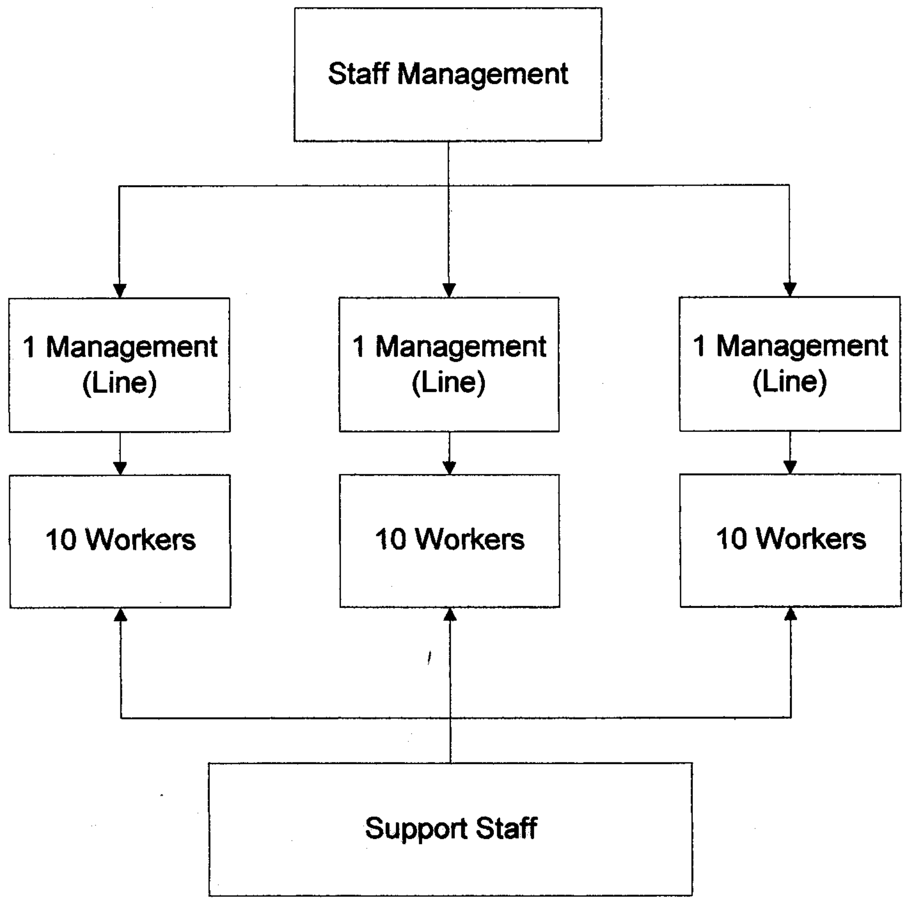

FIGURE 3

MODEL OF THE ORGANIZATION

The hierarchical organization was chosen because of its widespread use and simplicity. However, it begs a rather important question: what is the purpose of management? Why doesn't the organization consist of workers only?

According to Guillerma and Calvo (1987), economists have taken two basic approaches to this problem. The first, pioneered by Meyer, hypothesizes that hierarchies enhance labor productivity by facilitating the transmission of talents from more to less gifted workers. Thus, organizations form because most individuals get more income by teaming with others with different talent levels. The second, pioneered by Williamson, takes the appraoch that supervision is necessary to ensure the performance of the workers. The enhanced performance of supervised workers means that it becomes profitable to add layers of pure supervision to the organization.

The model of the organization created for this paper owes more to the organizational behavior literature, which observes that management at different levels perform certain coordination and planning functions that are essential to the performance of an organization. Consequently, two layers of management are included. The model requires that there be a certain balance in numbers between management and those managed. Too few managers and all their needed tasks are not performed, which reduces the overall production of the organization; too many managers and the managers have excess time to micromanage, which similarly has a negative impact on productivity. Consequently, when the required management ratio is not attained a penalty function reduces the organization's production.

Other organization forms could have been modeled—hierarchical, matrix, subdivisions, or whatever. The pyramid hierarchy modeled in this simulation was chosen for its simplicity, so that, if deterministic chaos was detected, it could not be attributed to complexities arising from the depiction of the management organization. The key concept is that, whichever type of organization is depicted, there is a penalty function assessed against organizational performance if the proper balance between managers, workers, and support personnel is violated.

Some researchers have attempted to establish the "best" management ratio. Researchers have proposed various numbers as the optimal subordinate-to-supervisor ratio. The model (arbitrarily) assumes that one line manager for each ten workers, and one staff manager for every ten line managers, is optimal. For the purposes of this investigation, the exact number is not important. What is important is that the number be exact and fixed, so as to not add an additional source of complication.

In the real world different people have different capabilities. One individual might be able to supervise eight people effectively, while another might be able to supervise twelve. In addition, in the real world that number is not fixed: a person's capacity to supervise would change over time, due to training, education, boredom, personal circumstances, or any other number of factors. However, such "real world" factors would incorporate elements into the model that would tend to change randomly, and thus reinforce any tendency to complex behavior. Thus, the model assumes that the numbers are fixed and invariant, and that the organization knows the perfect ratio so that optimal hiring decisions will always be made. When the organization departs from this perfect ratio, a penalty against production is assessed.

Some organizational theorists have examined organizations and discovered that the higher the management level, the further out in time is their perspective. Line managers operate in the here-and-now, while upper management is concerned about weeks and months into the future. One intriguing possibility would be to assess the staff management penalties against future production levels, several time cycles later. This is a suggestion for possible follow-on work.

Similarly, the necessary ratio between support staff and those that are supported can (in real life) change daily depending upon a myriad of factors. However, again, the purpose of the model is to create a classical, rationalist, deterministic model of the organization to see if it is chaotic; it would be easy to create a chaotic model if we used chaotic inputs. Thus, the model assumes that support personnel have the capacity to support a fixed number of personnel. One support person serves ten supported personnel. Internal workers, line managers and staff managers all require support. If there are insufficient support personnel, the production of the organization is penalized.

Too many support personnel do not result in a production penalty in the model. The assumption is that the support personnel would just have slack time when they are not working, and that this slack time does not impact on their efficiency in performing their duties. This assumption, too, may not hold in the real world. For example, in a study of the performance of personnel in a fire direction center, the U.S. Army found that when workload was low there was a higher incidence rate of errors, and individual tasks were performed more slowly. When the center was overloaded, the error rate was also high. There was an optimal workload where tasks were performed quickly, efficiently, and with the lowest error rate. However, for our purposes the simulation will ignore real world considerations in favor of classical simplifying assumptions.

For workers and support personnel the model considers two different classifications: internal and external. It is here where the basic principles of the New Institutional Economics come into play. Internal personnel are those that are brought into the organization. There is a single transaction cost assessed in the process of hiring these personnel. Once they are in the organization, they continue to perform their work without having an additional transaction cost assessed. Contracts for external workers, however, must be negotiated each time cycle. The cost of obtaining their services each turn consist of their salaries and the transaction cost of contracting. The model assumes that workers and support personnel can be hired on a turn-by-turn basis. Line and staff management must be brought into the organization.

The simulation assumes that external workers cannot be hired in fractional amounts. In this conceptualization, workers are not being contracted, but rather a contract for the product is being executed. In the simulation one worker has the capacity to produce one widget every time cycle. According to the literature of the New Institutional Economics, contracts are generally assumed to be for the performance of a certain task, not for a level of effort. So, in the simulation, a contract with an external worker is for the production of one widget at a given price.

In contrast, the model does allow support personnel to be hired in fractional amounts. This is to reflect the plethora of temporary help agencies that offer secretarial, accounting, janitorial, and other such services in amounts matching the needs of the organization. Such help must be negotiated for each time cycle in the model, and is more expensive per unit of labor than internal support personnel. An organization thus might find it more cost-effective to contract for external support when only, say, one-quarter of a support unit is required to bring the organization up to optimal efficiency, while if one-half were required, it would be more cost-effective to hire a new support person internal to the organization. The exact break point where it becomes more effective to bring new support personnel into the organization is determined by the relative costs of external and internal personnel. The model assumes that managers have perfect knowledge of these break points, and that they do not vary. This allows optimal hiring decisions to be made for internal and external support personnel.

Money is considered to be in arbitrary units that will be called, for familiarity's sake, "dollars." All internal personnel are paid one dollar per time cycle. Including different pay rates for managers, pay raises for workers in accordance with seniority, and other "real world" details would add complexity to the model, and thus, is ignored. The model assumes that pay rates are fixed and remain constant over time.

From the above description, our hypothesized organization will produce at its most efficient rate when there are ten workers, one line manager, one internal support personnel, and 0.1 external support personnel, at a cost of 12.2 dollars of salary per ten widgets produced (assuming that temporary help is twice as expensive as permanent help). As the organization gets larger and adds more workers, additional support and line managers will be added. Eventually the organization will get large enough to require staff management. Then, an optimal combination for production would be 100 workers, ten line managers, one staff manager, eleven internal support personnel and 0.1 external temporary support personnel.

Processes Executed in One Time Cycle

Figure 4 shows a flow chart of how the program is executed. The purpose of this section is to describe the flow of the program. A more detailed description of each process is provided later.

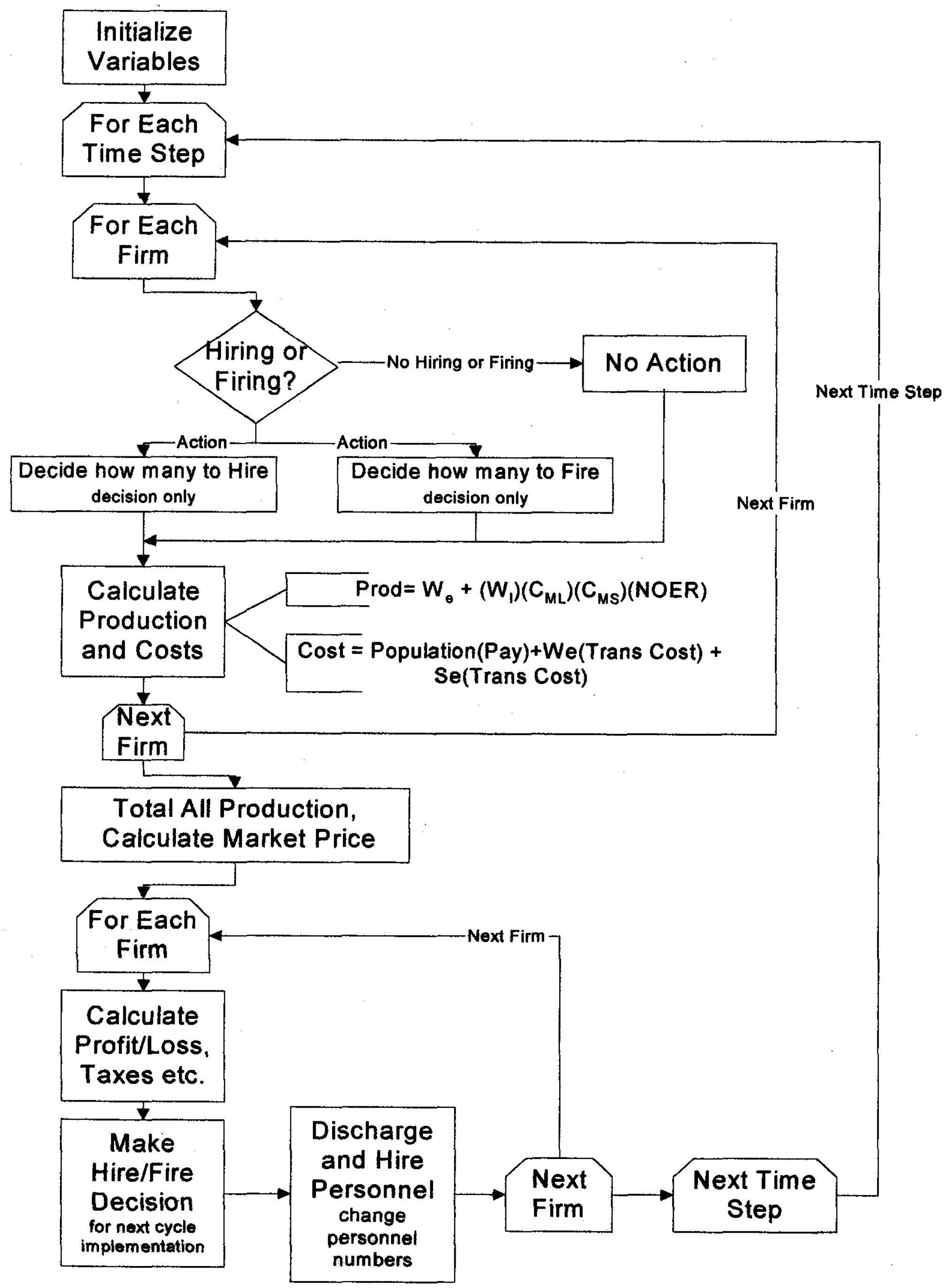

FIGURE 4

PROGRAM FLOW CHART

Beginning at the top:

First, all the variables are initiated and computer housekeeping is performed.

Next, the first time step is initiated.

An organization (firm) is selected. The program allows for up to three firms. There is no reason why more could not be included for some subsequent research.

For the selected firm, the program looks at the record of previous decisions and determines the hiring status of the firm. There are three possible situations: the firm is hiring, the firm is firing, or the firm is not taking any personnel actions. At the beginning of the program, all firms are in a "hiring" status.

If the firm is either hiring or firing, the program computes the number of each type of personnel that will be affected. The program does not execute the hires or fires yet; this is only a computational step. Look at it as if the advertisements for job openings are being placed in the newspapers; it will take time—all cycle—to locate, interview, hire, and train the personnel into crack widget producers.

Next, production is calculated, along with the cost of production.

These steps are executed for each organization in the simulation run. The program then totals the production from all organizations and determines the market price of widgets.

Then, for each organization, the program calculates the profit or loss for that cycle, and performs all accounting and bookkeeping tasks, such as paying taxes and allocating profits to organizational growth.

Based on the profit level for this cycle, the organization then decides what hiring action will be taken during the next cycle. The organization will hire, fire, or do nothing.

The organization then discharges or hires the personnel in accordance with the calculations executed at the beginning of the cycle.

Note that there is a time lag between the decision to hire or fire and the actual execution of the policy. For example, on cycle 2 a decision is made to execute a firing policy. In cycle 3, then number of personnel to be fired is calculated, but those personnel remain for the production phase of that cycle. After production is finished, the calculated numbers of personnel are actually deducted from the work force.

Production

For each cycle, the model will calculate the number of widgets that are produced by the organization. The basic equation is:

Production = (WI+WE) (Support correction) (ML correction) (MS Correction)

WI is the number of workers in the organization. WE is the number of workers external to the organization who have received production contracts to produce one widget for the cycle.

The support correction is a penalty function that reduces production if there are insufficient support personnel, including internal and external temporary support personnel. The organization always insures that there is sufficient support, so this factor is always equal to 1.0.

The ML correction is a penalty against production if there are an unbalanced number of line managers. First, as many groups as possible are formed with the correct management balance. These groups produce at optimal efficiency. Then, a penalty is assessed against the production of the remaining workers and managers.

For example, assume that there were 48 workers and five line managers. Since one line manager can optimally manage ten workers, four balanced groups could be formed, using four line managers and forty workers. This leaves one line manager supervising eight workers, or a group that is out of balance by two workers. A penalty would then be assessed against the production of this eight-worker group for being over supervised.

The penalty table is as follows; first, if there are too few supervisors:

# of Workers Too Many	Associated Production
1	0.9
2	1.7
3	2.4
4	3.0
5	3.5
6	3.9
7	4.2
8	4.4
9	4.5
10	4.55

If there are too many supervisors:

# of Workers in Partial Work Unit	Associated Production
1	.55
2	1.2
3	1.95
4	2.8
5	3.75
6	4.8
7	5.95
8	7.2
9	8.55
10	10 (in balance)

These tables are created by multiplying the number of workers by a percentage of efficiency. For the "too few managers" case, the penalty multipliers are .90, .85, .8, and so on, so that greater cases of unbalance have the greatest penalty. Similarly, for the "too many managers" case, the penalties are .95, .90, .85, and so on. The fact that the "over-supervised" case is penalized less than the "under-supervised" case is simply a matter of subjective judgment. The penalty function must, however, be large enough so that it makes economic sense to have managers. Management can only justify itself if the organization is better off for having paid their salaries.

Note that the line manager penalties were assessed only against partial work units. The other full units worked at full efficiency, subject only to the staff management penalty. In the problem space regions used in this study, line management penalties generally caused less than a 3% change in overall production.

A number of different penalty function were tried. The purpose was to see how robust the conclusions were to varying penalty functions for too many or too few supervisors. Overall, while the individual plots would change, the overall conclusions were intact.

The correction for out of balance staff management affected the entire production of the organization. The penalty was assessed at five percent times the number out of balance, divided by the number of staff management. The line managers could never exceed an out of balance of five. So, for a population with zero or one staff manager, the maximum penalty to production would be 25%; for two staff managers, 12%; for three staff managers, 8.3%. Thus, the penalties for having an incorrect number of staff managers go down proportional to the size of the organization.

The model does not incorporate any increase in production that might be associated with an "economy of scale" effect in larger organizations. The marginal cost in the model's organization actually increases slightly with scale: a ten-worker organization optimally produces widgets at a cost of 1.21 dollars per widget, while a 100-worker organization produces at 1.22 dollars per widget. The increase is associated with the need for the extra management layer of staff management as the organization grows.

This appears to be in agreement with neoclassical economics assumptions regarding the economy of scale effects. Ijiri and Simon (1977, p. 7) said:

"Decreasing returns to scale (increasing marginal cost) is an essential condition to the existence of a competitive equilibrium." The empirical evidence is mixed. Ijiri and Simon (p. 7) contended that the evidence did little to satisfy this requirement of theory. Walters, in 1963, conducted a comprehensive survey of empirical estimates of cost functions, and 14 of the 34 cases examined did not show signs of increasing marginal costs. They concluded, "an assumption of constant returns to scale is most consistent with the data." However, they admitted that there is a great deal of variation in the data, and that their conclusion is a generality that does not hold in every case. Simon (1995, p. 292) indicated, "in most large-scale enterprises, economies and diseconomies of scale are quite small."

So, I feel comfortable with the model conforming to the neoclassical assumption. I have not included any factor for economy (or inefficiency) of scale.

Only whole widgets are sold on the market. If the organization as a whole produces, say, 432.91 widgets, 432 are sold and the 0.91 is carried over to the next cycle, to be added in to that cycle's production.

Heuristics

The model has heuristic rules to trigger when to hire and when to fire, along with how many.

Hiring and Firing: Hiring and firing decisions are made based on the current cycle's financial performance. There are three possible decisions: to hire production workers, to fire production workers, or to do nothing.

If profits in the current cycle are greater than or equal to profits in the previous cycle, the organization will hire.

If profits in the current cycle are less than the profits in the previous cycle, but the organization is meeting return on capital requirements, nothing is done.

If profits in the current cycle are less than that needed to meet return on capital requirements, then the organization will fire workers.

The return on capital requirement recognizes that there is a competition for capital. If, for example, the interest rate for risk-free investments (such as bank deposit accounts or government bonds) is 5%, there is no incentive to invest in an organization that provides less than this rate at a risk. The organization must make more money than risk-free money could earn in order to make investment attractive. In the model, the risk-free interest rate is assumed to be 5%. The money put at risk (invested) is the salary of the personnel in the organization. So, if the organization has 100 personnel (each paid at 1 dollar per cycle) the organization must make a profit of at least five dollars per time step in order to justify reinvestment. Thus, 5% of the population level of the organization (in dollars) establishes the return on capital criteria for the organization.

If the profits of the organization drop for three consecutive turns, a firing decision will also be triggered. This would cover cases where profits were eroding but still above the return on capital criteria.

In all other situations, the organization will do nothing.

How Many to Hire: In the organization, there is a fund established for growth (represented by the variable GFUND). There is an initial amount put into this fund at the beginning of the simulation, representing the initial venture capital of the organization. When a hiring decision is triggered, in the next cycle workers will be hired in accordance with the following formula:

WI hired = (HF) (GFUND)

The variable HF ("Hire Factor") serves to moderate the influence of GFUND. Set HF to 1.0 and the model would hire the full amount of workers allowed by GFUND in one cycle. Setting the variable lower would cause some of the GFUND available to carry over to the next cycle and be available for later hiring. Thus HF could be equated to the availability of employees to join the organization. Lower HF settings would duplicate an environment where management would like to hire additional workers, but it might take more than one cycle to find personnel willing to join the organization in the numbers desired.

Two HF values were used, 0.8 and 0.4. The first allowed the organization to hire all the employees it desired in a balanced way, 80% workers and the remaining 20% of the GFUND available for management and support hiring. Similarly, when HF = 0.4, it allowed balanced hiring over several cycles, reducing the GFUND by approximately half each cycle.

As an example, if there were 100 dollars in GFUND and the HF was set at 0.8, 80 workers would be hired, with the remaining 20 dollars in the GFUND available to balance out the organization with hiring management and support workers.

For each personnel hired, one dollar is removed from GFUND. So, in the above example, if GFUND was 100, and 80 workers, 8 line managers, one staff manager, and 10 support personnel were hired, GFUND would be 100 – 80 – 8 – 1 – 10 = 1 dollar remaining in GFUND.

GFUND is replenished by a portion of the profits of the company, in accordance with the variable PLOWBK, which represents the "plowback" percentage of profits that are reinvested in the firm. In the simulation, a plowback percentage of 25% was used.

If the organization makes a decision to fire personnel, GFUND is set to zero.

How Many to Fire: The organization will fire workers based on the profit-loss figures of the cycle. For every one dollar of profit less than the return-on-capital figure for the size of the organization, one internal worker will be fired. For every ten workers fired, one support personnel and one line manager will also be fired. A computation is made to determine if the size of the organization has dropped so low as to require discharging any staff management.

Organization Growth: Background Information

Ijiri and Simon (1977, p. 157) created a stochastic model of the distributions of firm sizes. They attempted to prove that year-to-year changes in firm sizes could be explained by a simple Markoff process. They found that the probabilities of size changes of any specified percentage are independent of the firm's size—in other words, that increases in firm size are random. (An alternative possibility, not considered by them, is that the underlying process was chaotic, which could give similar results. Injiri and Simon's work predated most of the advances in chaos theory. I will return to this example later.)

In the model, I am taking a "rationalist" approach regarding organization growth. The model assumes that a rational decision is made whether to increase or decrease the size of the firm, based on current marketplace conditions; and, that the size of the change is based on the firm's established policy.

The heuristics for hiring and firing are significant parts of the model. Certainly they could be made much more complicated. If, for example, the population of the organization begins to oscillate in a periodic manner around a certain population, one could assume that managers would learn from their experience and change their decision rules to dampen the oscillations. However, the point of the experiment is to look at models with simple decision principles to see how they perform.

Other Financial Considerations

There are other financial realities included in the model. The following provides a short description of each and a listing of the values that were used. Unless otherwise indicated, the values remain constant in the various runs.

Transaction Cost: The transaction cost is rolled up into the cost to hire personnel external to the organization, on a cycle-by-cycle basis. While workers, management, and support personnel in the organization cost one dollar per cycle, temp workers and temp support personnel cost two dollar per cycle. There is also a cost associated with hiring new permanent employees.

Cost of Capital: If the organization does not have sufficient capital to meet all costs for the cycle, the balance must be borrowed at a cost of two percent per cycle. In all cases, the organizations were begun with an initial capital of 50 dollars, and that factor did not come into play except in the cases of organizational failure.

Hiring Cost: Personnel hired internal to an organization cost 0.25 dollars on the cycle that they are hired. They do not produce any widgets (or do any management, or provide any support) in the cycle in which they are hired.

Price of Widgets: The initial price of widget is 3 dollars. Nicholson (1978, pp. 10-11) presents the usual assumption in economics, that subsequent prices will be in accordance with a classical balance between supply and demand as originally developed by the English economist Alfred Marshall (1842-1924) in his *Principles of Economics*. The price curve is linear and declines with a slope of -.005 dollars per widget.

The model is set up to allow multiple regions for supply and demand, to allow the possibility of exploring the effects of positive returns, or the effect of competing products with different price curves (such as the situation that was observed with two technologies, VHS and Beta, competing for the home video market). As a side note, over the last decade there has been a great deal of interest in the economics of positive returns.

To look at positive returns, I experimented with using a positive price slope of widgets in the model for some runs. In those runs, the size of the organization rapidly exploded to infinity. I also experimented with a segmented price curve, where the price curve initially had a positive slope, and beyond some point would have a negative price slope (reflecting the fact that even markets with positive returns eventually reach market saturation). The organizations generally had rapidly accelerating growth until the negative portion of the curve was reached, and then, after a period of broad population oscillation, the populations behaved in a similar way as the experiments when the price slope was negative from the beginning.

While there may be something to be learned from additional experiments with this kind of price curve, for the purposes of this paper the price curve was restricted to negative slopes. Further investigations using nonlinear or piecewise segmented price curves is left for later investigations.

Tax Rate: Fifty percent of profits are sent to the government.

Losses: Losses are carried over onto subsequent cycles, to reduce taxes.

Layoff Survivor Sickness

One of the things that accompanies downsizing is what Noer (1993, p. 5) characterizes as "layoff survivor sickness." The syndrome originates from some of the assumptions that accompany an employment contract. New personnel believe that a job is available to them as long as they do not violate any of the norms of the organization. When a company goes through downsizing, it is subconsciously viewed by the employees as an event the equivalent of an accident or disaster. The "layoff survivors are no different than survivors of other forms of tragedy," who must deal with "anger, depression, fear, distrust, and guilt. People with survivor sickness have often been described as having a reduced desire to take risks, a lowered commitment to the job, and a lack of spontaneity" (pp. 12-13). Merry and Brown (1987, pp. 44-45) state that survivor sickness can infect entire organizations. Organizations "waste away" through behavior characterized by a negative self-image, a "failure script" that becomes a negative self-fulfilling prophecy, low energy, low motivation, and other deviations from organizational norms.

Many scholars have found that layoffs result in a decline in productivity in the remaining workers (Brockner, Konovsky, Cooper-Schnieder, & Folger, 1994; Caudron, 1996; Downs, 1995; Navran, 1995.) Ambrose (1996, p. 4) cites a 1994 American Management Association survey that indicated that about two-thirds of organizations that went through downsizing saw their productivity drop.

The simulation includes the variable NOER, which is a multiplier that represents this drop in productivity. When an organization is growing or at steady state NOER is set to 1.0 and there is no drop in production. When the conditions in the organization indicate the presence of layoff survivor sickness, NOER is set at a value less than 1.0, so that, for example, NOER set at 0.9 would represent the organization operating at 90% efficiency, or a 10% drop in productivity.

Quantifying this factor is difficult for a variety of reasons, including the problem of extracting a "before" and "after" productivity figure for an organization in the process of transition. Scholars have characterized the drop as "serious" or "the equivalent of a disaster," but have not provided quantification. In the absence of exact figures, I parameterized the variable and ran a number of simulations in order to see how the model reacted to different values of NOER.

The model includes factors for three different productivity levels, growth, steady state, and downsizing. The first two variables remained fixed at 1.0. Exploring the effects of different productivity factors associated with different growth states in the organization is left for possible future investigation.

When downsizing triggers a drop in productivity, the drop remains in place until the organization begins to rehire personnel.

In most of the simulation runs NOER was set at 1.0. Special runs were executed using other values of NOER to investigate the effect of this variable.

CHAPTER V

DESIGN OF THE EXPERIMENT

Objective

Up to this point I have described a simple model where the economics of transaction costs justifies the growth of an organization. If the model behavior is subject to deterministic chaos, there probably will be regions of steady state behavior, regions of periodic behavior, and regions of chaos. If that is so, it would support one of the key components of the hypothesis regarding the Scientific Method and the social sciences.

The primary measure will be the size of the organization, arrived at by adding the internal population of managers, workers, and support personnel. External personnel, such as temporary support personnel and outside contract workers, are not included in this total. There are many other ways in which the size of an organization can be described: by its revenues, profitability, production, net worth, among other measures. However, since the model is based on an economic theory that intends to justify the creation and growth of organizations based on a rational criteria, the population of the organization is the obvious and intuitive measure to use.

This measure is, in some ways, overly simplistic. Two organizations can have the same number of personnel and yet be very different in terms of productivity, funding, and resources, not to mention other more subjective measures of the internal health of an organization such as morale, experience, training, and motivation. However, concentrating on personnel size is a conservative approach. Including these other measures would add additional dimensions to the problem, and the results would be more likely to be complex. If the population of organizations in the simple model using a simple measure varies chaotically, it stands to reason that the multidimensional space would be even more chaotic.

Measuring Chaos

In regions of deterministic chaos, a chaotic system would have the following characteristics:

1. Non-periodic behavior;
2. Extreme sensitivity to initial conditions;
3. Non-monotonic behavior; and
4. A positive greatest Lyapunov exponent.

In addition, there might be repeating patterns similar to fractals.

Discovering Chaos

Model runs were executed in two different ways. The first was a "single run" method. In this method, all the initial conditions of the model were loaded into the model and a single run of a given number of cycles was executed. The total population of the organization(s) was recorded for each time cycle. The result was a dynamic plot that showed the growth of the organization(s) with respect to time.

The second execution method was a "batch" method. In this method, many complete 1000-cycle runs were executed, each with slightly different initial conditions. For example, a batch of 90,000 runs was executed to examine the effect of the variable GFUND, the initial venture capital funding of the organization. In each run GFUND was set at slightly different values for two organizations. At the end of each 1000-cycle run, the program would record some desired characteristic of the end state, for example, if one of the organizations failed (went bankrupt), or if the organization with the smaller initial funding turned out to be the larger organization. The initial conditions of the model would then reset, .001 would be added to one of the organization's initial GFUND, and another 1000-cycle run executed. The process would continue until all 90,000 runs were executed.

A batch run would generally take several hours of computer time on a high-end Pentium IV computer. A single run of 1000 cycles would be executed in less than three seconds.

To locate individual examples of particular behavior (such as one organization failing, or non-monotonic behavior) a batch run over the variable space of interest would be set up, and a line of code added to have the program record the initial conditions of a run which had the desired characteristics. When these variable spaces were located, an individual run could be performed with those initial conditions to examine the behavior in detail.

Individual runs were selected for illustration in this paper in order to make particular points or illustrate particular issues in the clearest way.

Using Computer Simulations in the Social Sciences

The sciences of chaos and complexity are children of the computer. While chaotic behavior was discovered decades ago (Poincaré's work was just over one hundred years past), it was impossible to pursue studies without a means of performing the millions of required computations.

Similarly, the use of computer simulations in the social sciences are in the early stages of what could be a very exciting period of development. A new field, "computational social science," addresses some of the possibilities, primarily through the use of agent-based modeling to explore theories of emergence. Sawyer (2002) discusses multi agent systems (MAS), a new computer simulation technology that has increasingly been used to describe and explain sociological phenomena. Computers have been used to create what are known as artificial societies, which are social simulations using MAS. Sawyer contends that MAS have attained a level of maturity where they can be useful tools for sociologists, and provide new perspectives on contemporary discussions of the micro-macro link in sociological theory.

Thus, computer simulations and mathematical approaches to addressing social science problems are an increasingly flexible tool, and growing in use. Rowe (1997, pp. 5-7) believes that approaches like this will be "normal science" in the future.

> Although it is true that, in the 300 years since Newton, most of the theoretical science has been done using the rigorous, analytical approach, the reason is that that is the only kind of science that could be done. . . . The lack of computational power meant that researchers could only answer questions that had clean, elegant solutions. . . . It is only now that we have the ability to do complex calculations and simulations that we are discovering that a great many systems seem to have an inherent complexity that cannot be simplified. . . . After another 300 years, we will no doubt feel as comfortable using computer simulations to analyze nature as scientists today feel using Newton's laws of motion to describe the trajectory of a falling stone.

The destination for this work has now been established. The following chapters will return to justify, expand, and document much of this material. The immediate objective is to demonstrate the following:

Determine if a computer simulation of an organization based on a rational economic model of the organization can behave in a chaotic manner.

CHAPTER VI

SIMULATION RESULTS AND ANALYSIS

Objective

This chapter shows the results of the modeling primarily though the use of plots. There are a large number of implications that might be shaken out of each one of the model runs. Some will be addressed later. At this point the objective is to determine if the system shows the attributes of deterministic chaos.

Terminology

First, a few definitions of terms.

"Market." The price-demand curves establish the demand and price relationship that the marketplace will pay for widgets. Since one worker at optimal productivity produces one widget, the population of the organization and the number of widgets made and sold should be nearly equal (considering that productivity will generally be close to 1.0). Consequently, as a shorthand term, the relative levels of population of competing organizations will sometimes be taken as the equivalent of the organization's share of the market.

"Stable Population Level." In cases where the population of an organization oscillates around some population figure for a length of time, that population figure will be referred to as the "stable population level" for those environmental conditions. This can also be very similar to an attractor, if the system is behaving chaotically.

"Environmental Conditions." This refers to the set of variables that are established in the model to depict a certain economic environment. These variables, described in the previous section, are things like the tax rate, cost of money, transaction cost, and hiring costs. These variables will not be altered in the runs shown in this chapter.

The variables that will be changed to depict unique organizational situations will be restricted to the size of the initial GFUND, the hiring factor HF, and the layoff survivor sickness NOER factor.

Individual runs will be numbered. If different plots are made from a run's data, the plots will be labeled alphabetically ("a," "b," etc.).

Results

Evidence of a Region with Equilibrium Behavior

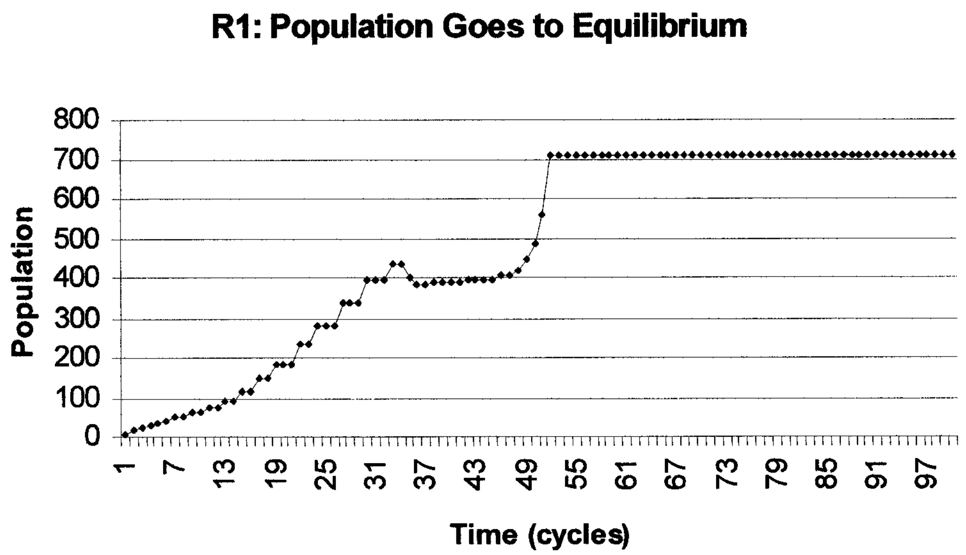

FIGURE 5

RUN 1

Run #1, Figure 5: In this model run, one organization was placed in the environment. The organization began with GFUND = 30, with HF set at 0.8. The plot shows the total internal population (WI + ML + MS + SI) of the organization plotted against time, for the first 100 cycles.

As is shown, the organization grows, has a certain period of consolidation, and then grows again. It eventually rises to an equilibrium population.

This behavior is typical of the expectations derived from neoclassical economics, where systems characteristically come into equilibrium with its (financial/market) environment.

Evidence of a Region with Periodic Behavior

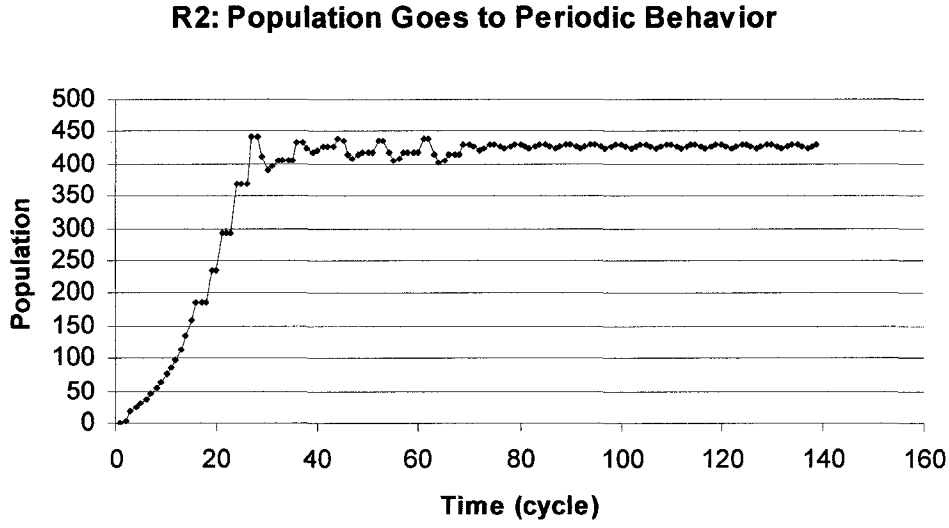

FIGURE 6

RUN 2

Run #2, Figure 6: In this run, two organizations were started. They each had an initial GFUND of 15 dollars, the same total amount of GFUND as in run #1, but now split between two organizations. The plot shows population vs. time.

Since the two organizations have exactly the same heuristics, the same environment, and the same initial conditions, their paths on the plot are perfectly superimposed, and show up as only one line on the plot.

The organizations' population grows, and eventually settles down into a periodic oscillation around a particular population. If viewed on a phase diagram, the points occupied by the organization over time would seem to circulate around a certain stable value; however, the organization's population never hits that stable value exactly.

Evidence of a Region with Chaotic Behavior

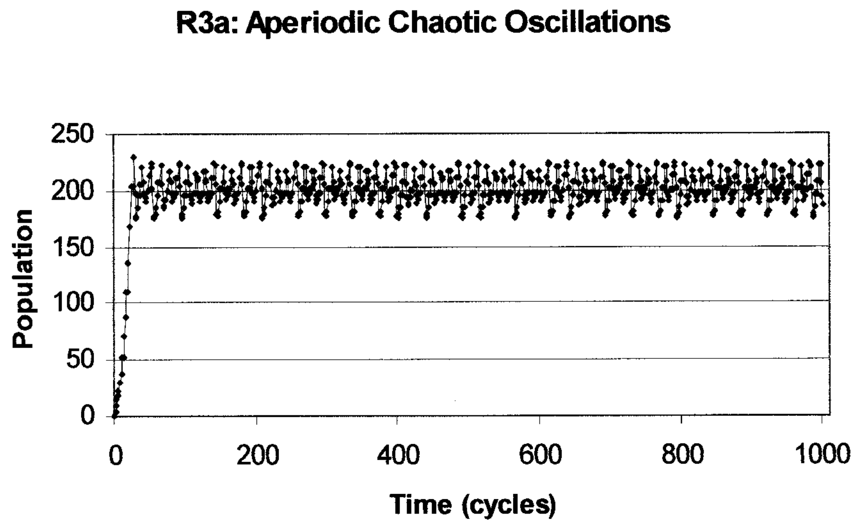

FIGURE 7

RUN 3A

Run #3, Plot a, Figure 7: This run was conducted with three organizations, each beginning with 10 dollars as an initial GFUND. The total GFUND, 30 dollars, is the same total GFUND as in runs #1 and #2. All other conditions were the same as in the earlier two runs.

As before, the paths of the three organizations are perfectly superimposed, so they show as one line on the population vs. time plot.

However, in this case the organizations, plotted over 1000 cycles, do not settle into either a single equilibrium population level or periodic oscillation around a stable population level. Instead, the system shows aperiodic oscillation around the stable population level. The greatest Lyapunov exponent was calculated at +0.513 +- 0.020. Positive Lyapunov exponents indicate the system is chaotic.

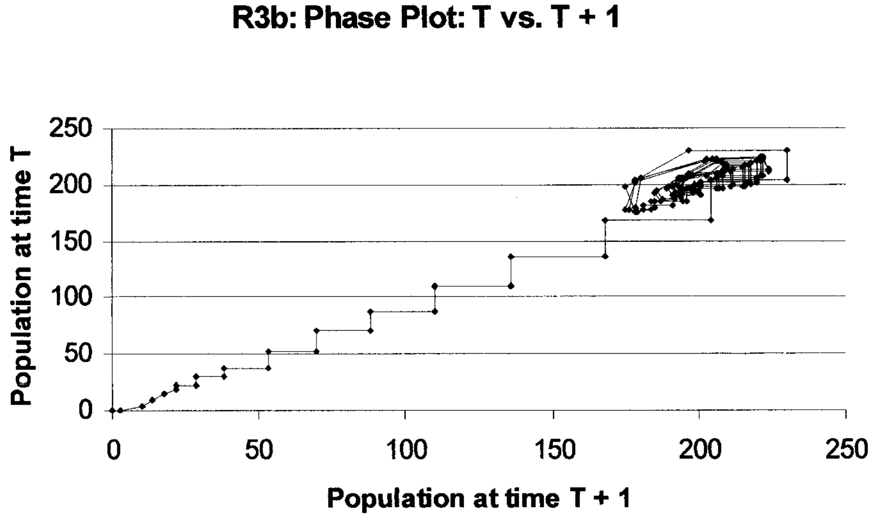

FIGURE 8

RUN 3B

Run #3 Plot b, Figure 8: This is a phase plot of the population of one of the organizations from run #3. A phase plot is a tool used in the analysis of chaotic data; this phase plot shows the population of the organization at time T plotted against the population of the organization at time T+1. The phase plot allows the behavior of the time series over time to be collapsed to show a different visual perspective on the behavior of the time series.

To make the path of the time series more evident, the successive points have been connected with straight lines. This is not usual procedure for a phase plot, but it is useful for visualization of the sequence of the behavior of the time series. It is not meant to imply that the organization actually inhabits the locations on the lines. The organization only inhabits those points at the end of the lines, as marked by the small diamonds.

As shown, the population of the organization begins at the lower left, and grows towards the upper right. Eventually the behavior of the time series begins to circulate around a region in the phase space.

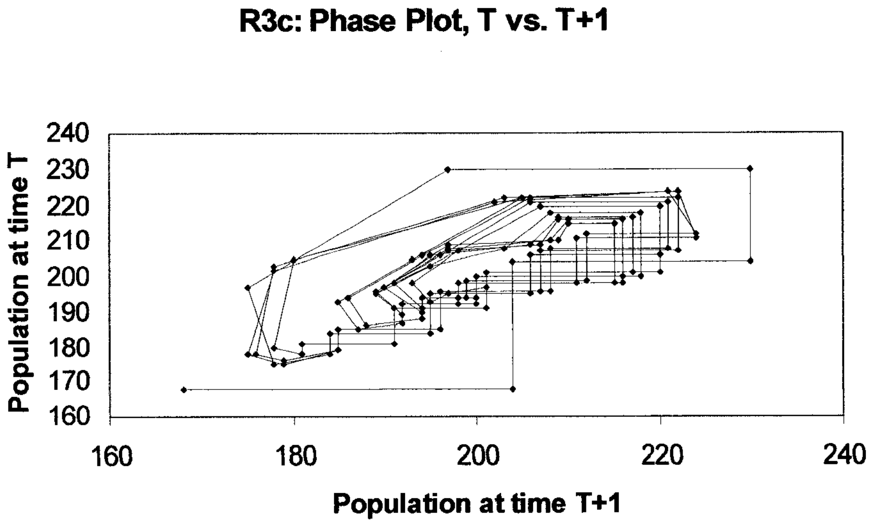

FIGURE 9

RUN 3C

Run 3, Plot c, Figure 9: This plot is a close-up of the phase plot, telescoping in on the upper right corner. The behavior of the time series is shown here to be very complicated. There are a multitude of paths available. The population circulates in a counterclockwise direction around a population level in the region of 200.

If there was only one line in to and out of each point, then the plot would be periodic, however complex. However, there are some points that can generate different destinations. Most of these can be seen in the lower right hand portion of the plot, during growth of the organization, where one point can have two possible destinations on the X-coordinate line. This is an indicator of the aperiodic behavior of the time series.

The behavior shown on these plots demonstrates that the organizations modeled can demonstrate deterministic chaotic behavior.

Effect of One Decision Variable: Hiring Factor (HF)

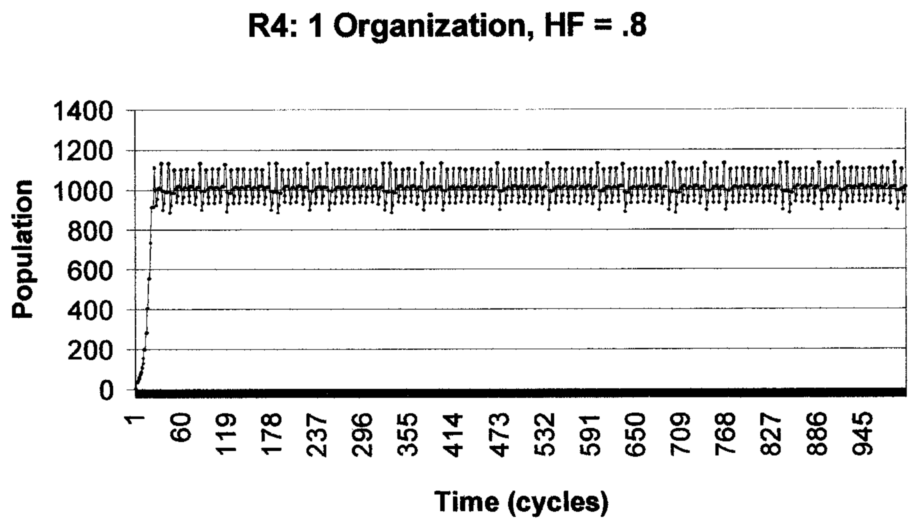

FIGURE 10

RUN 4

Run 4, Figure 10: This plot shows the population vs. time of one organization. As mentioned earlier, the factor HF moderates the rate in which personnel are hired. This plot and the next show the sensitivity of the behavior of the population to this factor. Run 4 sets HF = 0.8, so that all the desired personnel (as indicated by the funding in GFUND) are hired in one cycle. The plot shows the results, where the organization's population rises to a point where aperiodic (chaotic) oscillation occurs. The results of this plot are to be compared with the next plot, where HF is set at 0.4.

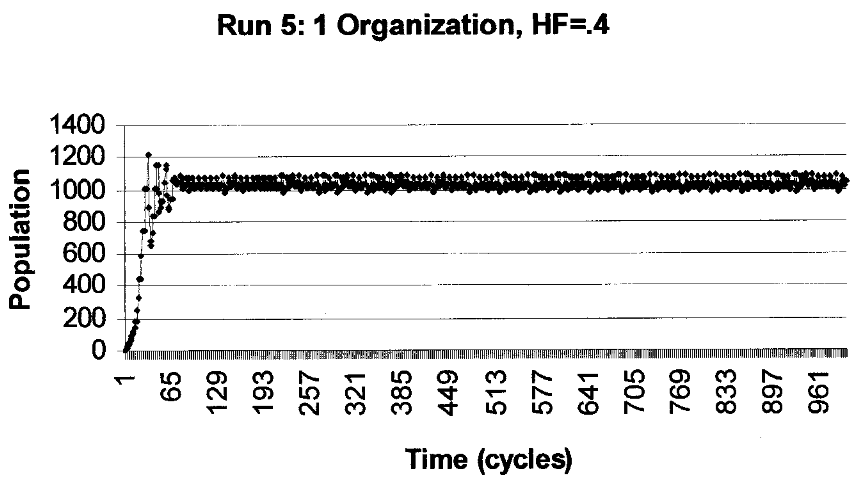

FIGURE 11

RUN 5

Run 5, Figure 11: In this run, all the conditions remained as in the previous run, except HF is now set at 0.4. This setting causes the hiring of desired personnel to occur over two cycles rather than one.

There are several interesting aspects to this plot. First, the population of the organization again rises to a point where there is an onset of aperiodic oscillations. With the lower hiring rate, however, the magnitude of the oscillation is reduced over the previous run. If the hiring rate were considered in terms of a system feedback loop, it would appear that the faster feedback resulting from the faster hiring rate increased the magnitude of the oscillations in this example. This point should be kept in mind when examining other plots, to see if there are other factors that might be the equivalent of adding magnitude to the oscillations of the system.

Second, note that the initial rise of the organization's population overshoots 1200. In the previous run, with a "faster" hiring cycle, the organization did not overshoot as much, a counterintuitive result.

Third, note that both organization's population seems to center around the same stable population level.

Evidence of Fractal Patterns

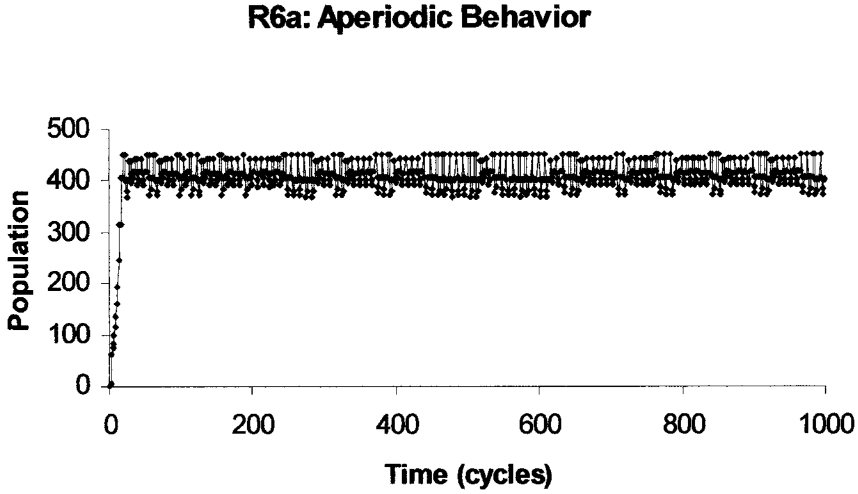

FIGURE 12

RUN 6A

Run 6, Figure 12: This run shows the path of two superimposed organization, both beginning with the same initial GFUND. The path shows a very interesting set of oscillations around the stable population level.

R6b: Detail View: Aperiodic Patterns

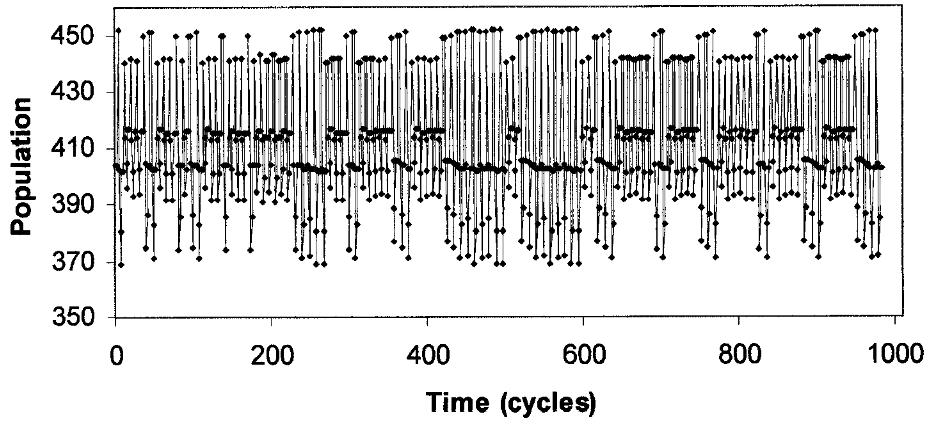

FIGURE 13

RUN 6B

Plot R6b, Figure 13, is a close-up of the oscillations. Note that there are repeating patterns that reoccur in a non-periodic manner. This appears to be the equivalent of a fractal pattern.

The greatest Lyapunov exponent calculated for a time series of 6,000 points from this time series was +0.492 +- 0.018.

Evidence of Non-Monotonicity

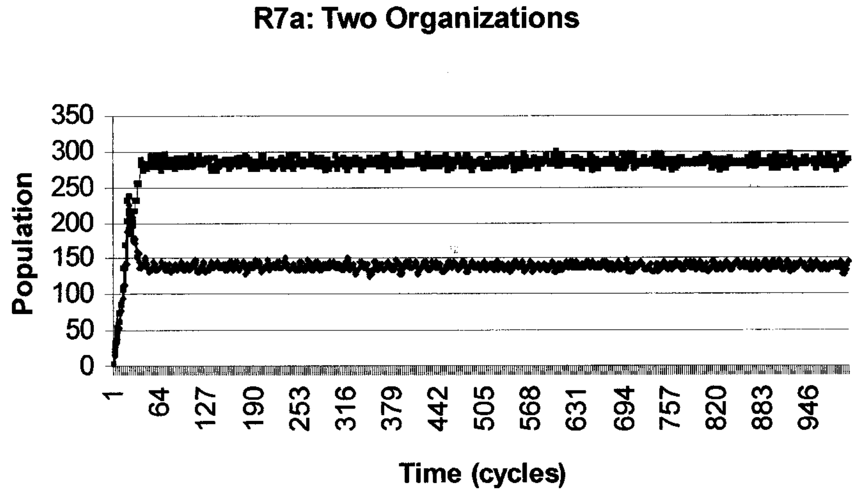

FIGURE 14

RUN 7A

Run 7, Plot a, Figure 14: This is a run where two different organizations were initiated in the same environment. The only difference between the organizations is that one organization began with 30 dollars in GFUND, and the other with 28 dollars in GFUND. Note that the two organizations divided the market in an approximately 1/3rd–2/3rd split. This is a counterintuitive result, considering that the initial funding of the two organizations was different by less than 10%.

Generally, in most situations, no matter what the initial conditions, two competing firms in the same environments would split the market and gravitate to these same split of stable population levels. I did not find any examples in the region that was examined (initial GFUND levels between 15 to 40 dollars) where two organizations would not split the market in this same manner.

A time series of 6,000 population points for the larger organization had a greatest Lyapunov exponent of +0.540 +- 0.018.

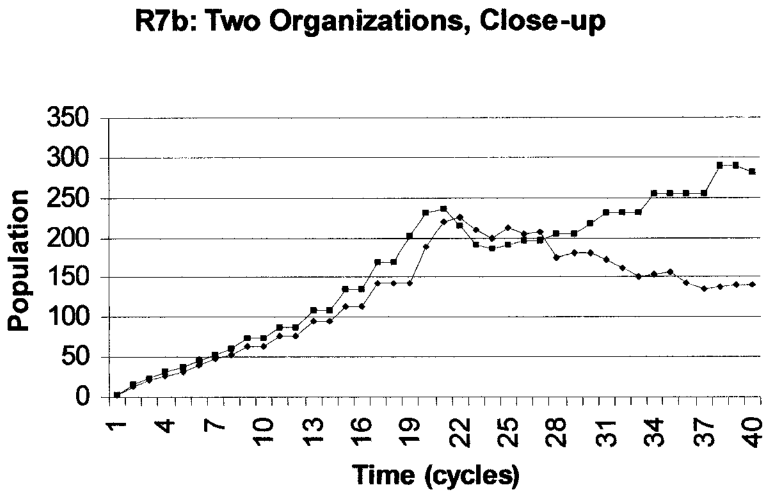

FIGURE 15

RUN 7B

Run 7, Plot b, Figure 15: This is a close-up of the previous run, showing the paths of the organizations over the first 40 cycles. Note that there is a period where the organization that began with the lower funding was actually larger than the other organization.

In general, there was a large variation in the possible paths to the equilibrium points; another example will be shown in the next plot.

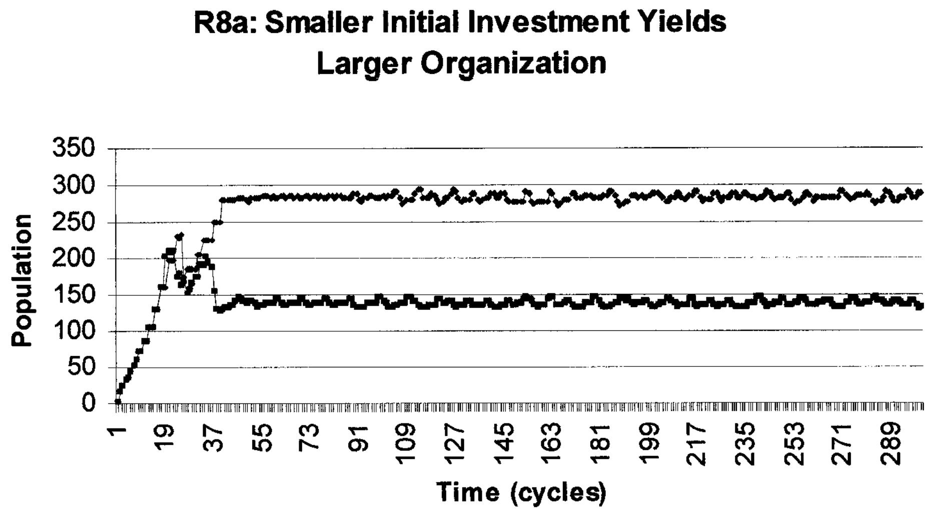

FIGURE 16

RUN 8A

Run 8, Plot a, Figure 16: This run is of two organizations, one with an initial GFUND equal to 30 dollars, and the second with 30.1 dollars. To place this in perspective, this is like having two 30 million dollar startup companies with a difference of one hundred thousand dollars in their initial capital, or a .0033 difference, a level that most would consider inconsequential. All other things being equal, neoclassical economic models would presumably predict that these organizations would equally split the market. Instead, again, the market is split 1/3rd–2/3^{rds} around stable population levels of about 280 and 140. Even more interesting and counterintuitive, it was the organization with the smaller initial GFUND that captured the largest position.

R8b: Detailed View

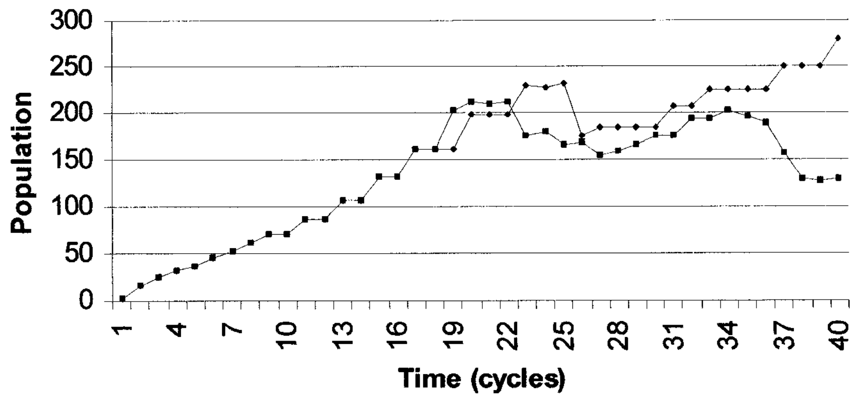

FIGURE 17

RUN 8B

Run 8, Plot b, Figure 17: This plot shows a detailed view of the first 40 cycles of the same run. Note that the paths of the two organizations superimpose until cycle 18, where they separate, the organization with the larger initial GFUND rising faster. But then, on cycle 22, the size rankings swap. The early leader never regains the lead. This is an example of sensitivity to initial conditions, and non-monotonicity.

The question that immediately comes to mind is: how prevalent are non-monotonicities?

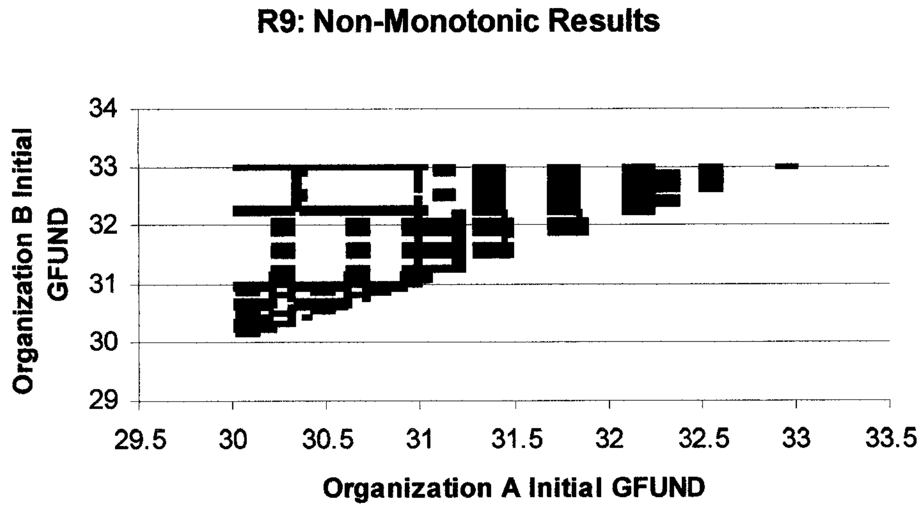

FIGURE 18

RUN 9

Run 9, Figure 18: The plot of run 9 is a display of the results of 45,000 model runs. For each one, two organizations (A and B) were begun in the same market environment and with the same characteristics, except for a difference in initial GFUND. Runs were performed with each company having between 30 to 33 dollars initially in their GFUND, with a resolution of .01 dollars (0.0333 percent) between runs. Since the results would be symmetrical, only the cases where organization B had more initial GFUND than organization A are shown.

A black mark is shown at every place where organization A turned out to be larger than organization B even though it started with a smaller GFUND, a counterintuitive result. Thus, every black mark indicates a non-monotonic result.

The first thing to note is the extent of the non-monotonicity. About 28% of runs resulted in non-monotonicities.

Second, the number of different areas indicates that there is a very high sensitivity to initial conditions—a demarcation between black and white areas indicates that there was a major difference in results for a very small difference in initial conditions.

Third, note the patterns in the plot. In particular, there appear to be fractal patterns that repeat themselves in different areas.

All of these factors tend to reinforce a conclusion that the system under investigation is demonstrating deterministic chaos.

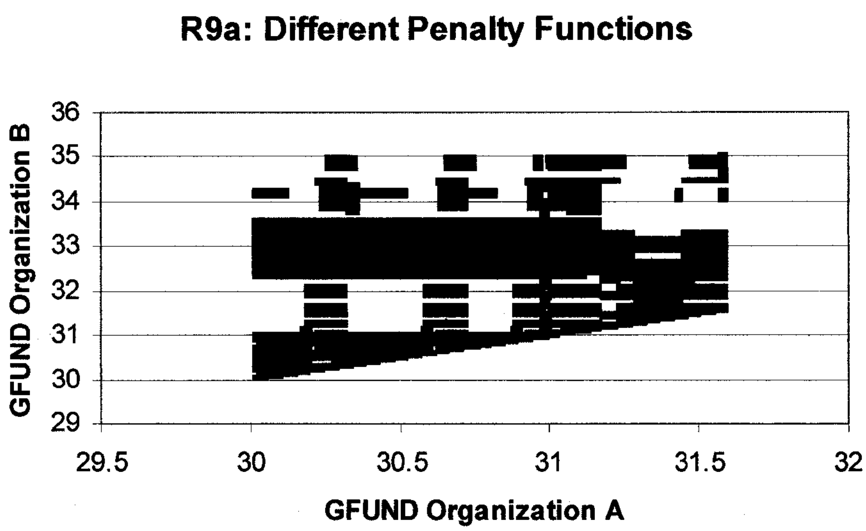

FIGURE 19

RUN 9A

Run 9a, Figure 19: This is the same as in run 9, except a very slightly different penalty function was used for line managers being out of balance, where having too many managers was penalized more than having too few, but with approximately the same magnitude of changes as in the earlier trial. Notice that there are some of the same characteristics as in run 9, i.e., repeating fractal-like patterns, but overall the pattern is substantially different. This reinforces a belief that the non-monotonic behavior is robust—in other words, it is not an artifact of just one unique set of conditions in the model.

Impact of Humanity: The NOER Effect

As mentioned earlier, the model included the capability to incorporate a change in productivity due to layoff survivor sickness, which I identified using the variable NOER. The runs shown up to this point set this variable at 1.0, so it did not have any effect. The next runs will explore the effect of this variable.

The NOER variable is set to a value lower than 1.0 when an organization fires personnel. It returns to 1.0 when the organization begins to grow again. Thus, NOER changes as the organization oscillates around stable population levels, and perhaps earlier if the organization must pause to consolidate during an initial growth phase. NOER is a factor that incorporates a representation of one aspect of having actual human beings in the organization, rather than "economic man" assumptions. There are many other factors like NOER that could be incorporated into a model of an organization, such as the effect of losing a contract or losing a bidding war, the impact of economic cycles on human performance, union activity and contractual work restrictions, absenteeism, waves of illness such as what occurs during cold and flu season, loss of key management or technical personnel, the effects of reorganizations and personnel transfers, and many others. It appears that most of these "humanness" factors would tend to increase the unpredictability of the performance of the organization, and thus increase the potential variability of results.

R10: Spontaneous Failure, NOER = 0.90

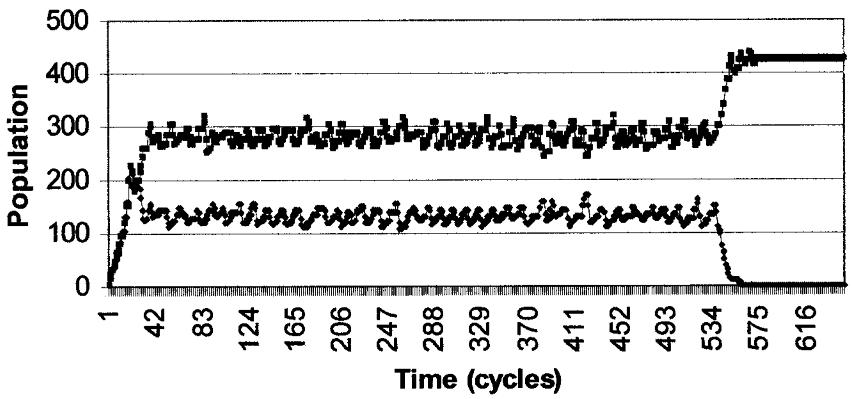

FIGURE 20

RUN 10

Run 10, Figure 20: In this run, two organizations are begun with very slightly different initial GFUND values. They both have a NOER factor of 0.90 assigned, so that, during firing cycles, production is reduced by 10% due to layoff survivor sickness.

Eventually, as seen in earlier examples, the organization's paths diverge and they end up splitting the market about $1/3^{rd}$–$2/3^{rd}$, with aperiodic oscillations around their stable population levels. This split of the market remains for a considerable period, about 500 cycles.

Then, right about cycle 540, it appears that the smaller organization spontaneously fails, and the larger organization captures the entire market.

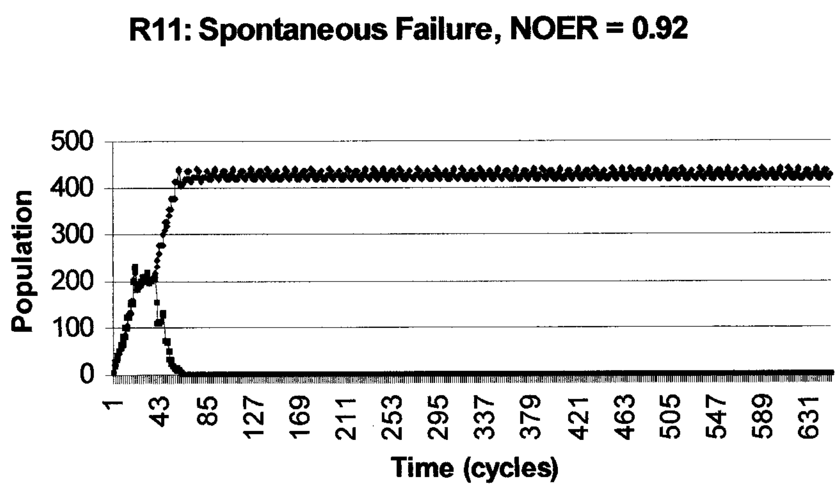

FIGURE 21

RUN 11

Run 11, Figure 21: In this run, two organizations were begun with the same initial conditions as in run 10, Figure 20. The difference was that NOER for both organizations was set at 0.92, so that periods of layoff survivor sickness would reduce production by 8%. The organizations in this run will have a smaller drop in productivity in reaction to downsizing than the organizations in the previous run.

In this case, the organizations never establish two stable population levels. There is an initial attempt to share the market near the 200 population level, approximately an even split of the market, but then one of the organizations fails and the other captures the entire market.

Many individual runs were examined where, under the influence of the NOER factor, one organization would fail. There did not appear to be any one particular cycle where organizations would fail. So, even though the entire model contains only deterministic decision making, the failure time of an organization could not be predicted before executing the run. Very slight differences in initial conditions could make the difference between a situation where one organization failed and a situation where the organizations split the market.

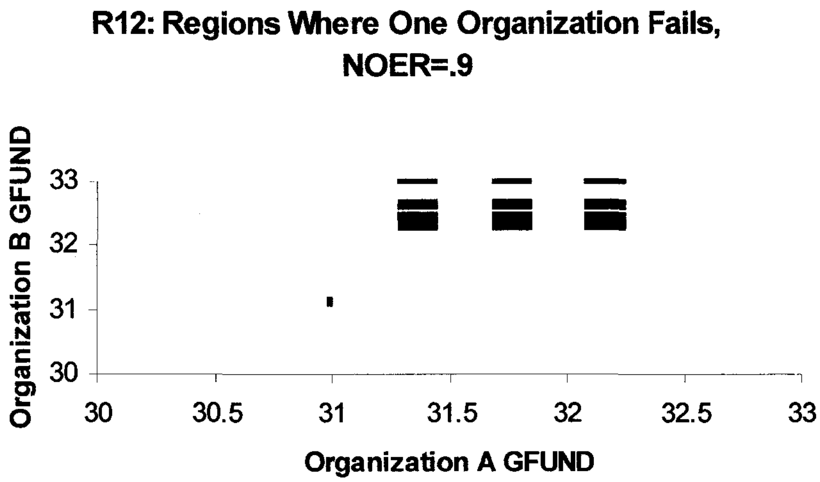

FIGURE 22

RUN 12

Run 12, Figure 22: This plot shows the results of 45,000 runs where the initial GFUND values for two organizations was varied. Resolution is every .01 dollars of GFUND, and only cases where B's GFUND was greater than A's were run, with values from 30 to 33 for each organization. NOER was set at 0.9. The black areas show the regions where either A or B failed.

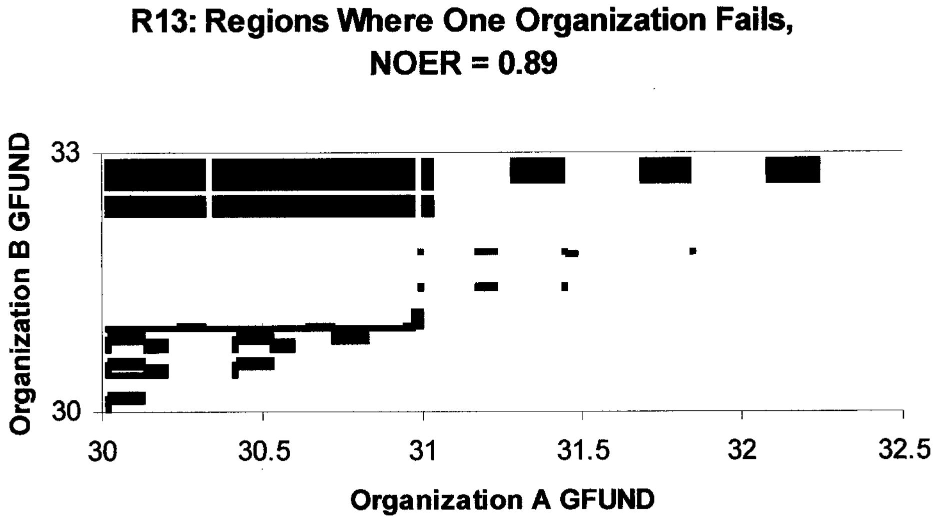

FIGURE 23

RUN 13

Run 13, Figure 23: Run 13 shows the same conditions as run 12, except that the NOER factor was set at 0.89. Because of memory limitations on the computer, the GFUND values range was slightly different than in run 12: A ranged from 30 to 32.5, while B ranged from 30 to 33. Note that there is a considerably larger set of regions where one of the organizations failed. The regions where organizations failed were generally different than in run 12, in spite of the fact that the NOER values were only slightly different. In practice it would be impossible to resolve empirically a difference of one percent of production efficiency due to layoff survivor sickness. Thus, a manager would have significant difficulty in determining, in practice, whether the plot from run 12 or run 13 applied to their particular situation.

Increasing Complexity with Increased Numbers of Competing Organizations

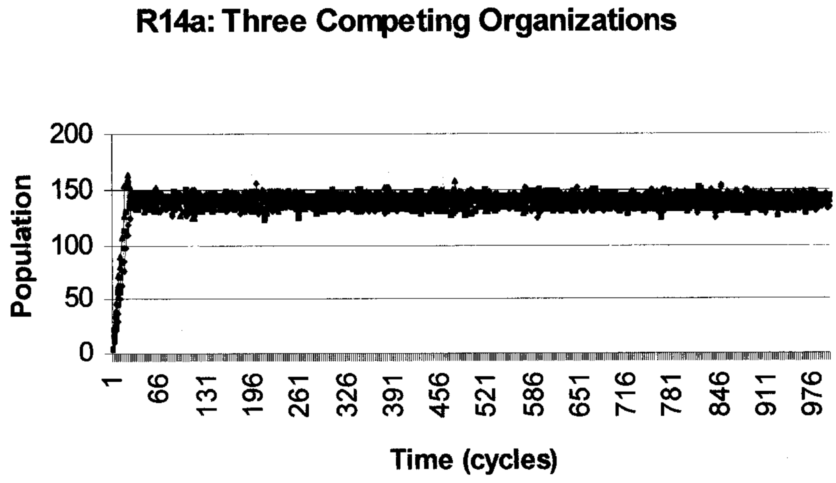

FIGURE 24

RUN 14A

Run 14, Plot a, Figure 24: This plot shows the populations vs. time of three organizations operating in the same environment and with the same internal characteristics, except they were started with different initial GFUND levels. The plots of all three organizations are shown, which emphasizes the jumbled nature of the results. In this case the three organizations divide the market approximately equally, oscillating around what appears to be a common stable population level. This is an interesting result compared to the previous examples where the two organizations would never split the market equally, no matter how close the differences in their initial GFUND.

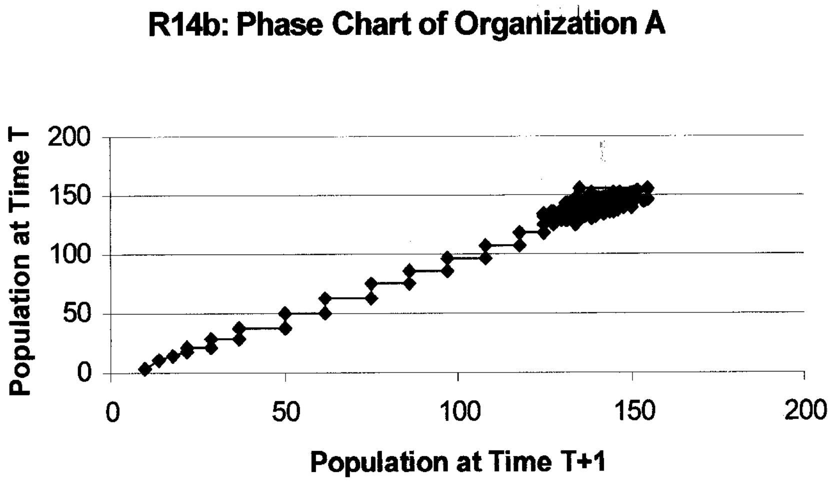

FIGURE 25

RUN 14B

Run 14, Plot b, Figure 25, shows the phase plot of one of the organizations from the run. As before, lines are connected between the successive points as an aid to visualization; this should not be taken that the organization ever inhabits points on the lines, only the points at the ends of the lines marked by the diamonds. Note that the behavior of the organization is approximately the same as that shown in the earlier plot, except that the final pattern is much more complex.

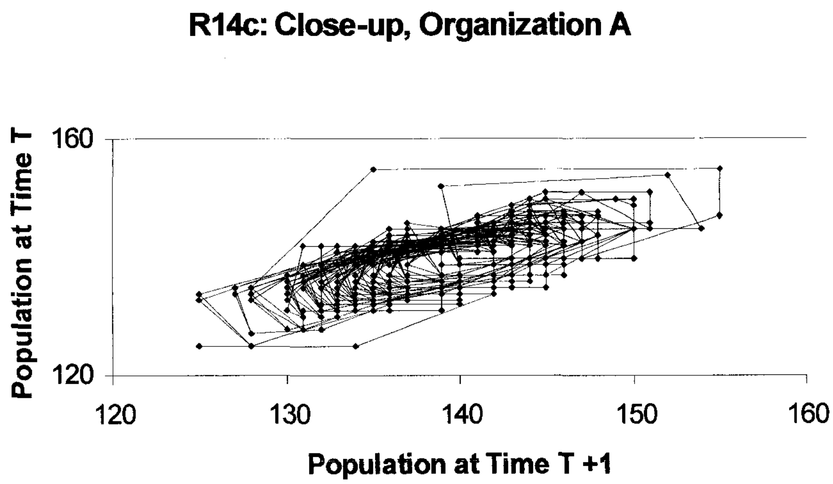

FIGURE 26

RUN 14C

Run 14, Plot c, Figure 26: This figure shows a close up of the phase plot, showing the circulation of the organization's population around a stable population level of approximately 137. Compare this with the circulation pattern shown in run 3. Run 14 has a much more complex pattern, occupying many more points. This appears to indicate that as the economic environment becomes more complex (three vs. two competitors), the resulting behavior also becomes more complex. However, this conclusion is premature, as will be discussed a little later.

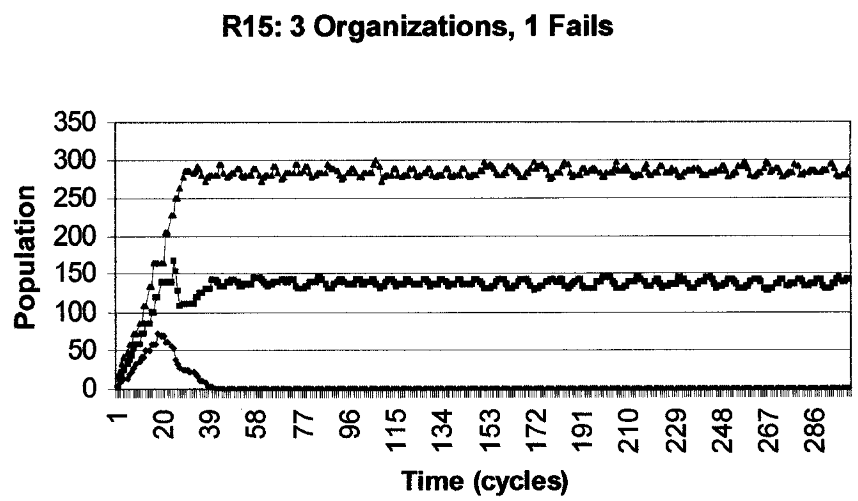

FIGURE 27

RUN 15

Run 15, Figure 27: In this run, three different organizations were started. Initial GFUND values were started at 7, 15, and 25 respectively. Note that one of the organizations failed after a short period of growth, leaving the remaining two organizations to split the market with the characteristic 1/3rd–2/3rd split. NOER remained equal to 1.0 in this run; in the runs with two organizations and NOER equal to 1.0, organizations never failed. It would appear that, in the given economic environment, having three competing organizations oscillating chaotically provides situations of sufficient complexity to trigger a failure reaction in one organization.

There are several analogies that are suggested by this behavior. One would be the threshold energy in a molecule. With three organizations, there was sufficient "energy" to kick one of the organizations beyond the stable limits of the system, to failure. A more vivid analogy might be marbles in a mixing bowl. As the marbles roll around inside the bowl, they sometimes collide, knocking one or the other towards the lip of the bowl. With two marbles hitting each other, one is never given enough kick to go out over the lip of the bowl. However, with three marbles, there is more potential energy rolling around in the bowl. There are situations where two marbles collide with one, giving that one enough of the energy, directed in the right way, to kick out over the lip, where it then falls to the floor.

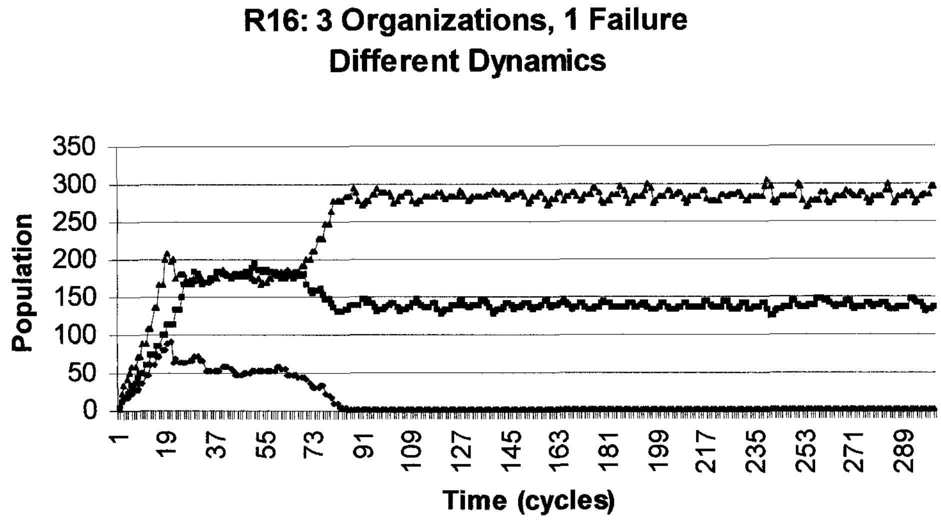

FIGURE 28

RUN 16

Run 16, Figure 28: In run 15, one organization failed and the other two reached the area of their stable population levels within 40 cycles. In run 16, three organizations were started, this time with initial GFUND values at 18, 20 and 22. Note that the results are the same—one organization fails, and the other organizations split the market 1/3rd–2/3rds—but this time the dynamics of the paths are considerably different. The organizations initially look like they are trying to settle into a split of the market, but the split does not appear to be stable. Eventually, around cycle 70, one organization begins the path to failure while the other two split the market.

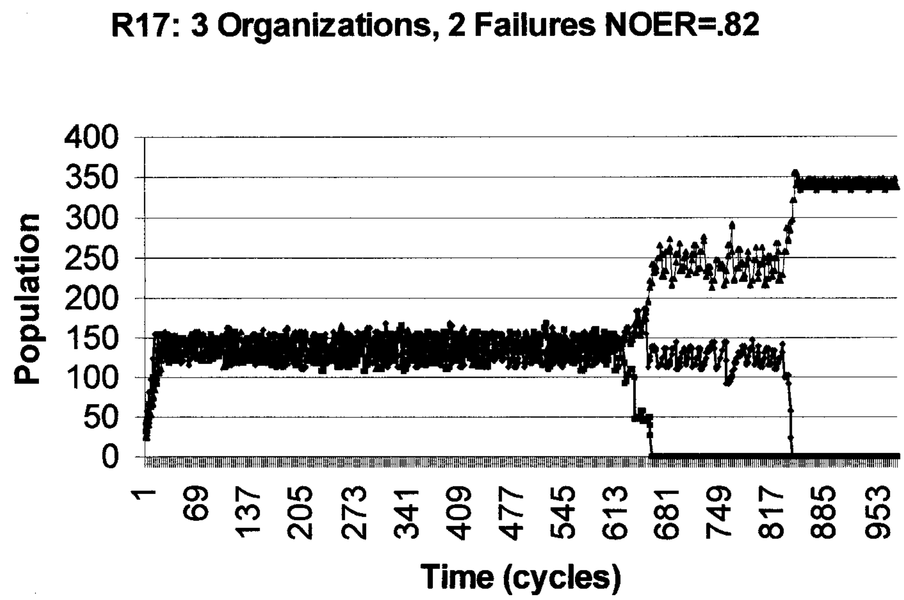

FIGURE 29

RUN 17

Run 17, Figure 29: This run began with three organizations with initial GFUND values of 26, 30, and 34. NOER was set at 0.82 for all organizations. Note that all three organizations attempt to equally split the market, into what looks to be a stable aperiodic pattern; but at about cycle 600 the pattern breaks down and one organization fails and another picks up its share of the market. Later, around cycle 830, another organization fails, and one organization captures the entire market.

These three examples show the diversity of behavior that can be observed as a result of only changing a few variables in the model.

Organizations in a Business Cycle Market Environment

The business cycle—a period of growth in the general economy, followed by a period of recession and contraction, and then a return to growth—is a well-known phenomenon, even when it is conspicuous by its absence (as it was during the 1989-2000 economic boom in the United States).

The model includes a price vs. demand curve in accordance with neoclassical theory. To simulate the different prices available in the course of a business cycle, the price from the demand curve was multiplied by a number between 0.9 and 1.1, which varied sinusoidally with a frequency of approximately 100 cycles.

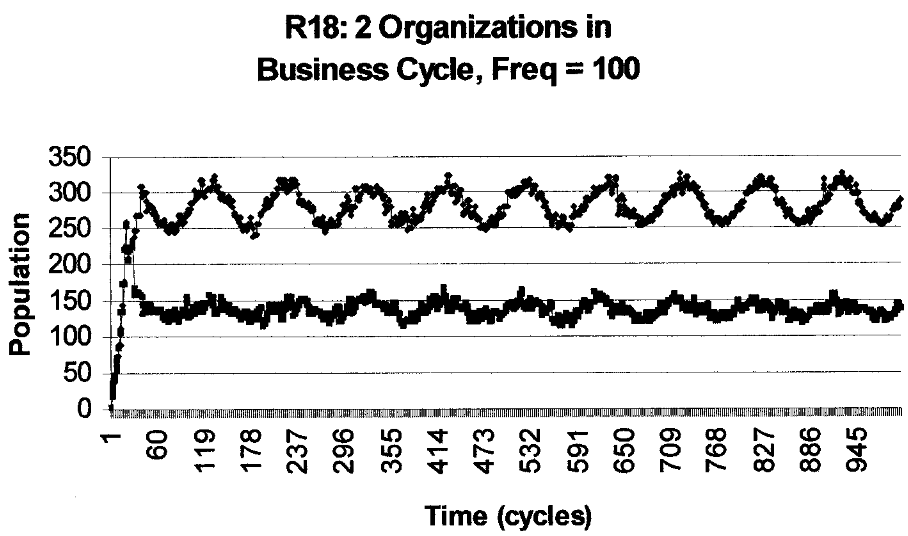

FIGURE 30

RUN 18

Run 18, Figure 30: This run shows the behavior of the populations of two organizations when in a business cycle environment. The organizations were begun with initial GFUND levels of 19 and 20. The sizes of the organizations oscillate around stable population levels that move up and down in phase with the business cycle. This makes sense, considering that greater demand for widgets yield higher prices, which can support greater production levels in an organization.

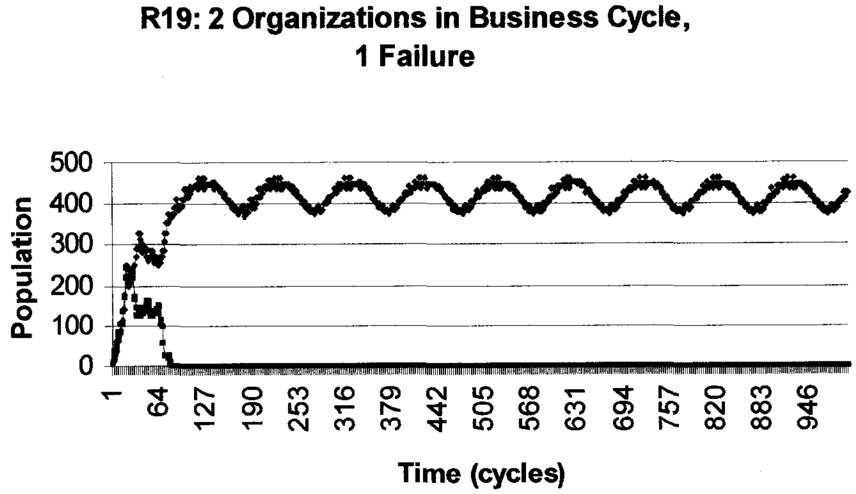

FIGURE 31

RUN 19

Run 19, Figure 31: This plot shows two organizations that were initiated with GFUND levels of 28 and 30. Note that one of the organizations fails right at the trough of the first business cycle. What makes this remarkable is that in other trials with two organizations, without the imposed business cycles but otherwise with all other factors equal, none of the organizations failed. It appears that the imposition of the business cycle added additional instability to the environment, resulting in the failure of one organization. The imposition of a price variation of plus or minus 10% also is in the range of the imposed NOER factor of 8% and 10%, which was shown earlier to also result in occasional organization failures.

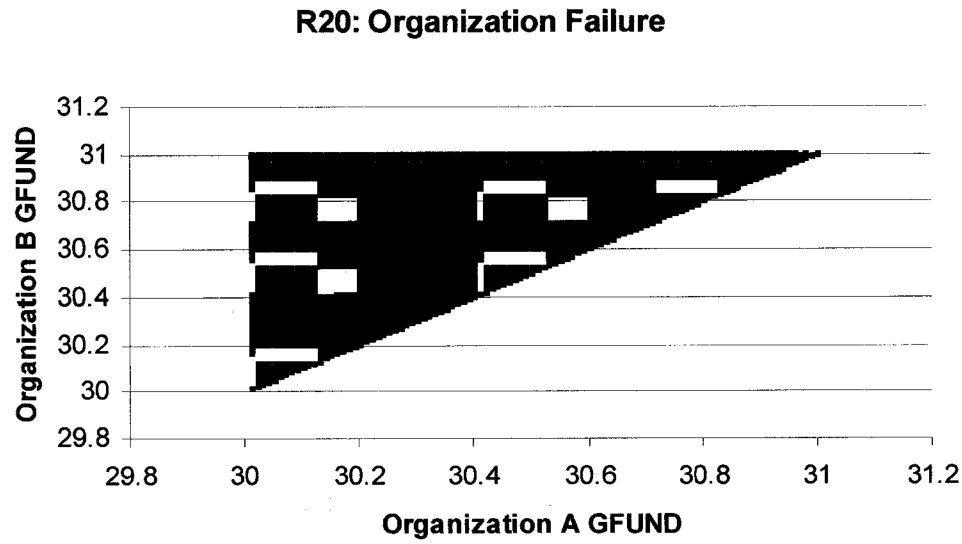

FIGURE 32

RUN 20

Run 20, Figure 32: This plot shows a sample of runs, again varying GFUND between two organizations. Runs were performed with GFUND between 30 and 31, with organization B always having the larger GFUND. A dot was placed on the plot when both organizations survived through 1000 cycles. Thus, a clear area shows where one of the organizations failed. Again, there appear to be repeating patterns that might be equated with fractal patterns.

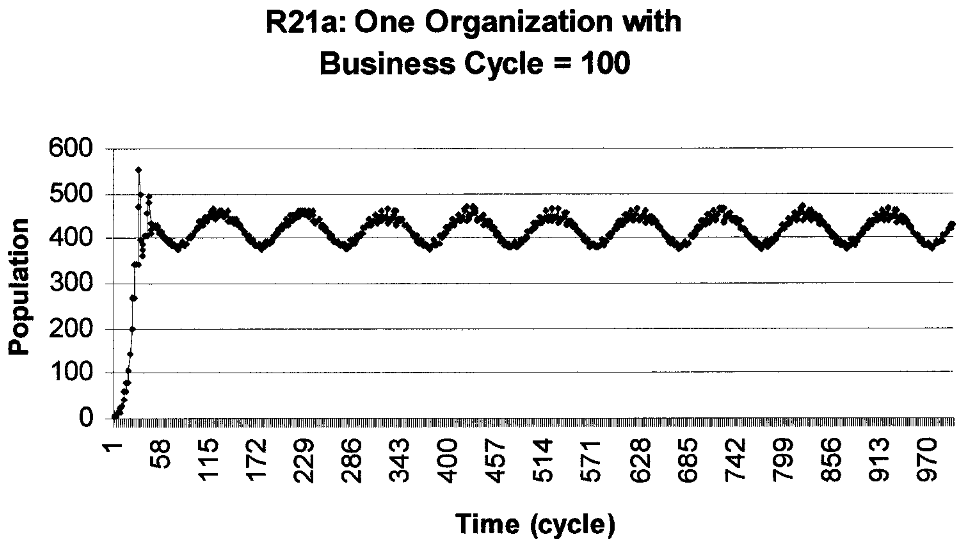

FIGURE 33

RUN 21A

Run 21, Plot a, Figure 33: At this point, it would be interesting to examine the behavior of an organization within an environment subject to the business cycle in more detail. The figure shows a plot of the behavior of one organization in this environment.

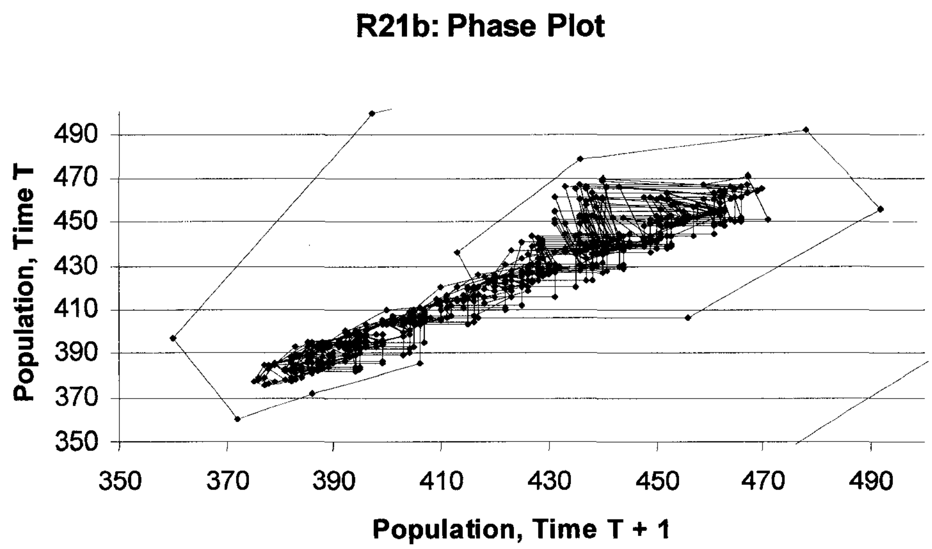

FIGURE 34

RUN 21B

Run 21, Plot b, Figure 34: This plot shows a close-up view of the phase plot. Compare it with plot R3c, Figure 14. The relative levels of complexity are an order of magnitude different. It would appear, from a simple examination, that the phase plot (at given price levels) both moves and transforms itself into different shapes.

One way to better examine this phenomenon is to look first at the pattern of the phase plot at the peak of the business cycle, and then to perturb widget prices by very small amounts. Then, plot the results to see what can be learned from the model.

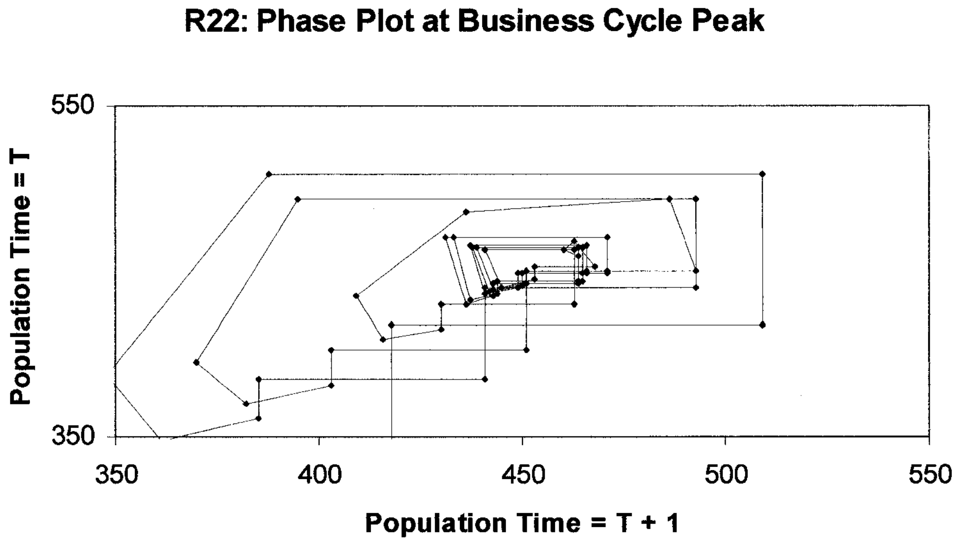

FIGURE 35

RUN 22

Run 22, Figure 35: This is a phase plot of a single organization in an economic environment where the demand curve for the price of widgets is set at the high end of the business cycle band, i.e., 3.3 dollars per widget with a price reduction slope of -.005 dollars per widget. The phase diagram shows a fairly simple pattern with about 60 points (model run duration 1000 cycles).

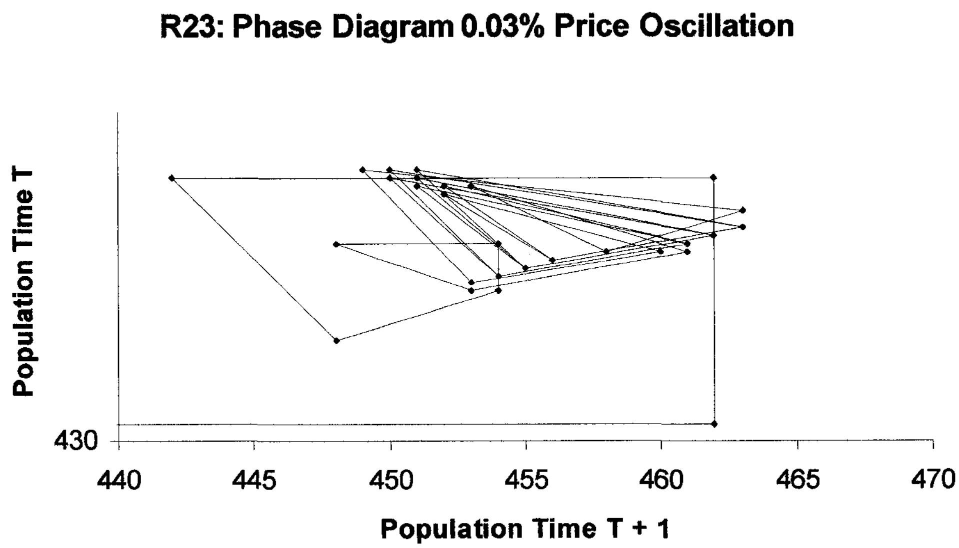

FIGURE 36

RUN 23

Run 23, Figure 36: This is a phase plot of a single organization again. The price is governed by the same demand curve as in the previous run, except that there is an imposed oscillation of plus or minus 0.033 percent at a frequency of approximately 100 cycles. This is a price oscillation on a $3.00 widget of less than 1/10th of a cent. Even for this tiny oscillation, the pattern of the phase plot has changed. It has actually become simpler, occupying less than half of the points as previously shown when there was no price oscillation.

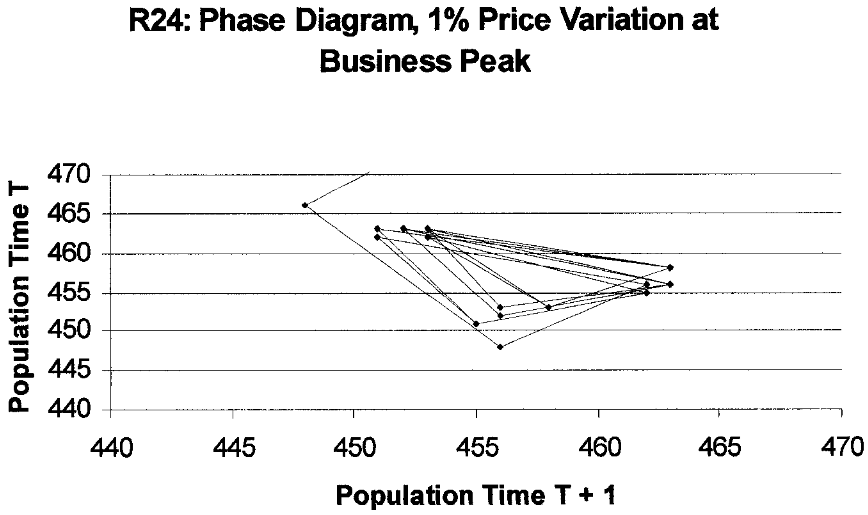

FIGURE 37

RUN 24

Run 24, Figure 37: This is a phase plot of a single organization, as in the previous run, except the magnitude of the price oscillation is increased to plus or minus 1 percent at a frequency of approximately 100 cycles. The pattern in the phase diagram is even simpler, occupying only 15 points.

R25: Phase Diagram, 2% Price Variation at Business Cycle Peak

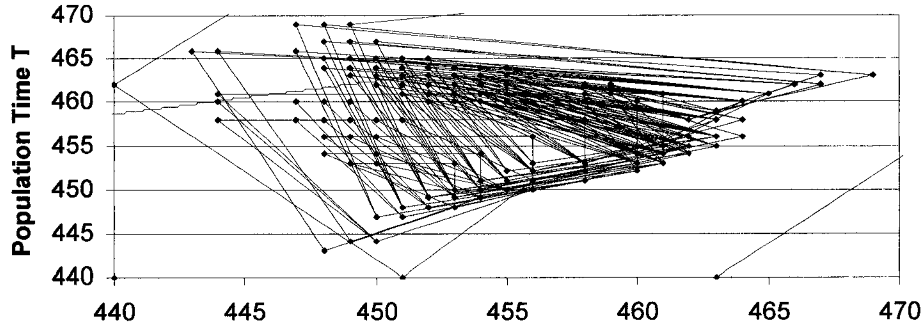

FIGURE 38

RUN 25

Run 25, Figure 38: This is a phase diagram of a single organization under the same conditions, except the magnitude of the price oscillation is increased to 2%. The complexity of the resulting behavior is increased dramatically, with the organization occupying over 100 points and with a wide diversity of paths.

This series of experiments shows that movement of the price curve not only causes the stable population level for the organization to move up and down, it also transforms the resulting oscillations between relatively simple patterns to complex patterns. It is almost as if the economic conditions had a certain resonance point. Away from the resonance point, the signal is very noisy; at or near that resonance point, the single is more "in phase" with the environment and the signal settles down into a simpler pattern.

Sensitivity to Initial Conditions

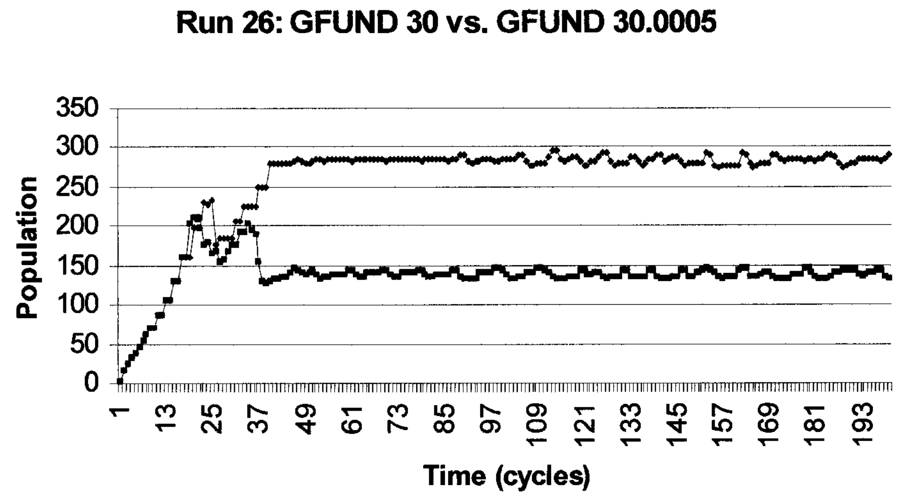

FIGURE 39

RUN 26

Run 26, Figure 39: The purpose of this run was to examine sensitivity to initial conditions. In the run, under the same economic conditions, organization A began with a GFUND of 30 dollars, while organization B began with 30.0005 dollars. This is the equivalent of a $500 difference between two 30 million dollar firms.

Note that separation between the two firms occurred at cycle 21. Eventually, the two organizations settled out at the now-familiar 1/3rd–2/3^{rds} market split. This again demonstrates an extreme sensitivity to initial conditions.

Immediate Conclusions and Implications

The model of the organization, constructed in accordance with principles of neoclassical economics and the idea of transaction costs taken from the New Institutional Economics, has been demonstrated to be chaotic. This deterministic model without any stochastic elements generated deterministic chaos. The behavior of the model showed:

- Regions of aperiodic oscillations;
- Examples where the aperiodic oscillations appear to be built up using fractal-like patterns;
- Examples of extreme sensitivity to initial conditions;
- Regions of non-monotonic behavior; and
- Positive greatest Lyapunov exponents on the order of +0.500 +- 0.020.

The time series generated had positive greatest Lyapunov exponents calculated using the Chaos Data Analyzer software. Positive greatest Lyapunov exponents are considered to be a reliable indicator of chaotic behavior. The greatest Lyapunov exponent values ranged between 0.44 to 0.61, calculated from a number of combinations of initial GFUND, number of organizations, reinvestment variables, and NOER values.

CHAPTER VII

IDENTIFYING THE SOURCE OF THE CHAOS: THE ORGANIZATION LOGISTIC EQUATION

Initial Experiments

As mention previously, there are a number of aspects of the model of the organization that could have contributed to the chaotic behavior. An obvious area to investigate would be to attempt to identify those specific things that resulted in chaos.

Two approaches were taken to identifying the source of the chaotic behavior in the model. The first approach was to take the model, in the configuration as exercised in the previous chapter, and cut out elements from the simulation that might be contributing to the chaotic behavior. With that element removed from the model, the simulation was then run again, and a time series tested for chaotic behavior by calculating the greatest Lyapunov exponent. Several such experiments were attempted.

First, a baseline configuration of the model was established. In that configuration, the greatest Lyapunov exponent for a representative run was +0.513 +- 0.020.

One possible source of chaos was the production penalty levied when management and worker numbers were out of balance. When this factor was removed, a representative run yielded a calculated greatest Lyapunov exponent of +0.321 +- 0.020, indicating the time series was still chaotic.

A second possibility was associated with the requirement that only integer widgets could be sold. When this restriction was removed, a representative run yielded a calculated greatest Lyapunov exponent of +0.474 +- 0.021, indicating the time series was still chaotic.

Another possibility was that the algorithms that calculated the number of personnel that would be hired or fired was introducing the chaotic elements into the model. A simplified heuristic was installed, where, if a hire or fire decision would made, the number of workers would be ten percent of the current size of the organization. With this heuristic, a representative run yielded a calculated greatest Lyapunov exponent of +0.397 +- 0.032, indicating the time series was still chaotic.

Another possible source of chaos was the algorithm used to trigger a hiring or firing decision. A simplified trigger was installed. If the organization showed a profit in the last cycle, hire; if the organization showed a loss, fire. With this heuristic, a representative run yielded a calculated greatest Lyapunov exponent of +0.360 +- 0.032, indicating the time series was still chaotic.

Combinations of these program modifications were also tried, but they all still generated regions of chaotic behavior. For example, taking the baseline of the model and simplifying both the hire/fire decision heuristic and the number of personnel hired or fired, a representative run yielded a calculated greatest Lyapunov exponent of +0.454 +- 0.031, indicating the time series was still chaotic.

Derivation of the Organization Logistic Equation

At this point, it appeared a better approach would be to begin with the simplest possible representation of the organization, and build from there. This motivated a search for a simple equation that would be representative of the processes in the simulation.

In such sciences as ecology and epidemiology there have been efforts to better understand the dynamics of populations using very simple mathematical models. Beltrami (2002, p. ix) justifies the approach.

> Mathematical modeling is viewed as an organizing principle that enables one to handle a vast and often confusing array of facts in a parsimonious manner. A model is useful when it reveals something of the underlying dynamics, providing insight into some complex process. Although models rarely replicate reality, they can serve as metaphors for what is going on in a simple and transparent manner, a bit of caricature perhaps but informative nonetheless.

Schaffer and Kot (1986, pp. 159-160) discuss one such effort.

> In the ecological literature, the most widely discussed model admitting to chaotic dynamics is the logistics equation
>
> (8.1) $$X_{i+1} = sX_i(1-X_i)$$
>
> Traditionally, eqn. (8.1) and its congeners are viewed as literal descriptions of single-species systems with discrete, non-overlapping generations, for example insects with one generation per year. Then *s* is closely related to the maximum *per capita* rate of increase, i.e., births minus deaths per individual.

In the social sciences there has been significant effort to discern chaotic behavior in data sets, but little effort to depict social systems in terms of simple equations. My research has not been able to discover any attempts to model the "populations" of social systems in a simple equation form. Searches were conducted on the Internet using the Google search engine, and three academic publications data bases covering mathematics, economics, and the social sciences journals, dissertations, and specialty publications. About 5,000 publication abstracts were screened.

A derivation of an equation representing the time-step population of an organization in a competitive environment can be made if a number of assumptions are employed.

Envision an organization with a population *P*. This population remains constant over some time period. At the end of each time period, the population is adjusted based on the success of the organization, e.g., if the organization is successful its population grows, if it is unsuccessful, its population shrinks.

The organization will be envisioned as a commercial organization producing and selling a product ("widgets"). The number of widgets produced and sold is represented by the variable *W*. The price received decreases in accordance with a classic linear Marshallian price-volume relationship.

Let P_n represent the population of the organization at time cycle n.

Let *DeltaP* represent the change in the population over one time period.

Then,

(1) $P_{n+1} = P_n + DeltaP$

Let S represent the success of the organization. In this case, success will be measured in terms of profit or loss.

Let R be the "reinforcement factor." This relates the success of the organization with how much it will grow or shrink. For example, if $R = 1.1$, and if the organization made a profit of 10, then the organization's population would grow by (10)(1.1) = 11 members. Thus,

(2) $DeltaP = (S)\,(R)$

Since we have identified S in this case as profit or loss, S can be expressed as

(3) S = Income - Costs

In the simulation model, the population of the organization produces widgets, and the widgets are sold in an economic environment. Thus,

(4) Income = (number of Widgets produced) (price per Widget)

Let a be a constant that represents the productivity of the population. Thus,

(5) (Widgets produced) = $a\ P_n$

The price per widget will be established by a Marshallian price-demand line, where

(6) (price per widget) = $b + c\ W$

where b is a constant, the price of the initial widget; c is a constant that represents the slope of the line; and W represents the number of widgets on the market at that time.

By substitution of (5) and (6) into equation (4),

Income = $(a\ P_n)\ (b + c\ W)$

(7) Income = $a\ b\ P_n + a\ c\ P_n\ W$

Assume that the only costs in the organization are the salaries of the workers (or, more generally, that costs are proportional to the population in the organization). Let d be a constant representing the costs per worker. Thus,

(8) Cost = $d\ P_n$

Now, by substitution of (7) and (8) into equation (3), the success of the organization (profit) can be represented as

$S = (a\ b\ P_n + a\ c\ P_n\ W) - (d\ P_n)$

(9) $S = (a\ b\text{-}d)\ P_n + a\ c\ P_n W$

And, again by substitution of (9) into (2),

$DeltaP = [(a\ b - d)\ P_n + a\ c\ P_n W]\ [R]$

(10) $DeltaP = (a\ b - d)P_n\ R + a\ c\ P_n\ W\ R$

Again, by substitution of equation (10) into (1),

(11) $P_{n+1} = P_n + (ab\text{-}d)\ P_n\ R + a\ c\ P_n W\ R$

As stated above in equation (5), the number of widgets produced is the productivity a times the population P, or $W = aP_n$. Substituting equation (5) for W in the equation (11), we get

$$P_{n+1} = P_n + (ab\text{-}d)\, P_n\, R + a\, c\, P_n\, (aP_n)\, R$$

(12) $\quad P_{n+1} = P_n + a\, b\, P_n R - d\, P_n\, R + a^2\, c\, P_n^{\,2}\, R$

To examine a "practical" case, the following values have been used to further simplify the equation:

One worker produces one widget per cycle, or $a = 1$.

The initial demand price of widgets is 3, or $b = 3$

The slope of the demand curve is -.005, or $c = -.005$.

The salary per worker per cycle is 1, or $d = 1$.

These values will remain constant through the course of this study, unless otherwise noted, over all variations of the models, when looking at the behavior of the system.

Substituting these values into equation (12), we get

(13) $\quad P_{n+1} = P_n + (1)\,(3)\, P_n\, R - (1)\, P_n\, R + (1)\, 2\, (-.005)\, P_n^{\,2}\, R$

Which reduces to

(14) $\quad P_{n+1} = P_n + 3\, P_n\, R - P_n\, R - .01\, P_n^{\,2} R$

(15) $\quad P_{n+1} = P_n + 2P_n R - .01 P_n^{\,2} R$

There are obvious and immediate parallels that appear between this equation and the Logistics equation developed by May (1976, 1980), which Lauwerier (1986: p. 41) cites in its more usual form as:

(16) $x_{n+1} = a\, x_n\, (1 - x_n),\ 0 < a <= 4$

The logistic equation can be made to look a bit more like our organization's population equation by multiplying through the value in the parenthesis:

(17) $x_{n+1} = a\, x_n \qquad - \ a\, x_n^2$ as compared to

(18) $P_{n+1} = (P_n + 2\, P_n\, R) \ - .01\, R\, P_n^2$

The organization population equation behaves in a way similar to May's logistics equation. With the test values substituted as above, when *R* is between 0 and 1 the population goes to 400. At values greater than 1, the population bifurcates, then bifurcates again, and continues until the plot is a near duplicate of the plot of May's equation.

Figure 40 shows the bifurcation diagram of the logistics map for May's equation, with 0 < R < 4.

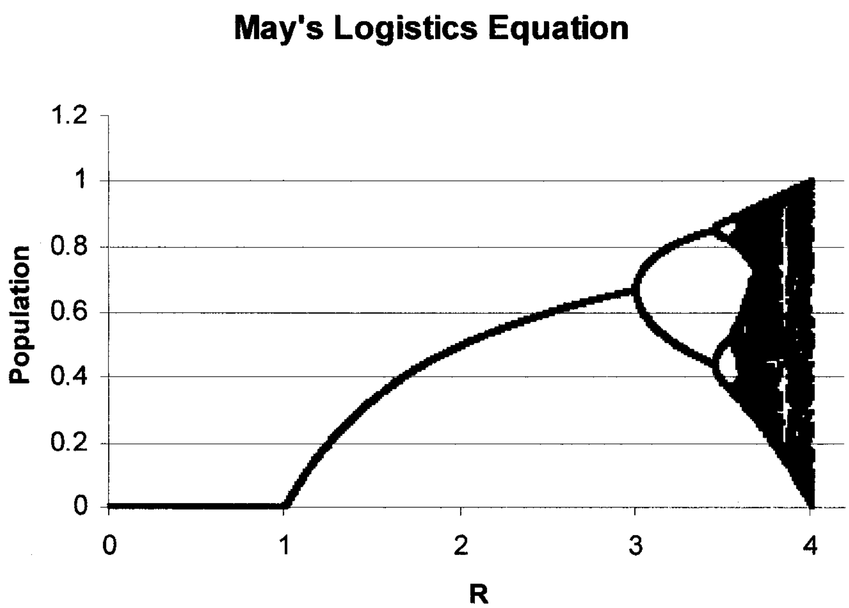

FIGURE 40

MAY'S LOGISTICS EQUATION

The Organization Logistic equation duplicates this behavior, with slight differences. Obviously, the range of R and the value of the population are different. The Organization Logistic equation ranges between $0 < R < 1.5$, different that May's equation, and with $R < 1$ it has a steady state value of 400. With values of R greater than 1.5, the Organization Logistic equation function goes to minus infinity. Otherwise, the similarity is obvious. To show the similarities in more detail, Figure 41 shows the organization logistic equation with $1.2 < R < 1.5$. This plot shows the same rich detail of behavior as in May's logistic equation. It is, as Berger (2001,

p. 29) would assert, part of a more generalized "logistics family" defined by

(19) $F_u(x) := ux\,(1\text{-}x).$

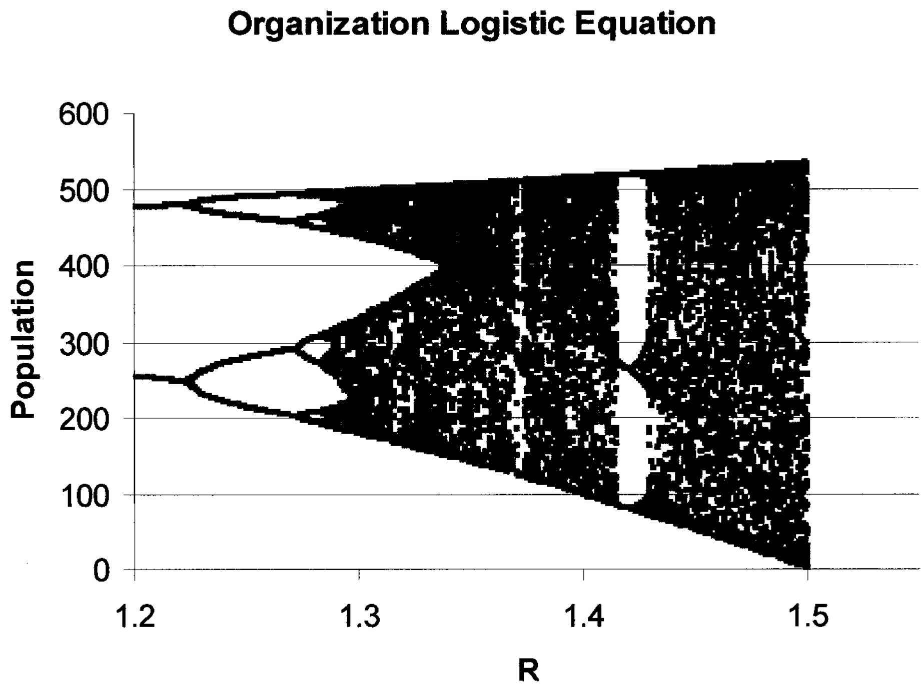

FIGURE 41

ORGANIZATION LOGISTIC EQUATION

Figures 42 and 43 show an equivalent plot for the simulation model. It shows a plot of one organization's population possibilities for a fixed set of economic conditions. The x-axis is the reinvestment rate, the equivalent of the variable R in the Organization Logistic equation. Figure 42 shows the plot when the reinvestment rate is between 0.05 to 0.37, while Figure 43 shows the results for a reinvestment rate of between 0.37 and 0.7. Note that there is a pattern of bifurcation, similar to the logistic equations.

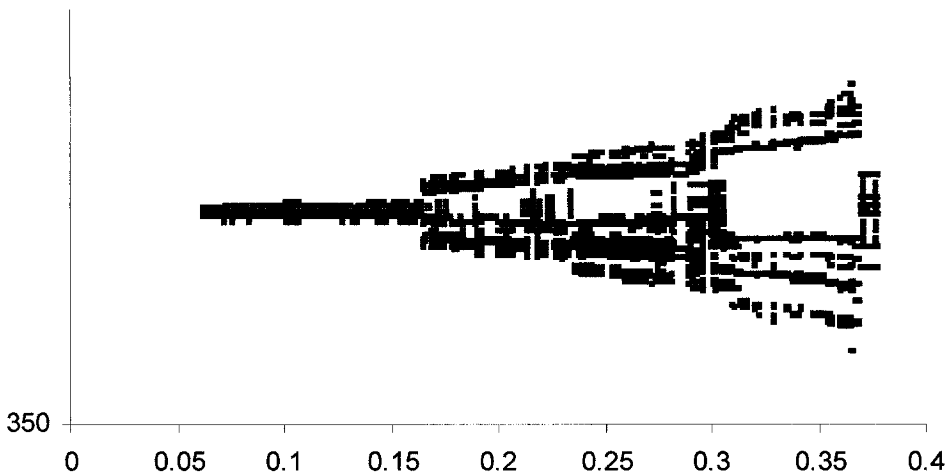

FIGURE 42

ORGANIZATION LOGISTIC EQUATION, R FROM .05 TO .37

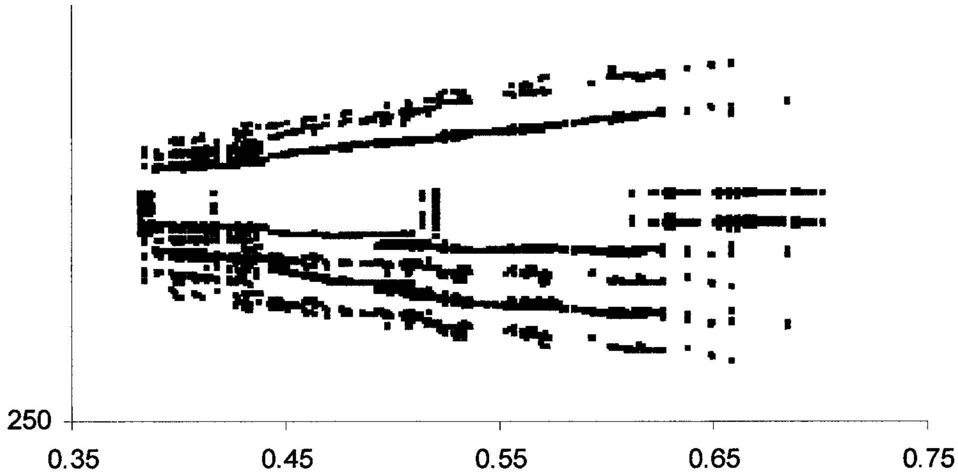

FIGURE 43

ORGANIZATION LOGISTIC EQUATION, R FROM .37 TO .70

The Organization Logistic Equation with Multiple Competing Organizations

The equations and results shown above hold when there is one organization in the economic environment dealing in the specified product. This equation can be generalized to the usual case in a competitive environment where there are several organizations that are marketing the same type of product.

For the case where there are two competing organizations, let the subscript "1" and "2" designate the two different organizations. Thus a_1 and a_2 would represent the productivity of the two organizations, b still represents the initial price point and c the slope of the price curve, and d_1 and d_2 represent the cost per population unit of the two organizations. $P_{1(n)}$ and $P_{2(n)}$ represent the population of the two organizations at time *n*, and $P_{1(n+1)}$ and $P_{2(n+1)}$ the population of the two organizations at time $n+1$.

In the case of one organization, through a simple process of substitution equation (12) was derived:

(12) $P_{n+1} = P_n + a\, b\, R\, P_n - d\, P_n\, R + a^2\, c\, R\, P_n^{\,2}$

The same process of substitution can be used to determine the population of each individual organization. The most significant difference in this case is that the price per widget is a function of the total production of each organization, or

(20) $W_t = W_1 + W_2$

Using the same process, the equivalent to equation (12) for two organizations is:

(21) $P_{1(n+1)} = P_{1(n)} + R_1\, P_{1(n)}\, [a_1\, b - d_1\,] \; + c\, a_1\, R_1\, P_{1(n)}\, [\, a_1 P_{1(n)} + a_2\, P_{2(n)}\,]$

The variable c, the slope of the price curve, is presumed to be a negative number, and small, between zero and one. This means that the last term in equation (21) tends to reduce the population, while the second term is the growth part of the equation.

There are differences between this equation and the "Organizational Growth: Competition Model" that is a standard form of the Logistic equation when there are competing populations. Marion (1999, p. 203) states:

> Building on [the Logistic equation], we can add one more term to represent the impact that a second, competing population would exert on the first. That second term is $aX_{old}Y$, and the new, modified equation is:

(22) $X_{new} = rX_{old}(1 - X_{old}) - aX_{old}Y$

In the extra term, *Y* represents the size of the second population expressed as a percentage of carrying capacity, and *a* represents the success of the second population in frustrating the efforts of the first (a high value of *a*, for example, might suggest that *Y* is outbidding *X* for market sales).

Equation 22 can be expanded into:

(23) $X_{new} = rX_{old} - X_{old}\, rX_{old} - aX_{old}Y$

Doing a side-by-side comparison between the two equations in similar forms, we have:

(21) $P_{1(n+1)} = P_{1(n)} + R_1\, P_{1(n)}\, [a_1\, b - d_1\,] + c\, a_1\, R_1\, P_{1(n)}\, [\, a_1 P_{1(n)} + a_2\, P_{2(n)}\,]$

(23) $X_{new} = rX_{old} \qquad\qquad - X_{old}\, rX_{old} - \quad aX_{old}Y$

This allows a term-by-term comparison.

In the first term, in equation 23 Marion identifies *r* as the birth rate, where in equation 21 the equivalent term is the reinvestment rate times productivity times the initial price point.

For the second term in equation 23, Marion shows the birth rate times the old population squared, while in equation 21 the equivalent term is the reinvestment rate times cost rate times the old population (unsquared).

For the last term in equation 23, Marion show the two competing populations multiplied, times a value *a* that is identified as some factor of the second population's success in "frustrating the efforts of the first," where in equation 21 the equivalent term is the (negative) price slope times the productivity of the first organization times the first organization's reinvestment rate times the sum of the product of the two organizations' population times productivity.

This equation can also be compared with the classical (scalar) Lotka-Volterra system, which Solomon (1999, p. 4) shows as:

(24) $w(t+1) = (1 + birth - death) * w(t) - competition * w(t)^2$

As can be seen, the terms of this are also different than that derived for the Organization Logistic equation.

Thus, the derivation of the Organization Logistic equation provides a significantly different interpretation of the constants than what was previously guessed by authors who used the Logistic equation by assumption. In particular, the term that Marion identified as an amorphous "outbidding" term is identified in terms of the market demand, productivity of the competing firms, and reinvestment rate.

To simplify things further, the values used in the simulation can be substituted for the variables. Using a = 1, b = 3, c = -.005, and d = 1 (as before), equation (23) becomes

(25) $P_{1(n+1)} = P_{1(n)} + R_1 P_{1(n)} [(1)(3) - (1)] - (.005)(1) R_1 P_{1(n)} [(1) P_{1(n)} + (1) P_{2(n)}]$

The equation becomes:

(26) $P_{1(n+1)} = P_{1(n)} + 2R_1 P_{1(n)} - .005 R_1 P_{1(n)} [P_{1(n)} + P_{2(n)}]$

The equation shows that the size of the organization tends to go up based on the population of its own organization, and tends to go down based on the organization's population times the total population in the market (assuming that the productivity of the two organizations are the same) . Eventually the population squared term on the right (with the small constant) overwhelms the linear term in the center (with the larger constant).

Figure 41 shows that, as R is varied, organization would have values of R that results in chaotic behavior, values that result in oscillation between eight, four, or two points, and values that stabilize on a single point.

One interesting characteristic of the equation is that, when there are several organizations in the system, the characteristics of the organization with the lowest R value eventually dominates the results. For example, Figure 44 shows a plot of the populations of three organization that all have R values in the chaotic region. The entire system is chaotic with a greatest Lyapunov exponent of + 0.517 +- 0.017.

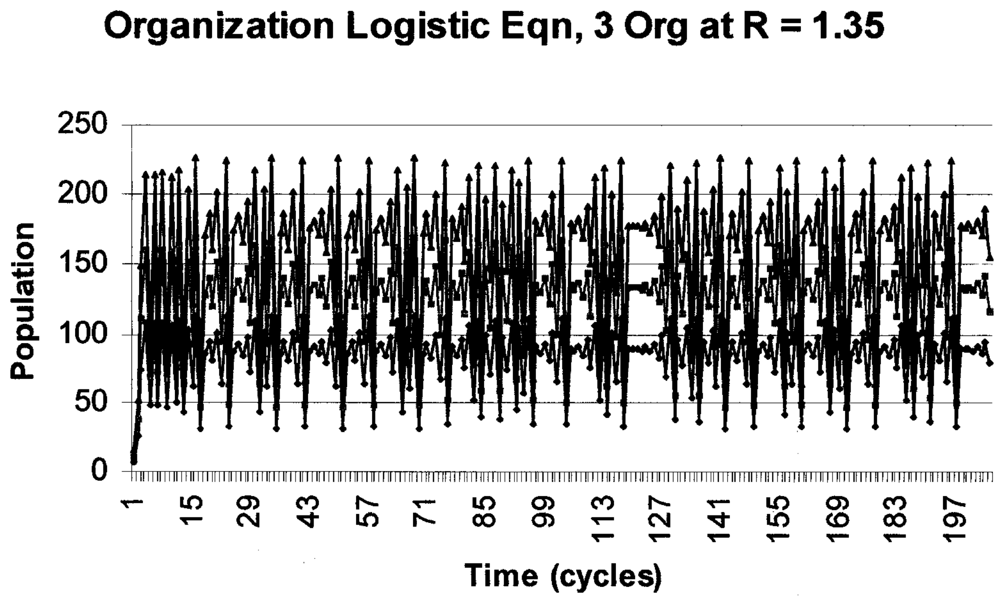

FIGURE 44

ORGANIZATION LOGISTIC EQUATION,
3 ORGANIZATION AT R = 1.35

Figure 45 shows the results when all the conditions remained the same, except the R value of the organization with the smallest initial population was change to 0.95, the area that results in a stable population. The system starts out oscillating, but eventually settles out to where all three organizations asymptotically approach single stable values. With two "chaotic" organizations and one "stable" organization, all three organizations in the system asymptotically approach stable values.

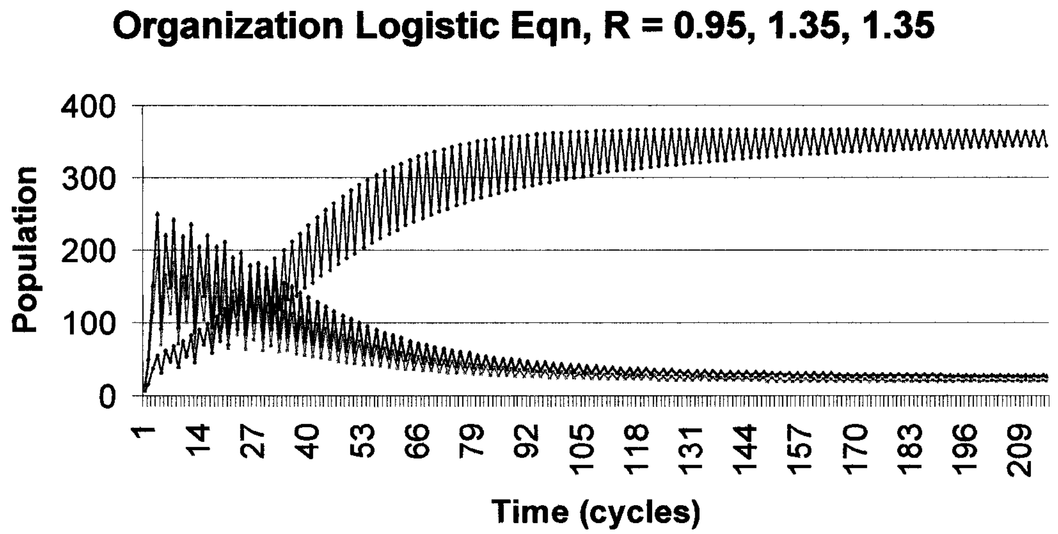

FIGURE 45

ORGANIZATION LOGISTIC EQUATION,
R = 0.95, 1.35, 1.35

This has interesting implications. If other social systems mirrored this kind of behavior, then a perturbation of the system could lead to an initial period of chaotic or oscillating behavior, which eventually dampens out to new stable values.

Other analogies between observed human behavior and model performance is possible. For example, Figure 46 shows a model run with three organizations, one with an R value in the chaotic region, one in the bifurcation area, and one in the stable area.

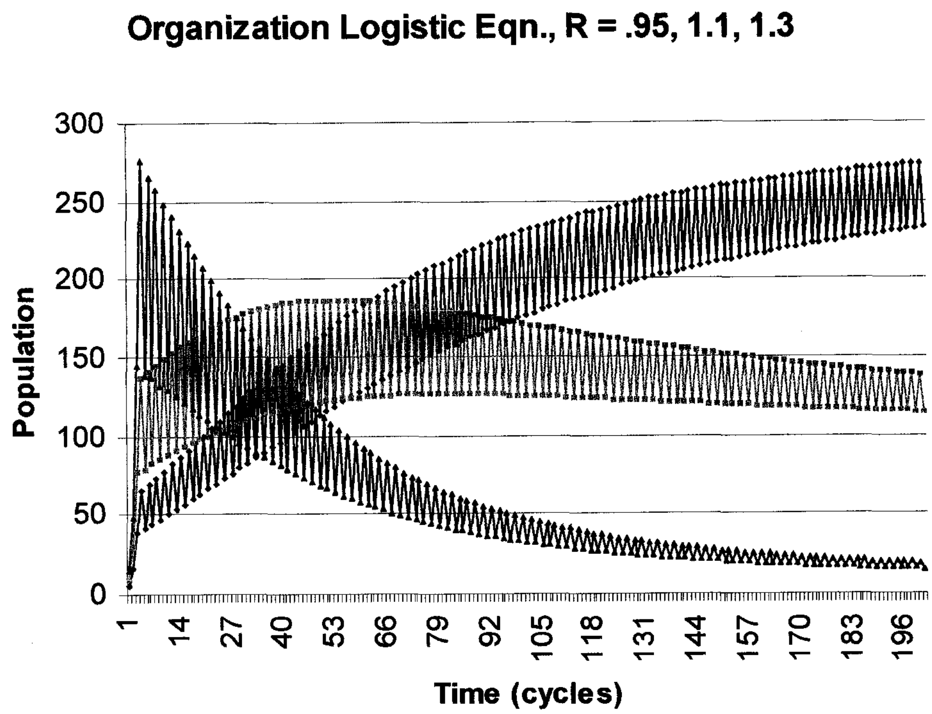

FIGURE 46

ORGANIZATION LOGISTIC EQUATION,
R = .95, 1.1, 1.3

The organization with the highest (chaotic) R value initially jumps to the highest population value. However, it does not stay that way for long; eventually, it recedes to become the smallest organization of the three. The organization with the lowest R value eventually becomes the largest organization.

The parallel between this plot and the "dot.bomb" phenomenon (the failure of high-growth internet commerce ventures) in the U.S. economy in the 1990's is striking. A new business area, internet commerce, was just opening. Some companies were given a large amount of growth feedback (in the form of added venture capital) at the slightest sign of success. While usual stock prices are generally valued at ten to twenty-five times current earnings, these internet stocks soared to multiples of thousands of times forecasted earning, with many of the forecasts ridiculously optimistic. Many of these companies rocketed up in organization size, only to eventually collapse. Other companies with more stable growth patterns later captured the market.

It is premature to make too many parallels between the behavior of the equations and actual human social systems. However, the possibilities are intriguing. There is certainly potential here for additional research.

Previous Research on the Logistic Equation

May's Logistic equation and a near cousin, the Lotka-Volterra model, are classic approaches to formulating population problems in the biological sciences. It has been applied to greatly-varied systems, anything from the growth of swimming crabs (Ariyama 1993), electrochemical dynamics (Sadkowski, 2000), to many medical applications.

The equations have also been used in economics, although not as extensively. McCane (2002) indicates that "economic models based on analogies between companies and species . . . have a comparatively long history in economics." Their use can be generalized onto one of three categories:

The equations are used in "curve fitting" exercises, where the generalize "S" shape of the logistic equation curve is employed. Examples can be seen in Sarno (2001), Radosavljevic and Horner (2002), Goodwin (1967), and Jarsulic (1989).

Or, the number and density of organizations is investigated, for example, the population of banks or brewers or newspapers in a particular geographic area (Hannan & Carroll, 1992). Scott (1992, p. 113) provides a short history of such efforts, citing Hofstadter in 1945, Hawley in 1950, Campbell in 1969, Hannan and Freeman in 1977, and Aldrich in 1979. In this case, the organization is the smallest unit of measure, and there is no differentiation between small or large organizations.

Or, the equations are taken directly as derived from the biological population studies, and conjecture offered as to what the variables and constants might represent in the new context. For example, Aoki (1998), Aoki and Yoshikawa (1999), Solomon and Richmond (2001), and Marion (1999) have taken this approach.

One effort that does not fit into these categories (and is the closest thing to the work done here) is a model by Farmer (2000), who looked at the dynamics of capital. He arrived at a set of dynamic equations for the evolution of capital that are analogous to the generalized Lotka-Volterra equation, using terms of liquidity, random change in prices, standard deviation of prices, non-equilibrium price formation rules, and other terms in a stochastic approach to the problem.

In contrast, the derivation of the Organization Logistic equation shown here makes the leap to considering the population *of* the organizations, relates the population with production of a product and costs to create the product, and relates individual parameters such as productivity and costs to each individual organization in a deterministic fashion. The equation is derived from a simple and accepted set of "first principles." I believe that this is the first time this kind of approach has been taken.

There could also be a link between the Organization Logistic equation and actual observed phenomenon. Many natural and man-made phenomenon exhibit power-law probability distributions. Solomon (1999, p. 1) provides a list of seventeen such studies. He goes on to show that generalized Lotka-Volterra models "provides a general method to simulate, analyze, and understand a wide class of phenomenon that are characterized by power-law probability distributions." The generalized Lotka-Volterra models have been shown to produce power-law distributions, both theoretically and using numerical simulations. According to Levy, Levy and Solomon (2002, pp. 196-197), Lotka-Volterra (LV) systems generally exhibit effects encountered in real markets, such as Pareto wealth distributions and stable-Paretian returns. A relationship between these two effects was predicted from the equations, and then precisely validated by the data.

> The LV interactions lead to the emergence of herding and collective adaptive behavior that, in turn, ensures the (financial) survival and global financial growth of the system. In fact, the emergent adaptive behavior is so resilient that [it] thrives even in conditions in which an average macroscopic view of the market would predict extinction/bankruptcy.

This indicates the existence of a very interesting set of connections:

- Many economic and social phenomenon generate power law distributions.
- Lotka-Volterra models (and, through extension, the Organization Logistic equation) generate power law distributions; thus, it could be that such models are good representations of the dynamics of the economic and social phenomenon.
- The Organization Logistic equation is derived from simple "first order" economic principles, thus, establishing a potential link between the first order principles in economics (and, again by extension, in at least one human social system, the organization) and observed power law distributions.

This set of connections is not in any way conclusive or could be considered proven. It is perilously close to a *post hoc ergo prompter hoc* argument. It is, however, a fascinating idea worthy of additional study.

The Organization Logistic equation deserves additional study; however, it is beyond the scope of this paper to take it further.

Significance of the Organization Logistic Equation

The Organization Logistic equation is a potentially significant discovery. It compliments the efforts of other researchers to discover an underlying process in economics that is fundamentally chaotic.

Summary

The last several chapters have demonstrated two possibilities:

First, that simple principles of behavior, in the context of a human organization, can generate complex and chaotic behavior. This was demonstrated by creating a simulation of a human organization and determining if its behavior is chaotic. The behavior of the model was determined to be chaotic.

Second, that a simplified equation representing the population of a human organization in a competitive economic environment can, when some reasonable assumptions are used, yield an equation that looks and performs similar to May's logistic equation, which is a classic example of chaotic behavior. This equation was created based on a further simplification of the underlying concepts used to build the simulation of the organization.

These two demonstrations argue the possibility that if fairly simple principles of human behavior exist, then resultant human behavior could be very complex and possibly chaotic. The connection between simple economic principles and chaotic behavior through the Organization Logistic equation offers the possibility that such behavior could be a fundamental underlying mechanism of human behavior, inherent in the principles of human interaction.

There are two additional pieces of background information necessary prior to presenting and discussing the overall hypothesis. In the next chapter, the concept of a paradigm will be introduced. That material will precede introduction of the last background topic, the Scientific Method.

CHAPTER VIII

PARADIGMS IN NORMAL SCIENCE

Paradigms

The concept of a paradigm is key to providing a framework to better understand the current environment of thinking in the social sciences. Thomas Kuhn popularized the idea of paradigms in scientific thought. His book, *The Structure of Scientific Revolutions*, was first published in 1962; this discussion will draw from the enlarged and corrected second edition, which appeared in 1970.

Kuhn (1970, p. 10) introduced the idea of a paradigm in normal science as follows:

> "Normal science" means research firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice. . . . Aristotle's *Physica,* Ptolemy's *Almagest,* Newton's *Principia* and *Opticks,* Franklin's *Electricity,* Lavoisier's *Chemistry,* and Lyell's *Geology*—these and many other works served for a time implicitly to define the legitimate problems and methods of a research field for succeeding generations of practitioners. . . . By choosing [the word "paradigm"] I mean to suggest that some accepted examples of actual scientific practice—examples which include law, theory, application, and instrumentation together—provide models from which spring particular coherent traditions of scientific research.

A paradigm thus represents a particular mind set in a scientific community. That mind set is not passive, but actively influences the parameters of future investigations and molds their interpretations. Stanesby (1985, pp. 136-138) used a different term to express a nuance of the idea: "Thus science is done from within a *Weltanschauung*, or conceptual perspective, which is intimately linked with the way our language conceptually shapes our understanding of the world." He went on to summarizes Kuhn's approach:

> History, according to Kuhn, does not display a steady accumulation of knowledge as the great men of science increasingly come to grips with the world through theory and experiment. Rather, history displays a variety of traditions or "paradigms" in which men are bound by shared presuppositions or underlying principles which determine the way they see the world. The replacement of one tradition or "paradigm" by another is more like a change of conceptual spectacles than a steady progression from the darkness of ignorance and superstition and prejudice into the increasing light cast by objective science. Scientific progress is viewed, not in terms of an end or goal such as increasing verisimilitude, but in terms of change.

Probably the best way to understand the concept and its implications is through an example. The following paragraphs will draw from *The Structure of Scientific Revolutions*, particularly Chapter 7, "Anomaly and the Emergence of Scientific Discoveries." Portions of this discussion were published in the *Proceedings of the Eighth Conference on Computer Generated Forces and Behavioral Representation* (Zimm, 1999).

In the Sixteenth and Seventeen Centuries "heat" was thought to be a type of fluid which was given the name *caloric*. A hot block of metal was thought to contain more of this fluid than a cold one. If a cold metal block were held over a fire, caloric would supposedly flow from the fire into the metal. If a cold metal block and a hot block were placed in contact, the caloric would flow from one to the other, just as water would flow from high to low.

Scientists wanted to better understand the properties of caloric. Establishing "fluid" as a metaphor for "heat" prompted the investigation to move in particular directions. The analogous properties of fluids structured the approach to the problem. As the metaphor suggested that heat ought to have weight, scientists began to design experiments to measure the weight of caloric.

Presuming that the weight of caloric must be very low (it could be seen rising in the air from a fire, thus caloric must weigh less than air), scales were constructed to measure very small changes in weight. The experiments were straightforward. The weight of a cold block of iron would be weighed. The block would then be heated, then again weighed.

Many investigators detected an ever-so-microscopic increase in weight. With a great deal of satisfaction they announced that, as anticipated, caloric had weight.

Problems emerged when the results of the various experiments were compared. Different scientists running similar experiments came up with different answers as to *how much* caloric weighed.

We know now that heat is not an invisible fluid. The change in weight detected in these experiments was the result of oxidation of the metal block, in the process:

(iron) + (oxygen from the air) = (rust)

The increase in weight was the weight of oxygen removed from the atmosphere and added to the block of iron in forming iron oxide.

The metaphor of heat as a fluid actively molded three things in the experimental process:

First, it shaped the general approach to the problem: heat is a fluid, fluids have weight, therefore heat must have weight. As Kuhn (1970, p. 18) asserted: “Both fact collection and theory articulation become highly directed activities” wherein the paradigm “suggested which experiments would be worth performing.”

Second, it shaped the experimental tools. Tools were created to detect a difference in weight, under the assumption that weight was a property of heat. If the original concept was wrong, the tools and the experiment would do nothing to advance knowledge of heat, except by elimination.

Third, it bounded the interpretation of the results. The theory expected an increase in weight, and the experiment detected an increase, *quod erat demonstrandum* and the theory was considered verified. There appeared to be no need to examine alternative explanations.

Currently, scientists have a different paradigm for understanding heat. Heat is conceptualized as energy, which is manifested as increased molecular vibration. Research grants are not awarded to perform experiments on "heat as a fluid," and any papers written dealing with the results of such an experiment would not see print in any (reputable) scientific journal. These are valuable social functions. They serve as effective filtering mechanisms so that research funds are not wasted, and, in an information-dense environment, serious scientists do not waste their time reading about obsolete concepts. However, filtering by the scientific community and restricted access to publication outlets also serve as an effective defense mechanism in favor of the established paradigm. As Waldrop (1992) noted:

> Martin Luther could nail his ninety-five theses to the church door of Wittenberg to be read by one and all. But in modern academia, there are no church doors; an idea that hasn't been published in an established journal doesn't officially exist.

When a particular paradigm is accepted, it tends to have a tenacious hold. Transitioning to a new paradigm is difficult. The example provided by the history of the exploration of "heat," and other cases mentioned by Kuhn, provide a number of specific reasons why it is difficult to break away from an established paradigm:

1. The physical reason: the experimental equipment investigating the phenomenon of "heat" was not designed to detect heat as energy. The experimental design had no relationship to any different conceptualization of heat. Running the experiments did not move them closer to a better understanding of heat.

2. The intellectual reason: the existing paradigm molded the interpretation of the results. The experiment was performed to further develop an existing paradigm. The puzzle was to fit the experimental results into the existing pattern. Results that could be jammed into the framework of the existing paradigm were accepted; results that could not be so jammed were discarded as failures of experimental technique. Results of experimental "failures" are rarely circulated.

3. The social reason: many influential scientists had centered their professional lives and reputations in the old paradigm. Change would involve great personal costs. There would be an obvious reluctance to abandon the investment of years of work.

4. The institutional reason: by the very nature of refereed journals, the same scientists who have so much of their professional life invested in the established paradigm also control publication resources. Thus, it is difficult for paradigm-breaking information or ideas to see print.

Particular note should be taken of a paradigm's ability to police its boundaries—in other words, to decide what ideas to admit into consideration. There are many aspects of this policing, from control of publications, to control of the professional education of scientists in training, control of grants and research resources, and (for aspiring doctoral candidates) control of what constitutes a successful dissertation. Policing actions can actually be overt and punitive, as examples from history testify. Stanesby (1985, p. 1) relates that, in the Fifth Century, Anaxagoras was expelled from Athens for describing the sun as a red-hot stone. Similarly, the Catholic Church during the Middle Ages and Renaissance dictated acceptable thinking for both religious and secular areas. The Modern History Source Book (2002) relates that in 1633 the Catholic Church indicted Galileo for the "absurd, philosophically false, and formally heretical" belief that the earth revolved around the sun.

It is not until many scientists achieve strange results and compare their data that an anomaly is established. An anomaly is something that cannot be explained under the current paradigm. Discovery of sufficient anomalies leads to a reexamination of the paradigm. This process opens the fortress gates to the discovery of other anomalies. Anomalies that were previously discounted as failures of experimental technique or controls are now given a second look, and have the credibility to be published and to receive additional attention. Kuhn (1970, pp. 52-53) wrote: "Discovery commences with the awareness of anomaly, i.e., with the recognition that nature has somehow violated the paradigm-induced expectations that govern normal science."

Kuhn believed that scientific advances progressed through several stages:

- First, there is an established paradigm;
- Then, anomalies are detected that are not easily explained by the existing paradigm;

- The paradigm is challenged, modified, stretched to its limits, and then discarded;
- A period of confusion occurs, with many ideas competing as replacements for the paradigm; and
- Then, eventually a new paradigm emerges, and scientific work goes forward under its aegis.

A classic example of this process, according to Kuhn (1970, pp. 68-69), was the overthrow of Ptolemaic astronomy.

The Ptolemaic system, developed from about 200 B.C. to 200 A.D., postulated that the earth was the center of the universe. Everything in the sky, the sun, stars, and planets, were thought to be fixed upon concentric spheres centered on the earth. Astronomical bodies moved as these spheres revolved.

The Ptolemaic system was remarkably successful in predicting future states of the astronomical system, considering that it was based on incorrect assumptions. It's accuracy is such that it still is widely used as a first order engineering approximation, especially for the locations of the planets. However, as observation technology improved, anomalies were noted. In particular, there were times when stars or planets would undergo precession, where the direction of movement of a star would reverse direction for a time before returning to its usual movement. To explain this behavior, Ptolemaic astronomers assumed there were compounding spheres mounted on the master sphere. Astronomical bodies were on these individual smaller spheres, which rotated independent of the master sphere. This modification of the theory helped reconcile some observations, but not all. The theory was further modified to include another set of spheres, attached to the smaller sphere, attached to the largest sphere, each with their own periods of revolution. Then, another set. To "explain" the observations, the Ptolemaic system eventually required a set of 80 nested circles.

Kuhn (1972, p. 78) wrote the following passage regarding those scientists entrenched in the old way of thinking:

> By themselves they cannot and will not falsify that philosophical theory [the old paradigm], for its defenders will do what we have already seen scientists doing when confronted by an anomaly. They will devise numerous articulations and *ad hoc* modifications of their theory in order to eliminate any apparent conflict.

Eventually the defenders of the old order had so many compounded spheres rotating in different directions that the system became incredibly cumbersome. Alfonso X, in the Thirteenth Century, was prompted to declare that if God would have been so good as to consult him when creating the universe, he could have offered some good advice. In the preface to the *De Revoluntionibus,* Copernicus wrote that the astronomical tradition that he inherited had created a monstrosity. Kuhn (1970, p. 69) asserted, "[that] recognition was prerequisite to Copernicus' rejection of the Ptolemaic paradigm and his search for a new one."

A different situation exists when a new branch of the sciences is opened. Then, there is no existing established paradigm. In this case, many ideas compete for primacy. Kuhn (1972, p. 4) contends that "the developmental stages of most sciences have been characterized by continual competition between a number of distinct views of nature." Many different and often completely contradictory theories are offered, each of which have their family of supporters who work within their own worldview.

Kuhn also believed that it was *often is impossible for proponents of different paradigms to debate*, because basic terms assumed different meanings in the different paradigms, making meaningful communication difficult. This point will be revisited later.

Kuhn's Critics

When *Structure* was first published, a review in Scientific American dismissed it as "much ado about very little." This was about as embarrassingly wrong as an assessment could be. The book attracted a firestorm of comment, support, and criticism.

Masterman (1970, p. 61) complained that Kuhn was imprecise. He found that Kuhn "uses 'paradigm' in not less than twenty-one different senses." Stanesby (1985, p. 140) refers to Suppe's argument that the plausibility of Kuhn's thesis is "guaranteed by the vague and ambiguous manner in which the term is used."

That criticism caused Kuhn (1977, pp. xix-xx) to expostulate three principle ways that "paradigm" should be used:

1. As “an exemplary problem solution”—a paradigm is a model of behavior, a concrete illustration of how to do good science;

2. Associated with classical books in which such exemplary problem solutions appeared; and

3. As a global set of commitments shared by members of a scientific community; a worldview or belief system; an ideological framework.

Stanesby (1985, p. 141) quotes Kuhn’s statement that “one sense of ‘paradigm’ is global, embracing all the shared commitments of a scientific group; the other isolates a particularly important sort of commitment and is thus a subset of the first.”

Kuhn’s modifications and clarifications of his theory did not halt criticism. Stanesby (1985, p. 148) provides a summary of many of the persistent objections:

> Kuhn’s account of scientific knowledge has been the subject of wide debate and severe criticism. First, despite his attempt at clarification, there are too many theses embedded in his notion of “paradigm” for it to retain much philosophical plausibility. Second, the distinction between normal science and revolutionary science is questioned as a matter of historical accuracy. Third, the incommensurability thesis implies a non-empirical subjectivism that leads to irrationality. Fourth, if the scientist can only view the world through his disciplinary matrix, or *Weltanschuung*, science is deprived of any sense of objectivity. Finally, Kuhn’s insistence that meanings are theory-dependent rests on the mistaken assumption that theories are simply linguistic entities.

Feyerabend (1981) was one prominent scientist who totally dismissed Kuhn’s ideas. He claimed that there is no logic to science at all; he believes that scientists adhere to particular theories for subjective, or even irrational, reasons. He argues that the progress of science displays no particular pattern. For that matter, he rejects objective reality as a metaphysical mistake. Feyerabend (p. 68) once mocked Kuhn's model of scientific change as actually being a better fit for the processes inherent in organized crime.

Additional criticism focused on any supposition that the paradigm shift theory might have universal application. The use of imprecise terminology made it impossible to know what exactly was shifting, so it was difficult to compare the theory with the historical record.

In addition, there was indignation regarding his statements that paradigms achieve prominence in the scientific community not on their merits, but because of social forces within the power structure of the existing scientific elite.

Using Kuhn's Research

In spite of these objections, some of which are valid, Kuhn's idea of paradigm shifts and his exposition of the characteristics associated with such shifts is still useful. Many of the criticisms take on a different light when the subject is viewed from a chaos and complexity worldview rather than a deterministic causality viewpoint.

For example, take the objection that not all changes in paradigms take the characteristics listed by Kuhn. If human behavior was subject to deterministic causality, it would be valid to expect that a certain type of event would have common repeating characteristics. However, if history was subject to deterministic chaos, then we would expect to see a wide variety of behavior to result. Characteristics that we might associate with a paradigm shift would not be expected to repeat in the same way each time. Sensitivity to initial conditions would indicate that there should be a wide range of expected behavior. However, since chaos is subject to a higher order of behavioral limitations, we would expect to see the same types of events repeated. For example, if the entire universe of events associated with a paradigm shift are events A, B, C, and D, a chaos and complexity worldview would expect that behavior would be limited to those four characteristics, but not all occurrences would show them all: one might show A and B, another B, C, and D, and one might show only D.

Cataloging the entire universe of events associated with a type of historical occurrence would be expected to be a daunting tasking, considering the number of variables that can impact on human behavior. However, certainly it should be possible to extract the major events and boundaries of any pattern; and, I believe, that is what Kuhn has accomplished.

If we are in the course of a paradigm shift in the social sciences, with anomalies being recognized in the ability of the Scientific Method to develop social science knowledge, then we would expect to see similar behaviors or variations on the behaviors observed in previous paradigm shifts. However, since this occurrence has its own set of initial conditions, we should also expect to see differences, in magnitude of the behaviors, rate of occurrences, and other characteristics. Since we are looking at a paradigm shift that encompasses all the fields in the social sciences, we should expect to see some, all, or none of the individual paradigm shift behaviors in each of the individual fields. The expectation of variability in responses essentially opens wider the boundaries of expected behavior. It eliminates any causality expectations that dictate a judgement that if one particular behavior is not observed, therefore the phenomenon is not present. Yes, the phenomenon may be present, only the expected behaviors may be muted, absent, or manifesting themselves in a different manner.

Otherwise, the employment of Kuhn's ideas in this paper does not depend heavily upon the disputed areas in Kuhn's philosophy of change, as opposed to his observations of change. There are historical examples that fit Kuhn's model of paradigm competition and replacement. There may be, in some cases, a progression from paradigm to paradigm (if not always as orderly as implied by Kuhn's model). It is still possible to look for these characteristic behaviors and postulate a connection between the behaviors and the process.

The following ideas deserve consideration as possible *in some cases*:

- The existence of paradigms;
- Established paradigms have mechanisms to enforce conformity;
- Some paradigms are ineffective in solving some types of problems;
- Periods of transitions between paradigms can show certain characteristics;

- Some practitioners will not abandon the old paradigm, in spite of revealed anomalies which the paradigm cannot explain; and
- Periods between or before the establishment of a dominant paradigm are characterized by a diversity of competing theories. Some of these theories can be quite strange, but they are not suppressed, because there is no generally accepted paradigm serving to police the field and banish them to scientific Coventry.

These ideas, I believe, while not universally applicable, have been demonstrated in some cases. If one believes in patterns in history, then it would be logical to contend that they hold the potential to be repeated.

CHAPTER IX

THE SCIENTIFIC METHOD AND CHAOTIC SYSTEMS

The Scientific Method

At this point it is appropriate to see exactly what constitutes the Scientific Method paradigm.

The following passage from Wolf (2002) explains the Scientific Method and some of the rules that make up its use:

> The scientific method is the process by which scientists, collectively and over time, endeavor to construct an accurate (that is, reliable, consistent and non-arbitrary) representation of the world. Recognizing that personal and cultural beliefs influence both our perceptions and our interpretations of natural phenomena, we aim through the use of standard procedures and criteria to minimize those influences when developing a theory. As a famous scientist once said, "Smart people (like smart lawyers) can come up with very good explanations for mistaken points of view." In summary, the scientific method attempts to minimize the influence of bias or prejudice in the experimenter when testing an hypothesis or a theory.
>
> The scientific method has four steps:
>
> 1. Observation and description of a phenomenon or group of phenomena.
> 2. Formulation of an hypothesis to explain the phenomena. In physics, the hypothesis often takes the form of a causal mechanism or a mathematical relation.
> 3. Use of the hypothesis to predict the existence of other phenomena, or to predict quantitatively the results of new observations.

4. Performance of experimental tests of the predictions by several independent experimenters and properly performed experiments.

If the experiments bear out the hypothesis it may come to be regarded as a theory or law of nature (more on the concepts of hypothesis, model, theory and law below). If the experiments do not bear out the hypothesis, it must be rejected or modified. What is key in the description of the scientific method just given is the predictive power (the ability to get more out of the theory than you put in; see Barrow, 1991) of the hypothesis or theory, as tested by experiment. It is often said in science that theories can never be proved, only disproved. There is always the possibility that a new observation or a new experiment will conflict with a long-standing theory.

Three important concepts are: (1) the idea of a *theory,* (2) the requirement for *repeatability*, (3) the concept of *sensitivity*, and (4) the criteria for *disproving the hypothesis*.

Theory

Bowditch and Buono (1994, p. 43) summarize the idea of a theory in saying: "Basically, a theory is an ordered relationship of data-supported hypotheses."

Reynolds (1971, pp. 10-11), in *A Primer in Theory Construction*, gives three classifications of theory:

First, theory can be the conception of scientific knowledge as a set of well-supported empirical generalizations, or "laws." This is called the "set of laws" form of theory.

Second, theory can be an interrelated set of definitions, axioms, and propositions that are derived from the axioms. This conception is borrowed from mathematical conceptions of theory and is called the "axiomatic" form of theory.

Third, if a sense of understanding is provided when a description of a causal process is presented, then theory can be a set of descriptions of causal processes, called the "causal process" form of theory.

Reynolds (1971) suggests that the causal process form is a separate classification of theory. In fact, all three types have embedded *causality models.* A "law of behavior" is certainly a causality model, as are a set of interrelated definitions and axioms. The difference is that the causality model of the first or second type of theory claims either general application or axiomatic antecedents, and is generally developed deductively; in the third type the causality model arises inductively from the data, is more specific to a particular situation, and sometimes is not quantified.

Pruitt and Snyder (1969, pp. 1-2) describe causality models in the following paragraph, wherein they describe "good theory":

> All theory is composed of symbols or terms, the most important of which refer to *variables*, i.e., abstracted dimensions or properties of the real world. Good theory consists of a set of interrelated *theoretical propositions* concerning the relationship between two or more variables. . . . Most theoretical propositions describe a causal relationship (directional influence) between one or more *antecedent variables* and a *consequent variable*. Antecedent variables come first in the causal chain described in a proposition. . . . Antecedent variables may alternately be called *independent variables, factors, determinants,* or *causes.* Consequent variables come last in the causal chain. . . . Consequent variables are sometimes called *dependent variables* or *effects.* Good theory, in addition, usually says something about *intervening variables*, i.e., variables that occupy an intermediate position in the causal chain between antecedent and consequent variables. . . . Intervening variables represent the *process* or

mechanisms underlying the relationship between antecedent and consequent variables.

In a previous publication, I summarized this process (Zimm, 1999, p. 230). A good causal model identifies:

- a set of interrelated theoretical propositions
  (if . . . then statements)
- describing a causal relationship
  (directional influence)
- between actions and/or conditions
  (antecedent variables)
- and outcomes
  (consequent variables).

A causality model is, when reduced to its simplest form, a cause and effect relationship that can be distilled down to "if-then" statements. The accepted form is, "*if* X, *then* Y." For example, a government agency might express a causality relationship in the following manner: "*If* prenatal medical care is provided to low income expectant mothers, *then* the incidence rate of low birth weight deliveries will be reduced." And, from the example of the physical sciences, these statements are intended to be predictive. To recall a famous quotation from Richard Feynman: "Physicists like to think that all you have to do is say, these are the conditions, now what happens next?"

Repeatability

One characteristic of most causality models—or of classical physical science theories in general—is that they are assumed to be deterministic. Given a definition of initial conditions and the hypothesized causality relationship, the scientific method states that—if the theory is correct—the system will end up at a given resulting state. When elementary school children are taught the scientific method, they are told that proper scientific experiments are repeatable: run the experiment twice, and the same results will emerge. In making the point with a more explicit example, Simon (1997, p. xi) wrote: "An experiment in chemistry derives its validity—its scientific authority—from its reproducibility."

Of course, to be reproducible the experiment must be published, with sufficient specificity to allow an experiment to be reproduced by other scientists. The combination of public access and reproducibility has been key to the current scientific paradigm's dominance. Brock (1990, p. 251) observed:

> Scientific standards demand that two scientists using the same information and the same technique must come up with the same results. Furthermore, the method that they use must be precisely definable in a manner that can be communicated to another scientist.

His context was in discussing the predictive power of certain economic charting techniques; "the same information" he was referring to was the same data and conditions being used in both analyses.

Casti and Karlqvist (1991, pp. vii-viii) point out how these interlocking characteristics undercut competing paradigms while assigning credibility to acceptable theory:

> A neutral skeptic might well object, saying that the other reality-generating schemes . . . are also based upon rules. For example, many brands of mysticism involve various sorts of meditation exercises, exercises which their adherents claim will result in a

> kind of self-realization and enlightenment. It is then claimed that the enlightenment, in turn, enables one to at least explain, if not predict, the unfolding of everyday events. And just about everyone is familiar with Sunday-supplement seers, stock market gurus, and other types of visionaries who predict events of the coming year on the basis of crystal-ball gazings, tarot card readings, the interpretations of astrological configurations, the outcome of the Super Bowl, divine inspiration, and/or other procedures that might charitably be thought of as being based on the following of a set of rules. So in what way *exactly* do these rule-following schemes part company from what is customarily thought of as being "scientific" procedure?
>
> Basically, there are two properties tending to distinguish scientific rule-based schemes for prediction and explanation from their competitors. The first is that scientific schemes are *explicit,* i.e., the rules and the way they are to be applied are spelled out in sufficient clarity and detail that they can be used by anyone. In short, it doesn't require any special insight or inspiration to make use of scientific laws or rules. . . . The second distinguishing characteristic of scientific rules is that they are *public.* Unlike many religions and other belief systems, there are no private truths in science.

"Explicit" and "public" are the two elements of repeatability. An experiment, based on theory, should be able to be duplicated (verified) by any other careful scientist.

Sensitivity

One of the foundational concepts in the classical scientific paradigm (or the Scientific Method paradigm—those terms will be used interchangeably in this paper), that goes hand-in-hand with the assumed repeatability of experiments, is the assumption that very small differences in initial conditions translate into negligible differences. As stated by Kellert (1993, p. xi), there is a methodological assumption in science that a small amount of vagueness in measurements will lead only to a small amount of vagueness in the predicted behavior.

Winfree (quoted in Gleick, 1987, p. 15) expressed the concept in the following way:

> The basic idea of Western science is that you don't have to take into account the falling of a leaf on some planet in another galaxy when you're trying to account for the motion of a billiard ball on a pool table on earth. Very small influences can be neglected. There's a convergence in the way things work, and arbitrarily small influences don't blow up to have arbitrarily large effects.

Given this idea, the resulting assumption is that smaller errors in measurement yield better predictions. So, as late as 1948, a scientist such as Churchman (1948, p. 173) could say: "We feel that it requires no defense to say that one of the purposes of scientific activity, *taken collectively,* is to reduce error to zero, i.e., to become absolutely precise."

This concept is the necessary corollary to the demand for repeatability. Since you do not have to have all the initial conditions precisely the same—meaning, to the tenth decimal place or better—some other scientist can set up the same experiment with his slightly-different equipment and expect to duplicate the results. If this property was absent and the experiments highly sensitive to initial conditions, then it would be impossible to duplicate results. The errors due to the microscopic differences between experimental equipment and measurement instruments would overwhelm any causal behavior in the system. The system would look like it was random or chaotic.

For most cases, however, the process of measurement does not materially affect the state of the things under observation, so physical systems are not generally hypersensitive to initial conditions.

Disproving the Hypothesis

As Wolf (2002) stated in the quotation that began this chapter, the experiments are expected to "bear out the hypothesis," with the possibility that "a new observation or new experiment will conflict with a long standing theory." The criteria embedded in the Scientific Method is that of negation. A hypothesis is made, and a prediction made from that hypothesis, and an experiment run to test that prediction. If the predicted result is not attained, then the hypothesis is considered disproved. In effect, any counterexample is sufficient to negate a hypothesis.

The Scientific Method is Not Designed for Chaotic Systems

At this point, it is readily apparent that the Scientific Method has a number of difficulties dealing with the special characteristics of systems that behave in a chaotic manner. The Scientific Method can lead to inappropriate conclusions. Clayton Thomas (2002, p. 53) quoted the following from *Scientific American*:

> The existence of chaos affects the scientific method itself. The classic approach to verifying a theory is to make predictions and test them against experimental data. If the phenomena are chaotic, however, long-term predictions are intrinsically impossible. This has to be taken into account in judging the merits of the theory. The process of verifying a theory becomes a much more delicate operation, relying in statistical and geometrical properties rather than on detailed prediction.

A few examples of the problems the Scientific Method has with chaotic systems demonstrate the truth of this statement.

Repeatability: The Scientific Method assumes that experiments will have consistent results if they are repeated. However, the property of sensitivity to initial conditions precludes that. A scientist repeating an experiment or study that was performed by another scientists will most likely obtain contradictory (or, at least, non-confirming) results. According to the standards of the scientific method, the contradictory results disproves the hypothesis. Consequently, a lot of valid scientific research may have been thrown out because the property of sensitivity to initial conditions was not recognized.

Disproving the Hypothesis: In the Scientific Method, a hypothesis is proposed, and experiments or studies conducted to confirm or disprove the hypothesis. A contradictory result is considered enough to disprove the hypothesis. So, in a system where a researcher proposes a antecedent variable, and hypothesizes that a consequent variable will linearly increase, a drop in the value of the consequent variable would disprove the hypothesis.

The hypothesis might actually be correct but not in a way that the scientist would be trained to recognize: the *attractor* might be moved, but not all the individual results. There might still be overlap between the field of possibilities under the first set of conditions and the subsequent set of possibilities. So, if in the first sample, where the consequent variable might be chaotically constrained to a range of values between 100 and 200 with the attractor at 150, in a single sample (experiment or study), a value of 170 might be obtained. In the second sample of the system, where the attractor has moved to 200 and the system is now constrained to values between 150 and 250 (an increase in the limits), a value of 155 might be obtained. So, even though the limits of the system responded to the change as hypothesized, the chaotic behavior of the system coupled with the small sample size led the researcher to an incorrect conclusion.

Misleading Interpretation of Results: To a scientist trained to expect non-chaotic responses in systems, the results of studies of a chaotic system can lead to incorrect conclusions. For example, Figures 20 and 21 showed the results of situations where the effect of layoff survivor sickness was incorporated into the model. In Figure 21, organization productivity scaled to drop by 10% when the organization downsized; one organization failed at about cycle 550. In Figure 21, organization productivity was scaled to drop by 8% when the organization downsized. This smaller drop in productivity might be associated with efforts by management to mitigate the effects of the downsizing. Yet, this organization failed much sooner, at about cycle 45. If this was an experiment, with the hypothesis that downsizing mitigation efforts by management are good for an organization, a scientist trained in the Scientific Method would conclude that the management's mitigation efforts were a failure, when in truth the earlier failure of the organization might have simply been an artifact of the chaotic behavior of the system.

Transitory Behavior: In the model runs of the Organization Logistic equation, plots were shown where organizations had different behavior based on their different values for the variable R. The behavior of the system could start out chaotic, but then moderate itself to more regular, stable behavior. This opens the possibility that human systems might sequence from chaotic to stable and back to chaotic behavior as certain environmental conditions change. Such a possibility would be very disruptive to anyone schooled to expect regular, non-chaotic behavior.

Consider an system of organizations where the Organization Logistic equation would apply. All the organizations initially have variables which result in behavior in the stable region. A scientist records information on the behavior of the system, and might very well find regular behavior conducive to developing any number of causality models. Hypotheses are made, and appear to be proven by the data at hand.

The scientist's work attracts the attention of other investigators, who attempt to confirm the work. However, for whatever reason, perhaps one of the organizations has a change in leadership or decides to expand more aggressively and use more of its income to grow, and one of the organization's variables shifts into the chaotic region. Suddenly the entire system has a burst of chaotic behavior which, as shown by the plots presented in Chapter 6, take some time to stabilize. However, this burst of chaos is recorded by the new investigators. They cannot see in this new data any justification for the original hypotheses. The original scientist's work must have been flawed, his experimental technique sloppy, his data collection inadequate. The new information is published, the previous work is, by implication, discredited, fingers are pointed. And, another attempt to apply the Scientific Method to develop social science knowledge dissolves into the confusions of another failed attempt. Reputations are shattered.

Consider what happens when this occurs again, and again, and again. Following the successful example of the physical science community, the Scientific Method is used in research, and continually, disproves every attempt at a hypothesis. The social science community "knows" that the Scientific Method is good because they see the evidence of its success daily as new inventions are marketed, engineering projects built, and new physical science breakthroughs announced. So, what is the matter?

The matter, they conclude, is not in the method but in the material. Social science itself must be different. Perhaps there is no such thing as "truth" in the social sciences. Perhaps everything is relative. Perhaps we can only strive to "understand" a single occurrence, and cannot derive conclusions that are applicable to human behavior as a whole. Perhaps we can only argue, and one argument is as good as another. Since if one argument is as good as another, one standard is as good as another. Perhaps there are no standards. If there are no universal standards, each person must make their own standards, and who is to say that their standards are not as good as yours? Everything must be subject to interpretation. Nothing is sure. There is no such thing as "truth," as "good," as "right," as "accurate," as "correct."

That, in a nutshell, is a possible explanation for the path that has occurred in the social sciences. The contradictory messages from the simultaneous success of the Scientific Method (in physical science applications) and failure of the Scientific Method (in social science applications) has led many social scientists to question the wrong things. Social scientists have accepted the Scientific Method and questioned everything else. Instead, the Scientific Method and mind set should be questioned, and we may find that there actually is truth (only in a different form than what we expected), and more of that truth in what many have rejected than in what many have been driven to accept.

If social systems are indeed subject to deterministic chaos, then it should have significant consequences. These consequences should lead to identifiable and observable behaviors in the social science communities as a whole. These consequences should themselves be manifested in a chaotic manner, i.e., present in some branches of the social sciences, not in others, stronger here than there.

The next step would be to extract a list of those behaviors.

Characteristics of Paradigm Shifts

It is possible to extract from Kuhn's work a list of characteristic behaviors that have accompanies paradigm shifts in the past. With this list, it will be possible to look into the overall state of the social sciences today and see if behaviors like them are present today. Note that these are generalized behaviors, subject to some interpretation in their specific applications. Also, from a viewpoint shaped by the assumption that human behavior will be manifested in complex and chaotic ways, there should be different combinations of these behaviors in each of the individual fields of the social sciences. It would not be expected that all of these behaviors would be present in all the fields in a uniform way.

There is some danger to such a search. Since variance in the characteristic behaviors is expected, there is the danger that the definition of these behaviors then becomes so broad that almost anything will be able to fit the definition and be counted as a "characteristic behavior." There does not appear to be an easy solution to this dilemma. The answer, most likely, resides in the scope of the perspective. If the overall hypothesis is correct, by maintaining a broad perspective all the individual behaviors should fall into an overall pattern. That pattern, unfortunately, cannot be objectively determined. It will be a subjective exercise, for each individual reader to see the evidence and determine if it is convincing. The objective of this paper will be achieved if there are those who see at least the possibility that the hypothesis is correct, enough so that the hypothesis deserves continued investigation.

Given those caveats, the following types of behavior are expected in a field undergoing a paradigm shift in methodology:

1. Emergence of many anomalies which, under the old paradigm, are not explained.

2. Complication of theories under the old paradigm, in attempts to explain the anomalies.

3. Emergence of competing methods of conducting "normal science."

4. Emergence of competing worldviews or *Weltanschauung*.

5. Failure of the established paradigm to be able to police its boundaries, meaning that publications and research emerges that is in direct competition with the established paradigm. This can be manifested by emergence of new competing paradigms as well as expansion of other paradigms that had formerly been restricted to a limited membership in the scientific community.

6. Argument over the standards of research. A change in standards of research, as the established paradigm cannot enforce the previous standards. Publications appear that are of doubtful quality, as measured by the standards of the old paradigm.

7. Combining the effects of #3 and #4, alternate paradigms emerge that would have been considered, under the old paradigm, to be ludicrous. However, the existing paradigm is no longer strong enough to suppress the emergence and growth of such ideas.

8. Difficulty of communicating between the alternate competing paradigms due to the changing meanings of words.

9. Changing general approach to problems, and a changing standard and/or changing interpretation of experimental results or collected data.

10. Changing standards of gatekeeping in professional publications.

A connection between paradigm shifts and emotions.

There is another type of behavior that has, to this point, not been mentioned. The failure of the old paradigm can cause, to many scientists, acute mental and professional discomfort. Kuhn (1972, pp. 68, 83-84) indicates that: "As one might expect, that insecurity is generated by the persistent failure of the puzzles of normal science to come out as they should. Failure of existing rules is the prelude to a search for new ones."

Kuhn uses quotations from respected scientists to serve as examples of these reactions. Einstein, confronting one such crisis of understanding, wrote: "It was as if the ground had been pulled out from under one, with no firm foundation to be seen anywhere, upon which one could have built." Pauli, in the time before Heisenberg's work in matrix mechanics transitioned into quantum theory, wrote: "At the moment physics is again terribly confused. In any case, it is too difficult for me, and I wish I had been a movie comedian or something of the sort and I had never heard of physics." His later words were a significant contrast: "Heisenberg's type of mechanics has again given me hope and joy in life. To be sure it does not supply the solution to the riddle, but I believe it is again possible to march forward."

This leads us to:

11. Evidence of acute mental and professional discomfort among some of the scientists over issues relating to the paradigm shift.

This last characteristic behavior provides a clue as to the source of many of the observed behaviors. Is there a connection between paradigm shift behavior and the emotions? Is there a connection between paradigm shift behavior and human cognitive behavior? The next chapter will explore this question, and through this connection add additional behaviors that can be associated with paradigm shifts.

CHAPTER X

COGNITIVE SCIENCE AND PARADIGM SHIFTS

The Paradigm as a Mental Model

One way that a paradigm can be discussed—another connection, of a different sort—is in terms of mental models. If a paradigm is a model of how to perform "good science," and if it is internalized as a mental model, then it would appear that there should be things in common between human cognitive behavior and how humans behave with respect to paradigms. And, since science is a creative process, there should also be things in common between the conduct of science and human creative mental processes. Indeed, as will be shown, there is convincing evidence of a connection between human behavior in the course of a paradigm shift and human behavior when any other mental model or creative process is disrupted.

The first step is to understand better how humans survive when surrounded with a surfeit of information, and how they make sense out of their environment.

Cognitive Processes

A mental model is a fundamental mechanism by which human beings approach the problem of understanding. It is a universal activity designed to extract meaning and comprehension from masses of data. It is a vital survival mechanism, which is important to remember when understanding how it is constructed and how it is used.

Humans build models that provide cause-and-effect relationships so that, in the future, when a "cause" is present they have a short cut to understanding the "effect" without having to go through the laborious process of rebuilding their understanding of the event.

For example, de Bono (1993, p. 53) tells us:

> Younger children continually ask, "Why?" They are not looking for a causal explanation in the adult sense of science. They want "connectors." They are looking for ways of filling in gaps and connecting up experience so that they get a more stable whole. . . . If there are no parents to provide the connectors . . . then the children have to create their own explanations and myths. The myths formed by adults who have no one to ask are of exactly the same nature. The history of science is full of connecting myths: "malaria" means the bad air from the swamp that gave people malaria. For a long time there was a theory of "Phlogiston" to explain why things burned.

Retrieving and applying the correct mental model to a situation is a major problem for the brain. When, for example, you hear something rattle, it is to the utmost importance for your survival (and, by extension, for the survival of the species) that you distinguish between the sound of a baby's toy rattle and that of a rattlesnake, and, moreover, to instantly retrieve the appropriate predictive mental model to guide your response. The connection between certain things and danger has to be retrieved accurately and efficiently.

The difficulty is that there is so much data available. Consider a decision maker driving a car through her neighborhood and processing a decision on where to park. Only a fraction of the available data is used. A distinctive mailbox and the right house color may be enough to trigger recognition of "my home." If the front lawn is noticed at all, the thousands of individual blades of grass are not counted and measured and classified but rather the general impression established that it seems a bit too long and it's time to fire up the lawnmower.

The power of a mental model to control and order this flow of information is prodigious. Consider for a moment an example used by Simon (1997, pp. 226-227):

> Saturation with information is no new thing. The movements of the stars, visible to us throughout the tens of thousands of years of our history, contain all the information that is needed for Newton's laws of motion or the law of gravitation. The information was there all along. What was lacking, until a few hundred years ago, was the basis for selecting the tiny fraction of it that could be used to establish powerful generalizations. . . . The example I have chosen is not an isolated one. In the scientific endeavor, "knowing" has always meant "knowing parsimoniously." The information that nature presents to us is unimaginably redundant. When we find the right way to summarize and characterize that information—when we find the pattern hidden in it—its vast bulk compresses into succinct laws, each one enormously informative.

Only a tiny fraction of the available data is generally needed to process a decision.

Mental models not only work toward understanding the environment—creating predictive mental models—but also down to such fundamental processes as recognizing the environment. Returning to the earlier example, humans store a template of "my house" in memory. Just a few clues lead the mind to that template, and the rest is filled in by recall. This process is confirmed by simple experiments where it is observed that people catch spelling errors better in the first half of a word than in the second half—the mind often recognizes the word from context and from clues in the first half of the word, matches it to the template of that word, and fills in the rest from memory. More intelligent people seem to do this more often, a point that reconciles me to my inadequacies as a proofreader.

Pinker (1997, pp. 28-29) illustrates the complexity of employing stored mental models, using the sense of sight as an example.

> Ordinary optics is the branch of physics that allows one to predict how an object with a certain shape, material, and illumination projects the mosaic of colors we call the retinal image. Optics is a well-understood subject, put to use in drawing, photography, television engineering, and more recently computer graphics and virtual reality. But the brain must solve the opposite problem. The input is the retinal image, and the output is a specification of the objects in the world and what they are made of—this is, what we know we are seeing. And there's the rub. Inverse optics is what the engineers call an "ill-posed problem." It literally has no solution. Just as it is easy to multiply some numbers and announce the product but impossible to take a product and announce the numbers that were multiplied to get it, optics is easy but inverse optics impossible. Yet your brain does it every time you open the refrigerator and pull out a jar. How can this be? The answer is that the brain supplies the missing information, information about the world we evolved in and how it reflects light. If the visual brain "assumes" that it is living in a certain kind of world—an evenly lit world made mostly of rigid parts with smooth, uniformly colored surfaces—it can make good guesses about what is out there. . . . If the surface-perception module assumes that the illumination is even, it could even be seduced into hallucinating objects that aren't there. Could that really happen? It happens every day. We call these hallucinations

> slide shows and movies and television. . . . When we watch TV, we stare at a shimmering piece of glass, but our surface-perception module tells the rest of our brain that we are seeing real people and places.

Strauch (1989, p. 189) summarizes the interaction between vision and internal models:

> It now seems clear that vision is not the direct, objective sense it appears to be. We see what we know how to see, and what we expect to see. From our visual experience we build internal models of the world and the things it contains, and we use those models to filter and interpret the stream of optical images which flow across our retinas. . . . Our other senses operate in much the same way.

Models and mental templates are so important in organizing and storing experiences that we actually have difficulty recognizing, or even actually seeing, things that are not consistent with our existing models. Strauch (1989, p. 112) states, "We can fail to see what we do not expect; and we may see something which is not there at all." An interesting experiment that illustrates this point has to do with the blind spot that exists in the eye where the optic nerve penetrates the retina. At that spot there are no vision cells to detect light. When people are shown patterns, like that of a brick wall, with a hole exactly corresponding to the blind spot in the eye, the brain will automatically extend the pattern of bricks to cover the hole.

While the brain can fill in data to match a mental model or template, it can also reject information that is not in accordance with that mental model. Middlebrook (1972) provides a tragic example of this taken from the Battle of the Somme during the First World War. The British has arranged for a tremendous artillery barrage to precede a major attack on the German trench lines. They believed—had an established mental model—that the "artillery takes the ground, and the infantry occupies it." They expected nothing except dazed remnants of the enemy to survive the bombardment. They were so confident that they told their infantry that they were not to run through no-man's land, but walk.

When the attack began, the British headquarters received a number of reports over the first twelve hours. Some of them reported success. A large number reported horrific casualties and entire British units wiped out by German machine guns.

After the battle, the British attempted to understand their failure. They questioned the headquarters staff about their reaction to the reports of high casualties. The headquarters staff stated that no such reports were received in the first twenty-four hours of the battle. When shown the reports, they refused to believe that they had been sent to headquarters. When that was proven, they refused to believe that they themselves had seen the reports.

In this case, the officers of the headquarters staff were so confident of success that they mentally screened off all reports that were not in accordance with their established mental models of how the battle should proceed.

Buffet (2001) spoke of one scientist's recognition of the mind's resistence to contradictory evidence:

> Charles Darwin used to say that whenever he ran into something that contradicted a conclusion he cherished, he was obliged to

> write the new finding down within 30 minutes. Otherwise his mind would work to reject the discordant information, much as the body rejects transplants. Man's natural inclination is to cling to his beliefs, particularly if they are reinforced by recent experience.

These things are now generally well known, as reflected in popular management thinking. For example, Senge (1990, p. 112), in *The Fifth Discipline,* makes Mental Models, and "understanding the deeply ingrained assumptions, generalizations . . . that influence how we understand the world and how we take action" the third of his five disciplines.

The point to particularly note is that the models in the mind can actually guide the brain into believing certain things. These things may not correspond to actual reality (e.g., watching a flat TV screen and believe that you are observing three dimensional objects).

As related by Pinker (1997, p. 14), the problem is more complex than just recognition. Mental models must also be predictive, not only of the direct effects of actions, but also of side effects. He refers to a thought experiment conducted by the philosopher Daniel Dennet, who

> . . . asks us to imagine a robot designed to fetch a spare battery from a room that also contained a time bomb. Version 1 saw that the battery was on a wagon and that if it pulled the wagon out of the room, the battery would come with it. Unfortunately, the bomb was also on the wagon, and the robot failed to deduce that pulling the wagon out brought the bomb out, too. Version 2 was programmed to consider all the side effects of its actions. It had just finished computing that pulling the wagon would not change the color of the room's walls and was proving that the wheels would turn more revolutions than there are wheels on the wagon, when the bomb went off. Version 3 was programmed to

> distinguish between relevant implications and putting all the relevant ones on a list of facts to consider and all the irrelevant ones on a list of facts to ignore, as the bomb ticked away. An intelligent being has to deduce the implications of what it knows, but only the relevant implications.

Obviously, like Phlogiston and mal-aria, not all mental models are accurate. The following is a story of a situation encountered by a U.S. naval officer during the Second World War, taken from Bryant (1954, pp. 53-54). It provides an example of how people can generate incorrect internal models of thought.

> Mac Bridges staggered along under the onerous title of Officer in Charge, Department of Commerce and Industry, Naval Military Government. Mac loves his job and Guam and the Guamanians, and stories about them bubbled out of him. My favorite concerned the old lady from Yido. The accuracy of our bombing in the "softening up" period before the invasion had impressed her tremendously—so much so that she developed a theory that the bombs were attracted by the scent of taro root. Neighbors, less naive, tried to enlighten her, but the old lady stood firm. "You'll see" she said stoutly. Presently a party of [Japanese soldiers] took refuge in a cave behind her house. This was her chance. As soon as night fell, she crept out and smeared the cave mouth with taro, then went home and waited. First thing next morning, a direct hit buried the [Japanese soldiers], "and now," Mac said, "Nimitz, MacArthur, and Einstein together couldn't convince her that her theory is wrong."

All of the above considerations underscore the importance of the accuracy of the mental model. If the model is not accurate, accurate data might actually be ignored or transformed into something better aligned with the established mental model. The same thing happens when paradigms serve as the mental model. This provides a hint as to why the classical Scientific Method model is so persistent in the social sciences, and has resisted the flood of anomalies regarding its employment:

many social scientists simply do not perceive data that are not in accordance with the model.

In mental models, there is ample evidence, through research into such things as Expert Systems, that humans form a set of causal connections between a limited set of "key indicators" and conclusions. The broad mass of data is generally ignored in favor of concentrating on one or two key elements, often subconsciously. This is an important human survival characteristic: when under stress, concentrate only on what is most important.

There is a large amount of research on decision making that underscores this conclusion. According to a number of researchers (Beach & Mitchell, 1978; Newell & Simon, 1972; Payne, 1976; Fischhoff, Slovic, & Lichtenstein., 1978; Kahneman & Tversky, 1979; Yates, Jagacinski, & Faber, 1978), human performance is constrained by cognitive limitations imposed by the human information processing capabilities, the decision environment, the structure of the decision task, and the kinds and amounts of information available. A growing literature supports the idea that people adapt to these constraints by developing heuristics that simplify complex decision processes. They will "satisfice," limit the amount of information they seek, and sometimes ignore potentially useful information.

The process generally works; sometimes it can fail. For example, in the tragic engagement where the *USS Vincennes* shot down Iran Air Flight 655, according to Rogers, Rogers and Gregston (1992, pp. 15-16), in the last two and a half minutes the captain concentrated on the key indicators of the aircraft's course, speed, range to target, and altitude trend as displayed on the combat information center consoles, along with radio broadcasts. That is a lot of information to receive and process when under stress and subject to time constraints; but it is nowhere near the total mass of data available from the Aegis combat system. If more data had been considered—the *right* data, specifically that from the fire control radar which showed that the aircraft was gaining in altitude and was higher than had been reported to the Commanding Officer verbally—the decision to fire might not have been made. The sensitivity to circumstances of this decision making process is illustrated by the fact that there was another ship in the area, and that ship's commanding officer, based on nearly the same data, simultaneously came to the conclusion not to fire at the aircraft. The only differences were the order in which the data were presented and the personal background—the internal mental models—of the decision makers.

Cognitive scientists have discovered that humans subconsciously employ causality models in most of their thinking. In decision making, humans employ these mental models to predict anticipated outcomes of various courses of action, and make decisions in favor of those that yield the most favorable results. This process is a universal human characteristic. The differences in thinking between various nationalities, races or ethnic groups are contained not in the process but in the details of the mental models and in the assessment of the relative worth of the various predicted outcomes. This point—the universal human process of problem solving—will be important later when we look at the consequences of such behavior.

It appears that the mind uses models and templates as a survival technique to get rapid, short term answers. As part of that strategy, Mintzberg, Raisinghani and Theoret (1976, pp. 246-275) have documented that people will search for a ready-made solution before they design a new solution. According to Abelson and Levi (1985, pp. 231-309), people favor their initial set of beliefs. These beliefs may serve as psychological anchors for other alternatives, perhaps because of the cognitive effort required to generate them in the first place. Klayman and Ha (1987, pp. 211-228) have shown that people tend to search primarily for evidence that is most likely to support their current beliefs. Schustack and Sternberg (1981, pp. 101-120) indicate that people tend to discredit or reinterpret information that contradicts their current beliefs. These mental information-processing tendencies underscore the importance of the mental model/paradigm, as the paradigm will thus serve as a major guide, constraint, and boundary for the creative scientific process.

For more difficult problems, especially those requiring an element of creativity, it appears that the mind begins to try more and more combinations within the established model—matching more and more templates—in order to get a solution. Most of this work is actually accomplished subconsciously. In the subconscious, a "randomizer" continually comes up with possible solutions which are compared to established models and weighted with a value that corresponds to the goodness of the match with other good models, to determine if the proposed solution deserves further consideration. If it does, it is kicked up to the conscious level, along with a random number of other possibilities—"wild ideas"—for further consideration.

Tipler (1994, pp.199-200) summarizes the current ideas on how this creative process works:

> As has been emphasized by Hofstadter and Dennett, the various levels of implementation interact with each other in a human being. The conscious level is always issuing orders to the lower levels: the randomizer, for example, is constantly being ordered to change the probability weights in the decision matrix. Consider an important decision, one which the agent ponders over for some time. . . . The consciousness level send down orders to the lower levels to bring to consciousness any information stored in memory. . . . But the randomizer is also active, connecting—at random—various memory traces on previously unrelated subjects, and sending these connections to the higher levels, which act in a deterministic fashion. Almost all of these connections are nonsense, completely useless, and are rejected without being sent up to the consciousness level. But a very few are considered by the deterministic programs to be promising and sent up to the consciousness level for further consideration. While this is going on, a deterministic decision is made that more information from outside would be useful. Again, the order is passed down for suggestions on where to look for more information. Again, most of the suggestions are made by deterministic algorithms—information should be looked for in the places where one previously obtained information on the subject—but again the randomizer makes some unlikely suggestions. Probability weights are attached to these ridiculous suggestions by the deterministic algorithms, and sent back down to the randomizer, which selects some according to the probability weights. Collectively, all the resulting proposals, including those few selected by the randomizer, are sent up to the consciousness level, which acts on all of them. The information collected is worked on by the deterministic algorithms, the conclusions are sent up to

> the consciousness level, which deterministically with the help of these algorithms selects the final probability weights to give the various possible actions. If the probability weights are nonzero at more than one possibility, these weights are sent down to the randomizer, which picks one (at random) according to the probability weights selected in the end by the higher levels. This pick is sent to the consciousness level and acted upon.

It is possible to see this creative process underway in the stories told about various scientific creative efforts. For example, *L'Intermediaire des Mathematiciens* in 1905 investigated the question of mathematical discovery. The greatest mathematician of the time, Henri Poincaré, provided his comments on the process of discovery in a lecture in 1908 (quoted in Taton, 1957, pp. 16-17).

First, Poincaré admitted that he did not have the type of memory that would make him a good chess player, where it is necessary to remember many moves and combinations of moves.

> "Why then," he asked, "does [my memory] not fail me in a difficult mathematical argument?" Clearly because it is guided by the general trend of the argument. A mathematical demonstration is not a simple juxtaposition of syllogisms; it consists of syllogisms *placed in a certain order,* and the order in which these elements are placed is much more important that the elements themselves. If I have the feeling, so to speak, the intuition of this order, so that I perceive the whole of the argument at a glance, I need no longer be afraid of forgetting one of the elements; each of them will place itself naturally into the position prepared for it, without my having to make any effort of memory.

In other words, Poincaré's mind had created a pattern. This pattern was judged to be correct by comparison with other patterns, already resident in the mind, representing the paradigm of "good mathematics." That template was so powerful that it, in effect, replaces memory, by allowing a "natural ordering" of the mind. It was also a key component in Poincaré's creative process.

Poincaré revealed this creative process when he related the account of one discovery, as quoted in Taton (1957, p. 21):

> For a fortnight I had been attempting to prove that there could not be any functions analogous to what I have since called Fuchsian functions. I was, at the time, very ignorant. Every day I sat down at my table and spent an hour or two trying a great number of combinations and arriving at no results. One night I took some black coffee, contrary to my custom, and was unable to sleep. A host of ideas kept surging in my head; I could almost feel them jostling one another, until two of them coalesced, so to speak, to form a stable combination. When morning came I had established the existence of one class of Fuchsian functions. . . . Then I wished to represent these functions by the quotient of two series. This idea was perfectly conscious and deliberate; I was guided by the analogy with elliptical functions. I asked myself what must be the properties of these series, if they existed, and I succeeded without difficulty in forming the series which I have called the Theta-Fuchsian. At this moment I left Caen where I was then living, in order to take part in a geological conference arranged by the School of Mines. The accidents of the journey made me forget my work. When we arrived at Coutances, we got onto a brake to go for a drive, and, just as I put my foot on the step, the idea came to me, though nothing in my former thoughts seemed to have prepared me for it; that the transformations I have used to define the Fuchsian functions were identical with those of non-Euclidean geometry. I made no verification and had not time to do so, since I took up the conversation again as soon as I had sat down in the brake, but I felt absolute certainty at once. When I got back to Caen I verified the result at my leisure to satisfy my conscience.

It is evident that the mind has stored in it a huge number of mental models. It constantly compares incoming data or current problems against these stored models, looking for a "good" match. This process appears to be remarkably like what has developed in the social sciences. In the absence of the possibility of isolating society in a laboratory, many of the social sciences' advances have been made *by the use of metaphor.* A metaphor is a type of mental model. Comparing things metaphorically is exactly the same as the mind's process in comparing templates to come up with new thing—the process of creativity.

According to Tipler (1994, p. 199): "There is a growing consensus among cognitive scientists that all human originality is due to an essentially random mixing of ideas in the human creator's mind, with subconscious elimination (natural selection) of the bad ideas." This subconscious elimination is obviously controlled in part by the mental models that populate the mind. Thus, it stands to reason that if the mental model, the paradigm, is disrupted, so the entire creative process will be disrupted. Ideas that a proper paradigm would have eliminated as absurd are nominated to the conscious mind for serious consideration. I believe that this is the source of many of the truly dysfunctional ideas in some of the branches of the social sciences. Ideas that clearly violate self-evident truths are no longer eliminated, because the model of self-evident truth has been disrupted. Then, the human language is convoluted enough, and some concepts can be so taken out of context (such as Popper's Impossibility Theorem), that suddenly a mental model of self-evident truth has been swept aside and a new model—of Postmodernism—has been substituted. And, like the old lady of Yido and her taro root model of warfare, the model sticks fast and resists displacement.

There is another way that new mental models are created, and that is based on the foundation of existing mental models. When an old belief is proven unfounded, the mind begins the process of readjusting its models. It does not do this process from a clean slate, however. Such other mental models that have survived form a basis for the new models.

Templates are a survival mechanism. Humans are weak and vulnerable compared to most of nature's other creatures; recognition of threats based on limited information, performed quickly, is the difference between being a survivor and being a meal. But beyond simple survival, the constant jostling and comparing of mental models and templates in different orders or combinations seems to be a key component in the creative process. Thus, the paradigm that one has, that molds and constrains the entire population of a person's mental models, can also limit, constrain, or direct the creative process. Consequently, if a scientific problem is attacked by someone operating within an incorrect or inappropriate paradigm, the chances that the problem will be solved in a creative manner are limited. What is even more stunning is the realization that these internal mental models can actually have a major—perhaps dominant—role in decision making. Tipler (1994, p. 201) reported:

> Libet et al. have demonstrated that a "person's" brain makes a decision to act before the "person" is aware of having decided to act; that is, the brain makes the decision and then informs the person of the decision, who (mistakenly) believes he or she actually "made" the decision. In the experiment to show this, a spot rotating on a TV screen at a rate of 2.5 cycles per second is

> watched by an experimental subject. The subject is asked to decide of his or her free will to bend a finger, and note the position of the spot when the decision is made. An electrode attached to the head shows that, on the average, potential change in the brain occurred 0.35 seconds *before* the person said he or she "intended" to act.

In other words, the mental model can have a dominant role in decision making. Presumably, this can also extend to attitude towards new information. So, scientists who is presented with information that is in conflict with their established mental models may, quite literally, have their mind made up before they realize it. The paradigm can dominate a person's thinking without their even realizing it.

Waldrop (1992, p. 168) makes reference to landmark studies conducted by Allen Newell and Herbert Simon on human problem solving.

> By asking experimental subjects to verbalize their thoughts as they struggled through a wide variety of puzzles and games including chess, Newell and Simon had concluded that problem-solving always involves a step-by-step mental search through a vast "problem-space" of possibilities, with each step guided by a heuristic rule of thumb: "if *this* is the situation, then *that* step is worth taking."

Waldrop (1992, pp. 177, 178, 193) went on to say: "We use these 'mental models' so often, in fact, that many psychologists are convinced that they are the basis of all conscious thought." And, taking this progression even further, he refers to one of the pioneers of artificial intelligence, John Holland.

> In the cognitive realm, says Holland, anything that we call a "skill" or "expertise" is an implicit model—or more precisely, a huge, interlocking set of standard operating procedures that have been inscribed on the nervous system and refined by years of experience.

Waldrop further discussed Holland's beliefs.

> Knowledge can be expressed in terms of mental structures that behave very much like rules. . . . The cluster of rules forming a default hierarchy is essentially synonymous with what Holland calls an internal model. We use weak general rules with stronger exceptions to make predictions about how things should be assigned to categories: "If it's streamlined and has fins and lives in the water, then it's a fish"—but "if it also has hair and breathes air and is big, then it's a whale." We use the same structure to make predictions about how things should be done: "its always 'I' before 'e' except after 'c'"—but "if it's a word like neighbor, weigh, or weird, then it's 'e' before 'I.'" And we use the same structure again to make predictions about causality: "If you whistle to a dog, then it will come to you"—but "if the dog is growling and raising its hackles, then it probably won't come."

In psychology, this kind of knowledge organization has been observed repeatedly, and is known as a default hierarchy.

Holland further believes that these rules are created through an inductive process of linking experience with observed outcomes. There are a number of illustrations of this. One of the most compelling has to do with "expert systems." In the past there were certain experts that were thought to possess almost mystical powers in their field. There were (are!) "water dowsers" that can walk through a field and locate the best place to drill a well. Young (1976, p. 11) relates stories of the Cornish mining "cap'm" who would go down into the mines, sniff the air, taste the rock, and find gold; the best were known for their "nose for ore." It was always assumed that such talents were a gift from God.

Then, in the 1980's, researchers in Expert Systems desired to take these gifts and reduce them to algorithms that could be placed on a computer. Inevitably, after extensive interviews, it was determined that the experts generally relied on a small number of key indicators. Often the experts did not even understand how they come to their conclusions; for expert miners, or oil drillers, or fishermen, or any other number of fields, the expert would just observe all the signs and then the thing would "feel right."

This "feeling" is a very important part of the survival mechanism. It serves as reinforcement to the individual that they have chosen a correct path. When a model is violated, there are likewise feelings of distress, anger, annoyance, and discomfort—exactly the kind of feelings associated with cognitive dissonance, another situation where data does not correspond to established internal mental models.

Thus, there is a linkage between these mental models and human emotions.

During my undergraduate education, I hated mathematical proofs. I never could pull the right string to get them to come out as they should. A proof to me was red-lettered, dark-visaged, weekend-smashing trouble. Yet, constantly, my instructors would put certain proofs on the blackboard and step back and look at them in admiration and pronounce them "elegant" or "lovely." Clearly, they were comparing such things to a beauty that had as yet eluded me.

Later, when I began programming computers, certain programs would be passed from hand to hand among the students and admired as "beautiful." In this case I understood; I could see how the programs were particularly economical of

CPU time, or of memory, or simply had an elegance of approach. They were simply—lovely. They elicited an emotional response.

According to Livio (2000, p. 14), in ancient Greece the term "beautiful" was identified with "good" and "real" or "truthful."

Horgan (1996, p. 2), when he interviewed the theoretical physicist Roger Penrose, had this to say of Penrose's attitude: "Penrose is an admitted Platonist. Scientists do not invent the truth; they discover it. Genuine truths exude a beauty, a rightness, a self-evident quality that gives them the power of revelation."

Others have observed this connection. The astrophysicist Livio (2000) asks the pertinent question: "When is a theory in the physical sciences beautiful?" He believes that physical theories are considered beautiful when they conform to a set of principles, described as "symmetry," "simplicity," and "the Copernican Principle" (this last is meant to convey the sense that the theory is not human-centric).

Livio's (2000) principles for beautiful theories correspond to what our internal mental models have already created, through the inductive process of observation, and identified as "correct" (i.e., what is in accordance with observations). In other words, Livio has reversed the causality: he believes that theories are right because they are beautiful, when actually they are beautiful because they are right.

It is interesting to note that modern art, abstract art, cubism, and other forms of non-classical art that could not be described as adhering to Livio's principles, are often described as an "acquired taste." I interpret this as meaning that there is a period of time where a person's internal mental models have to be realigned to conform to the new sense of what is "right" in the different genre.

Thus, there is a connection between human emotions, including the human sense of beauty, and our internal mental models, those that we recognize as "truths." Our brain uses emotion as a signal to confirm whether something is right or not, whether it conforms to our sets of internal mental models and templates. Our brain correlates an observation (and idea, a theory, an observation, a picture, whatever) with an internal model of "truth." Those that correlate well are interpreted as correct. That is transmitted to our emotions as a sense of "elegance" or "beauty," in proportion to its degree of correlation.

Livio (2000, p. 254) quotes the German mathematical physicist Hermann Weyl, who contributed greatly to the theory of gravitation, in saying: "My work always tried to unite the true with the beautiful; but when I had to choose one or the other, I usually chose the beautiful."

The linkage between beauty and correctness is explicit in particular sports such as ice-skating, gymnastics, diving, and ballet. There are certain rules that define whether a move is proper (and thus beautiful) or not. The uninitiated may think that a triple lutz performed by an ice skater is miraculous, but the judges will note if the leg is at the required angle or if the hand is held just so. The difference between beauty and a blown effort is judged by their adherence to these (sometimes arcane) rules.

This feeling of "rightness" can come even when what is "right" is not understood. Witness this passage where Wheatley (1992, p. 2) describes her first encounters with chaos theory: "I couldn't always draw immediate corollaries between [the new] science and my dilemmas, but I noticed myself developing a new serenity in response to the questions that surround me."

So, when things adhere to the rules of an internalized model of "rightness"—the paradigm—the mind rewards you with a positive emotional response.

This can also extend to the other extreme, when things do not adhere to a "good" internal model. Livio (2000, p. 18) states that there are theories that are not considered beautiful—and, again, he reinforces the belief that beauty is associated with rightness: "The reluctance to associate beauty with such theories is a bit like the disbelief that we would surely feel if someone told us that he can flip a coin and make it land on its side."

Beyond thinking such things "ugly," the mind can actually go to greater extremes: it can totally reject the experience. As explained by Bowditch and Buono (1994, p. 122), "people strive to achieve a sense of balance between their beliefs, attitudes, and behaviors." This attempt to achieve "cognitive consistency" is mirrored by those times

> . . . when you might be forced into a position or unwittingly do something that does not "fit" with your beliefs and attitudes. This

situation creates cognitive inconsistency or imbalance. Since the resulting psychological imbalance is unpleasant or uncomfortable, we try to reduce that imbalance to attain cognitive consistency once again.

An example of negative emotional feedback can be drawn from the clash of U. S. military doctrines between the "attrition" and the "maneuver" schools in the last decade. Generally, ground commanders have a different paradigm on how to defeat an enemy than air commanders. In the Desert Storm battle planning, when the commander in chief (a ground officer) confronted his air commander about doctrine, Khaled bin Sultan reported he began "throwing a tantrum" and "threatening to fire" the other. During the Kosovo operation, "NATO commanders at 13 bases across Europe watched with growing discomfort during daily video conferences as tensions between their chief [and] his top air force officer broke into the open. . . . Among the commanders there were sharp divisions and frustrations" (Zimm, 2000a, pp. 33-35). What reinforces the significance of these events is that the military trains decision makers to suppress emotions and to adhere strictly to a decision methodology. The reversion to emotionalism, in so public a venue as a video teleconference with international participation, reinforces the belief that the emotional forces involved with the conflicting paradigms were powerful indeed.

There are many examples of how the cognitive inconsistency of competing paradigms yields powerful emotional responses. The military commanders at Desert Storm and Kosovo, fighting with inconsistent cognitive models of how to attain victory, screaming and fighting with each other in public, and the great physicist

Pauli's confusion and depression while dealing with the paradigm-shifting anomalies of quantum theory, are only two examples.

The economist Arthur began dealing with the paradigm-breaking idea of positive returns. This was a concept that was anathema to neoclassical economists, because neoclassical economics is based on finding equilibrium points, which in turn is fundamentally dependent on an environment of decreasing returns for scale. In other words, one candy bar might taste good, but the next candy bar would not be quite so good, and too many will make you sick, and this establishes the pattern of payment that people would be willing to make for products.

The reaction of his colleagues to the opposite scheme, that obtaining more of the same item might be worth more rather than less, was that "many of them seemed to find ideas on increasing-returns economics outrageous . . . whenever I talked about these ideas in the United States, there was hell to pay. People got angry at the very notion that anything like this could happen" (Waldrop. 1992, p. 47). As he explored the source of their anger more deeply, Arthur touched on additional evidence consistent with the idea of a current paradigm shift in progress.

> After all, if you spent your career proving theorems about the existence of market equilibrium, and the uniqueness of market equilibrium, and the efficiency of market equilibrium, you aren't likely to be very happy when someone comes along and tells you that there's something fishy about market equilibrium. As the economist John R. Hicks had written in 1939, when he looked aghast at the implications of increasing returns, "the threatened wreckage is that of the greater part of economic theory."

Recapitulation

At this point, it is necessary to step back and recapitulate.

The material presented in this chapter suggests that the human mind builds mental models as part of its fundamental survival strategy. To enter the correct mental model, the mind must sort through a huge amount of available data. To facilitate this process, to make it rapid, the mind uses templates. These templates are very similar to metaphors, as they allow a small amount of characteristic information to cover a broader ranger of actual information. These models are built though an inductive process of taking experiences and outcomes and linking them together. The construction and use of these models is consistent with the individual's paradigm, or the context in which they understand the problem. These mental models are so powerful that they can actually fool the mind into seeing things that do not exist, or not remember information that conflicts with the mental model. Thus these mental models have a powerful influence on how the individual perceives the world. If the mental model is wrong, an individual can draw wrong conclusions and have every feeling that the conclusion is nonetheless correct. The reasoning and creative portions of our brain that performs scientific work is likewise guided by the paradigm (model) resident in the brain.

The importance of these mental models is profound. Equally profound is the effect when the mental models are found to be false. I have already discussed some of the consequences, associated with things like cognitive dissonance, where there are emotional reactions to conflicts between truth and the mental metaphor. Extreme mental trauma can cause the human mind to go into denial. Disruption of mental models can also cause such trauma.

The Scientific Method, Metaphors, and Paradigms (Right and Wrong)

The concept of a paradigm as an established methodology to approach a problem predates Kuhn, although previous authors did not use the same terminology. Probably one of the most powerful statements of paradigms, which also established a cultural connection, is from Spengler in his monumental *The Decline of the West.* In discussing the investigations of Copernicus and the Paris Occamists (Buridan, Albert of Saxony, and Oresme), Spengler (1928, p. 301) said:

> Let us not deceive ourselves as to the fundamental motive-power of these explorations. Pure Contemplative philosophy could have dispensed with experiment for ever, but not so the Faustian symbol of the machine, which urged us to mechanical constructions even in the twelfth century and made *"Perpetuum mobile"* the Prometheus-idea of the Western intellect. For us the first thing is ever the *working hypothesis*—the very kind of thought-product that is meaningless to other Cultures.

The key part of this passage is the last sentence. Here, Spengler identifies a paradigm of thinking that developed in the Western world—the paradigm of the working hypothesis—that was driven by a metaphor, that of the machine. Spengler believed that this metaphor was the key. Other cultures developed with other metaphors guiding their thinking, which in turn molded them into different ways to approach problems.

A unique example of this is the Japanese. Their culture was almost totally insulated from the machine metaphor way of thinking. As related by Harries and Harries (1991, pp. 5-6), in the late fifteenth and early sixteenth centuries the Tokugawa dynasty sealed Japan away from outside influences. For 250 years—the years of the industrial revolution in the West—Japan was isolated from Western culture, their machines, and their metaphors. This allowed the Japanese to build (or retain) their own, unique set of mental models and metaphors.

When Perry's ships steamed into Tokyo Bay in 1853 and intercourse was forcibly opened, the acquisition of Western machines did not mean a corresponding acquisition of Western imagery and mental models. The traditional, ingrained ways of thinking were too well established, part of the warp and woof of Japanese culture. The metaphor of the machine, with all its implications of mechanistic causality models and a reductionist approach to problem solving that was so much a part of Western culture, had no comparable impact on Japanese thinking. Instead, spiritual power was thought to be superior to physical power, not only in framing moral and spiritual outcomes but in deciding the outcome of physical events as well.

The primacy of the spiritual power metaphor as the approach to problem solving was further reinforced by official government action. In the 1880's and thereafter, the Japanese had to come to terms with their impoverishment in the natural resources needed to sustain a modern Western-style economy and military establishment. Japan lacked ore, quality coal, oil, and even sufficient arable land to feed her own population. And, in that era of Western imperialist expansion, the Japanese developed the perception of being surrounded and cut off from these resources by enemies.

One resource that Japan did have was a relatively large population. In an era of massive armies, they thought to develop that resource to build and employ an army in an imperialist manner to acquire resources by conquest. To develop the needed nationalistic commitment in the general population, the national government actively promoted the spiritual cult of bushido and the philosophically complementary religion of Shintoism. The confluence of these factors—an abiding spiritual power metaphor, material weakness, an international ethic of imperialism, and a government-mandated religion that reinforced the spiritual power metaphor—served to coalesce an unique Japanese attitude of spiritualism and stoic fatalism in government and individual problem solving.

This was evident in the Japanese approach to warfare during the Second World War. The usual Western-style methodology to prepare for an operation involved calculating logistics requirements, planning movement rates, pre-establishing transportation needs, and forecasting combat results—a reductionist, machine metaphor approach. The Japanese approach was very different. Lieutenant General Ryotaro Nagai of the Japanese Army wrote in 1943 in an essay entitled "On Generals": "It is a fact that victory or defeat in warfare is ascribable to something transcending logic, namely, fate or the *grace of Heaven.*"

In the following passage from Harries and Harries (1991, p. 336), note the different metaphors that Japanese leaders applied to problem solving:

> Japan's entry into [World War II] was a reckless advance into the unknown—a leap, in Tojo's phrase, from the veranda of the Kiyomizu Temple. He himself admitted to having no idea in 1941 of what the future might bring; the army, he thought, would "manage somehow" in 1942 and even 1943, but beyond that anything might happen. The word he used to describe the leadership once the decision for war had been made was "carefree": but "unthinking" might have been more fitting. "From fatalism to passivity to the giving up of thought—the path is distinctly cut." This was a Buddhist attitude reflecting the religious leanings of most of the officer corps, including Tojo. . . . [The British] General Slim noted with Western-style incredulity that [the Japanese] attacked with a very narrow margin of administrative safety, generally with no more than nine days' supplies in terrain where operations could reasonably be expected to last for weeks. If it was heaven's will that they should win, then something would turn up—perhaps the supply dumps of their enemies; the same cast of mind may have lain beneath the army's dogged refusal to negotiate long after defeat was inevitable. . . . Risk, as a correlate of strategic objectives, stakes, and consequences, is not a word that one finds in the ordinary vocabulary of the Japanese military.

This passage illustrates a rich diversity of metaphors to guide behavior that, if examined as a whole, constitute a problem-solving paradigm. This Japanese paradigm of problem solving was certainly a dramatically different approach to the problems of Twentieth-Century warfare. By a string of incredible good fortune, spiritual power did indeed overcome material power in the Japanese conquest of the Philippines and Singapore; however, the grace of heaven did not translate into the food and weapons needed in New Guinea, Guadalcanal, Burma, China, and a host of other situations from the middle of 1942 until the end of the Second World War.

Spiritual fatalism was not an effective problem-solving paradigm for Twentieth-Century military problems.

The mental process required to build a mental model is an exact parallel to the Scientific Method. Just as the Scientific Method builds and tests models employing a predictive "if–then" structure, so does the mind. Indeed, to flip this relationship over, Arthur has been quoted in Waldrop (1992, p. 327) as asserting that "nonscientists tend to think that science works by deduction. But actually science works mainly by metaphor."

The Scientific Method paradigm appears to be aligned with the brain's natural problem-solving methodology. That suggests one of the reasons for the success of the Scientific Method: to humans, it is "natural"—it works in the same manner as the brain. This could also be a contributing factor for the success of those Western cultures that assimilated the machine metaphor and adopted the scientific method as normal procedure. Western thinking correlated best with the mind's natural problem solving methodology, which also correlated with the best paradigm to approach the "easy" problems in the physical sciences and engineering. The result contributed to the remarkable success of the West in the physical sciences.

It also suggests that an incorrect paradigm inhibits the process of problem solving. We can see this in the progress that Oriental or Middle Eastern cultures have made, and the fact that they have been relatively less creative in the physical sciences.

Hart (2001, p. 12) related the story of the famous debate between Thomas Aquinas, theologian, and Averoes, the Arab philosopher and physician.

> [Averoes] held, with Islam, that there is a truth of religion and a truth of science. He compartmentalized them against each other. Aquinas, in his great "Contra Averoes," held that there is a single truth of fact and logic. A statement cannot be true and false at the same time. There is no compartmentalization of truth.

This passage highlights the constant tension in the Arab world between religious truth and material truth, a compartmentalization in conflict with itself and in tension with natural mental processes. As a byproduct of this tension, witness that, for example, there is not one world-class university of the physical sciences in the modern Arab world.

As we have seen, the brain has, as a survival trait, the ability to build internal models or metaphors of the world around it. These models are a short cut from input data to expected outcome or expected consequences. They exist so that the brain does not have to consider every piece of data available to it in order to come to a judgment. There are two aspects involved in this process: using fragmentary or partial data, fitted to a template, to make an identification; and rejecting data that is *non sequitur* to the expected, that does not fit in to the established mental model. The brain does this primarily by an inductive process of observation, of experience in connecting observations with consequences. It observes specifics, and extends this to the general. So, observing one or more fires to be hot and potentially damaging, it concludes that all fires are potentially damaging and resists the idea of placing one's hand in the fire to test the idea again. And, if someone is in a position where they can anticipate that contact with a fire will happen, there can be the extreme physical reaction of fear: trembling, sweating, and all sorts of related physical and emotional feedback.

Consequent Behavior

Various experiments with human point to other potential behaviors that might fall out from the possibility that human behavior can be chaotic or fuzzy. Pinker (1997) provides some information on experiments that can give a clue to this behavior. In one set of experiments, people were presented with completely random data. Almost invariably, they saw some kind of pattern. In another experiment, people were shown a movie clip showing a woman walking from room to room. When asked to describe what they saw, they almost invariably attributed a motive to the woman's action, e.g., "I saw a woman looking for her child." Human beings are so oriented to operate from the basis of internal mental models that they will impose a mental model on a set of observations even when there is no basis to do so.

From this information, consider the implications when a human attempts to build a mental model out of data that is chaotic. The propensity would be to build a model even though the data would not support such an effort. Since the mind will tend to filter and adjust the received data to make it conform to an established mental model, then there would be a propensity to build models that would be like self-fulfilling prophecies. The scientist would tend to see in the model what conformed to his or her established worldview. This propensity would tend to entrench scientists further into whatever worldview they currently inhabited, *regardless of the accuracy of that worldview*.

Consider the situation when a scientist publishes a new theory taken from data that is, unknown to him, chaotic. Often, immediate objections are raised from other scientists who have their natural filters set to screen the incoming stream of data in a different way. Others, "revisionists," also have a vested interest in having the results come out against the hypothesis, so that they might gain academic stature as "the one who disproved X." Kuhn's observation on paradigm shifts then comes into play. He noted that (in some cases, as fitting the chaotic nature of human behavior), when the reigning paradigm is displaced, there follows a period of disorder, where many competing paradigms jostle for position, experiments are conducted without a guiding paradigm and show disparate results, and the landscape in that academic field is uncertain.

This disorder would tend to be accentuated by some of the fundamental pitfalls of using metaphors.

Let us suppose that we are investigating phenomenon "A." We would like to understand all the characteristics of A. A has three characteristics, #1, #2, and #3. We know about #1, but we do not know about #2 and #3. The let us suppose that there is another phenomenon, "B." B is well known, and we know that it has characteristics #1, #2, and #4.

	A		B
Unknown	Known	<u>Characteristics</u>	Known
#2 #3	#1		#1 #2 #4

FIGURE 47

METAPHORS

System A has characteristic #1 in common with B. This suggests that we can make a metaphorical connection between A and B. We do so. We then look at B, see that it also has characteristic #2, and so we look in A to see if it also has characteristic #2. And Lo! We find that it does. The process of the metaphor has led us to additional understanding.

However, there are also traps in the process.

In A there is still characteristic #3, which we have not discovered through the metaphor. The metaphor has not led us to a *comprehensive* understanding.

Also, in B, there is characteristic #4, which is not in A. If we take the metaphor too seriously, and expect more of it than it is capable of giving, then we might incorrectly be led to the conclusion that A also has characteristic #4. The metaphor in that case has led us astray. In some circles that is known as "negative learning."

In general, however, we know not to strain the limits of a metaphor. Not to do so is common knowledge, captured in some common saying such as "Don't stretch it too far." How do we know when we have gone too far? When asked, people will talk about the comparison being silly, or it just not feeling right.

One of the disciplines imposed by a prevailing paradigm is to police the metaphorical process, to ensure that the use of a metaphor is limited and rigorous. After all, the metaphor is a poor substitute for an experiment or real data. There needs to be limits.

Return for a moment to the example of Poincaré's creative process. His subconscious was continually comparing templates and testing analogies. How did it know which to accept and which to reject? As Poincaré said, he was "guided by the analogy of elliptical functions," a mental comparison (exemplar) he was using for "good mathematics." When there was a good match, there was also an associated emotional response—as he said, ". . . I have the feeling . . ."—of rightness. In my terminology, the analogy had made a match that was in accord with Poincaré's internal heuristic rules of "good mathematics," and there was an associated positive emotional reinforcement. Poincaré's existing paradigm served as the mental standard to police the comparison and ensure that the analogy was reasonable.

Now, take the situation where an individual does not have an accurate paradigm. Other connections can be proposed in the brain, but there is no master exemplar to police its acceptance. In its absence, incorrect connections can be made. When an incorrect internal mental model is formulated, it then provides the same feeling of "rightness" as a more correct model. Thus, people can see beauty after acquiring the taste for modern art, and the old lady from Yido smears her taro root with a feeling of confidence that the enemy will be destroyed.

I believe that the metaphorical approach to the social sciences is so attractive because it parallels the natural mental processing strategy of the brain—the way the brain creates models and templates in order to handle masses of information in a rapid fashion. The reversion to this natural strategy has become a natural "fall-back position" in response to the failure of the scientific method to yield social science theory.

But that is not something unique to the social sciences. Remember the statement by Arthur that "actually science works mainly by metaphor." A case can be made that scientific breakthroughs are through insights triggered by mental metaphors. But in the physical sciences there remains the scientific method that disciplines the use of metaphor and makes it much more controlled. In addition, if we are to believe Livio's construction of what constitutes beauty in a physical science theory, beauty (rightness) consists of conforming to standards that we know work in the non-chaotic branches of the physical sciences. In the social sciences, where (chaotic or fuzzy) behavior does not correspond to these standards of rightness on theory, we do not see metaphor making any particular breakthroughs.

In addition, in the social sciences the employment of metaphors is more unconstrained, because the restraint provided by the ruling paradigm, and the standards it imposes, is much less powerful.

In a discussion with Dr. Robert Schirmer of the Johns Hopkins University Applied Physics Laboratory, he recalled instances where social scientists would attend seminars in quantum physics. Invariably, he stated, there would be some question regarding the assertion that quantum physics has now "proven" some notion or another in the social sciences. The physicist, of whom the question was asked, would back-pedal ferociously and try to lead the social scientist back from the edge of that particular cliff. "Surely, that is taking the idea too far, into a realm beyond which it is appropriately applied," they would assert. Their "feelings" of how far the metaphor could extend, I assert, would be far more accurate than those of an outsider trying to apply the metaphor based on an incomplete knowledge of the exemplar.

Thus, when mental models or paradigms are disrupted, much negative learning through over-extended metaphors is possible.

Expected Behaviors

At this point, a quick review of the chain of logic that caused us to arrive at this point would be appropriate.

Human behavior can be complex and chaotic. Human behavior could be based on fundamental mechanisms, like principles of behavior and the Organization Logistic equation, that result in regions of deterministic chaos. There also could be other mechanisms of human behavior, such as fuzzy logic and behavior based on human fuzzy decision making processes, that might also yield complex or chaotic human behavior. These additional mechanisms have not been discussed here and are beyond the scope of this dissertation, but need to be mentioned to leave open the possibility that there are other underlying mechanisms that drive human behavior to be complex and chaotic, and are incompatible with the Scientific Method.

Consequently, human behavior cannot be properly investigated using the usual processes and logic inherent in the Scientific Method. This has resulted in the failure of the Scientific Method to develop social science knowledge. This failure should result in the same type of behaviors that are seen when any other human mental model is in conflict with reality. Behaviors that would be characteristic of this problem can be identified.

This chapter has discussed human cognitive processes, concentrating on mental models, metaphors, and the creative mental process. With this information, characteristic behaviors can be identified that would be expected if the social sciences were currently undergoing a conflict where the scientific method paradigm was being assailed by anomalies.

Some of the expected behaviors would be:

1. A large portion of the social science community retaining the old (Scientific Method) paradigm, in spite of large numbers of observed anomalies;
2. The development of new methodologies based on new mental models;
3. The increasing use of metaphor as a substitute for theory development;
4. The increasing employment of very wild metaphors;

5. Rejection of obvious truths by some elements of the communities;
6. Increasing inability of members of different factions to communicate; and
7. Elements of the debate becoming very emotional.

Justification of these behaviors are as follows:

1. *A large portion of the social science community retaining the old (Scientific Method) paradigm, in spite of large numbers of observed anomalies:* Mental models become entrenched in some individuals. The models are to a degree "self-protecting," because of the tendency of the brain to filter out conflicting data. There is a documented propensity for humans to stay with established methods or plans, rather than develop new ones.

2. *The development of new methodologies based on new mental models:* Some portions of the community will see the stream of anomalies. They will inductively begin to attempt to build new mental models based on these observations.

3. *The increasing use of metaphor as a substitute for theory development:* It has already been shown that much of the progress in the sciences (both physical and social) have been through the medium of the metaphor. In the absence of a base of theory from which to emulate, the mind with have recourse to pulling up additional metaphors from the "randomizer" and testing them out.

4. *The increasing employment of very wild metaphors:* Two factors are at work here. First, these new trial metaphors are kicked up into the conscious mind for consideration in increasing numbers as earlier attempts to come up with satisfactory

metaphors and theory fail. Second, the underlying mental models that provide the emotional feed back as to the "rightness" of the proposed solution have been disrupted by the previous failures. Some people will have substituted different (and wrong) templates as the standard of "rightness" (just as the old lady of Yido had developed a wrong mental model). Some stream of data (or an established anomaly) might reinforce a particular model incorrectly (from the propensity of humans to incorrectly see a pattern in data that is random or chaotic). Thus, some wild metaphors will strike some people as "right." And, taken from Kuhn's work, the established paradigm will have been weakened by the anomalies, and will not be able to police its borders and reject the new, wild metaphor. Thus, the metaphor is published, and other people who are searching for "rightness" may latch on to it and install it as their new standard of "rightness."

5. *Rejection of obvious truths by some elements of the communities:* As internal mental models are disrupted, and new substitutes developed to deal with anomalies, sometimes the new mental models that are accepted in some people are just plain wrong. They have been developed and accepted in the person based on inductive creation from a set of experiences and information which, if they are chaotic or complex, may be resolved into a "pattern" that is random. These models may conflict with self-evident truth, but because the individuals underlying mental model of "rightness" has been disrupted, the individual may accept the model that "explains" the new "pattern of anomalies" in spite of the fact that they conflict with

self-evident truths. This propensity would be accentuated if others have gone before him in the process and made statements that the self-evident truths are wrong, thus allowing the random internal model to be reinforced and eventually established as a new standard of "rightness."

Some of these new models will be dominated by other, older mental models. Thus, there will be seen such things as cases where a political mental model will dominate over lessor mental models that might require the adherence to truth, or honesty, or some other value.

6. *Increasing inability of members of different factions to communicate:* As an individual begins to populate the new mental model, words begin to take on new meanings. This is because the new mental model may need to communicate new concepts for which there are no existing words. Existing words then become preempted. They are used in a slightly different context, to mean different things than originally. Eventually these new meanings become entrenched in their new meanings within the particular community, while others retain the word in its old context.

7. *Elements of the debate becoming very emotional:* Since one way the way the brain communicates the "rightness" of a mental model is through the emotions, people will get negative emotional feedback when looking at others' models. This negative emotional feedback will become accentuated by frustration when two groups try to communicate, and their words end up meaning different things to

different people, without their recognizing that this is happening. Thus, one group might feel that they have clearly made their case, while the other group totally misinterprets what has been said, since they are attributing different meanings to different words.

Some of the behaviors described above are the same as some of the behaviors associated with past paradigm shifts. There appears to be an obvious cognitive connection between the behaviors and the brain's natural workings.

Thus, we have two lists of expected behaviors: a list taken from the history of past paradigm shifts, and one taken from expected cognitive behaviors. The next step will be to take a broad overview of the social sciences to see if these behaviors are evident. Again, not all the behaviors are expected to be evident in all fields of the social sciences. If human behavior is complex or chaotic, a great deal of (chaotic) variation would be expected within each individual field. However, as will be seen, a broad sweep over the social science landscape will show that all of these behaviors are present in some or all of the individual fields.

CHAPTER XI

PARADIGMS IN THE SOCIAL SCIENCES

Taking a Lead from the Physical Sciences

The social sciences have been, from the first, greatly influenced by the physical sciences, both in methodology and metaphors. Elliot and Kiel (1996, p. 1) recognized this:

> The social sciences, historically, have emulated both the intellectual and methodological paradigms of the natural sciences. From the behavioral revolution, to applications such as cybernetics, to the predominant reliance on the certainty and stability of the Newtonian paradigm, the social sciences have followed the lead of the natural sciences.

Compared to the physical sciences, the social sciences are relatively new. Perhaps the first would be economics, which might be dated from the publication of Adam Smith's *An inquiry into the nature and causes of the wealth of nations* in 1776. Otherwise, Shafritz and Hyde (1992, p. 40) point to Max Weber (1864-1920) as the father of modern sociology, which is believed to be the first of the social sciences other than economics. Weber's great work, *Bureaucracy,* began to receive wide distribution in 1922. While Weber did not use the scientific method in his study of authority and its organizational manifestations, *Bureaucracy* appeared in a society well versed in reductionism and the machine metaphor. During this time, schoolchildren were taught the Scientific Method in elementary school, and indoctrinated in the glories of the physical sciences. It is thus inevitable that the social sciences initially would model themselves after this contemporary success story, and take on its forms and protocols.

This point was underscored by Burke (1989, p. 47), in "Themes in American Public Administration," where she indicates that taking on the label of "science" was a critical part of the development of Public Administration.

> It provided a basis for the legitimacy of expertise, which could then be taught and could be used to improve conditions of life in an industrialized and urbanized society. Acceptance of this position was a major step in the development of the government of the United States. It lead to development of large bodies of technical knowledge that have allowed the "administrative state" (and industrial societies it serves) to survive and flourish.

Thus, the acquisition of this large body of technical knowledge, acquired under the trappings of "science," reinforced the aspirations of the field to scientific legitimacy, leading to further alignment with the existing Scientific Method paradigm.

In the early development of the physical sciences, the discovery of laws of behavior using the scientific method had an immense effect on the thinking of early scientists, which Gleick (1987, p. 14) characterized as "Newtonian fever." Laplace, the great Eighteenth-Century mathematician, is an example of this early enthusiasm.

> Such an intelligence would embrace in the same formula the movements of the greatest bodies in the universe and those of the lightest atom; for it, nothing would be uncertain and the future, as the past, would be present to its eyes.

There were, of course, immediate specific implications of the conversion of the "social arts" into the "social sciences" with the associated theory-creation form of exemplar.

Casti and Karlqvist (1991, p. vii) mention some of the baggage that accompanies being identified as a science.

> Prediction and explanation are the twin pillars upon which the scientific enterprise rests. The whole point of science, along with religion, mysticism, and all of science's other competitors in the reality-generation game, is to make sense somehow of the worldly events we observe in everyday life. Thus, the whole point of the practice of science as science, is to offer *convincing* explanations for these events, as well as to back up those explanations with accurate predictions about what will be seen next. . . . Scientific explanation and prediction is explanation and prediction *by rule*, or as it's sometimes put, *by law*. . . . For instance, we can accurately predict that there will be a full moon on October 30, 1993 by appealing to the laws of celestial mechanics, essentially Newton's laws of gravitation and motion. And, in fact, when a rule like Newton's proves useful enough over a long enough period of time, it's often enshrined in the lexicon of science by being upgraded to a "law," viz., Newton's *law* of gravitation.

In other words, being identified as a science pushed the social sciences from being an ad hoc collection of "arts" (the "art of leadership," or the "art of war") into the prediction and explanation business—or, more simply, into the theory business, with all implied therein.

The tendency of the social sciences to align itself with the physical sciences' paradigm has been reinforced by cross-pollination of practitioners. In one example, Bygrave (1989, p. 16) observed that a number of management strategy theorists had migrated to the field from engineering. He observed a close connection between their scientific training and their approach to business strategy, featuring systematic, rational approaches to problems.

Wheatley (1992, p. 6), an organization theorist, has written a passage that is revealing on several different levels.

> I began college with full intent to major in biology, but my initial encounters with advanced chemistry ended that career, and I turned to the greater ambiguity of the social sciences. Like many social scientists, I am at heart a lapsed scientist, still hoping that the world will yield up its secrets to me in predictable formulations.

There are several points revealed in this quotation. First, it provides another indication of the extent of the cross-pollination between the "hard" and "soft" sciences—the phrase "like many social scientists" reinforces that this is not an isolated phenomenon. Second, through its disarming admission of failure in the discipline of chemistry, it adds a touch of sublimated envy to her relationship with her former area of study. Last is the indication that, even if she was not able to master chemistry, the desire to emulate the hard sciences remained. The broad objective of discovering "secrets in predictable formulations" was internalized as an exemplar of science to be emulated.

There are other examples. Mathematicians have often gravitated to the field of Economics. Economists have begun to direct their attention to explaining motivations for organizations, to the extent that they have developed an entire new school of thought, the New Institutional Economics.

I can add my own experiences. I began my college career as a physicist, my masters degree is in the mathematically-oriented field of Operations Research, and I spent the first twenty years of my working life as a nuclear power engineer. Later, in the USC Public Administration program, I received my first real concentrated exposure to the practice and practitioners in the social sciences. I have seen many students who have migrated from the physical sciences to the social sciences, but few who have gone in the other direction. When I was the head of the Operations Research curriculum at the Naval Postgraduate School, I observed people with social science degrees that wanted to transition into this "hard science" field. Most were thwarted by the difficult abstract mathematics prerequisites. The success rate of this group was low, even among the most highly motivated students.

Flow has generally been into the social sciences by students that have been thoroughly indoctrinated in the fundamentals of the physical science paradigm.

These factors make it understandable why there is so much of the physical sciences approach and methodology in the social sciences.

Another point deserves notice. Just like Wheatley's linguistic skirting around the admission that she was unsuccessful in chemistry, most of the students that I have known to make the shift in disciplines did so because of a "failure" in the hard sciences. The difficult disciplines of mathematics, organic chemistry, and physics, subjects that generally serve as prerequisites for advanced study in the physical sciences, are the major stumbling blocks. A failure to meet the standards of one discipline could transfer feelings of envy coupled with determination: "I can be just as good and rigorous a scientist in my new field as I might have been in the old." What the students absorb from the physical sciences before their migration—the physical science paradigm, physical science standards of research, and idea of what constitutes "good, rigorous, acceptable science"—is then applied in the new field with the zeal of a young child attempting to convince her parents that she, too, is an adult. Add to this the observation, as related by Burke (2002), that scholars in the physical sciences are afforded more prestige and often are better paid than those in the social sciences, and a picture is completed of one field that can be considered the exemplar for another.

From these three influences, then,

(1) the modeling of the new social sciences after the established physical sciences at their creation;

(2) the cross-pollination of scholars; and

(3) the desire of some to prove that the social sciences could meet the standards of the physical sciences,

it was perhaps inevitable that the social sciences would take on the forms and protocols of the physical sciences.

It is intriguing to consider what might have been. There is a story that I have been told by several scholars that I have not been able to confirm in any reference, but would like to relate because it has fascinating implications.

The story goes that, in the 1950's, scholars had fields of study that they proudly labeled the "liberal arts." Social studies could be legitimately approached as forms of the art of being human. Management was an art, leadership was an art, and so on. It was a legitimate pursuit to try situational solutions for unique problems. There was an entirely different ethos involved in studying the liberal arts, a different standard of what was considered the exemplars of good practice.

Then the Russians launched Sputnik. In the post-WW II era of nuclear weapons competition, this event was of earthshaking magnitude. The physical sciences became the center of attention; physicists began to gain the notoriety generally reserved for rock-and-roll entertainers. The U.S. government executed new policies for grants and studies. The emphasis was on science. In the atmosphere of national emergency, there just was not much funding available for anything labeled "art." Grant money for liberal arts vanished.

To counter this problem with gaining funding, so the story goes, historians and sociologists and anthropologists and all the others who formerly were gathered under the rubric of "arts" began to call themselves the "social *sciences.*"

A rose might not smell any different for being called another name, but, in human society, the label can become the message. Perhaps the more applicable quote is, "you have named me Caesar, so I shall act the part." The applicable liberal arts became sciences, both in name and aspiration.

It is intriguing to speculate what might have been if the social sciences had not capitulated to financial necessity and assumed the paradigm of the physical sciences. Without Sputnik and all that it caused, it is possible that the social sciences could have developed in a different way, without the baggage of the necessity of uncritical acceptance of all things from the physical sciences' cupboard. As I will discuss later, the social sciences have been able to go only so far by hanging on to the physical sciences' coattails; the round peg does not become square upon being labeled square.

The Conduct of the Social Sciences Under the Prevailing "Classical Science" Paradigm

The physical sciences were fortunate in having a myriad of problems that could be successfully addressed with relatively simple means and described in relatively simple terms. According to Ijiri and Simon (1977, p. 1):

> Nature, as it presents itself to the physical scientist, is full of clearly defined patterns. The planets move in ellipses about the sun. The distance traveled by a falling body varies with the square of the time. . . . The nineteenth century brought forth a whole array of partial differential equations to describe in exquisite detail the motions of waves: light, sound, electromagnetism.

In general, the social sciences were guided by the propensity of *all* the sciences to take their lead from the example of physics. The social sciences have generally adhered to the classical Scientific Method and accepted its precepts. Indeed, as related by Marion (1999, p. 42):

> Perhaps the earliest theory of organization and society (other than philosophy) evolved shortly after Newton wrote *Principia*, during the infancy of the Industrial Revolution. Known as Social Physics, the theory compares society to a machine with its complex of different but interdependent parts. Just as machines require engines, cog wheels, and levers, a society requires governors, bankers, and police, and a factory requires leaders, line workers, and a sales force, the parts of such systems are held together by attraction and differentiated by repulsion. Social Physicists wrote of social space (analogous to phase space in physics), centrifugal forces, attraction and attractors, countervailing pressures, and social equilibrium.

Currently most social science research is based on the presumption that the world is governed by deterministic cause and effect relationships that can be described in relatively simple causality models. Humphreys (1989, p. 48) reinforces this understanding by broadening it to the underlying philosophy of science, to which the social sciences aspired:

> It is not surprising that the philosophy of science, which has traditionally seen physics as the central science, should have (1) taken causation in physics as exemplary, (2) used laws of physics in its logical reconstruction of causation, as a result, (3) been concerned almost exclusively with laws formulated primarily for application in closed experimental systems, involving single causal influences, and consequently (4) focused largely on causal chains.

Thus, many scholars looked to discover the social science analogs to Newton's Laws. Attempts to derive causality models and laws of behavior appear in the early literature of all the social sciences.

Examples are legion. In the organizational and administrative sciences, the earliest attempts were associated with Henri Fayol (1949, p. 52). In 1916 Fayol asserted, "The soundness and good working order of the body corporate depend on a certain number of conditions termed indiscriminately principles, laws, rules." Marx (1947, p. 65), in a review of Simon's *Administrative Behavior,* was looking for the creation of "a general theory of administration." The Hierarchy of Needs, Theory X and Theory Y, Equity Theory, Exchange Theory, Attribution Theory, Expectancy Theory, Path-Goal Theory, the Managerial Grid, and Immaturity-Maturity Theory all are examples of causality models that were, at one time or another, proposed as the equivalent of laws of human behavior.

Public administration also initially followed the lead of classical science in a search for laws. In 1947, Dahl (1947, p. 1) wrote: "The effort to create a science of public administration has often lead to the formulation of universal laws or, more commonly, to the assertion that such universal laws *could* be formulated for public administration." Dahl's list of references to support this assertion ranged from Merson in 1923; Watson's "The Elements of Public Administration, a Dogmatic Introduction" in *Public Administration*, 1932; Gulick's "Science, Values and Public Administration" in 1937; and Renwick's "Public Administration: Towards a Science" in 1944. Dahl quoted Willoughby as making "perhaps the best known expression of this kind," as saying "in administration, there are certain fundamental principles of general application analogous to those characterizing any science." Dahl went on to state, "others argue that it is possible to discover general principles of wide, although not necessarily of universal validity."

In public administration overall, Denhardt (1993, p. vi) has observed that "the mainstream work in public administration theory has centered on elaborating a so-called rational model of administration," a model that stresses predictable, law-governed responses by human beings. Wheatley (1992, p. 6) makes this same point in the narrower example of organizational theory.

> Each of us lives and works in organizations designed from Newtonian images of the universe. We manage by separating things into parts, we believe that influence occurs as a direct result of force exerted from one person to another, we engage in complex planning for a world that we keep expecting to be predictable, and we search continually for better methods of objectively perceiving the world. These assumptions . . . come to us from seventeenth-century physics, from Newtonian mechanics. They are the base from which we design and manage organizations, and from which we do research in all of the social sciences. Intentionally or not, we work from a worldview that has been derived from the natural sciences.

The tendency of the social sciences to cast their investigations into the mold of classical science can be seen in just about any survey social science text. For example, a chapter in Bowditch's and Buono's (1994, p. 39) *A Primer on Organizational Behavior,* entitled "The Research Process in Organizational Behavior," begins: "One of the ways in which people attempt to simplify the world is to break down the complex sets of information that surround them into manageable portions." This statement is clearly a reductionist assumption borne from the machine metaphor. They go on to say: "Within the field of Organizational Behavior (OB), this process often takes the form of propositions about human behavior and its causes." These "propositions" represent an effort to discover laws of behavior.

Such causality models are widely prevalent in the social sciences, and are found even when they are not explicitly placed in an "if-then" form. For example, the following quotation comes from the introductory chapter of a popular management textbook by Turban and Meredith (1994, p. 2):

> Productivity is a major concern for any organization since it determines the well-being of the organization and its participants. . . . The level of productivity, or the success of management, depends primarily on the execution of certain managerial functions such as planning, organizing, directing, and controlling.

This can be converted to the following causality relationships:

If the execution of the managerial function of planning, organizing, directing, and controlling are improved,

then the level of productivity will improve;

and

if the level of productivity improves,

then the well-being of the organization and its participants will improve.

An important question is how one goes about determining these "if–then" laws of behavior. Ijiri and Simon (1977, p. 116) have described the "law-finding" process as a three-stage process: "(1) finding simple generalizations that describe the facts to some degree of approximation; (2) finding limiting conditions under which the deviations of facts from generalizations might be expected to decrease . . ., (3) explaining why the generalization 'should' fit the facts." This process can be seen as very compatible with the Scientific Method paradigm.

In addition to forms in common with the current science paradigm, the research methodologies themselves appear to be lifted whole from the classical physical science hymnal, without transposition. For example, O'Sullivan and Rassel's *Research Methods for Public Administrators* (1989) could just as easily have been a textbook in statistics. It contains an initial research methodology that mirrors the scientific method, and contains chapters on "Index Construction," "Charts, Graphs and Tables," "Statistics," "Regression Analysis" and "Inferential Statistics for Hypothesis Testing" that would warm any statistician's heart, just as if the data was being collected from any classical physical system.

The examples given above are just a sampling of the prevalent effort by the social sciences, from its inception, to discover social science analogs to Newton's Laws. Trained in the scientific method in elementary school, and with many social science investigators with physical science undergraduate or graduate degrees, and steeped in a desire to emulate their more successful cousins, it was inevitable that social scientists would embrace the paradigm of the classical physical sciences and work hard to live up to the rules and standards of that community.

A major problem exists, however, as outlined by Meyer (1999, pp. 71-73) in an article entitled *A User's Guide to Politics*. The ideas expressed there can easily be generalized to the social sciences as a whole.

Meyer begins by asking: "What do we know about politics?" He suggests that the correct answer is: "We don't know what we know about politics." We have never taken the trouble to codify our knowledge, to bring it all together into a coherent, shared body of knowledge. In referring to political scientists, he stated:

> With no accepted body of knowledge to guide us, each of us is left to work out our own set of axioms. And because we tend to do this implicitly, rather than explicitly—if indeed we do it at all—usually we don't even know whether our own grasp of how things work matches the grasp of those with whom we are dealing. If we find out at all, it is through the experience of working together and arguing about this or that issue. Generally, when we "agree" about politics, we mean that a common understanding of how things work shapes our views on issues. Mind you, this doesn't mean we agree on every issue; merely that we reached our positions by traveling down the same intellectual track. . . . The fault lies in the culture of our discipline. In the hard sciences, the overriding objective is to develop new insights into how things work. Despite the ferocious competition that marks their daily work, hard scientists all seem to share an insatiable curiosity, an extraordinary sense of enthusiasm, above all an overriding feeling of purpose. What drives them forward is precisely this hope of adding one more piece to the puzzle, of coming one step closer to a genuine understanding of how things work. Thus in the end they celebrate any individual's triumph as a victory for the entire enterprise. When a new insight is shown to be true, that insight is accepted by scientists—embraced, actually—along with the individual who figured it out. And when an insight is later shown to be false, it is discarded—quickly, brutally, and often accompanied by the careers of those who develop it and who continued to defend it after its falsity had become apparent. . . . Unlike our hard-science counterparts, [political scientists] see ourselves more as competitors than collaborators. Our object isn't to add one more piece to the puzzle; rather, it is to push forward our own perceptions and viewpoints. Political scientists conduct their research, publish their books and essays—but they never resolve their differences, and thus they fail to create a shared body of knowledge. . . . [N]owhere does the profession separate true insights from false ones. Each political scientist does his or her own thing, the good ones and the bad ones working side by side, often sullenly, without the sense of shared enterprise that is so striking among the hard scientists. . . . Our objective is to win acceptance of our own policy prescriptions or policy strategies, which are based on whatever insights we may have developed from our personal study and

> experience. For us there is no such thing as a discredited idea or insight; there are only varying perceptions and realities.

There are ideas of substance here applicable to this study. First, that since there exists truth for the physical sciences, there exists an ultimate arbiter over the correctness of the ideas proposed by physical scientists. This has generated a psychology of its own within the community, where all are contributing toward discovering that truth.

With the social sciences, that arbiter has not been discovered. All ideas can be equally likely to be correct. As such, a different psychology has been developed, one of competition of ideas and salesmanship of concepts.

Note that in political science there is no overriding paradigm of "good political science." If there was, the various ideas could be compared to this benchmark and either accepted or rejected. Instead, many ideas—many models of what is "good political science"—are allowed to co-exist. The prevailing condition in political science is one where many models, many paradigms, are competing. This type of situation can be seen in many of the other social sciences.

The "Other Part" of the Physical Sciences

While the social sciences were trying to create their disciplines and establish credibility by adopting the physical sciences' paradigm, the physical sciences were discovering that the classical paradigm had limitations.

In the physical sciences—taking physics as an example—it is now recognized that there are two overarching classifications of problems. First there are the problems amenable to the techniques of classical physics. In classical physics, nature behaves in accordance with fixed laws of behavior, and causality models can be formulated which completely describe the cause-and-effect relationships within a given system. Often these causality models can be expressed in terms of relatively simple mathematical relationships or laws of behavior. Examples would be Newton's Laws of Motion or fluid dynamics' equations of laminar flow. As discussed previously in the section on the construction of theory, these laws can be used to predict the future state of the system.

The physical sciences in general have been blessed with a broad and rich universe of practical problems that are amenable to relatively simple causality models. Fortunately for the beginnings of science, most of the immediate problems addressed by scientists and engineers—how to float a boat, make a steam engine, light a light bulb, forge a plow—were problems amenable to simple methods. The classical scientific methodology was thus able to crawl before it was called on to walk. It achieved great prestige in solving these relatively undemanding problems. The underlying scientific models have proven their predictive power, and currently form the basis of the practical sciences of engineering, manufacturing, and other daily pursuits.

However, there is also another set of problems in the hard sciences. There are problems, just as numerous, that have proven totally intractable to the classical methods of science. These are the non-classical problems in the physical sciences.

By non-classical I mean problems of systems behave chaotically or in a complex manner. Because of extreme sensitivity to initial conditions, future states of the system cannot be predicted exactly, if at all. For example, a scientist specializing in fluid flow can deal quite nicely with mass flow rates and fluid velocities as long as the flow remains laminar; should the flow transition into turbulence, his instruments can never measure to a sufficient accuracy to allow the future states of the system to be predicted. Meteorologists can predict the weather for, at best, a few hours; predictions for next week are often embarrassingly wrong, in spite of employing batteries of sensitive instruments, radars, satellites, and sophisticated computer models.

So, historically, there was first a paradigm—the classical science paradigm—that developed a record of success within a certain domain of problems. The scientific community at one time thought that it was just a matter of time before thoughtful scientists would be able to address all scientific problems using the same approaches. The classical science paradigm set the standards for good scientific inquiry. Then, there appeared a set of problems that challenged the universality of this approach. Waldrop (1992, p. 64) relates:

> In part because of their computer simulations, and in part because of new mathematical insights, physicists had begun to realize by

> the early 1980s that a lot of messy, complicated systems could be described by a powerful theory known as "nonlinear dynamics." And in the process, they had been forced to face up to a disconcerting fact: that the whole really can be greater than the sum of the parts. Now, for most people that fact sounds pretty obvious. It was disconcerting for the physicists only because they had spent the past 300 years having a love affair with linear systems—in which the whole is precisely *equal* to the sum of the parts.

Gleick (1992, p. 12) reinforces the idea that there are classical and non-classical realms in the physical sciences when he writes:

> Where chaos begins, classical science stops. For as long as the world has had physicists inquiring into the laws of nature, it has suffered a special ignorance about disorder in the atmosphere, in the fluctuations of the wildlife populations, in the oscillations of the heart and the brain. The irregular side of nature, the discontinuous and erratic side—these have been puzzles to science, or worse, monstrosities.

In the face of this challenge, there have been many different reactions.

Most scientists continued to attempt to apply the old paradigm in the domain of problems where the paradigm still applied.

Others created theories that most would describe as "wild-eyed ranting," and the most charitable would consider to be "out of the box."

Others created new metaphors, building linkages between things that no one before had considered as linked.

Others became what Bloom (1973, p. 22) chides as "mere rebels," who "childishly" invert conventional moral categories, challenging the dominant theories of science as social fabrications. The methodology of this group is to create fantasies rather than to rigorously test hypotheses about natural processes.

There is also the path of what Horgan (1996, p. 7) calls "ironic science." In this view,

> . . . ironic science resembles literary criticism in that it offers points of view, opinions, which are, at best, interesting, which provoke further comment. But it does not converge on the truth. It cannot achieve empirically verifiable surprises that force scientists to make substantial revisions in their basic description of reality.

However, in the main, the physical sciences adapted by understanding that there were two domains of problems, each with different characteristics. Perhaps it would be more accurate to say that the physical scientists embraced this solution. Many scientists eagerly addressed this new realm of problems as a new frontier. As part of this adaptation, the sciences of chaos and complexity theory developed, and have established an entirely new paradigm to govern its portion of the problem space.

To broadly generalize, there are two active domains in the physical sciences, the classical and the non-classical, each with their legitimate realm of problems. One would expect that the social sciences would sit up and take notice; indeed, some social scientists have. There is a growing literature on the application of chaos and complexity theory methodologies in the social sciences. However, the cross-pollination between the non-classical physical sciences into a presumed non-classical arena in the social sciences has been very limited. As I will discuss later, the social science literature in the areas of chaos and complexity or similar "non-classical" approaches is very limited, in scope, application, and accuracy. Non-classical metaphors have not caught on in the broad social science community.

I can postulate a few reasons for this. First, remember why the social sciences assumed the physical sciences' paradigm: (1) because it was inculcated in the general education of schoolchildren and thus something every social scientist grew up with; (2) because it demonstrably worked on a wide range of practical problems; (3) because of governmental pressures; and (4) because of a cross-pollination of physical scientists migrating to the social sciences.

Reasons 1 and 2 remain, pressing the social sciences to maintain the current course. There is no appreciable primary or secondary education in the non-classical physical science problems, and classical science proves its validity daily. Reason 3 remains intact; government generally has accepted classical science methodology, but your local congressman or senator is generally uneducated in non-classical problems. As for reason 4, I suspect that few non-classical scientists have migrated to the social sciences. This is because, first, there are few non-classical scientist to migrate, and second, since the non-classical sciences are advanced topics that are generally under-populated with investigators. Scientists who reach a level that they are able to study such problems generally have little motivation to change fields. They have not been unsuccessful in the physical sciences or driven to shift to the social sciences because they cannot overcome calculus or organic chemistry or whatever.

To these reasons can be added the cognitive reasons: the tendency for people to hang on to established mental models, and to filter out information that conflicts with it.

CHAPTER XII

SOCIAL SCIENCE: THE CRACKS IN THE VENEER OF THE CLASSICAL SCIENCE PARADIGM

Failure of Social Science Under the Classical Paradigm

Simon (1957, p. 42), in his classic book *Administrative Behavior,* indicated that there are "two indispensable conditions" necessary for empirical research in administrative science. The first is to define the objectives of the administrative organization in "concrete terms," and the second, "that sufficient experimental control be exercised to make possible the isolation of the particular effect under study from other disturbing factors that might be operating on the organization at the same time." Both of these are typical requirement of any classical science problem addressed under a reductionist metaphor. The problem is that, in the social sciences, they are both quite impossible to attain.

This has been recognized, at least for the last half century. Banfield (1957, p. 280) wrote, in a review of *Administrative Behavior*, that one could do nothing to eliminate "disturbing elements" that destroy the controlled environment of public administration studies. This conclusion can be generalized to apply to *any* social science experiment. It could be something so small, as Banfield noted, as a difference in personality of the test subject.

Banfield went on to acknowledge that even Simon recognized that "the principles of the usual theorists" (the "usual suspects," I gather, such as Gulick et al.) were "essentially useless."

Dahl (1947) identified three particular problems. His discussion of one of them deserves to be quoted in length.

> But most problems of public administration revolve around human beings; and the study of public administration is therefore essentially a study of human beings as they have behaved, and as they may be expected or predicted to behave, under certain special circumstances. . . . This concern with human behavior greatly limits the immediate potentialities of a science of public administration. First, it diminishes the possibility of using experimental procedures; and experiment, though perhaps not indispensable to the scientific method, is of enormous aid. Second, concern with human behavior seriously limits the uniformity of data, since the datum is the discrete and highly variable man or woman. Third, because the data concerning human behavior constitutes an incredibly vast and complex mass, the part played by the preferences of the observer is exaggerated, and the possibilities of independent verification diminished. Fourth, concern with human action weakens the reliability of all "laws of public administration," since too little is known of the mainsprings of human action to insure certitude, or even high probability, in predictions about man's conduct.

Dahl went further to point to the work of Lasswell (1930), which Dahl identified as "pioneering," which "described the irrational and unconscious elements in the successful and unsuccessful administrator"—in other words, confirming that there was a problem in identifying and measuring human behavior. This is not quite the identification of human behavior as "non-linear, discontinuous, stochastic, and complex," which are characteristics associated with deterministic chaotic behavior, but it is very close.

So, many of the luminaries of the founding period of Public Administration understood that there were problems in applying classical science methodologies in the social science realm. The difficulties with performing social science research to the same standards as held in the physical sciences—to emulate the physical sciences' paradigm—is one of those "dirty little secrets" that all know of but few acknowledge. There are no instruments to measure human commitment, or anger, or dedication, or any of the other elements of human behavior that can have such an impact on the course of human affairs. We are left with the human judgment of the experimenter, which, in many cases, is judged to be a dispassionate instrument based on the experimenter's word that their judgment was unbiased. Those that are less convincing are taken to task by the standards of the paradigm. For example, Yin (1989, p. 21), in the context of case study research, relates:

> Perhaps the greatest concern has been over the lack of rigor in case study research. Too many times, the case study investigator has been sloppy, and has allowed equivocal evidence or biased views to influence the direction of findings and conclusions. . . . What is often forgotten is that bias can also enter into the conduct of experiments (see Rosenthal, 1966) and in using other research strategies, such as designing questionnaires for surveys (Sudman & Bradburn, 1982), or in conducting historical research (Gottschalk, 1968).

To put a slightly different viewpoint on the issue, the work of Rosenthal, Sudman & Bradburn, and Gottschalk referred to in the passage can be seen as efforts to police the disciple to the standards of the reigning paradigm. The fact that the problem seems so pervasive can be approached from two directions: either the investigators were inferior or lazy scientists and incompetent to execute research, or there exists particular difficulties in applying the physical science paradigm to the social science sort of problem.

As for the first possibility, Deming (1986) teaches that whenever there is a consistent problem within a system, it is generally the system's fault. People want to do a good job; and, in the academic business, where reputation is so valued and so fragile, scholars would certainly want to execute their studies as rigorously as possible. If a consistent and enduring problem emerges, as Yin says exists in the field of case study research, Deming would guide us to the belief that the problem lies with the standards, not with those attempting to execute the standards. This suggests that the paradigm is at fault, not the people.

One response to these problems from the social science community has been to develop extensions of classical methodologies. Simon (1958, p. 61), in a response to Banfield's criticisms, drew an analogy with celestial mechanics as a "nonexperimental science," a science based solely on observations. He stated:

> We badly need reliable general methods for observing and drawing inferences from the single case; but we need methods that are more objective, less subject to the "filtering" of the

> observer, than those the arts of history, biography, and journalism have provided us.

Simon's answer was to *extend the rules of the current paradigm* by developing new "normal science" rules for this new environment. Simon went on to assert that "studying important matters [in administrative science] scientifically"—i.e., using the Scientific Method paradigm—"is a steep and rocky [route], but the only one, I am convinced, that leads to our destination."

This behavior is the same response as outlined in Kuhn's observations on a paradigm under assault: when anomalies emerge, the first attempt is to fix the existing paradigm.

That the "fix" was not effective can be seen by the controversy that surrounded dissertation research in public administration in the late 1980s. Articles by McCurdy and Cleary (1984), and White (1986), in *Public Administration Review,* revolved around the issue, "Why Can't We Resolve the Research Issue in Public Administration?" White (p. 232) stated: "Much of the dissertation research fails to satisfy the criteria for mainstream social science research," in agreement with McCurdy's and Cleary's (p. 50) conclusion that "few of these doctoral projects meet the criteria that conventionally define careful, systematic study in the social sciences." White (p. 229) reported:

> Roughly half of the research does not conform to the standards of mainstream social science and therefore does not have the potential to contribute to the growth of knowledge in public administration, at least as knowledge is defined from the mainstream perspective.

Obviously, these quotations indicate a problem with the basic paradigm in social science, at least in the field of Public Administration. If there is anyplace where a dominant paradigm should be able to police its ranks, it should be among doctoral candidates. The criticism that the research "fails to satisfy the criteria for mainstream social science research" indicates a defection by the student's committee chairmen, who allowed as acceptable research not in accordance with traditional research values. This indicates a fundamental weakening in the classical paradigm among those that would be most expected to enforce it.

This problem is not unique to public administration. Andreski (1972, p. 11, 16) extended the problem over all the social sciences. He contends that 95% of the social science research is "re-search for things that have been found long ago and many times since," and that the average quality of publications has declines "in a number of fields." Most telling, he claims that "one could spend a whole life and fill and encyclopaedia trying to expose all the foolish antics which pass for a scientific study of human conduct."

The anomalies found in the social sciences have driven some to challenge the basic paradigm. In 1988, Cleveland (pp. 681-686) wrote the type of strangely contradictory article that one would expect from one who's basic mental models have been challenged and found wanting. He asserted that "The 'inner logic' of the Scientific Revolution is now in serious question, and a new kind of Reformation is seemingly at hand." His article was a marvelous example of someone who has seen the anomalies, found the current paradigm inadequate, yet had nothing to put in its place. Instead, he proposed a kind of scientific nihilism: "it is time to put behind us the idea that the politics and administration of human endeavors are some kind of science." He correctly identified the existence of anomalies and the current paradigm's inadequate response to them: "What can't be counted is treated as an 'externality'—that is, a disturbingly relevant factor that doesn't fit the current fashion in systematic thought and is therefore put on the shelf to think about later."

Cleveland did not see the full implications of what he was observing. While in the one hand he favored sweeping the current paradigm off the pedestal, he did not see what was needed as a replacement. Instead of a clear concept, he trotted out a strange mixed breed of ideas. First, he offered something like nihilism or postmodernism ("Above all, let's put behind us the idea that the politics and administration of human endeavors are some kind of science"); then appealed to morality ("an emerging ethic of fairness"); then, like Pilate, he washed his hands of it all ("science leads us to . . . technoporn").

What makes his views internally inconsistent is his denial, on the one hand, of any possibility of a social science, while at the same time acknowledging: "Aristotle in his *Politics* and Lao Tsu in the *Tao Te Ching* were already giving advice that is helpful today in the administration of complexity." One wonders why he ignored the possibility that the advice of these luminaries could form the basis of a substitute science of administration—perhaps, its "first principles"—given that they are so enduring.

But the point here is not what Cleveland is proposing, but rather what he is disposing: in his mind, the Scientific Method paradigm has been rejected.

Yet, there remain a hard core of individuals still dedicated to the existing system. The problem of anomalies has not reached crisis proportions, at least to them. The majority of social scientists have yet to put to the question their training in the fundamental assumptions of the sciences. There are not yet enough social scientists screaming that their methodology is deficient and that the issue is important. However, the Scientific Method is no longer so dominant as to be able to exclude other methodological paradigms. Prime evidence for this can again be found in the standards for writing doctoral dissertations, which should be seen as a major gatekeeper of scientific standards. At least one guidebook, Davis' and Parker's (1997, pp. 68-71) *Writing the doctoral dissertation*, explicitly gives the alternatives:

> *Positivist versus postpositivist:* the scientific method is positivist: A problem is identified, data are collected, and hypotheses are formulated and empirically tested. There is a strong positivist, quantitative research tradition in the sciences. . . . Many problems, especially in the social sciences and humanities, do not lend themselves to hypothesis testing and statistical analysis. Other methods of collecting and analyzing data reflect the richness of complex human situations and allow the researcher to apply alternative interpretations or develop alternative theories. The orientation of the post-positivist approach is qualitative, rather than quantitative.

However, even given their apparent acceptance of alternatives to the Scientific Method, Parker and Davis still acknowledge that there is some remaining gatekeeping power in the Scientific Method paradigm. In discussing the reasons for choosing one methodology over the other, they assert that "using positivist methods in the dissertation is often better for career development. . . . A positivist dissertation is easier for colleagues to understand. It is easier to defend and to get results published."

In public administration, alternative methodologies to the Scientific Method appear ingrained and accepted as a natural part of the landscape of the field. For example, a 1995 article by Behn (pp. 313-315) identified "The Big Questions of Public Management." There was the "micromanagement" question, the "motivation" question, and the "measurement" question, all typical questions of organizational behavior. The "measurement" question, which asked "how public managers can measure their achievements of their agencies in ways to increase those achievements," dealt more with motivational theory and the challenge of identifying measures of effectiveness, with no reference to problems like the "butterfly effect" or the Hawthorne-Western Electric effect or anything other than how to go from achievement to measurement to more achievement. His discussion of the scientific method paid more attention to "scientific luck" and skirted the issue of methodology, implying that proper methodology did exist, it was just an issue of selecting the right one.

Instead of an uproar over the inconsistences of having two different guiding paradigms for research, there appears in this case to be passive acceptance. Where some are outraged, some just consider two methodologies in one field to be perfectly acceptable, a natural part of the scientific landscape. To this group, the Scientific Method works just as well as any alternative approach—it is just a matter of style.

However, the evidence of anomalies when employing the Scientific Method in social science is building. Bowditch and Buono's (1994) narrative of the history of organizational behavior theories is a catalog of successive unsuccessful attempts to apply the Scientific Method to organizational behavior problems. A quick leafing through *A Primer on Organizational Behavior* reveals at least three dozen theories or models that have been advanced at one time or another, only to be greatly modified or superseded when confronted by experimental results.

Often, a field will have a number of theories active simultaneously, sometimes with competing viewpoints, sometimes complementary, but usually contradictory. There has been little success in adjudicating between competing theories.

For example, Bozeman and Bretschneider (1994, p. 207) reported on a theory that higher levels of red tape are found in public organizations because of a lack of goal clarity. However, Rainey (1983) presents counter evidence. In a different area, Lowery (1993, p. 202) documents where different investigators were coming to diametrically opposed conclusions, even when the experiments were "by authors employing a common . . . approach," in this case, the New Institutionalism. Lowery (p. 206) concluded that, "until we elaborate and reconcile the methods, substantive foci, and the broader theoretical context of our research program on bureaucratic politics," there would continue to be a shortfall in the intellectual rigor and consistency of these investigations.

Moe (1994, p. 17), in an article discussing "the state of theory in public administration," claimed that not much progress had been made on the political foundations of bureaucratic organizations "due to underlying weaknesses in the field's theory and methods."

As was mentioned earlier, the physical sciences are blessed with a number of simple problems with simple solutions—"starter" problems, if you will—upon which to establish methodologies and the practice of theory building. However, there is no equivalent body of "starter" social science problems. Ijiri and Simon (1977, p. 1) comment on the situation this way:

> The patterns that have been discovered in social phenomenon are much less neat. To be sure, economics has evolved a highly sophisticated body of mathematical laws, but for the most part, these laws bear a rather distant relation to empirical phenomena, and usually imply only qualitative relations among observables.

A similar progression is repeated in other branches of the social sciences. A model or theory is advanced with enthusiasm, tested with hopefulness, and then eclipsed by some experimenter's data. There are continual attempts to square theory with fact, with little success. *Primer* (1994, p. 239) is replete with statements of capitulation, like the following taken from the section on leadership: "While we have numerous middle-range theories and models of leader behavior, the field has been less successful in creating a comprehensive, generalizable model of leadership." And, in fact, an examination of the "middle-range theories" shows that they are more exercises in description of special circumstances rather than causal models that have predictive value.

While the mainstream of the behavioral sciences seems to have discounted the existence of classical principles of human behavior, as late as 1995 there still was evidence that the roots of the classical science paradigm were still so deep as to stimulate attempts to find such laws of behavior. Behn (1995, p. 322) reported in an article in Public Administration Review on the "Big Questions of Public Management," that "'Scientific Management' may have lost much of its intellectual stature, but its legacy lives on; people still search for the 'one best way.'"

For another, more specific example, Bergquist (1996, p. 578) relates:

> Tom Peters acknowledges that in the early 1980s he knew something about how organizations achieved excellence (Peters and Waterman, 1982). By the late 1980s, he discovered that he had been mistaken. Many of the "excellent" organizations of the early 1980s had become troubled institutions by the late 1980s. Other theorists and social observers have been similarly humbled by the extraordinary events of the 1980s and the early 1990s.

Bowditch and Buono (1994, pp. 20-21) expressed the general problem as follows:

> The move toward increased empirical research produced inconsistent support for many of our generalized concepts. In

> other words, while these general theories were supported in some studies, other research efforts produced different results. The major impact of these trends has been the move toward more complicated models of human nature and the resultant implications of how to manage these individuals. A set of complex assumptions about people began to emerge.

The reaction of social scientists to the failure of their theories to hold up to repeated examination has usually been to either reject the underlying theory as flawed, to criticize the experiments as poorly conducted, or to assume that the system was more complex than first believed and that other variables disturbed the experiment. The other possible alternative—that simple principles could result in complex or chaotic behavior—is not mentioned.

Recall Ptolemy's astronomy. First, there was a theory; then, when observations were taken, there emerged anomalies. The first reaction was to add additional layers of complication to the basic theory to account for the anomalies. In the social sciences, initially there were simple assumptions about human behavior; then, increased empirical research that produced inconsistent support for these assumptions. A set of more complex assumptions about people begins to emerge.

The parallel between these descriptions is striking.

In certain social science fields, the domain of solvable problems has been embarrassingly small. For example, economics (called "the most rational of the social sciences") can model supply and demand for a single product, say, milk; and perhaps even two semi-competing products, milk and skim milk; but, according to Saari (1995a), the moment we try to add a third or fourth or (horrors) try to model the behavior of the total market for beverages, our models give results that could just as easily have been the output of random (or chaotic) number generators.

The above example is from the area of microeconomics. Some feel that it is unfair to judge economics on its ability to predict small changes in price or demand for small niche products. However, they feel more confident in predictions for the economy as a whole, presumably because the "law of large numbers" would come into play and the twitches of small parts of the economy would be smoothed by looking at the economy as a whole. Unfortunately, economic predictions do not have a good track record. For example, according to the *Elliott Wave Financial Forecast* (2001), a publication that tracks economic predictions, "In January 2001, only 1 economist in 50 called for a recession. They've changed their minds only after the evidence became indisputable."

Various economic rules of behavior that have been given the aura of laws have fallen in the last several years. One such set has to do with linking the stock market performance with the interest rate actions of central banking. According to Fosback (1985, p. 41), the "Two Tumbles and a Jump" rule states that when the Federal Reserve eases the monetary climate by decreasing one of the three basic policy variables (discount rate, margin requirement, or reserve requirement) two times in succession, conditions are favorable for an ensuing jump in stock prices. This rule was wrong in the 1990s. The Japanese central bank dropped interest rates from 3 ½% to 0% in over nine rate cuts, and yet the Japanese Nikkai Index fell from over 32,000 to under 15,000. Ten years later it has yet to recover. Similarly, in 2000-2002, eleven successive Federal Reserve rate cuts, beginning from 6 ½% down to 1 ¾%, did not prevent the NASDAQ index from being cut in half.

As an over-generalization, which I believe to be more than less accurate, the predictions of economists have been accurate mostly when economic conditions have been stable. And, generally, those predictions were for more stability. According to the Elliot Wave Theorist (2001), economic predictions during periods of change have mostly been little more sophisticated than straight-line projections off historical data.

Economics has not been able to handle the rich diversity of human behavior. Brian Arthur is one of the economists who have been frustrated by such limitations. His views on this problem, quoted in Waldrop (1992, pp. 22-23), have fascinating applicability to the issues of this dissertation.

> In the lecture halls of Berkeley, economics seemed to be a branch of pure mathematics. "Neoclassical" economics, as the fundamental theory was known, had reduced the rich complexity of the world to a narrow set of abstract principles that could be written on a few pages. Whole textbooks were practically solid with equations. The brightest young economists seemed to be devoting their careers to proving theorem after theorem after theorem—whether or not those theorems had much to do with the world. . . . The mathematical economists had been so successful at turning their discipline into ersatz physics that they had leached their theories clean of all human frailty and passion. Their theories described the human animal as a kind of elementary particle: "economic man," a godlike being whose reasoning is always perfect, and whose goals are always pursued with serenely predictable self-interest. And just as physicists could predict how a particle will respond to any given set of forces, economists could predict how economic man will respond to any given economic situation: he (or it) will just optimize his "utility

> function." . . . [Arthur] just couldn't buy it. Granted, the free market was a wonderful thing, and Adam Smith had been a brilliant man. In fairness, however, neo-classical theorists had embroidered the basic model with all sorts of elaborations to cover things like uncertainty about the future, or the transfer of property from one generation to the next. . . . But none of that changed any of the fundamental assumptions. The theory still didn't describe the messiness and the irrationality of the human world that Arthur had seen [as a business consultant] in the valley of the Ruhr—or, for that matter, that he could see every day on the streets of Berkeley.

There are several things to note in this passage. First, there is the adherence of neoclassical economics to the classical Scientific Method paradigm—in this case, close adherence to Reynold's second type of theory, that is, theory based on an interrelated set of definitions, axioms, and propositions, the axiomatic form of theory.

Second, there is the complicating of the theory, making it "embroidered," in order to account for anomalies while still retaining the same fundamental assumptions.

Last, there is the failure of the theory to deal with the messy data of real life. Arthur went on to assert, in the context of a particular real-world problem upon which he was working,

> . . . the quantitative engineering approach—the idea that human beings will respond to abstract economic incentives like machines—was highly limited at best. Economics, as any historian or anthropologist could have told him instantly, was hopelessly intertwined with politics and culture. . . . You could measure something like income or childbearing in one country, and find that another country had the same levels of one, and totally different levels of the other.

Simon (1995, p. 290) discusses one of the key assumptions (and problems) in neoclassical economics.

> The difficulty that economics has had in giving a good account of organizations and their predominance is traceable in no small part to the fascination of economists with systems in equilibrium. Analysis under the assumptions of perfect knowledge and certain expectations has little relevance, surely, for such issues of economic organization as explaining how an economy is structured between organizations and markets. Prices provide only one of the mechanisms for coordination of behavior, either between organizations or within them. Coordination by adjustment of quantities is probably a far more important mechanism from a day-to-day standpoint, and in many circumstances will do a better job of allocation than coordination by prices. . . . From a conceptual standpoint, it is entirely feasible to construct economies in which prices are based on costs and final demands are limited wholly by budget constraints, with demand vectors that are otherwise insensitive to prices, provide the information for coordinating these systems. Many observers of business scheduling and pricing practices have claimed that (with the possible exception of the agricultural and mining sectors) models that use quantities as signal approximate first-world national economies more closely than do models in which prices are the principle mechanisms of coordination.

In other words, in order to obtain problems amenable to current tools and methodologies, economists have made some extra-ordinary assumptions. These assumptions depart from what we observe in the real world, and ignore significant possibilities. Just as the old paradigm of caloric molded the tools and defined the experiments, here too the paradigm has molded the problem.

Dahl (1947, p. 7) attacked any tendency of those in public administration to follow the path of economics. He stated:

> We cannot achieve a science by creating in a mechanized "administrative man" a modern descendent of the eighteenth century's rational man, whose only existence is in books on public administration and whose only activity is strict obedience to "universal laws of the science of administration."

He concluded his article:

> No science of public administration is possible unless: (1) the place of normative values is made clear; (2) the nature of man in the area of public administration is better understood and his conduct is more predictable; and (3) there is a body of comparative studies from which it may be possible to discover principles and generalities that transcend national boundaries and peculiar historical experiences.

One of the most important ideas embedded in the Scientific Method is, as stated by Pruitt and Snyder (1969, p. 2), that "A good theoretical proposition must be capable of passing an *empirical test* by which it is confirmed or disconfirmed."

The problems with economics have been featured here for two reasons: first, because economics is the most "rational" of the social sciences, where one would expect to see the classical scientific paradigm most comfortably employed. The failure of the Scientific Method to generate explanations for human behavior is, thus, a telling blow against the applicability of the classical paradigm to the scientific investigation of human behavior in general. Second, the action of economists in defense of the established paradigm adheres closely to Kuhn's descriptions of a paradigm in transition. Anomalies emerge, followed by elaboration of the existing theory attempting to make it right by adding another layer of complication.

This situation is not restricted to economics. There has been a failure of any number of attempts to build theories of human behavior. Management theory has been particularly unsuccessful in establishing an enduring linkage between theory and practice, to the extent that the field has been dominated by a succession of transitory fads, heavily promoted by management "consultants."

Abrahamson (1996, p. 254) addressed this phenomenon.

> Management fashion setters disseminate management fashions, transitory collective beliefs that certain management techniques are at the forefront of management progress. These fashion setters—consulting firms, management gurus, business mass-media publications, and business schools—do not simply force fashions onto gullible managers. To sustain their images as fashion setters, they must lead in a race (a) to sense the emergent collective preferences of managers for new management techniques, (b) to develop rhetorics that describe these techniques as the forefront of management progress, and (c) to disseminate these rhetorics back to managers and organizational stakeholders before other fashion setters.

This kind of "pop theory" dynamic could only develop in the absence of a paradigm that establishes credible theory and enforces the discipline of a normal science.

Abrahamson (1996, pp. 257-258) used the history of Quality Circles to exemplify this process. In the early 1980's, Quality Circles were heavily promoted as a leading edge management technique. Articles on Quality Circles were a prominent part of academic and business journals, peaking at over 40 articles in 1981. By 1986, the number of articles on Quality Circles had declined precipitously.

A survey indicated that more than 80% of the Fortune 500 companies that had embraced Quality Circles had abandoned them by 1987.

Clearly, there was no anchor of an established management paradigm when so many fads and fashions could find a ready audience among managers, many of whom were trained in management theory by reputable academic institutions.

There appears to be an increasing recognition of anomaly in the social sciences. LaPorte (1994, pp. 7-10), for example, prepared a discussion paper for a panel discussion on the state of theory in public management and bureaucracy. In it, he stated:

> What we draw from sociology, social psychology, and economics is fractured, and when it joined with concerns for problems of operations or political ideology it splays out in a messy pattern with limited cumulative effect. . . . Our grasp of the dynamics of public organizations is slipping further and further away; *we know less of what we need to know, even as we know more than what we did*—and even as prescriptions for change and improvement proliferate.

He went on to ask to what extent, based on current theory, could the capacity of public organizations to operate coherently under the new conditions be predicted. His response was not encouraging. In high reliability organizations, he "found unexpected behavioral responses in decision making, in patterns of authority, in processes of discovery . . . and in responses to regulation." He quoted four publications underscoring this statement. He concluded that he "could derive only modest assistance from the empirical and theoretical literatures in providing plausible hypotheses or explanations."

For clarification, a more rigorous definition of a "failed" theory is appropriate here. Taking one specific example, that of management science, I can offer two definitions of "failed."

One definition would involve a theory that cannot explain, or is disconnected from, the real world that is supposed to be explained. Priesmeyer (1992, p. 153) is so bold as to place all of the management theories developed in academia in this category.

> Academic texts provide a collection of well-accepted concepts that constitute the most common denominators in the field; they contain those things that most academicians recognize and accept. What is in the academic texts, however, is *not* what is being discussed in corporate offices; the current hot management topics in industry are found on the back shelves of commercial bookstores. On those shelves one finds a collection of unconventional, highly effective approaches to management, sold by consultants and practiced by some. That is where one finds the real innovations in human resource management. The difference between what is taught and what is practiced is probably greater today than at any time during the past twenty years.

A second definition for a failed theory could be "superseded by another theory that claims to better describe the system being examined." For example, Priesmeyer (1992, p. 154) provides a list of a succession of management theories spawned and rejected:

Scientific Management	1912
Human Relations Movement	1927
Bureaucratic Management	1947
Fayol's Principles of Management	1949
Systems Theory	1960
Management by Objectives	1965
Intrapreneurship	1985
Empowerment	1990
Total Quality Management	1991

Priesmeyer (1992, p. 155) goes on to explain this phenomenon: "The acceptance of new ideas depends upon experience with older ones. Old ideas are modified to create new ones. The history of management is a trail of ever-changing concepts and practices."

One cannot be anything other than struck by the fit between this sentiment and what Kuhn expressed earlier, that the adherents to existing paradigms man the ramparts and continually modify their paradigm in an attempt to make it viable in the face of new anomalies. This process seems remarkably similar to the actions of the proponents of Ptolemy's heliocentric universe. As their data showed more complex behavior than could be explained by their theory, their solution was to add more complexity to the theory. Initially no thought was given to attempting another solution apart from the current paradigm.

Strategic Planning: A Case Study

One particular example of this process is seen in the field of Strategic Planning. A good source for information in this area is Mintzberg's (1994) disarmingly honest book, *The Rise and Fall of Strategic Planning.* In it he describes the progression of the ideas of strategic planning in the business world.

When formal planning was first introduced in the post-World War II years, there was no shortage of enthusiasm for the concept. Academics and consultants embraced the concept with gusto. Mintzberg (1994, pp. 91-95) measures this enthusiasm by the length of the bibliography in a book by Steiner, *Top Management Planning*, which appeared in 1969: the list of publications filled 38 pages.

Of the initial studies, most were "strongly favorable" to the theory that "strategic planning was good for organizations." However,

> . . . the first crack appeared in a paper by Rue and Fulmer . . . which concluded that while planning paid in durable goods businesses, a lack of planning paid in service businesses, and it was not clear which paid in nondurable goods businesses. From there, the relationship was up for grabs. In 1975, Malik and Karger found favorable results, while Sheehan (1975) and Grinyer and Norburn (1974) . . . did not. Later, Wood and Forge (1979) found a positive relationship for large banks, followed by Robinson and Pearce who found (1983) no such relationship for small banks.

Studies continued on motor carriers, dry cleaning businesses, and others. The research continued, with the addition of more and more variables, with the results more and more variable. They ranged from "no relationship," "weak," to "strong." Some researchers found the strategic planning process mandatory for a company if it expected to survive; some found that such planning did nothing other than provide a record of decisions; some found that planning was not of value, but the process was retained by some companies as a means of political control.

Clearly the body of evidence appeared inconsistent. Anomalies were jumping out of the woodwork and bashing each other on the head fighting for attention: why was strategic planning good for big banks, and yet not good for small banks?

From the viewpoint of anyone who is looking at the situation from a paradigm shift viewpoint, the predicted next step after recognition of anomalies would be that the basic theories of strategic planning would be made more complicated, and new planning models developed.

That is what occurred. Mintzberg (1990) lists ten different approaches that eventually emerged in what he refers to as "schools of thought" on strategic planning. These schools of thought generally aspire to the status of theory by assuming the trappings of theory. Mintzberg (1994, pp. 2-4) provides a short summary of each.

> Three are prescriptive, seeking to explain the "proper" ways of going about the making of strategy. The first I call the "design school," which considers strategy making as an informal process of conception, typically in a leader's conscious mind. The design school model . . . also underlies the second, which I call the "planning school" and which accepts the premises of the former, save two. . . . The third, which I call the "positioning school," focuses on the content of the strategies (differentiation, diversification, etc.) more than on the processes by which they are prescribed. . . . [S]even other schools of strategy formation [are] schools more *des*criptive than prescriptive in nature . . . the "cognitive school" considers what happens in the human head that tries to cope with strategy; the "entrepreneurial school" depicts strategy making as the visionary process of a strong leader; the "learning school" finds strategy to emerge in a process of collective learning; the "political school" focuses on conflict and the exploitation of power in the process; the "cultural school" considers the collective, cooperative dimension of the process; the "environmental school" sees strategy making as a passive response to external forces; and the "configurational school"

> seeks to put all the other schools into the specific episodes in the process.

Thus, we see the paradigm shift process at work: the initial theory failed when confronted by the facts, and as a result there emerged a bevy of competitors, generally each more complicated than its predecessors, attempting to reconcile the data. And, predictably, there is increasing complexity. The early "Design School" model could be outlined in a flow chart containing 11 processes; the later "Ansoff Model" contained 57 processes; "The Planning Process at Kaiser Aluminum" contained 86 (Mintzberg, 1994, pp. 37, 41, 50). The increasing complexity of the theories can be likened to the spheres within spheres in Ptolemy's model of the universe.

The other interesting thing is that Mintzberg could discuss the ten different approaches as if they coexisted in time. How could we have ten different best strategic planning processes existing simultaneously? Why weren't the disproved methodologies rejected? From a viewpoint where we attempt to apply the rules of "good science," one reason might be that each system is so specialized and complicated that they never are subjected to test in their purest form. After all, strategic planning is a complicated process that must be executed in fairly large human organizations that have objectives other than to test a theory. So, even if some test results might contest the claims of the method's proponents, the tests are never pure enough to meet scientific criteria for a proper experiment.

From a paradigm shift viewpoint, the observation is that so many theories could not exist unless there was no governing paradigm strong enough to discipline the academic process—there was no standard separating the wheat from the chaff. Mintzberg essentially documented ten different approaches to the same problem that appear totally self-referential, learning nothing from the earlier theories other than to avoid (or embellish) their particular approach. Without a paradigm strong enough to be able to boot a competitor off the pedestal, many paradigms can aspire to occupy the place at the top, simultaneously.

Over it all—from a careful reading of Mintzberg's work, and a sampling of some of the works that he quoted (see Fulmer & Rue, 1974; Starbuck, 1985)—there appears to be two additional things happening. First, studies did not learn from each other. It is like a mechanic trying out a tool, finding it doesn't work, and throwing it away without first discerning why it didn't work.

Second, in the face of this steady stream of contradictory results, no one seems to have questioned, "Why?" Why does one study say that banks need strategic planning, and another say that banks do not need strategic planning? No one, to my knowledge, ever addressed, much less challenged, the rules of the game, the *very assumptions inherent in the classical causality models that were generating these conflicting results*.

This problem is not just prevalent in the field of strategic planning. Similar situations occur in all the social sciences: theory, test, confirmation, retest, failure.

Yet no one appears to question that causality may be working in a different manner that the common deterministic models that we know and love; no one appears to question that the Scientific Method may be leading us to errors in thinking.

Going from this specific case to the general, I believe that the diagnosis could be applied to all the social sciences.

Just as another example, Priesmeyer (1992, pp. 154-155) talked about a similar phenomenon in the management field. In the following passage, he unwittingly reinforces this diagnosis:

> *Empowerment* and *total quality management* are continuations of the intrapreneurship theme—empowerment being the vesting of substantial decision-making authority in employees and total quality management being a philosophy and management system that calls for personnel participation with a focus on continuous improvements in quality. Total quality management requires empowerment; empowerment is an attempt to capture intrapreneurship quality in personnel. The acceptance of new ideas depends upon experience with older ones. Old ideas are modified to create new ones. The history of management is a trail of ever-changing concepts and practices. . . . We cannot expect consistency; we must expect change.

The line that "old ideas are modified to create new ones" is certainly disingenuous. It is a whitewash over the fact that studies appear to be consistently coming up with different results. Certainly, if one were to suggest to a physicist that the laws of motion should be continually updated to reflect current conditions, the response would be that it is the object of scientific laws to predict current conditions, not to adapt to them.

That Priesmeyer would think nothing of an environment of continually changing management theory to encompass the passion du jour is a confirmation of two points: first, there exists a weakness in the current paradigm's ability to police the discipline; second, there exists among many scholars a dogged refusal to consider that something else might be happening.

Kuhn (1970, p. 52) stated: "Normal science does not aim at novelties of fact or theory and, when successful, finds none." In these branches of the social sciences, it is clear that many novelties have been found. The initial reaction of scientists steeped in the current paradigm is either to complicate existing theory to cover the anomaly, or to assemble their own theory (*theory du jour*). Clearly, these branches of the social science have not found success in their theory formulation. Clearly, not all of the inconsistency in results can be attributed to poor experimental method. Clearly, there is something else going on.

Metaphors in One Branch of the Social Sciences: Public Administration

The metaphor has served as an important way of studying public administration, and is used extensively in other social sciences.

As early as 1897, Muller contended that metaphors exert an important influence on the development of language. In 1954, Burke added that metaphors help forge connections about reality.

In 1966, Hesse contended that metaphors have played an important role in the development of science. He beehives that metaphors serve as an image for studying the subject, and the basis for detailed scientific research. The logic of the metaphor suggests that no one metaphor can capture the total nature of organizational life. New ones see into the blind spots of others.

Hill (1991, p. 262) quotes Gareth Morgan in saying that "the use of metaphor implies *a way of thinking* and *a way of seeing* that pervades how we understand our world generally."

Hill (1991, pp. 261-294) discussed the role that metaphors have in providing order in a world over saturated with stimuli.

> Many . . . are best thought of as "images of politics," which, despite their theoretical inadequacies, people use to develop "pictures in our heads" that form a "pseudo environment," in Walter Lippman's terms, that helps make sense of the swirling mass of political stimuli in which they are exposed. Psychologists discuss this phenomenon in similar terms, saying that once human beings attend to a given stimulus they must then integrate it into their "perceptual set," that is, their predisposition to interpret events in certain ways based upon past experience and the context of the situation. Talk of politics as a "game" or a "system," for example, uses metaphorical language that plays a crucial role in thought processes.

Hill (1991, p. 292) goes on to place the metaphor as a critical tool in understanding the world. "The prevalence of such metaphors in the social sciences tends to reinforce Lippman's conclusion that they are necessary." He then quotes Lippman (1922, p. 16):

> For the real environment is altogether too big, too complex, and too fleeting for direct acquaintance. We are not equipped to deal with so much subtlety, so much variety, so many permutations and combinations. And although we have to act in that environment, we have to reconstruct it on a simpler model before

> we can manage with it. To traverse the world men must have maps of the world. Their persistent difficulty is to secure maps on which their own need, or someone else's need, has not sketched in the coast of Bohemia.

Treadwell (1995, p. 91) underscores the importance of metaphor in our basic language when he asserts: "When we speak of what something means, we are always referring to a relationship between similarities and differences." He further observed that much of the public administration discourse is in terms of bi-polar "comparisons of contrasting relationships that are framed in a language of dichotomous symbolism." Examples of this frame of thinking are such familiar constructs as Treadwell's (1995, p. 91) list of "supervisor-worker, democracy-autocracy, teacher-student, public-private, democracy-autocracy, conservative-liberalism, patronage-merit, individualism-collectivism, and centralized-decentralized."

In organizational theory, there has been a flood of metaphors. The classical management theorists like Taylor, Fayol and Weber employed a machine metaphor. The "organization as an organism" depicts the organization as a living thing attempting to survive in the context of a wider environment, and has been employed by the Structural Functionalist Selznick (1948), by Parsons (1956), by the Socio-technical Systems school pioneered by Trist (1951), in the general systems approach formulated by Katz and Kahn (1966), and in the Contingency Theory of Burns and Stalker (1961), which carried the organic metaphor to its logical conclusion.

Other metaphors have ranged widely: organizations have been likened to cybernetic systems (Hage, 1974; Argyris, 1978), loosely coupled systems (Weick, 1976), ecological systems (Hannan & Freeman, 1977), orchestras (Drucker, n.d.), theaters, political systems, and (my personal favorites) psychic prisons and instruments of domination. There have been cultural metaphors, concentrating on the symbolic aspects of organizational life, language, stories and myths.

So, obviously, the use of metaphors in organizational theory is widespread. Other fields of the social sciences also rely on the metaphor as a fundamental tool for expressing ideas. I offer as one piece of evidence the work of Pierre Saint-Amand, *The Libertine's Progress: Seduction in the Eighteenth-Century Novel* (1994, pp. 3-4). The constant recourse to metaphor stands out in a sampling of a few passages:

> But seduction, I would argue, must be apprehended somewhat closer to its origin. It is above all fascination, mesmerism, sorcery. My interest, then, lies in the magic of magnetic gestures, in the intimidation of the gaze that transfixes. . . . I propose to read the eighteenth-century novel as the locus in which the so-called Enlightenment reveals its underside, foundering on the blind reefs of desire, Atheistic philosophers (except perhaps Diderot) have no place in this world of obscure desires and ancient fears. Their place is taken by creatures of the night under the spell of evil. My aim is to restore all their satanic excess to these beings. . . . The coquette appears in these novels as one of the modern figures of sorcery. . . . As for the seducer—what is he but the Devil with a modern face and worldly tastes?

In this short passage I count ten major metaphors. Continuous recourse to metaphor is found throughout this specific work.

Why do we use metaphors? A cynical answer could be that with a surfeit of social scientists all vying for limited publication space, a well-turned metaphor can serve as both entrée and stimulant for discussion, an agent provocateur, certainly something to gain the attention of tenure committees.

For example, I would suggest that March and Olsen's "Garbage Can" model of institutional decision making is inaccurately named (March, 1962; March & Olsen, 1979). The model describes a decision making environment where problems are in a constant flux, emerging and submerging at random times and with random intensity. In this setting decisions are not the result of a reasoned rational process, but rather the by-product of a "collision" between a problem and a solution in time and space. Decisions are represented as a function of the simultaneous time of arrival of the problem and the solution, the flux of allocation of energy in the decision making process, the structure of the organization, and the structure of the problem process.

I would imagine that there are few that have encountered real garbage cans that remain in constant flux, allocate energy, and impose any structure on their contents other than constraining it with sides and a bottom (and, on occasion, a top). The characteristics of a typical garbage can just are not a good fit with March and Olsen's descriptions. A more accurate title might be "A Random Flux Variably-Structured Decision Generation Process (RFVSDGP)." A better metaphor might be a comparison to the Brownian movement of particles in a fluid. However "more accurate" or "better" other suggestions might be, the "garbage can" beats them cold in terms of controversy, attention-getting value, and marketing pizzazz. Likening any type of human decision making to a garbage can is provocative; doing so to an academic audience, when your primary example is the decision processes of the university, is a guarantee of notoriety (and, perhaps, tenure).

A less cynical individual can point to the value of metaphors in their ability to organize thought and suggest connections that might otherwise not occur to a researcher. In a field where theory has been an ineffective mechanism to developing knowledge, the metaphor is a natural fallback. It is also natural, in that mental models are, in effect, metaphors.

If the reigning paradigm is weak, one would expect that strange concepts will pop up under the guise of "learning metaphorically." For example, in the field of organizational theory, organizations have been connected metaphorically to some things that an outsider to the field would consider to be very unlikely, like theaters, psychic prisons, and instruments of domination. Wheatley (1992), as a perpetrator of out-of-the-box analogies, is a prime example of stretching metaphors beyond reasonable limits. She takes an analogy and extracts broad statements of universal truth. She looks at quantum theory, which is concerned with the action of matter at the scale of a billionth of a billionth of a billionth of a second, and metaphorically extracts lessons for the management of organizations.

She does not restrict herself to quantum theory. In a look at the domain of chaos, she concludes: "Chaos theory has proven . . . *[t]he world* is far more sensitive than we had ever thought" (1992, p. 127, emphasis added). Later she looks at fractals and concludes that *nothing* can ever be definitively measured.

While on a hike in the Rockies, Wheatley (1992, p. 127) sits and soaks her feet in a stream.

> Finally, I ask directly: What is it that streams can teach me about organizations? . . . The Colorado realized that there were ways to get ahead other than by staying broad and expansive. . . . Streams have a different relationship with natural forces. With sparkling confidence they know that their intense yearning for ocean will be fulfilled, that nature creates not only the call, but the answer.

There are words here that can exude a "warm and fuzzy" feeling—"sparkling confidence," "intense yearning," a "call" that will be answered—that can substitute for the feeling of "rightness" that correspondence with the feelings that an accurate mental model of "good organization" can generate. However, that is all they are—a substitute. Her words are devoid of meaning in an organizational context. Like a baby duck that becomes imprinted with the mistaken notion that a human is its mother, individuals like Wheatley have lost the imprint of an accurate scientific paradigm and have instead imprinted, in her case, a New Age philosophy paradigm. Anthropomorphic images that elicit positive emotional feedback to New Age concerns are also inappropriately providing positive emotional feedback when New Age symbols are being applied against Organizational Theory concerns. And, like patriotism being inappropriately invoked to justify a wartime atrocity, such mixed metaphors do not yield clear thinking.

The question remains, why are so many of the new social science metaphors so bizarre? A key consideration is that the current paradigm competition is not over just explanations of phenomena, but instead involves a fundamental challenge to the paradigm of "how to do science." It is a challenge to the scientific method itself. Without the scientific method to establish the rules of evidence, anything goes. Under the old classical paradigm theory must be predictive, evidence must be experimentally repeatable, and if something can't be quantified it was an opinion, and opinions carry no weight. In this new environment, things like Wheatley's "feelings of quality" are offered as evidence of a "morphogenic field" and they can be considered as valid evidence. The classical paradigm is not strong enough to cry "foul" and make it stick.

Wheatley bemoans the rigidity of many of the organizations that she has observed, and the fear of departing from set methods. In her view, by "lusting for order" we have failed to understand the true nature of organizations. She attributes these problems to the pervasiveness of the metaphor of the machine (Wheatley, 1992, pp. 17, 21). The issue here is not whether or not Wheatley is correct, but rather her acceptance that a metaphor can be that powerful. That might not be a fair generalization—after all, most of Wheatley's work appears to consist of taking odd metaphors and using them to stretch our credulity—but it does serve as a data point. It also serves to allow us to question the extent in which science by metaphor has captured the social sciences and led it astray. Surely streams sparkle, but do they have confidence? Do they yearn? Are those like a characteristic #2, in common with organizations, or a characteristic #4, leading our understanding astray?

Reaction to Failure

The reactions of social scientists to the consistent failure of their theories have been varied.

Traditionalists have argued that the social sciences are not yet matured enough to make the necessary breakthroughs into predictive causality models. One example of this line of argument is from Pruitt and Snyder (1969, p. 4).

> People who make invidious comparisons between theory and practice are often reacting to theory building in its undramatic earlier stages, when evidence is incomplete and applications have not been worked out. Yet one generation's Einstein, thinking while walking in the park, may develop the insights necessary for the next generation to master an important new source of physical power. Significant rewards will often be reaped in the long run by a society that is willing to tolerate what looks like unproductive activity in the early stages of theory development.

Other social scientists have reacted with the approach that "if we haven't been able to discern truth with our previous attempts at developing rules of behavior, then there must be no rules at all." One example of this is Contingency Theory. Bowditch and Buono (1994, p. 25) relate that

> . . . the central thesis of Contingency Theory is that there are *no* universal principles of management that can be applied uncritically in all situations. Organization and management approaches must vary from one firm to the next because it *depends* on the unique environmental conditions and internal factors which are inherent to each organization.

Saying "it depends" is just a dressed-up surrender. The missing component of the thought process of Contingency Theory, an understanding of "it depends *on what*," is left unanswered. Good theory would identify and deal with this problem. Instead, Contingency Theory leaves it to be addressed by highly paid consultants who would be pleased to visit each and every corporation in America to cast their bones and read the Tarot cards and provide site-specific "solutions."

As was mentioned earlier, proper theory identifies antecedent and dependent variables in a causality model, or at least in a set of "if–then" statements. By definition, Contingency Theory is no theory at all.

Denhardt (1994, p. 417) went on to discuss another contender, Critical Theory.

> Critical Theory has its roots in Hegelian philosophy, particularly Hegel's notion of history as the unfolding of reason. The task of theory in this view is not to build on that which exists, but rather to critique and possibly remove those aspects of the present that impede movement toward freedom and reason. . . . Just as individuals may repress certain aspects of their life history yet still be troubled by them, so may society be troubled by conditions that are not apparent on the surface. In such cases, the theorist must engage in the type of critique that removes the veil concealing our present though unrecognized condition of domination, thus allowing reason to move toward greater freedom.

This entire concept is interesting from two viewpoints. First, because it is an attempt at developing a competing paradigm, an indication of dissatisfaction with the existing paradigm. Second, and more significant, is that it is a direct challenge to the current paradigm at the most fundamental level: the definition of theory. Hegel's notion of theory does not conform to the classical idea of what a theory should be, and what the Scientific Method could test. In Critical Theory we do not see a comprehensive theory of behavior, but rather an attempt to identify certain factors in order to shape the system toward certain outcomes or values that are in some way identified as "good" by the practitioner, and without any theory that would substantiate that the actions taken would actually result in the desired state. It is normative rather than predictive. To many Critical Theorists, it is the actions that are what is desired—the impositions of government controls, or certain taxation policies, or additional regulations—not a particular end state. Theory is not intended to lead to a full understanding of cause and effect, but rather to promote an program of treatments that are, in themselves, the goal. As such, because there is certainly an absence of any agreement on what is good, it is less a theory and more a political agenda dressed up in the trappings of theory.

The point is, again, to remember to place this in the context of a struggle between paradigms. A dominant paradigm makes the rules of what constitutes "good science." Critical Theory is clearly not "good science" under classical rules. The fact that Critical Theory was not quashed has clear implications. The paradigm that defines the criteria for "good science" has been weakened so much that even things that are not theories can lay claim to that status.

CHAPTER XIII

THE PARADIGM SHATTERS: PHILOSOPHY, INDUCTION, POSTMODERNISM, AND ALL THAT . . .

A Philosophy of Science

The power of the Scientific Method has been so great as to spill into the realm of philosophy.

Probably the most egregious case of scientific cheerleading was Charles Sanders Pierce and the philosophy of Pragmatism. To a Pragmatist, "absolute truth" was defined as being, well, whatever the scientists say absolute truth is.

Later efforts became a little less subjective. In Europe, Logical Positivism asserted that something is true if it can be logically or empirically demonstrated. According to Stanesby (1985, p. 13), Logical Positivism

> . . . attempts to relate the logical and experiential nature of the scientific enterprise. It is empiricist and positivist in that it admits of only knowledge from experience, which rests on what is immediately given; and it is logical in its method of reduction of scientific statements, step by step, to the given. Metaphysical entities and concepts were avoided by insisting that the only theoretical terms allowed were those that could be proven with correspondence rules to give them explicit phenomenal description.

The analysis of theories as axiomatic calculi interpreted through correspondence rules is called the Received View on Theories, which Stanesby (1985, pp. 13-14) says was the dominant philosophy of science from the 1920s to 1950. The Received View was eventually extended into a general doctrine of cognitive significance. Munz (1985, p. 3) noted that "Positivism claimed to contain two certainties. First, the certainty that all philosophical problems were scientific ones; and second, the certainty that the first certainty could be known with certainty." As an example of the pervasiveness of this view, Stanesby (1985, p. 53) could cite a 1943 quote from Carnap in saying that "psychology is a branch of physics."

These philosophies largely ignored what Macrone (1994, pp. 33-36) calls "The Scandal of Induction." He states:

> The scientific method pioneered in the seventeenth century by Francis Bacon, Rene Descartes, and others, is essentially an inductive process. As opposed to deduction (which derives new truths from established ones), induction passes from particular observations to general conclusions. Bacon and his followers insisted that scientific knowledge can never rest on given truths, whether mathematical or metaphysical, but must ground itself in observation and experiment. True scientists regard the natural world, seek out its patterns, propose hypotheses, and then test them by experimentation. A hypothesis becomes theory if established through repeated experiment but is rejected if contradicted ("falsified") by experiment. Philosopher David Hume, while agreeing that knowledge can only be gained through experience, thought the scientific method had big problems.

In *A Treatise of Human Nature* (1739) and *An Enquiry Concerning Human Understanding* (1748), Hume challenged the basic premise of inductive logic. To continue from Macrone (1994): "Exactly what does it mean, he asked, to 'know' something by observation or experiment—that is, by induction? All you know is that whenever X appears to happen, Y also appears to happen."

So, if you put something on an inclined plane, it will slide down. Repeat it a thousand times, and one thousand objects slide down. You conclude that placing an object on an include plane causes it to slide down. But what if the 10,001st object you place is coated with adhesive, and it sticks?

Thus, Hume claimed that experience alone was insufficient to justify accepting generalizations about the world.

However, there was no discernable effect of this argument on the conduct of the physical sciences. Stanesby (1985, p. 23) quotes Russell's statement that "subsequent British philosophers rejected [Hume's] skepticism without refuting it."

This is an important point. Why could the physical sciences shrug off this challenge? I submit that *the classical scientific paradigm was so strong that it was able to overcome this significant challenge to the foundation of its underlying philosophy*. Scientists read Hume and noted his objection, but did not change how they thought about their approach to scientific problems. For that matter, they did not even recognize themselves (at least publicly) that what they were actually doing was not in accordance with what they said they were doing.

This was not the only challenge—there have been others. Postmodernism, developed in France, was present in some forms as early as the 1900s. The power of the prevailing paradigm restricted postmodernism's appeal to a small circle.

Similarly, there was the *Methodenstreit*, the conflict originating in Germany between those who thought that social science should be humanistic and those who thought it ought to mirror the physical sciences. The classical paradigm weathered these challenges unscathed.

Then came Karl Popper (1968).

According to Horgan (1996, p. 34), Popper's philosophy

> . . . stemmed from his efforts to distinguish pseudo-science, such as Marxism or astrology or Freudian psychology, from genuine science, such as Einstein's theory of relativity. The latter, Popper decided, was testable; it made predictions about the world that could be empirically checked. The logical positivists had said as much. But Popper, like Hume, denied the positivist assertion that scientists could *prove* a theory through induction, or repeated empirical tests or observations. He believed that it was impossible to prove anything unless *every* case is tested.

According to this view, to prove that 1 + 1 = 2, one must test every conceivable set of 1's, testing for the possibility that, in some other universe or situation, 1 apple plus 1 orange might equal 1.5 fruit. One never knows if sufficient observations have been gathered, because the next observation might contradict all that preceded it. Observations can never prove a theory but can only disprove, or falsify, it—a proposition that has been labeled as Popper's Impossibility Theorem.

Popper rightfully claimed that he had "'killed' logical positivism with this argument."

Popper took Hume's attack on inductive logic to the next level. He challenged *all* inductively-derived theory. As stated by Stanesby (1985, p. 67):

> Popper totally rejected induction and replaced it by falsification as the critical method of science, as Darwin had previously rejected the inductivism of Lamarck and replaced it by natural selection. Popper's thesis rests on the logical asymmetry that exists between verification and falsification. Although theories cannot be verified, because of their unrestricted generality, or universal nature, they can be falsified by a single negative instance.

Popper (1968) had, in essence, a quantum leap in the definition of "good science." It resulted in a new theory of the method of science. Instead of seeing science as a logical problem of inductive generalization, where new theories spring from careful examination and the logical consequences of empirical evidence, he saw new theories as imaginative conjectures that must hold its own in competition with other theories through a process of critical analysis through trial and error. According to Popper, as quoted in Stanesby (1985, p. 68):

> We choose some interesting problem. We propose a bold theory as a tentative solution. We try our very best to criticize the theory; and this means that we try to refute it. If we succeed in our refutation, we then try to produce a new theory, which we shall again criticize; and so on. In this way, even if we do not succeed in producing a satisfactory theory, we shall have learned a great deal; we shall have learned something about the problem. We shall know where its difficulties lie. The whole procedure can be summed up in the words: bold conjectures, controlled by severe criticism which includes severe tests. And criticism, and tests, are attempted refutations.

Popper's description of the process of acquiring knowledge (and, normatively, the process that scientists of all flavors should use to approach problems) is accurate. There is a very interesting correlation between this process and the natural cognitive processes of the human brain, a correlation that has fascinating implications.

However, there was some unfortunate baggage with Popper's ideas. As stated by Stanesby (1985, p. 73): "The Popperian account of the growth of human knowledge through criticism rules out the notion of *episteme*, of sure and certain knowledge. The edifice of human knowledge is open and non-authoritative in its structure. . . . All human knowledge is fallible."

It is a very tiny step to go from the statement, "we cannot absolutely know truth," to "there is no such thing as truth." There are those, however inappropriately, who were willing to take that step.

In an interview with Horgan (1996, pp. 37-38), Popper stated that

> . . . he believed that a scientific theory could be "absolutely" true. In fact, he had "no doubt" that some current scientific theories were absolutely true (although he refused to say which ones). But he rejected the positivist belief that we can ever *know* that a theory is true. "We must distinguish between truth, which is objective and absolute, and certainty, which is subjective." . . . Yet Popper abhorred those modern philosophers and sociologists who claim that science is incapable of achieving *any* truth and who argue that scientists adhere to theories for cultural and political reasons rather than rational ones. Such critics, Popper charged, resented being viewed as inferior to genuine scientists and are trying to "change their status in the pecking order. . . . There is a certain corruption, unfortunately."

Popper's (1968) theories did not address whether absolute truth existed or not. His topic was about methods of scientific inquiry, and subjecting theories to criticism and falsification as a means of growing knowledge. However, there were those ready to corrupt his views into the interpretation that there was no such thing as truth.

Stove (1982) argues that the foundational argument of these misinterpreters of Popper's ideas is based on what he calls a "necessary truth." A necessary truth is not true because it is a natural law or based on reality, but rather is based on nothing more than semantics, a fallout of the language used. Windschuttle (1994, p. 214) states:

> Necessary truths are void of empirical meaning. So when any skeptic claims that a flame found tomorrow *might* not be hot like those of the past, or that the next baby born *might* not have circulating blood, he has no genuine reason for this doubt, only an empty necessary truth.

Stove continues the argument by asking the consequences if he should believe in a theory, which the Impossibility Theorem says cannot be absolutely proven. As quoted by Windschuttle (1994, p. 215), Stove argued, as Popper himself believed, that the doctrine of falsification should not be taken too far.

> [D]espite all the actual or possible empirical evidence in its favor, the theory *might* be false. But this is nothing but a harmless necessary truth; and to take *it* as a reason for not believing scientific theories is simply a frivolous species of irrationality.

In other words, Popper was talking about the growth of knowledge. Others took his words further—too much further—to challenge the very existence of knowledge, of truth. The process of the growth of knowledge and the existence of knowledge itself are two very different things.

Popper's ideas on the growth of knowledge successfully challenged the reigning paradigm, the part that talked about how to do good science. By 1994, Denhardt (1994, p. 417) could state that Positivism, "which had dominated the field for many years, began to be recognized [in the late seventies and eighties] as much for its shortcomings as for its virtues, while other approaches, primarily phenomenology and critical theory, bid for prominence." Popper's arguments, because of their persuasive power, set against an environment where more and more anomalies in the social sciences emerged that also challenged the classical paradigm, found fertile ground. The paradigm was no longer so strong as to hold back this argument, as it had thwarted a similar argument against induction mounted by Hume two hundred years before.

White (1986, p. 231), in his article on public administration dissertation research, acknowledged the contemporary challenge explicitly.

> Philosophers of science once believed that the truth of a scientific hypothesis, generalization, or theory could be ascertained by strict adherence to the rules of inductive and deductive explanation or by correspondence to an objective "thing" language devoid of any theoretical interpretation. Some influential contemporary philosophers of science have rejected these guarantors of truth and rationality because they fail to resolve issues of incommensurability among theories, research programs, and paradigms.

Just as Hume's attack on induction predated Popper's, Pascal's (1623-1662) belief that it was "impossible to definitively prove anything" predating the Impossibility Theorem by several centuries. Pascal believed that it was impossible for human reason to prove anything for certain—as Stove would say, "a harmless necessary truth"—and then, as reported by O'Connor and Robertson (2001), Pascal dedicated a good part of his life to mathematical proofs in the emerging science of probability.

This is not inconsistent thinking. Pascal knew that, if you are restricted from making an assumption that something is true, it is impossible to prove that *anything* is true. It is like attempting to perform a mathematical proof without the necessary first line, which goes: "*Given that* . . ." However, if the existence of a truth is accepted, if you are allowed to begin with a *given*, then it is possible to prove that truth exists. Pascal accepted the validity of *given,* and his mathematical work is a practical example of overcoming the necessary truth.

This is, in essence, a circular argument on both sides: given that nothing is true in the beginning, we cannot prove anything to be true; given something is true, we can prove that truth exists. Circular arguments such as this should be the province of philosophy, not science. The essence of being a scientist means that you accept the possibility of truth—or else why pursue science? The alternative is to argue for the sake of argument.

The point that should also be recognized is that Popper's Impossibility Theorem does not prove that truth does not exist. It is like looking at a box and saying that, since the top is on the box and it is impossible to look inside, therefore there is nothing in the box. The Impossibility Theorem says nothing about whether truth exists or not, whether there is anything in the box; all it says is that you cannot look in the box by an inductive process. The fact that, as will be seen later, some Postmodernists cite this theorem as a proof that truth does not exist, is simply an example of wooly thinking.

Regardless of the validity of the argument, the issue is, *why now?* Hume's argument was made 200 years ago, Pascal's 350 years ago. Yet the classical paradigm shrugged these challenges off.

Why, then, could Popper have such wide-ranging influence with what was essentially a recapitulation of Hume's and Pascal's questions?

I suggest that the difference appears to be in the life experiences of the audience. In 1650 and 1750, scientists were making great strides in understanding the physical world. There was then no real hint of problems that might be intractable to the scientific method. The scientific community was imbued with a bounding optimism and the assumption that, eventually, given time and application, all problems could be solved.

Today, we do not have that feeling of success. The problems in the physical science non-classical realm are only now beginning to be addressed, while the humanities' successes and breakthroughs in the observational and descriptive aspects of social science have not carried over into theory development. Several generations of social scientists have spent their lives looking for the equivalent of Newton's laws, and failed. People have been conditioned to this failure. They have spent a generation observing the failure of their paradigm. Then, when the anomalies develop, it is very easy to react by saying, "the reason why I could not discover 'truth' is that there is no 'truth' to discover."

The process can also be looked at from a cognitive viewpoint. First, we have a group of social scientists with a certain mental model of how science should be conducted, the Scientific Method. However, they found, as we have seen in the previous chapter, that this model does not result in the growth of social science knowledge. They were bombarded by anomalies. Their internal mental model of "how to do good science" was disrupted. Then, Popper offers a criticism of how science is being conducted. A number of people, vulnerable to new mental models, seize upon some or all of Popper's argument, and coalesce a new mental model that essentially says, "the reason why we were not able to determine truth is that you cannot discover truth inductively, and if you cannot discover truth, it is possible/ probable/self evident that truth does not exist." And, as mentioned by Finney (2002), like Lewin's management change model, these individuals' minds have gone through a "unfreezing, changing, re-freezing" process, where it has rejected the old model and settled upon a new one. Because the mind's model of "good science" was involved, rejecting the previous model of "goodness," there was no negative emotional feedback to indicate that this new model was incorrect. The result was that a new community grew around a new paradigm, and offered it as an alternative to other scientists in the grips of trying to reconcile their own mental models of good science against the anomalies of the constant failure of theory to predict human behavior. The result was Postmodernism, whose growth in the social sciences can be thus related to the pathological failure of the Scientific Method to grow social science knowledge.

The Standard of Repeatability

Ijiri and Simon (1977, p. 117) have stated: "It has often been demonstrated in the psychological laboratory that men—and even pigeons—can be made to imagine patterns in stimuli which the experimenter has carefully constructed by random processes." In the Scientific Method paradigm, the standard of experimental repeatability serves as a check on such errors.

The unfortunate point for the social scientist is that, with any experiment involving human beings, it is impossible to repeat an experiment. You cannot repeat the same experiment with the same people, because your subjects have learned from their experience; you cannot repeat the same experiment with different people without acknowledging that the "control" in the experiment is now different. Even using large numbers in the test (relying on the Law of Large Numbers to settle the results into a predictable mean), it is impossible to fully define and control the system of variables to be accounted for in the experiment. Even using segregated groups simultaneously does not work, since it is impossible to duplicate all the external environmental conditions and sensory and information sources to all groups. Context and experience will always be different.

I can relate the experience of participating in a test of human reaction time while I was in graduate school. The experimenter was a student in a class that was studying human factors in the design of aircraft cockpits. He wished to discover if color had an effect on reaction time. He created a simple experiment where a light of a certain color would flash and the subject would react by pushing a button.

He arranged for test subjects to arrive on two successive days, by asking two groups of students to come by immediately after their classes adjourned, one class on Thursday, the other on Friday.

The data for the first group showed the fastest reaction time in response to the red light, followed by yellow and then blue. The experimenter felt confident that he understood why. The bulk of that day's students were pilots who had been trained under a "red: danger, yellow: caution, white: information" color-coding scheme. There was a training variable for which to account. He expected the next day's group, which did not have any pilots, to show a different distribution. He also expected the non-pilots to show slower reaction times, due to the nature of their work when compared to the need for fast reaction times in pilots.

The second day's test subjects, much to his puzzlement, showed hardly any differentiation between colors. And, the subjects had markedly faster reaction times than the first day's results.

As it turns out, the two classes of students had played a softball game on the evening between the tests. The second day's test subjects lost. They went into the test with a determination to show that they were better than the first group in *something*. The factor of competition and motivation totally distorted the results.

To extend this example, a problem with any testing with human subjects is that it is impossible to get a complete grasp on all the variables. In this case, test results were changed by motivation; in other cases, it could be sleep, or diet, or emotion, or a news event, or a personal tragedy. It is impossible to know all the possible factors; even if it were, it is impossible to quantify, measure, and control them. How does one quantify emotion? How does one establish a control condition for an emotional state? How does one establish a causal linkage between these variables and the result?

When standards of repeatability are not met, scholars often concluded that the problem must be in the execution rather than in the method. O'Toole (1995, pp. 295-297), in a review of *Research in Public Administration,* stated that "the articles clearly document the conclusions of scholars of widely differing perspectives [who] nevertheless agree that much of the [public administration] field's research has been of dubious quality." O'Toole mentions the book's "emphasis on diversity" in research, another indication that there is no governing paradigm that adjudicates between acceptable and unacceptable research methodology, allowing "diverse" research methodologies to coexist without one or the other being culled by the dictates of normal science. The existence of more than one research approach indicates the weakness of the previous dominant concept of the "correct" approach.

This point was underscored by O'Toole (1995, pp. 295-297).

> Some readers might interpret the brief for diversity [in research methods] itself, based as it is on a post-modern consciousness, as acceptance of the disappointing best to be hoped for: investigators exploring questions highlighted by their own nearly solipsistic "local narratives" and reaching results that are individually interesting but collectively muddled and discordant.

It is generally considered the place of theory to connect the observations of 'local narratives' into a clear and non-discordant whole. Given this observation, one would then expect an article on research to issue a call for development of theory and a demand for the establishment of scientific standards, i.e., a new paradigm. To continue with the passage: "However, without a serious effort at scholarly dialogue both within and across perspectives, diversity is not enough."

In this context, diversity could very well be the root of the problem.

> Its likely product, more cacophony than enlightenment, could hardly offer a way to ameliorate broadly public problems. The postmodern trap of ennui is thus to be avoided, as the editors of this volume clearly recognize. If scholarship means anything, and if public administration scholarship is not to become simply the last object of privatization in the postmodern landscape, researchers must reach beyond diversity to engagement—with the pressing contemporary agenda, with each others' viewpoints, and with the prospects of mutual enlightenment.

The statement that "researchers must reach beyond diversity to engagement" is somewhat slippery and can have a multitude of meanings, but it is clear that the passage supports the case that there is no established overriding paradigm in effect. The implication—to "engage" or learn from each other's viewpoint, implies that there is no established, "correct" paradigm. If there were a viewpoint of established truth, then there would not be an appeal to learn from another viewpoint, because no truth would be excluded from a correct viewpoint. Rather, one viewpoint or the other would be defined as "truth," i.e., in accordance with the established paradigm, and the others would not. According to Kuhn's historical examples, established paradigms are not tolerant of other viewpoints. Other viewpoints are suppressed, not published, attributed to bad experimental method, or cognitively not recognized. The absence of a ruling paradigm opens the way to a practice of seeking diverse viewpoints.

This idea is further reinforced by Harmon (1995, p. 297).

> The last quarter century has witnessed a steady decline in the positivist hegemony over social science scholarship. This has, of course, been welcome news to positivism's detractors; but the range of alternative theoretical and methodological approaches legitimized by that decline has bewildered new graduate students and dismayed established social scientists about what now should qualify as acceptable scholarly practice.

Cook, in *Criteria of Social Scientific Knowledge* (1994), attempts to identify the disagreements between some of the contending paradigms. He indicates that the three major families of social theory offer different criteria of what is expected from successful theory. These three criteria are interpretation, prediction, and praxis. The fact that these criteria exist is another element of evidence that there is no dominant paradigm. Only one—"Prediction"—meets the previously-defined understanding of what should constitute theory under classical science.

"Interpretation," which includes, according to Harmon (1995, p. 297), "hermaneutics, phenomenology, ethnomethodology, symbolic interactionism, various 'standpoint epistemologies' (feminist theory being the most prominent), depth psychology (mainly Freudian and Jungian), . . . structuralism . . . and . . . poststructuralism" all are generally viewpoint oriented agenda-seeking, rather than truth-seeking. "Praxis" is similar in that it desires to be prescriptive, to "fix" what is perceived as being broken.

In general, these types of viewpoints execute what Zimm (2001), in an article dealing with current developments in the analytic communities, has labeled as "Advocacy Analysis."

An example of postmodern analytic processes can be found in *Evidence, Argument, & Persuasion in the Policy Process* by Giandomenico Majone (1989), from which the quotes in the following paragraphs are taken. Majone makes explicit the thought process involved in the Postmodernist mental model of "good science." The development of the postmodernist argument in policy analysis can be seen as consisting of three steps:

Step One begins with epistemology, the study of the nature of knowledge, especially with regards to limits and validity. In modern epistemology (or "postmodern", as "advanced thinkers" are identified), there is no such thing as universal transcendent truth. Majone (1989, p. 42) states that "few scientists and philosophers of science still believe that scientific knowledge is, or can be, proven knowledge."

To Majone, a keystone to this view is Karl Popper's Impossibility Theorem. The fact that Majone invokes Popper as justification for the postmodern viewpoint is a point of evidence establishing the connection between Popper and Postmodernism (no matter how erroneous the interpretation). The Postmodernist position recognizes, according the Majone, that "even mathematical knowledge is fallible, tentative, and evolving, as is every other kind of human knowledge. . . . Scientific knowledge is always tentative and open to refutation."

So, truth is not a transcendent fundamental reality, but an "agreement between reasonable people." Majone (1989, p. 43) states that truth "can only be established by convention: through a consensus of experts in the field and the fulfillment of certain methodological and professional norms—the rules of the scientific game."

Step Two logically builds on the death of truth. Since policy analysis cannot produce "universal truths," then what does it produce?

The answer comes from the ancient Greek idea of the dialectic. The starting point of the dialectic is not axioms or propositions (as would be the case in a mathematical proof), but rather points of view already prevalent in the community. The purpose of the dialectic is to evolve a line of reasoning built upon the nature of the premises and the social context of an argument. The product is not a formal proof of a truth, but rather a shared understanding of an issue that is accepted as a truth. The dialectic is a method of clarifying controversial issues.

Within this context Postmodernists believe that policy analysis is a form of dialectic. And, "like dialectic, policy analysis usually starts with plausible premises, with contestable and shifting viewpoints, not with indisputable principles or hard facts. Like dialectic, it does not produce formal proofs but only persuasive arguments."

Step Three completes the transformation. Majone asserts that "the policy analyst is a producer of policy arguments, more similar to a lawyer—a specialist in legal arguments—than to an engineer or a scientist. His basic skills are not algorithmic but argumentative." Argumentation is based not on bedrock of immutable truth, but on truth as a relativistic creation based on "evidence." "Evidence" is not a collection of all the relevant facts, but rather "information *selected from the available stock* and introduced at a specific point in an argument to 'persuade the mind that a given factual proposition is true or false.'" I have added the italics to emphasize Majone's belief that evidence is selected in accordance with its power to argue a certain point. If evidence does not contribute to the object of the argument, in his view, it is irrelevant.

Thus, rationality and truth are skewed in postmodern theory to where an action is judged to be rational if it can be defended with "arguments acceptable to a reasonable audience."

Majone (1989, p. 43) went on to state:

> If advocacy and persuasion play such an important role in the development of scientific ideas, can policy analysts afford to slight them in the name of a historically mistaken view of the scientific method? . . . The practical question, therefore, is not whether to use persuasion, but which form of persuasion to use and when.

Thus, in postmodern thinking, since there is no objective truth, the inevitable avalanche of logic brings us to the place where the analyst's role is to develop arguments in favor or against a particular policy—what I call "Advocacy Analysis."

In Advocacy Analysis, the rules are lawyer's rules. In a courtroom, a lawyer selectively presents information and arguments to bolster a case. Information that can damage that case is either ignored or (if presented by the opposition) belittled. The objective is not to expose objective transcendent truths, but to convince the jury to come to your point of view. Analysis is more of politics than of science.

A great deal of policy analysis in the non-military branches of the government falls into the category of Advocacy Analysis. I monitored the news for a week. A total of three "polls" and four "studies" were reported. A typical example was a Baltimore *Sun* report on a study that concluded that adding a prescription drug benefit to welfare payments would be "less expensive to the taxpayer" than the current system without the benefit. How this remarkable result was attained was not reported. The study was performed by an unnamed "advocacy group for the poor." Those wishing to promote a certain viewpoint take such counterintuitive results at face value; presumably it would be accepted as truth unless another study made the opposite argument with its own set of data, assumptions, and calculations.

The position of the postmodernists regarding policy analysis is a subset of their overall attitude towards scientific research; it is representative of their approach in general. Since truth does not exist, the purpose of research is to make an argument. To make such an argument, scientists are justified in selecting only that part of the evidence that makes their case. The other evidence is buried, so that some other researcher cannot employ it in a counter-argument in the dialectic. The object of research is no longer to chase the chimera of transcendental Truth, because it does not exist; instead, one strives to win the argument, and the rules of debate, rather than the rules of the scientific method, apply.

It is obvious that, with this attitude, *scientific experiments are no longer repeatable.* Science is instead to be conducted by a different set of "rules of the game" depending on the investigator's viewpoint to support a dialectic constructed to convince the current set of decision makers that an argument is correct. Should a different group of scientists become the reigning decision makers, and they find a different set of arguments persuasive, then the analysis changes.

Since experiments and research and analysis are based on making an argument, it is a legitimate practice to ignore evidence that would prove to be an anomaly to the argument. Under these conditions experiments would not be repeatable, thus undermining one of the primary pillars of the Scientific Method.

The fact that such a methodology is now extensively used in public policy "analyses" and "studies" without challenge as to their methodology and objectives is an indication that the current paradigm has become so weak that it cannot defend itself against such a blatant violation of its fundamental precepts.

Obviously, this construct represents a radical departure from the Scientific Method. What is even more interesting in the context of this study is that such analyses are only seen in "social science" circles, areas like human policy studies conducted for branches of the government dealing with welfare, education, and the like. "Postmodernist" study methodologies are categorically rejected in the military operations research community. Most military studies are performed to determine the potential effectiveness of weapons systems.

Thus, there is a very strong connection between the engineering/physics community, which establishes the operating parameters of the system, and the analytic community that measures the systems' effectiveness within the context of various scenarios. Independent analytic organizations are characteristically employed to confirm and verify any operations research studies that might be performed by weapons manufacturers in an attempt to "sell" their product. Independent organizations that succumb to the pressure of a manufacturer and manipulate a study to make their pet system look good are quickly identified by the community and ostracized.

The operations research community as a whole is represented by two organizations: the Military Operations Research Society (MORS), and the Institute for Operations Research and the Management Science (INFORMS). MORS is dedicated exclusively to military subjects, while INFORMS is involved with medical, business, education, logistics, marketing, social policy, public affairs, and other social science applications.

Over the last year I attended symposiums organized by both groups. At each meeting, I conducted an informal survey of the members with which I fell into conversion, asking them about their attitude towards the "postmodernist" approaches to conducting analysis.

At MORS, I questioned twenty-seven people, none of whom knew anything about the "dialectical" approach to analysis. All definitely subscribed to the view that the purpose of analysis was to reveal the truth, that all evidence should be included in an analysis, and the analysts' task was to provide the study sponsor with information and analysis in order to facilitate a proper decision. When I presented to them a passage from Majone's book describing analysis as "making an argument in favor of a program," and allowing selective use of evidence, they all reacted negatively. All of them had "hard science" education backgrounds.

At the INFORMS meeting, of twenty-four people informally interviewed, eighteen knew of and subscribed to the "dialectical" approach to analysis. They generally felt that the purpose of a study was to help the sponsor substantiate and justify a program decision that was generally already made but needed to be "sold" to Congress or some other public agency decision maker. Sixteen of the eighteen felt that it was acceptable to ignore evidence that did not agree with their conclusions. Fourteen of the eighteen had at least one social science degree, either as their bachelor's or master's degree.

This study was not conducted in a scientific manner. At the INFORMS meeting, I purposely attended "social science" oriented presentations. Since my past affiliation (for nearly twenty years) was with MORS, I wanted to find out if there was any penetration at all in the analytic community of postmodern ideas. My suspicion was that Majone was an academic, and that academic postmodernist leanings may be fashionable on campus but would certainly not have penetrated into the practicing analytic community.

From my MORS background, my supposition was that I would not find any postmodernists at either the MORS or the INFORMS meetings. As noted, I was partially wrong: at MORS I found none; at INFORMS I found well over half. The fact that the postmodernists were generally those with social science degrees was to me significant, and a confirmation of an ongoing conflict between paradigms in the social sciences. None of the analysts with exclusively hard science backgrounds were found to be insecure in their attachment to the Scientific Method.

Rejection of Truth: The Postmodernists

Postmodernism represents an entirely different paradigm of how to conduct social science. Both theory and the comparative analysis of competing ideas is considered irrelevant. Bergquist (1996, p. 578) observed: "In the postmodern camp, there is neither interest in the systematic building of theory (through what Thomas Kuhn, 1962, calls 'normal science') nor in a warfare between competing paradigms (what Kuhn calls 'scientific revolutions'). Rather, everything is preparadigmatic." Thus postmodernism represents another symptom of a breakdown in the governing Scientific Method paradigm. This breakdown has spilled past the boundaries of social science into the humanities in general. The postmodernists, once a minor branch of the philosophies centered in France, broke out beyond their formerly restricted borders to begin disruption of all of the humanities.

Spiro (1996, pp. 759-780) provides a taste of the postmodernist approach, in this case regarding the study of anthropology.

> The postmodernist critique of science consists of two interrelated arguments, epistemological and ideological. Both are based on subjectivity. First, because of the subjectivity of the human object, anthropology, according to the epistemological argument cannot be a science; and in any event the subjectivity of the human subject precludes the possibility of science discovering objective truth. Second, since objectivity is an illusion, science, according to the ideological argument, is subverting those of oppressed groups, females, ethnics, third-world peoples.

This approach is very close to the views of the scientific Luddite Feyerabend, who, according to Cook (1994, p. 7), "would give no rule at all for the doing of science, save for the implied rule that none should give any dictated rules, except that one."

The following passage from Ashley (1990, p. 22) provides additional clues to the appeal of postmodern ideas. In particular, he states a reason for postmodernism that is very much attuned to the cognitive approach proposed in this paper: "Modern, overloaded individuals, desperately trying to maintain rootedness and integrity . . . ultimately are pushed to the point where there is little reason not to believe that all value-orientations are equally well-founded. Therefore, increasingly, choice becomes meaningless."

Ashley goes on to quote Baudrillard in saying that we must now come to terms with the second revolution, "that of the Twentieth Century, of postmodernity, which is the immense process of the destruction of meaning equal to the earlier destruction of appearances. Whoever lives by meaning dies by meaning."

These descriptions hint at what I believe is true: that the genesis of the acceptance of postmodern thought is rooted in the failure of the social sciences to generate achievements comparable to what was accomplished in the physical sciences. The social sciences could not discover truth; therefore, truth must not exist. To paraphrase his first statement, modern, overloaded individuals, who have been driven to jettison their established mental models due to an overload of anomaly, grasp on to another mental model, that there is no truth or no differences in values—the postmodern mental model. If there was a mental model that could have explained the anomalies, they likely would have accepted that; since there was no model that provided explanation, they accepted the model that explanation was not possible.

As Kimball (1990, p. xv) put it: "Who could have predicted that the ideals of objectivity and the disinterested pursuit of knowledge would not only be abandoned but pilloried as products of a repressive bourgeois society?"

The rejection of the concept of truth is deep-seated in postmodernism. Indeed, any traditional value that smacks of being a fundamental truth is considered fair game. For example, one would expect that a field such as architecture, which attempts to meld sound engineering practice, beauty, functionality, and comfort, would be outside the purview of postmodernism. After all, a "truth" might be unsubstantial—can anyone ever touch a philosophical or political truth?—but a building either stands or it falls, it is either functional and efficient or it is not, it is either comfortable or it is not.

However, according the Kimball (1990, p. 119), "semiotic" innovations and "deconstructivism" are part of a new theory of architecture drawn from

> . . . chic literary theory. One thus sees architects obsessed with language, rejecting traditional aesthetic values such as clarity, order, and harmony, and designing buildings that seek to undermine or deconstruct such conventional "prejudices" as the desire for comfort, stability, and commodiousness.

The postmodernist architect Eisenman (1988, p. 169) designed several homes to illustrate his theories about "narrative architecture" and "unsettling the traditional meaning of 'home.'" One was designed to explore

> . . . an alternative process of making occupiable form . . . a process specifically developed to operate as freely as possible from functional considerations. From a traditional point of view, several columns "intrude on" and "disrupt" the living and dining areas as a result of this process.

The expression of Eisenman's "metaphysic" consists of dining rooms placed remotely from kitchens, with pillars located in such a way as to make it impossible to place a dining table; rooms that are claustrophobically small, irregularly shaped, or both; convoluted passageways and restricted access to rooms; all supposedly express "dominant vectors of truth" towards "an attempt to alienate the individual from the known way in which he perceives and understands his environment." (As a side note, I understand that two of Eisenman's designs were actually built. I have no information as to whether the homes were eventually remodeled.)

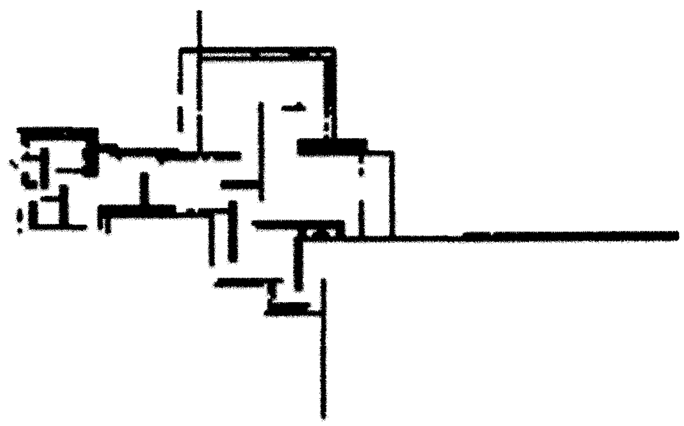

FIGURE 48

MIES VAN DER ROHE'S "PROJECT FOR A BRICK HOUSE"

Source: Bermudez, Julio (1995). *Aesthetics of Information: Cyberizing the architectural artifact.* Available at: http://www.arch.utah.edu/people/faculty/julio/conne1.htm.

An example of Postmodernist architecture is shown in Figure 48, taken from Bermudez (1995). Bermudez explains the design as follows:

> Using television and computers as exemplars for developing an architecture of/for the information-age leads to the key concept of *"screen."* First, as argued, it is the screen which practically and theoretically defines the most powerful cultural artifacts of our time. Second the screen can be architecturally construed as a plane, that is, an essential compositional element able to generate complete architectural orders. There are excellent examples of architectures developed solely on the basis of planes (e.g., Mies van der Rohe's work in Europe, Van Doesburg's theoretical studies, some of Richard Neutra's houses, etc.). In other words, the couple screen-plane may act as the conceptual and concrete device that, while making the architectural artifact sensitive to the pulses and nature of information, can be used to deliver credible architectural orders. I will call this type of construction *"architecture of screens."* . . . [A]n architecture of screens offers multiple and continuously changing interfaces that transcend the

> static nature of the Postmodern facades while addressing the information concept behind them. At the same time, the syntax of an architecture of screens brings the Deconstructivist insight into play but transcends it by offering many possible architectural organizations. . . . Finally, an architecture of screens transcends its own physicality by offering buildings of a multi-dimensional character. Interfacing an architecture of screens means accessing a hyper-environment because there are many more (virtual) environments available than the one physically present. The immediateness and multiplicity of environments challenge the traditional concepts of presence, distance, and time and delivers an architecture of singular simultaneity, that is an architectural version of Borges' aleph: a non-place where anything and everything is (re)present(ed) not least in theory. An architecture of screens is a hypermedia holder that creates an anomaly in the urban universe. . . . Clearly, an architecture of screens demands an upheaval of the solid, stable, enclosing, and semiotic nature of the wall and architecture as we have understood them for millennia.

This passage has been quoted in length because it demonstrates several of the points made earlier. First, there is the string of metaphors that begin with “information age,” progresses through “television screens” to “screens” to “planes” until suddenly the author wants the reader to accept that what the design represents an “architectural artifact sensitive to the pulses and nature of information [that] can be used to deliver credible architectural orders.” Apparently, nowhere in this progression has the author’s internal mental models screamed for him to stop, that the metaphor is being overextended.

Also, note that there are a number of terms that evidently mean special things to the Postmodern architect, beyond what Mr. Webster has recorded. How a place can be a non-place simultaneously is a concept which would need explaining to anyone subject to the tyranny of the Scientific Method.

The interesting thing is what the article does not say. Through context and some of the language used in the explanation, one can come to the suspicion that the design consists primarily of moving partitions or screens that can change the shape of the rooms. However, this is never made clear. It is almost as if the article purposely avoids such clarity to instead concentrate on rather nebulous concepts, like a building that "transcends its own physicality" or "addresses the information content behind them." Lost is any idea that a house should be comfortable and functional.

The denial of self-evident values as comfort and functionality in a home in favor of creating "an anomaly in the urban universe" and "upheaval" is one example of the denial of a self-evident truth; other Postmodernists go on to deny self-evident medical truths. For example, it is a self-evident truth that there are biological differences between men and women. Shalit (1999, p. 87) writes of her encounter with those who deny such differences. She quotes Robert S. McElavaine, who defines "essentialism" as the "heresy" that "there are biological differences between males and females." She also mentions Judith Butler from Johns Hopkins University, a well-regarded "antiessentialist," in her criticism of feminists for even claiming to support the "fictive" category called "women."

Feminist theory even encompasses an explicit paradigm shift in scientific methodology. According to Grosz (2002):

> Feminist theory is necessarily implicated in a series of complex negotiations between a number of tense and antagonistic forces that are often unrecognized and unelaborated. It is a self-conscious reaction on the one hand to the overwhelming

> masculinity of privileged and historically dominant knowledges, acting as a kind of counterweight to the imbalances resulting from the male monopoly of the production and reception of knowledges. On the other hand, it is also a response to the broad political aims and objectives of feminist struggles. Feminist theory is thus bound to two kinds of goals, two commitments or undertakings that exist only in an uneasy and problematic relationship. This tension means that feminists have had to tread a fine line either between intellectual rigor (as it has been defined in male terms) and political commitment (as feminists see it)—that is, between the risks posed by patriarchal recuperation and those of a conceptual sloppiness inadequate to the long-term needs of feminist struggles—or between acceptance in male terms and commitment to women's terms. . . .

In this conception, the rigor associated with the Scientific Method is revealed as a male method of domination. The Scientific Method is not an objective scientific paradigm for "good science," it is instead a subjective means to maintain female subjugation to patriarchal powers. There is no issue of truth, just patterns of domination.

There are various traumas that are known to cause people to deny truth. Losing a loved one, being diagnosed with a fatal disease, experiencing a major financial tragedy, all have been known to trigger a reaction in humans to deny the self-evident. The denial of self-evident truths on a scale encompassing all of the social sciences is indicative of mental trauma on a societal level.

Postmodernist Interpretations of Paradigm Shifts

Some of Kuhn's ideas expressed in *The Structure of Scientific Revolutions* (1970) have also added to the mischief. Kuhn believed that there were different reasons why a new paradigm supplants an old one, beyond the obvious explanation that the new paradigm better explains nature. Kuhn believed that scientists' personal values and relationships within the scientific community could also come into play.

Into this tiny crack, Windschuttle (1994, p. 189) has seen where

> . . . a bevy of sociologists have entered the field to take up what they see as one of the most enticing consequences of [Kuhn's] position: the idea that what is believed in science is determined by the customs and power relations prevailing within a particular scientific community. One of these sociologists, David Bloor, has gone so far as to suggest that the content and nature of science can be explained through the methods of the sociology of knowledge, and that scientists accept scientific laws primarily for reasons of justification, legitimization and control. Another, H. M. Collins, [claims that] all experiments are subject to "experimenter's regress." This argument contends that experiments cannot perform the function that scientists claim for them, that is, to independently assess the success or otherwise of competing scientific theories. This is because the theories themselves determine what counts as an effective experiment. Hence, there are no objective criteria that can be derived from experiments to separate the outcome of the experiment from the theory that it has been designed to test. Collins further claimed that scientists were strongly influenced by "the size and prestige of the university where the experiment was done; the personality, nationality, and reputation of the scientist; whether the experiment was performed within a university or within industry; and the way the results were presented."

Following this exchange in the literature is fascinating, if one steps back and looks at the battlefield from the vantage point of one interested in paradigms and the enforcement of standards of normal science.

First, there is the outrage of the critics of Kuhn, who rail against his supposed "assault" against the orthodox order. Read Kuhn carefully and you do not find anything near what these critics are against; rather, it is an *implication*, that they are largely reading into the work themselves, that they find upsetting. One cannot help but think that the upset is more from a sense of vulnerability rather than from an issue of truth—"Methinks he doth protest too much."

Second, there is the glee with which the revisionists, like Collins, seize upon the work and extend it to say that not only is there a crack in the dike, but the entire country is flooded, too. That is fairly representative of the entire approach of postmodernism. Like a child who cannot find his shoes next denies that his shoes ever existed, social scientists who have not been able to find any working social science theories have segued into postmodernists who deny that there are any such theories to find. Indeed, their technique is extended to deny anything that appears obvious. According to Fish (1989, pp. 4, 14, 164), a leading postmodernist in interpretation of the law, "there is no such thing as literal meaning . . . there can be no such thing as theory . . . there is no such thing as intrinsic merit."

This appears to be the type of turmoil that Kuhn describes when an old paradigm is found wanting, and new paradigms begin to compete for attention. The guardians of the old paradigm are indignant, because that in which they have been trained, which they have accepted and conditioned themselves into a feeling of "rightness," is now being challenged; they react with the ferocity of a mother guarding her young, or the buggy-whip manufacturer guarding his livelihood. On the other side are the revisionists, who have lost the feeling of "rightness" regarding the old paradigm, and now seek a replacement without having a common standard against which to measure goodness. As such, the revelation of goodness can be very individualistic. Some may agree, but many will follow their own Siren.

Windschuttle (1994, p. 200) went on to look at some of those who have argued against the revisionists. One of the most prominent has been David Stove (1982). Stove has pointed out that the problem arises from intermixing the philosophy of science with the sociology of science. The philosophy of science is

> . . . concerned with the logical relations between scientific statements, that is, what could and could not be legitimately transmitted from one statement to another. The sociology of science, on the other hand, is about scientific practice, what scientists do and think. The two fields are distinct areas of inquiry. The philosophy of science is *presumptive* in that it aims to establish what relations *must* hold between statements. The sociology of science is *descriptive* in that it is simply an account of scientific activity, irrespective of the degree of logic that prevails within it. . . . [O]ne of the central problems in the recent debate is the conflation of the two fields. . . . Popper, Lakatos and Feyerabend all derive the evidence for their claims from the history of science. This would be acceptable if they used this evidence simply to provide examples of statements whose logic they were examining. Time and again, however, Stove shows them *appearing* to be making logical statements about the relations between scientific propositions, while *actually* making statements about what scientists believe and accept, that is, using the latter as if they were examples of the former. There is a constant, subtle elision from one kind of statement to the other. Their radical skepticism derives from their attempt to resolve questions of logical value by appealing to matters of historical fact.

Stove (1982) also discusses the rather slippery tendency of some postmodernists to play fast and loose with words, using terminology to appropriately imply legitimacy where none exists. Consequently, a scientific theory is spoken of as "defeated," or "eliminated," or "removed," as if those observational terms were the logical equivalent of "is falsified" or "has been disproved." As Stove (p. 41) states, just because a theory has been abandoned does not mean that it "has been refuted, in the logical sense of having been proven to be false."

Arguments Against Postmodernism

As outlined above, there are many sound arguments that counter the Postmodernist paradigm. These arguments can be convincing. In the 1940's, similar arguments, backed by the power of the dominant paradigm, were considered convincing by the majority of social scientists, keeping the postmodernists' intellectual ancestors in check.

However, by the 1980's the ruling paradigm was greatly weakened. The arguments of Stove and those of like philosophical beliefs have been unsuccessful in squelching the revolutionists; otherwise, postmodernism would still be a fringe movement. Counter arguments have been marginalized by the Postmodernists' inherent beliefs that, *as a matter of faith*, their beliefs are not subject to falsification.

In previous times, under the stronger "classical science" paradigm, such beliefs were handled using "Occam's Razor" and "Hume's Fork."

William of Ockham, in *Quodlibeta,* Book V, circa 1324, stated: "Plurality is not to be assumed without necessity." This has been translated into the principle of parsimony, formulated as "Occam's Razor," and most commonly articulated as:

"What can be done with fewer [assumptions] is done in vain with more" (Encyclopedia.com, 2001). In practical application, it means that if there are competing explanations, the simplest is the one most likely to be true.

This assumption has stood physical scientists in good stead, and is departed from only for good reason. For example, according to Ijiri and Simon (1977, pp. 114-115), it took Kepler almost ten years to retreat from the simplicity of the circle to the complexity of the ellipse in investigating orbital mechanics. After presenting other examples, they conclude by saying, "we can see what importance the physical and biological sciences attach to finding simple generalizations that will describe data approximately under some set of limiting conditions." Albert Einstein was quoted in Prector (1999, p. 34) as saying: "Our experience until now justifies our belief that nature is the realization of the simplest mathematical ideas that are reasonable." The complexity of the postmodernist explanations—indeed, their assumption that all explanations are equally valid—clearly cuts against these dictates.

As for Hume's second contribution to this debate, Macrone (1994, p. 30) observed:

> Hume's Fork is a philosophical utensil for separating interesting problems from bogus ones. . . . The basic idea is that every statement or claim falls into one of three categories: 1) either true or false by definition, 2) dependent upon experience, or 3) just nonsense. These are the fork's three tines.

The social science community has not been able to stick the tine of "just nonsense" into the nonsense—the "non-sense"—of postmodernism. It is just nonsense to make a building uncomfortable, non-functional, and ugly, and that should be about all the effort that should be spent on such ideas. However, if the paradigm of rationality is under assault, Hume's Fork is lost under the dining room table.

A Debate Fraught with Emotions

In the predictions of paradigm shift behavior based on cognitive considerations, I predicted that one of the consequences of a paradigm shift challenging the Scientific Method would be extreme emotionalism in the debate. That extreme emotionalism appears prevalent in the conflicts over Postmodernism. Debate is not ordered and collegial; emotional responses are at the forefront.

Homi K. Bhabha is a Postmodernist academic dealing in colonialist theory. Kimball (2001, pp. 56-57), in an article in a major national magazine, attacked Bhabha's postmodernism with an emotional diatribe that flung epithets like grapeshot, with an added leavening of finger-pointing and name-calling. Bhabha was labeled as "post-structuralist," "anti-hegemonic," "neo-Marxist," "radical-feminist," and "America-hating," while his work was labeled as "a staggering abundance of really bad writing," "awe-inspiring, in a horrible way," "angry politicized gibberish," "reader-proof," "seamless unintelligibility," "academic wooly-headedness," and "unintelligible"—not the words exchanged by colleagues over a faculty tea.

Another example is Windschuttle (1994). His descriptions of the clash between Postmodernism and more traditional historical methods are couched in the terms of war, fighting, clashes, destruction, killing, and similar terms. Some of Windschuttle's arguments, along with other anti-postmodernists like Kimball, will be quoted at length later. The reader is encouraged in the task of picking out the venom from between the lines.

This is fairly representative of the sweep of the discourse. The "classical" scientists fight with all the terminology of war, while the Postmodernists attack with similar tools, confident that their conscience has already approved even the most extreme language and manners.

Other Indicators of a Paradigm Clash

Windschuttle (1994, pp. 1-2), in *The Killing of History,* discusses the assault of postmodern thought on the "intellectual discipline" of history, and on the social sciences in general.

> For most of the last 2400 years, the essence of history has continued to be that it should try to tell the truth, to describe as best as possible what really happened. . . . [C]ritics still operated on the assumption that the truth was within the historian's grasp. Today these assumptions are widely rejected, even among some people employed as historians themselves. In the 1990s, the newly dominant theorists within the humanities and social sciences assert that it is impossible to tell the truth about the past or to use history to produce knowledge in any objective sense at all. They claim we can only see the past through the perspective of our own culture and, hence, what we see in history are our own interests and concerns reflected back at us. The central point upon which history was founded no longer holds: there is no fundamental distinction any more between history and myth. This view is not itself new. It was forcefully argued more than one hundred years ago by the German Philosopher Friedrich Nietzsche, and has been nurtured by his followers ever since.

> What is new is the success these ideas have had among English-speaking universities and academic publishers in the last ten years. . . . Sociology, anthropology, and psychology have always been prey to fashionable and sometimes bizarre theories, but, while history remained intact, the humanities and social sciences had some claim to being intellectually respectable. It is amazing how quickly this has changed.

Windschuttle (1994) attributes the changes to the "rejection of empiricism and induction" by "old New Left crowd from the 1960's, . . . just as addicted to the latest fashions as they were in the days of hippy beads and flared trousers." One of the reasons that the humanities and social sciences were unable to resist the "putsch" was that the new beliefs were impossible to understand (p. 5).

> The uninitiated reader who opens a typical book on postmodernism, hermeneutics, poststructuralism et al. must think he or she has stumbled onto a new foreign language, so obscure and dense is the prose. Now, this happens to be a very effective tactic to adopt in academic circles where there is always an expectation that things are never simple and that anyone who writes clearly is thereby being shallow. Obscurity is often assumed to equal profundity.

Windschuttle's point is certainly powerful, especially when we can see examples in print. The following three paragraphs are excerpts from the works of Homi K. Bhabha, as quoted by Kimball (2001, pp. 56-58):

> Within that conflictual economy of colonial discourse, which Edward Said describes as the tension between the synchronic panoptical vision of domination—the demand for identity, stasis—and the counter-pressure of the diachrony of history—change, difference—mimicry represents the *ironic* compromise. If I may adapt Samuel Weber's formulation of the marginalizing vision of castration . . .
>
> . . . on the margins of metropolitan desire, the *founding objects* of the Western World become the erratic, eccentric,

> accidental *objets trouves* of the colonial discourse—the part-objects of presence. . . .
>
> It is the emergence of the interstices—the overlap and displacement of domains of difference—that the intersubjective and collective experience of *nationness*, community interest, or cultural value are negotiated. How are subjects formed "in-between," or in excess of, the sum of "parts" of difference (usually isolated as race/class/gender, etc.)? How do strategies of representation or empowerment come to be formulated in the competing claims of communities where, despite shared histories of deprivation and discrimination, the exchanges of rules, meanings, and priorities may not always be collaborative and dialogic, but may be profoundly antagonistic, conflictual and even incommensurable?

From Genesis chapter 11, verse 7: "Come, let us go down and confuse their language so they will not understand each other." To the uninitiated, Bhabha's prose appears to be the basement of the Tower of Babel.

Had these excerpts been from some fringe radical from outside the mainstream of academic standards, it could be dismissed as some fringe radical from outside the mainstream of academic standards. However, Professor Bhabha holds the Chester D. Tripp chair in the humanities at the University of Chicago, teaches at the University of London, is an advisor to the Institute of Contemporary Arts, the Whitney Museum of American Art, and the Rockefeller Foundation, and sits on the editorial board of a number of publications.

Windschuttle (1994) believes that the dense prose and unintelligible sentences are part of a plot to purposefully deceive others as to the worth of their material—what he characterizes as "a very effective tactic." Andreski (1972, p. 63) agrees, saying: "Sometimes the verbal substitutions masquerading as contributions to knowledge are so inept and gross that it is difficult to believe that the authors . . . are not laughing up their sleeves at the gullibility of the audience." Possibly. It is also possible that the unintelligibility of postmodernist prose comes from three factors acting together. First is the postmodernist rejection of all standards of quality. Kimball (1990, p. xii) relates:

> The notion that some works are better and more important than others . . . is anathema to the forces arrayed against the traditional understanding of the humanities. The very idea that the works of Shakespeare might be indisputably greater than the collected cartoons of Bugs Bunny is often rejected as antidemocratic and an imposition on the freedom and political interests of various groups.

As such, to a Postmodernist, there is no differentiation possible between postmodern "good" prose and postmodern "bad" prose, since Postmodernism refuses to acknowledge that such a difference exists. Thus, those postmodernists with less than acceptable communications skills will find no one willing to say so—the postmodern emperor's new clothes, updated and revised edition.

The second factor is the postmodernist's attitude toward writing. As Kimball (1990, p. xv) relates: "*Writing* no longer means attempting to express oneself as clearly and precisely as possible, but is rather a deliberately 'subversive' activity meant to challenge the 'bourgeois' and 'logocentric' faith in clarity, intelligibility, and communication."

The third factor is the possibility that authors such as Bhabha are marching to a different drummer. Their writing is a symptom of an entirely different phenomenon being played out. Recall that Kuhn mentioned that members of different paradigms could not communicate with each other because they did not share common concepts and common meanings for many words. Within Bhabha's scholarship community, there may very well be significance placed on terms such as "conflictual economy," "diachrony," "part-objects," and "*nationness*" in italics, beyond that found in a common dictionary. The references to other writers indicate a community of scholarship, with common beliefs and foundations, who presumably understand this language. In terms of paradigms, Argyrous (1999, p. 828) states that paradigms are incommensurable, because it is not possible to map terms of one paradigm into the terms of another—there is a loss of content in the translation.

The point is, however, that all of these points can be traced back to the disruption of mental models associated with the discrediting of the Scientific Method paradigm. Thus, the emergence of postmodernism could very well be a huge, species-wide artifact of a huge, species-wide case of cognitive dissonance.

The fact that much of this reaction includes putting out unintelligible material (as illustrated in the writing of Homi K. Bhabha) is another symptom connecting the root problem with the disruption of mental models. I do not believe that the Bhabhas of the world are willfully putting out unintelligible material as some kind of smoke screen to hide a lack of substance, or as a plot to fool their colleagues in order to attain tenure (although I admit a willingness to be convinced otherwise, on a case-by-case basis). I believe that what is occurring is that when the normal deterministic cognitive model is disrupted, the mind then goes into an "anything goes" search for the correct mental models to apply. The mind then seizes upon some other model that, for whatever reason, makes sense at the moment. Like the taro root extract that was "proven" to be a bomb attractor, some random observations of the environment have convinced the mind that the model du jour is correct.

It is significant that the postmodernists are mostly academics. Windschuttle (1994, p. 214) states that few people believe in such ideas, "outside the world of philosophers and sociologists of science and their students." Postmodernism has not become a popular movement. These academics were exposed to misinterpretations and over-extensions of the arguments of Popper and Hume and, not having an adept answer, succumbed to cognitive dissonance. The rest of the world, either not exposed to their arguments or uncaring, continues to muddle through life fairly successfully employing inductive-reasoning mental models.

However, history shows at least one time when there was a popular movement that could be causally linked to cognitive dissonance. The nihilism, rise of communism, and isolationism of the post-World War I years is commonly assumed to be a result of a trauma on the popular level, the horrors and massacres of trench warfare. Given that historical event, it is not then surprising that the postmodernists are generally associated with the Viet Nam War generation, the "injured souls" of the counterculture movement.

There is a connection between the rejection of the Scientific Method with the generation that created bumper stickers like: "Question Authority." What they were doing in the political arena, they were also doing in the social sciences. When the "authority" of the scientific method failed to do what it was supposed to do, reveal truth, then the authority must be questioned, as well as the truth that it was reputed to be able to find. So, alternate models were created.

As these alternative models are promulgated, other minds similarly prepared to accept alternative models clustered about them. The validity of these other models is not mentally evaluated against some standard, because the standard has been destroyed. Consequently, their only attraction might be the feeling of unity with another mind and the sense of security that others have found this model to be correct. This sort of behavior is correlated to the combat behavior of soldiers when their cohesion has been broken and they are in panic and disorder: they instinctually tend to cluster around their leaders (Zimm, 2000b, p. 50).

If there were indeed a paradigm shift in the making, Kuhn's model would lead us to expect just such an emergence of groups employing language that is not intelligible to members of the old paradigm.

Kuhn's model is reinforced by observations of the use of language in other examples of competing domains. Probably the greatest example of competing paradigms in modern U.S. society is between the worldviews of political parties and their philosophies. In that context, Sowell (2001, p. 14A) made this observation regarding one powerful term:

> Everybody is for "fairness"—because we all use the word to mean very different things. Most of us think you have been treated fairly when you have been treated the same as everyone else—subjected to the same rules and judged by the same standards. But some think that you have been treated fairly only if you had the same chances as everyone else. These are very different and completely incompatible notions. . . . People at opposite sides of political and legal issues often talk right past each other when they are using the same words to mean totally different and mutually contradictory things.

Thus, the use of language can be offered as another piece of evidence of a paradigm shift in progress.

There is a very interesting passage from Weiss' and Adler's (1984, pp. 2-4) article on the state of personality research within the field of organizational behavior that can be fruitfully examined from the viewpoint of the use of language and the competition between paradigms. A commentary on this passage can serve as a summary of the many problems of, and reactions to, the lack of a dominant social science paradigm. As the passage unfolds, I will highlight some of the pertinent points.

"Our discussion will focus almost totally on the use of trait conceptions of personality within a nomothetic framework."

On initial reading of this passage I stumbled on the word "nomothetic." Recourse to the dictionary informed me that the word meant "relating to, involving, or dealing with abstract, general, or universal statements or laws." As I read on through the article, however, I came to realized through context that the word meant, to the specialists in this field, something more than was contained in the dictionary definition. As I mentioned before when discussing Kuhn's thoughts on competing paradigms, it is difficult for members of different paradigms to communicate because of their assignment of different meanings to the same terms. I offer this as additional evidence that we are operating in a period of competing paradigms.

To continue with Weiss and Adler (1984): "Our preference for a nomothetic viewpoint is less arbitrary. We have been aware of a number of recent papers advocating ideographic approaches to the organizational analyses of various sorts."

I had the same problem with regards to the term "ideographic." Per Mr. Webster, "ideographic" is "relating to or dealing with something concrete, individual, or unique." Again, "ideographic," in an Organizational Behavior context, evidently means that and something more. Fortunately, Weiss and Adler provide some explanation: "[T]he core of an ideographic position is that every individual, indeed every object, has a certain essence or 'thisness' . . ."

Unfortunately, Mr. Webster did not cover "thisness."

When Kuhn discussed the problems of communications between members of different paradigms he did not mention the invention of new terms, but I believe that the same spirit applies. Here is an example where the definition of "ideographic" has been expanded by the members of the paradigm to include "thisness."

To continue with Weiss and Adler (1984):

> . . . and that there can be no real understanding of the object without understanding its essence. Additionally, since each essence is unique and cannot be resolved into class concepts, the search for scientific laws and generalizations is an impediment to real knowledge. A true ideographic position precludes as meaningful knowledge the acceptance of lawful relationships which generalize across individuals.

I note here the similarities between the ideographic approach and that of the postmodern literary theorists discussed earlier, where there is a fundamental assumption that "thisness" represents a universal uniqueness that defies all attempts to be governed by rules or laws of behavior. One thisness cannot be used to meaningfully interpret another thisness.

I question whether anything totally unique can be understood. Something totally unique cannot be experimented upon, because it would acquire a different uniqueness in the process of undergoing the experiment; and, since human beings understand things by a process of mental metaphors (more on this later), something totally unique cannot have meaningful metaphors and thus cannot be totally understood. And, since theory involves the ability to predict, thus there can be no theory in the ideographic approach, only observations that are worthless to apply to any other thisness.

However, Weiss and Adler offer hope: "We should note that Allport's position was never so extreme. He did not reject the meaningfulness of scientific generalizations and envisioned nomothetic and ideographic 'sciences' coexisting in the study of personality."

Here we have a repeat of the proposition that competing paradigms should be allowed to coexist, similar to Cook's plea that the interpretive, predictive, and praxiological schools should each be retained because they each contribute something unique to the discourse. On the surface, this might seem reasonable, and might also seem to have precedent in classical science; after all, a photon is considered to be simultaneously a particle and an energy wave, and we do allow Ptolemaic astronomy to remain in the literature as quick-and-dirty "engineering estimates" of planetary positions.

Asking paradigms to coexist, however, is like asking Dracula to coexist in the coffin with crosses, wooden stakes, a bag of garlic, and perhaps a nice east-facing window to view the dawn: one inevitably must destroy the other, because their unalterable thisness is antagonistic to the others.

Weiss and Adler (1984) go on to make this point, albeit in a different way:

> Frank argues that most so-called ideographic analyses are based upon hidden nomothetic principles. He cites history, which is often taken as a prototype ideographic approach, and notes that the historian based his analyses on unstated generalizations and assumed regularities. Similarly therapists are described as taking an ideographic approach, but essentially depend upon nomothetic generalizations. In arguing for a nomothetic viewpoint we are not denying the practical or scientific importance of analyzing particular cases.

The chaos of results of previous investigations have been compared to expectations of what should be achieved by a law-driven approach, and found wanting. The absence of law-driven predictability has driven them to the conclusion that there are no laws. Yet, Frank is cited as implicitly believing that such laws exist because therapists implicitly use them—a rather strange bit of logic, somehow warping *cogito ergo sum,* "I think, therefore I am" into "*He* thinks about *it*, therefore *it* is." Descartes cannot be pleased with this development.

The problem, as we can see by reading Weiss' and Adler's following remarks with a jaundiced eye, is that there is no established paradigm to judge whether the research is up to normal science standards or violates those standards:

"To us it seems clear that ideographic approaches are nomothetic at heart. The difference between the two lies in whether the matrix of generalizations and set of categories are theory based and public or implicit and private to the observer."

Certainly, if that were true, a paradigm of "normal science" would have nipped ideography in the bud. It would have been strangled for recognition by the process of selection for refereed journals of the field. Why print a commentary on a unique thisness that cannot be translated into knowledge of any other unique thisness?

However, since there is currently no such powerful standard for normal social science, no paradigm, such material is included in journals and published by the academic community based on the "big tent" assumption: we can't all understand it, but it doesn't violate any of the standards of good science (because those standards do not exist), so we'd better let it go to print—who knows, it might be right.

Jewett (2002, p. 86) discusses this problem, and mentions one of the classic (and hilariously embarrassing) examples:

> Scholarly publication involves three levels of filtering: the "norms of the academy," the use of peer review by scholarly journals, and the presence of an expert audience. But why then, after all this filtering, the poor quality of so much of the work that is published in the thousands of peer-reviewed academic journals, particularly in the humanities and social sciences? Has a public intellectual publication ever failed in its gatekeeping function as spectacularly as the academic journal *Social Text*, where New York University physicist Alan Sokal published a hoax article that was deliberately "salted with nonsense" but employed au courant academic jargon and was consistent with the editors' ideology?

In *Social Text,* Sokal (1996) offered an article entitled *Transgressing the boundaries: Towards a transformative hermeneutics of quantum gravity.* According to Sokal (pp. 62-64), his purpose was to "test the prevailing intellectual standards."

> So, to test the prevailing intellectual standards, I decided to try a modest (though admittedly uncontrolled) experiment: Would a leading North American journal of cultural studies—whose editorial collective includes such luminaries as Fredric Jameson and Andrew Ross—publish an article liberally salted with nonsense if (a) it sounded good and (b) it flattered the editors' ideological preconceptions?

The answer was "yes."

In his first paragraph, Sokal derided: "The dogma imposed by the long post-Enlightenment hegemony over the Western intellectual outlook." Then, he went on:

> Throughout the article, I employ scientific and mathematical concepts in ways that few scientists or mathematicians could

> possibly take seriously. For example, I suggest that the "morphogenetic field"—a bizarre New Age idea due to Rupert Sheldrake—constitutes a cutting-edge theory of quantum gravity. This connection is pure invention; even Sheldrake makes no such claim. I assert that Lacan's psychoanalytic speculations have been confirmed by recent work in quantum field theory. Even nonscientist readers might well wonder what in heavens' name quantum field theory has to do with psychoanalysis; certainly my article gives no reasoned argument to support such a link. . . .
>
> Later in the article I propose that the axiom of equality in mathematical set theory is somehow analogous to the homonymous concept in feminist politics. In reality, all the axiom of equality states is that two sets are identical if and only if they have the same elements. Even readers without mathematical training might well be suspicious of the claim that the axiom of equality reflects set theory's "nineteenth-century liberal origins."
>
> The fundamental silliness of my article lies, however, not in its numerous solecisms but in the dubiousness of its central thesis and of the "reasoning" adduced to support it. Basically, I claim that quantum gravity—the still-speculative theory of space and time on scales of a millionth of a billionth of a billionth of a billionth of a centimeter—has profound *political* implications (which, of course, are "progressive"). In support of this improbable proposition, I proceed as follows: First, I quote some controversial philosophical pronouncements of Heisenberg and Bohr, and assert (without argument) that quantum physics is profoundly consonant with "postmodernist epistemology." Next, I assemble a pastiche—Derrida and general relativity, Lacan and topology, Irigaray and quantum gravity—held together by vague rhetoric about "nonlinearity," "flux" and "interconnectedness." Finally, I jump (again without argument) to the assertion that "postmodern science" has abolished the concept of objective reality. Nowhere in all of this is there anything resembling a logical sequence of thought; one finds only citations of authority, plays on words, strained analogies, and bald assertions.

Sokal (1996) offered this explanation of his motivation, which dovetails nicely with the thesis of this paper:

> In short, my concern over the spread of subjectivist thinking is both intellectual and political. Intellectually, the problem with

> such doctrines is that they are false (when not simply meaningless). There *is* a real world; its properties are *not* merely social constructions; facts and evidence *do* matter. What sane person would contend otherwise? And yet, much contemporary academic theorizing consists precisely of attempts to blur these obvious truths—the utter absurdity of it all being concealed through obscure and pretentious language. *Social Text*'s acceptance of my article exemplifies the intellectual arrogance of Theory—meaning postmodernist *literary* theory—carried to its logical extreme. No wonder they didn't bother to consult a physicist. If all is discourse and "text," then knowledge of the real world is superfluous; even physics becomes just another branch of Cultural Studies.

This event provides another "real world" example of the destruction of the Scientific Method paradigm.

Another of the most unfortunate aspects of the conflict in paradigms is the reduction in scientific integrity. There are paradigms, as shown above, that value the supposed end result over integrity in the means of achieving the goals. As such, there has recently been a spate of cases where a political agenda has trumped honesty and veracity. As documented by Roberts (2001):

> Recently, a history professor wrote a book citing sources that don't exist. What was important to the professor was not truth, but making a case against gun ownership. To further their agendas, other professors have fabricated life stories for themselves. A professor in Maryland passed himself off as a Vietnam veteran and told stories about events that never happened. Another at Columbia created a history of himself as a Palestinian refugee. One woman won a Nobel Prize . . . for a fabricated biography. Even some scientists have made up a global warming scenario in order to achieve their environmental objectives. In a civilization in which so much depends upon adherence to fact, it is a scary thing to experience fact playing second fiddle to emotion.

Of the above examples, perhaps the most famous was the awarding of the Nobel Prize for *I, Rigoberta Menchu,* an autobiography documenting a Guatemalan Indian's torment at the hands of the Guatemalan military. However, as Coulter (2002, pp. 108-109) reports,

> . . . it was exposed as a hoax by anthopologists and Guatemalan expert David Stoll of Middlebury College, who examined archival material and interviewed survivors of the events described by Menchu. He found that her book was a fantasy—an utter fraud from beginning to end. Thus, for example, on the very first page of her Nobel Prize-winning book, Menchu claimed to be an uneducated, illiterate peasant. Indeed, the book had to be translated from tapes of Menchu's oral history. It turned out that Menchu attended prestigious boarding schools run by nuns. She claimed to have engaged in arduous low-wage labor in coffee and cotton fields as a child. In fact, she belonged to a relatively prosperous farming family. She vividly described being forced to watch family members starve or burn to death. These events, too, never occurred. As one of Menchu's native countrymen described it, "the book is one lie after another, and she knows it." Stoll's refutation of Menchu's account was later confirmed by the *New York Times.*

Menchu, evidently, was motivated (at least in part) by politics. She delivered a speech to the United Nations on political themes, and used the prestige of a Nobel laureate to arrange a meeting with the United States State Department to lobby for an end to American aid to Guatemala. In a press conference reported by Newsdesk (1992), she said that her role would be to "push for a stable and lasting political solutions to the nation's armed conflict."

Falsehoods issued for political ends are not unusual. What makes this event noteworthy is the reaction of the academic community. According to the Pacific

News Service (1999): "In U.S. academic circles there is debate around the difference, if any, between literal truth and the truth of testimony intended to reveal the reality of hidden lives." Pacific News Service's McConahay, commenting on her personal knowledge of Stoll and Menchu, recognized the fault line.

> It has always been clear to me that these two tireless workers operate on completely different planes. Stoll is wholeheartedly concerned with verifiable fact and has a bulldog yen to smash icons; Menchu's strength comes from a vision of herself as duty-bound to represent the Indian as she sees the Indian to be.

The director of the Norwegian Nobel Institute, when asked to comment on the scandal, minimized the situation with a statement to the effect that all autobiographies embellish to a greater or lesser extent.

This is a remarkable comment, particularly from the head of an Institute responsible for arguably the most prestigious awards in the hard sciences. Certainly the Nobel Institute is applying two different standards to its requirements for awards.

The controversy on the academic level regarding the difference between "literal truth" and the "truth of testimony" makes explicit the conflict between paradigms. I would suggest that, at the beginning of the Twentieth Century, there would have been no question of awarding a Nobel Prize for a fabricated autobiography. However, the standards of the interpretive, and particularly the praxiological schools would offer no objection to such falsification as long as the ultimate social objective was deemed good.

Another example is the scandal surrounding *Arming America,* a scholarly revisionist history by Michael Bellesiles that aspired to counter many beliefs about the role of gun ownership in America during the colonial and westward expansion eras. According to Sullivan (2002) in a laudatory review in *Metroactive Books*:

> What he finds is so startling as to be revolutionary. Popular fancy locates the birth of our national obsession with guns in the rugged nature of early American life. Our movies and popular fiction present life in 18th- and early-19th-Century America as a constant struggle of the firearm-toting frontiersmen against British soldiers, aggressive Indians and dangerous wild animals. But Bellesiles argues that guns were actually fairly rare in early America. Drawing on a mountain of probate, military and business records, as well as travel accounts and personal letters, he makes the case that gun ownership was once the exception.

Bellisiles claimed to have reviewed over ten thousand probate and criminal records in his research to support the claim that only about 14 percent of men owned guns in colonial America, and that most of those guns were unusable. Bellesiles was awarded a 2001 Bancroft Prize for history.

When other scholars tried to review Bellesile's research, the data did not match. Some of the records that he claimed to have reviewed do not exist. When asked to corroborate his claims through his notes, he claimed to have lost his notes in a flood of his office at Emory University (an event not substantiated by any other faculty). San Francisco probate court records that he claimed to have reviewed were actually destroyed in the 1906 earthquake.

McCain (2002) reported that Emory University placed Bellesiles on paid leave pending an investigation into possible "research fraud." According to an

Emory spokesman: "Scholars say they have found evidence of research fraud in Mr. Bellesiles' book, 'Arming America: The Origins of a National Gun Culture,' which one critic says is full of 'massive misrepresentation' and another says is 'riddled with errors.'"

The contrast between the Menchu and the Bellesiles cases is marked. In the Menchu case, a scan of the internet using the Google search engine found many articles defending Menchu after her fraud was revealed, mostly emanating from the academic community. Scanning the first 150 Google returns on the Bellesiles case did not find one defender.

It is hard to draw anything conclusive from the two cases, because both were so fraught with political overtones. However, the fact remains that there were two distinctly different treatments in the cases, both concerning the treatment of truth by the authors. In one, fabrication of facts was acceptable, at least to the Nobel Institution and some elements of the academic community; in the other, fabrication attracted no defenders.

The viewpoint of cognitive models, however, does provide some perspective on what is occurring. Formerly, there was a mental model of the existence of something called "truth," and this concept was a dominant force in the perspective of building scientific knowledge. When mental models were in conflict, the mental model that required people to adhere to "truth," dominated the process. Popper's arguments regarding the inability to prove truth shattered this mental model in many people. When this dominant mental model was destroyed, other existing mental models took over primacy. So, for example, when a "political" mental model conflicted with the "adherence to truth" mental model, the "adherence to truth" mental model dominated, and required the political mental model to conform. When the concept of truth was shattered, then the "political" mental models were no longer governed by the "adherence to truth" model, and there arises a new culture where adherence to factualness, adherence to truth, instead is dominated by the political mental model. Thus, we see where people find it justifiable to lie in support of their political beliefs.

Lying for political gain is nothing new—it has been happening ever since the invention of politics. What is new, however, is the widespread practice of scientists to sacrifice truth on the alter of their other mental models, and of political movements holding to principles that are contradictory. Thus, environmentalist scientists feel justified in promoting "global warming" scenarios that they know are unjustifiable, and conservationists can place the survival of microscopic "endangered species" above the survival of human beings. Flynn (2002, p. 58) can cite Marcuse as calling for "violent revolution in the era of peace and love, [and at the same time can convince] his followers that the . . . principles were harmonious." The Free Speech movement can call for the censorship of opposing views. Critical Theorists can transforms the concept of "tolerance" into "Liberating" tolerance, which consists of “intolerance against movements from the Right, and toleration of movements from the Left.”

When the idea of truth is jettisoned, all values become relative, and it becomes the responsibility of the individual to choose which values are more important, which mental models are to dominate. It is not a coincidence that the New Left movements, that are the home for most of these contradictions, are also most often associated with those academics who have rejected the idea of truth.

Competing Paradigms

There are several areas of direct competition between alternative paradigms. For example, Cook (1994, pp. vii-viii), in his *Criteria of Social Scientific Knowledge,* identifies three broad “families” of “competing criteria of social science knowledge.” He lists:

1. The “interpretive,” where “some would have us use the vehicle of words to attain understanding of people”;

2. The “predictive,” intended to “successfully predict what people do next”; and

3. The “praxiological,” where “researchers act to attempt large-scale changes in social structure, . . . chiefly the Marxist outlook.”

In this listing Cook is presenting these “competing criteria” as individual, unique paradigms, with their own individual, unique standards of normal science. No answer is given to a direct challenge, “which is right?” Instead, Cook asserts that all three have something to bring to the table and warns against closure to any one of them.

A first examination would suggest that one, the predictive, is the classical scientific method, while the other two are subsets, capturing elements of what would also be part of the predictive/classical paradigm. Certainly one would think that it would be impossible to attempt large-scale praxiological changes in social structure to any particular end without a theory that could predict the results. As for the "interpretive" school of knowledge, it should provide information that would be a prerequisite to developing the "predictive" school's model. In other words, you certainly ought to be able to interpret if you have achieved such a depth of understanding that you can predict, and you have to be able to predict if you want to induce action that will close on a praxiological goal.

While that appears logical, the members of the interpretive and praxiological schools would not agree. The interpretive school, Geneva's Jean-Jacque Rousseau and Germany's Herder, Kant, Fiche, Schelling, and Hegel, according the Cook (1994, p. 16), "urged understanding of human activity through the meanings of its actors." Historians such as Dilthey (1883-1911) pursued "understanding (*Verstehen*) in contrast to the rival school of natural scientific explanation (*Enklaren*)." However, I would suggest that this is an artificial distinction, since understanding the *why* of the interpretive school certainly is part and parcel of understanding the *how* per the predictive, classical paradigm. The interpretive school appears to be an attempt to concentrate on *why* part of the problem while dismissing the *how* part as irrelevant.

Members of the praxiological school assert that the most thorough kinds of understanding come through efforts to exact social change. Cook (1994, p. 100) indicates that one of the most important figures of this movement, Karl Marx, believed that "the quest for knowledge is not isolable from social practice. Individual social scientists do not and can not stand aside from a social context." Marx would speak of "laws," but not in the same manner as the classical paradigm: Marx saw laws of behavior "arising at a certain historical conjecture," later to fall "when replaced by human choices." Thus, these laws are not of a universal nature. However, the substitute proposed by the praxiologists is to take certain large-scale social actions and assume that they are correct based on their ideology. Consequently, it appears that praxiologists are simply making predictions relying on a kind of faith rather than knowledge. Praxiologists claim that such actions are the only way to true knowledge. However, Cook (p. 118), referring to the Soviet experiment, countered with the plea: "Was there some other and perhaps better way to learn whatever we think we have learned without going through such suffering?"

The praxiological and interpretive schools thus appear to be "classical paradigm light," where they have selectively retained certain elements of the classical paradigm and jettisoned elements that are inconvenient, wearisome, or too restrictive to their other aims. Each desires to be identified as a separate school, with separate standards of scholarship; and certainly the standards of scholarship vary mightily between the three. The predictive school would necessarily have the most rigorous/ rigid standards, because it would presumably have a very measurable indicator of success or failure: either the prediction happens, or it does not. The interpretive represents an abandonment of standards, because a scholar in that field, if challenged as to the validity of their interpretation, can fall back into the fortress of "it's unique and can only be understood subjectively." There is no valid test for truth, only subjective "understanding." As for the praxiological school, the objective here appears to be primarily to justify the imposition of certain treatments (associated with socialism or communism) regardless of any evidence that they may or may not be causally connected to the desired social condition.

The interpretive and praxiological schools both represent a departure from the classical paradigm. By departing from the existing paradigm they can establish their own standards, different in intent or rigor from classical science standards. As such, they are clearly competitors with the classical science paradigm and, implicitly, with each other, because they operate under different standards. As Cook (1994, p. 99) stated: "The isolation from practical test of some non-Marxist social scientific inquiry is not an accidental aspect of liberal social science, but something quite integral to its value viewpoint."

Cook's (p. vii) plea to retain them all is rather naive, considering that he also acknowledges that the "interpretive, predictive, and praxiological outlooks have each long challenged the validity or at least the relative value of the other two epistemological 'families.'" Again, the debate between Thomas Aquinas and Averoes, the Arab physician and philosopher, comes to mind: can a statement be both true and false at the same time? How can a statement be true under the interpretive school yet false under the praxiological?

Other groups representing alternate intellectual approaches are consciously striving to unseat the ruling paradigm. Their desire is to make their beliefs the mainstream, or, as Bernstein and Rorty have rather idealistically asserted, to reach consensus in the scientific community (Cook, 1994, p. 3).

There are those that contend that postmodernist views on the nature of truth are *already* the mainstream. Witness this statement from Curthoys (1991, p. 391), a Professor at the University of Technology in Sidney:

> Most academics in the humanities and social sciences, and as far as I know in the physical and natural sciences as well, now reject positivist concepts of knowledge, the notion that one can objectively know the facts. The process of knowing, and the production of an object that is known, are seen as intertwined. Many take this even further, and argue that knowledge is entirely an effect of power, that we can no longer have any concept of truth at all.

Relaxation of the Standards of Science

When a paradigm has lost its power to enforce discipline on its field, the result is to open the door to many other competing visions. When paradigms can no longer enforce quality standards, it would be expected that some disreputable ideas will gain an audience, and even gain a standing in those portions of the scientific community searching for new normative mental models. Current times provide ample evidence of this phenomenon.

Rupert Sheldrake (1987) believes in "morphogenic fields," invisible, low energy fields that serve as "species' memory" and help shape behavior. Sheldrake asserts that some of what we know how to do comes not from our own acquired learning, but from knowledge that has accumulated in the human species morphogenic field. When part of the human species learns how to ride a bicycle, the morphogenic field assumes a pattern consonant with that learning. Other humans access the field through their individual energy, and their learning is faster and less laborious. Sheldrake's evidence for this is the fact that some cultures have developed faster than others.

Wheatley (1992, pp. 52-53) takes the idea of "fields" one step further.

> As a generation of managers, we have been focused on many of the ethereal qualities of organizations—cultures, values, vision, ethics. Each of these words describe a *quality of* organizational life that we can observe in our experience, yet find elusive to pin down in specifics. Recently, while doing work on customer service for a retail chain, I asked employees to visit several stores. After spending time in many stores, we all compared notes. To a person, we agreed that we could "feel" good customer service just by walking into the store. We tried to get more specific by looking at visual clues, merchandise layouts, facial expressions—but none of that could explain the sense we had when we walked into the store that we would be treated well. Something else was going on. Something else was in the air. We could feel it, we just couldn't describe *why* we felt it. It seems to me that field theory provides a useful explanation to this and many other organizational mysteries. At one level, thinking about

> organizational fields is metaphoric, and interesting concept to play with. But the longer I have thought about it, the more I am willing to believe that there are literal fields in organizations. I can imagine an invisible customer service field filling the spaces of those stores we visited, helping to structure employees' activities, and generating service behaviors whenever the energy of an employee intersected with that field. . . . With such a powerful structuring field, certain types of individual behaviors and events were guaranteed.

Wheatley goes on to forcefully assert: "This is not a fantasy image."

Another example is the extent that devotees of particular approaches attempt to stretch their pet theory to be all things to all people. For instance, there are the proponents of Social Darwinism, specifically Geoffrey Miller (2000), who proposes that art, music, and literature are produced largely by men between the ages of 20 and 40 to attract mates. Culture, in his view, is a sexual display. The image of *Antigone* or *The Texas Chainsaw Massacre* as social displays intended to entice one of the opposite sex into a mood to reproduce is daunting. And, of course, the theory would also imply that the productivity of bachelors would be greater than that of married men, a proposition yet to be demonstrated.

Probably one of the most interesting examples is the Gaia hypothesis, because it serves as a case where a serious scientific proposal was co-opted by those with unorthodox mental models. As related by Schneider and Boston (1991), the Gaia Hypothesis was first mentioned in a brief paper by Lovelock that appeared in 1972 in *Atmospheric Environment*. Lovelock's idea was that the biological organisms take an active part in molding the environment of a planet. According to Lenton (1998,

p. 439): "The Gaia theory proposes that organisms contribute to self-regulating feedback mechanisms that have kept the Earth's surface environment stable and habitable for life." Using computer modeling, Lovelock showed a model where Earth, without its organic population, would have a very different temperature, weather, and distribution of water than one with living plants and animals. In effect, without biota, the Earth would have an unlivable atmosphere and temperature profile.

The response across the various communities to the Gaia hypothesis was fascinating. There were serious scientists who were interested in testing the hypothesis and examining its implications. There were also others with distinctly different agendas and different mental models of reality. According to Schneider and Boston (1991, p. xiii),

> . . . the Gaia hypothesis attracted the most attention from theologians interested in the possibility that the Earth controlled its environment on purpose (i.e., teleological implications), from those looking for "oneness" in nature, and from those defending polluting industries. . . . [N]onscientific side issues diverted attention in the scientific community away from a serious analysis of the Gaia hypothesis and its implications.

One example of the fringe elements that have fluttered around the Gaia hypothesis is Ralph Abraham, the author of *Chaos, Gaia, Eros*. As quoted by Durham (1997, p. 51):

> The peaceful partnership society of our Cro-Magnon ancestors was characterized by the three-way cooperation of three principles, Chaos, Gaia, and Eros, symbolized by the triple-headed goddess TriVia, the Pythagorean Y, and the victory symbol that emerged during World War II, and in the peace movement of the 1960s. The healing of our planetary society

> from scars of the past six millennia, the Periodic Epoch, will be an Orphic enterprise. We must welcome the Chaotic Epoch.

Rose X (2002) provides the following additional information on the video version:

> A stunning alchemical computer assemblage of thought, sound and image based on Abraham's work as a leading theorist of the dynamics of chaos. He is founder of the Visual Mathematics Project and Professor of Mathematics at the University of California, Santa Cruze [sic]. His curiosity led him to dig deep into the mystic past where he found Orphism, a feminine psychedelic worldview based on the deities of Chaos, Gaia and Eros.

By most standards, one of Abraham's qualifications would be placed in the ranks of the "hard scientists." His grounding in mathematics certainly has not constrained him to the classical paradigm.

The point that is again very effectively made by the situation of the Gaia hypothesis is that the classical Scientific Method paradigm has been so weakened that non-scientific elements of mysticism can be found co-existing with hard scientific proposals. In the example shown above, even a "hard" scientist can have mental models of a "feminine psychedelic worldview" coexisting with the "hard science" mental models associated with mathematics. The fact that Abraham has not been marginalized in the scientific community, and that his feminist psychedelic worldview is allowed to stand next to serious scientific study of an interesting scientific questions, is indicative of the weakness of the Scientific Method paradigm to dominate the field of science and marginalize competitors, as it was able to do for so long to the Postmodernists in the first seven decades of the Twentieth Century.

The Scientific Method paradigm requires that hypotheses be subjected to experimental verification and empirical proof. In the absence of an equivalent social sciences paradigm there is no objective way to pass judgment on hypotheses. Arguments are substituted for proofs, which opens the way for self-fulfilling observations: someone proposes that A causes B, I look out in the world and see lots of B, which (in the absence of scientific standards) validates the existence of A. It is like the scientists looking for the weight of *caloric*: temperature went up, the weight of a hot block of metal went up, therefore, Q.E.D. A governing paradigm guards against conceptual silliness. It establishes and maintains rules of "good science." In its absence, *post hoc ergo prompter hoc* becomes the rule rather than the error.

Wheatley can be used again as a fine example of conceptual silliness. In *Leadership and the New Science* (1992), she writes more like a poet than as a serious scholar. Anthropomorphism in her work is rife. She speculates on the "motives" of mountain streams; she contemplates a moose standing next to a sapling and draws conclusions regarding how organizations should be controlled. Yet, rather than being banished for foolishness, Wheatley was, at one time, on the faculty of a prestigious institution of learning (in Management Science, not the Poetry Department). She found a sufficient market for her ideas to eventually set up as an independent consultant. Her book evidently resonated with a large part of the community at large, because it sold hundreds of thousands of copies.

Yet, to one applying Scientific Method thinking, all it consisted of was unproven speculation, overextended metaphors, New Age mysticism, and a great deal of appeal to the emotions. This book was assigned in one of my doctoral program classes. All of the members of the class who had a "hard science" background reacted negatively to the book. Others in the class felt it was very good (although they became rather evasive when asked for specifics). While this is certainly not a scientific study, it does provide an additional data point regarding the existence of multiple paradigms. The fact that this book was a best-seller indicates the extent to which the alternative New Age paradigm has penetrated into society in general and, specifically, the social sciences community.

Belief in phlogiston is still nurtured in some corners of the world. There are witches and covens that cast spells. However, the market for such ideas among reputable physical scientists should be nonexistent. (To paraphrase that great apostate realist, Scrooge: "Are there not madhouses? Do I not support them with my taxes?")

In the social sciences the classical paradigm is under attack. The weakness of the paradigm results in standards that are painfully in flux. Anything becomes acceptable because nothing can be ruled out. Stanesby (1985, p. 155) quotes Krige as saying that "anything goes . . . means that, in practice, *everything stays.*" There are no standards or rules to separate the wheat from the chaff. When the social sciences tried Logical Positivism and failed, the classical science paradigm itself was challenged. Hume's Fork was lost. The gates were opened to any number of quack nostrums.

One point must be made clear. The purpose of this section has not been to belittle social science or social science scholars. That would be like belittling Plato or Aristotle because they sometimes pursued wrong paths. The point is that an effective paradigm provides a means of enforcing a standard; it defines what is acceptable as "rational." The prevalence of postmodernism, radical spins on the Gaia hypothesis, and other disparate views indicate that there is no ruling paradigm strong enough to jettison such concepts into the "silly" pile. If the existing paradigm has become so broad that morphogenic fields and administrative management by moose are all considered normal science, then the paradigm has broadened itself out of existence.

Heraclitus the Obscure lived in the later part of the Sixth Century, B.C. He believed that the world is like the flame of a candle, ever the same in appearance, but ever changing in substance. Like a river, the world may look the same, yet you cannot step into the same river twice. But Heraclitus did not mean that all is chaos; he believed that behind the flux and strife there was a guiding principle, an organizing force, which he called *logos,* the Greek word for "reason" or "logic."

It would appear that the social sciences have returned to Heraclitus' vision of flux and strife, but have abandoned *logos*.

Spengler (1926, pp. 117-121), in his monumental *The Decline of the West,* foresaw the deterioration of logic as a natural progression of the course of history. He described a duality of concepts that he called "destiny" and "causality."

> The Destiny-idea demands life-experience and not scientific experience, the power of seeing and not that of calculating, depth and not intellect. There is an *organic logic*, an instinctive, dream-sure logic of all existence as opposed to the *logic of the inorganic,* the logic of understanding and of things understood. . . . Between them there is all the difference between a feeling of life and a method of knowledge.

Horgan (1996, p. 24) noted that Spengler predicted "the decline of science and the resurgence of irrationality would begin at the end of this millennium." If Wheatley, Bhabha, Sheldrake, Abraham, and the postmodernists can be seen as representing the irrational, Spengler's prediction is fulfilled.

CHAPTER XIV

SOCIAL SCIENTISTS APPLYING THE CHAOS PARADIGM

There are scholars who have recognized that human behavior can be chaotic. There has been a considerable body of literature in the social sciences grappling with the application of chaos and complexity theory in the social sciences. The quality of these efforts ranges from excellent to abysmal. If the principles of medical triage were to be applied to these works, they could be sorted into several classifications:

Exploitive: The exploitive literature employs the words "chaos" or "complexity" without any pretense at addressing the underlying theory. To many of these works, the terms are used to describe the environment or simply employed for marketing purposes.

An example is Peters' (1988) *Thriving on Chaos: Handbook for a Management Revolution.* For some reason Peters has come to believe that the business environment consists of chaos. He sees "An Accelerating American Decline," "A Decline in Service Too," and other supposed indicators that the American economy was about to self-destruct. With the failure of his theories put forward in his previous book *In Search of Excellence*—most of the companies that he named as exemplars were in serious financial trouble during the succeeding recession in the late 1980's—Peters evidently concluded that if his theories did not work, well, then, the world must be submerging into chaos. Thus, the premise of his book was an attitude change: "The true objective is to take the chaos as given and learn to thrive *on* it."

Clearly, Peters is not in tune with anything regarding chaos or complexity theory. To him, chaos is just disorder, a term to exploit in marketing a book.

Adaptive: Many see chaos theory as if it were a mirror: they look deeply and see another manifestation of what they have always believed. Their own internal mental models dominate and filter the concept.

A foremost example of this, one who has already been beaten upon rather unmercifully, is Wheatley. In *Leadership and the New Science* (1992, p. xii), she looks at chaos theory and sees things quite differently than most mortals. She sees yearning streams, and turns moose into metaphysical exemplars of how to manage organizations. Her work features mystical sentences of lyric beauty but vacuous content. Some statements are phrased so that one *feels* that she has plumbed some new and secret depths and discovered a truth that has eluded lesser mortals. But, upon analysis, reading her work is like biting into a cream puff and discovering all puff and no cream.

Wheatley obviously does not *understand* what she has read about chaos in the same way that a mathematician or a physicist understands. Her filters and lenses are entirely different. She does not recognize that quantum physics is not a suitable place to learn lessons about human organizations. However, she is using an area that many other people do not understand to promote ideas that are similarly incomprehensible. In general, Wheatley's enthusiasms are simply overblown, a fine example of the fate of science without the discipline of a strong governing paradigm.

Abraham's *Chaos, Gaia, Eros,* also mentioned in an earlier chapter, serves as another example of melding chaos studies with the mystical. According to Durham's (1997, p. 51), *Chaos, Gaia, Eros*

> . . . is an example of the increasing number of books that portray Chaos Theory as some Grand Unifying Metaphor (GUM) for everything. . . . Chaos is considered by many to be "a load of fashionable number crunching" at best, or a kind of theory du jour of cocktail party scientists, at worse. Unfortunately, there is a vast swell of books on Chaos Theory that gives nothing more than lip service to the real underlying mathematical principles of Chaos.

Confused: There are many scholars who make sincere efforts to incorporate chaos or complexity theory viewpoints. For example, according to Durham (1997, p. 38): "It has also become very popular for political scientists and military strategists to apply the tenets of Chaos Theory to such soft-science issues as political science and international relations." Unfortunately, many of them just come out wrong. Mostly, these false starts are caused by a lack of depth of understanding of chaos or complexity phenomenon. Analogies are drawn where analogies just are not appropriate.

An example of this school is Stacey's *Complex Responsive Processes in Organizations*.

Stacey (2001, p. x) says some of the right words in his introduction, when he talks about voices that

> . . . emphasize the radically unpredictable aspects of self-organized agency, which talk about agents and the social world in which they live as mutually created and sustained. This way of thinking weaves together relationship psychologies and the work of complexity theorists who focus on emergent and radically unpredictable aspects of complex systems.

However, when he begins to draw conclusions from this material, he begins to go astray. For example, his conclusion from the paragraph quoted above is, "The result is a participative approach to understanding the complexities of organizational life." This is flat wrong. Computer simulation experiments have taken complex adaptive agents and have demonstrated some properties of self-organization. However, the existence of self-organization in response to the principles imbued into the artificial agents does not prove that humans work the same way, that human organizations ever emerged from this process, or that such characteristics suggest that a "participative approach" is in any way better than other approaches. This is an example of taking a "type four" characteristic from one end of an metaphor and assuming that it holds on the other end.

Stacey (2001), in taking some lessons from emergent behavior of computerized agents, carries his suppositions too far. For example:

> . . . a key insight from the complexity sciences relates to the intrinsic properties of interaction. The modeling of complex systems demonstrates the possibility that interactions between large numbers of entities, each entity responding to others on the basis of its own local organizing principles, will produce coherent

> patterns with the potential for novelty in certain conditions, namely, the paradoxical dynamics at the edge of chaos. In other words, the very process of self-organizing interaction, when richly connected enough, has the inherent capability to spontaneously produce coherent pattern in itself, without any blueprint or program. (p. 93)

Stacey, it speaks of "patterns" and "self-organizing" as if they are automatically good ("richly connected" yielding a "coherent pattern"). The models show that patterns and self-organization can indeed emerge, but they may not be optimal to a situation, or even good. There are destructive patterns that can self-organize. Stacey assumes a quality that may not exist.

In addition, he does not understand the computer models. Many models of complex adaptive agents have the idealized agents learning through huge numbers of successive *failures* to develop new and more appropriate behavior rules based on the particular fixed conditions modeled. An individual agent must fail a large number of times in order to evolve a successful strategy, and must be in a position to constantly adapt its internal mental models to new situations. This can be executed in computer simulations where we can define our own conditions and rules, and make the agents infinitely flexible, but it does not match well with what we know about the way humans function.

Recall that, in discussing cognitive functions, I pointed out that humans tenaciously hold to established mental models, to the extent that false identifications will be made, or information not in line with the model will actually be rejected and not processed. These confirmed human mental processes are not in line with the model that Stacey develops, based on some sociological theories of communications developed by Mead in the 1930s. Most computer models of reactive agents evolve through things like genetic algorithms, which have an optimization process based on the death of unsuccessful agents (!!!), or some cognitive models that allow learning through massive numbers of previous failures. To propose that organizations function through this method, by learning through continual failure, is a recipe for bankruptcy. Any organization with a usual turnover of, say 20% per year would require constant failure in order to assimilate new employees.

In other words, Stacey has uncritically taken some computer simulations and applied them to organizations without understanding their underlying assumptions and constraints. While complex adaptive agents in simulations can show that some self-organization is possible, that does not mean that is a good strategy for a human organization to employ. Before applying any computer simulation "insights" one must reconcile the assumptions inherent in the behavior of the agents with what we know about human cognitive processes and human behavior.

The process of analogy is very slippery. One demonstration of this is the old elementary school lesson where the teacher lines up all the children, and to the first child whispers a truth. The exercise is for each child to whisper the truth to the next in line, and then we are to be impressed with the result that comes out at the end, invariably very different from the initial truth. Similarly, some social scientists are comparing A with B, B with C, C with D, and expecting in the end to have D still have some connection with A. Stacey plays such a game with attempting to combine complexity theory with established social science metaphors; the result is something far astray from common sense. Stacey (2001, p. 116), in a section offering conclusions, states:

> Knowledge, it follows, cannot be stored away nor shared simply because it is bodily action. . . . This is clearly a notion of knowledge that differs fundamentally from that in mainstream thinking. The alternative notion I am suggesting leads to the conclusion that it is impossible to measure knowledge or manage it.

As indicated, Stacey concludes, quite rigorously from his assumptions and models, that

knowledge = bodily action.

Clearly, the combination of chaos or complexity theories with social science metaphors, combined with an undisciplined process, can generate the bizarre and unusual. A serious problem results when people take conclusions drawn from computer models and simulations of chaotic processes without understanding the underlying details. To make an analogy, in these processes the Butterfly Effect holds: small misunderstandings in the constituents of the experiments can be magnified into large errors when transferring concepts outside their intended venue.

That is not to say that chaos will not be better understood in the future, and that better mental models will not be created. As chaos and complexity is better understood, and as it is encountered more and more, it will become a part of our culture. Appropriate metaphors will, eventually, emerge.

The following is from Csikszentmihalyi's *Creativity: Flow and the Psychology of Discovery and Invention* (1996, p. 7):

> In cultural evolution there are no mechanisms equivalent to genes and chromosomes. Therefore, a new idea or invention is not automatically passed on to the next generation. Instructions for how to use fire, or the wheel, or atomic energy are not built into the nervous systems of the children born after such discoveries. Each child has to learn them again from the start. The analogy to genes in the evolution of culture are *memes,* or units of information that we must learn if culture is to continue. Languages, numbers, theories, songs, recipes, laws, and values are all memes that we pass on to our children so they will be remembered. It is these memes that a creative person changes, and if enough of the right people see the change as an improvement, it will become part of the culture.

I believe that mental models can be added to the list that includes songs and laws and recipes. Our mental models and their associated abbreviated processing methods were optimized for survival in an environment requiring rapid recall and quick prediction. But they are deficient in their ability to handle certain classes of experiences. In the past, when confronted with phenomena beyond our understanding, like the old woman of Yido we might build incorrect models.

Models are corrected by experience and learning. Our predominantly deterministic Scientific Method mental models are demonstrably inadequate when applied to social science phenomenon. We have a great deal of difficulty coming to grips with the nonlinear, chaotic, stochastic world, because the very nature of mental modeling is to impose a "first estimate" of linearity, regularity and determinism. The brain does this as a survival mechanism to facilitate rapid recall and decision making.

As a rapid approximation, in dealing with sabre tooth tigers and woolly mammoths, it facilitates survival. In such cases of survival, to quote the Soviet Admiral Gorshikov, "Better is the enemy of good enough." But in addressing man's complex social relationships, "good enough" is a barrier to full comprehension.

Of course, I am here discounting the effects of knowledge gained from morphogenic fields; confirming evidence is awaited.

Reasonable First Steps: The last classification of social science literature dealing with chaos and complexity theory are those that appear to be taking rigorous and reasonable steps to come to grips with chaos and complexity. Most of them are in what could be described as the "exploratory" phase, where scientists are examining old anomalies and discovering chaotic roots. In the chapter on chaos theory, I included a few pages of examples of the advances being made through chaos and complexity theory in the fields of economics, political science, sociology, organization science, and other areas. I expect that this trend will continue.

Previously, I quoted an eminent scientist in saying that a new paradigm becomes established when the older generation gives way to a generation that is familiar with the new paradigm from the outset and trained in its intricacies. The explosion of chaos and complexity theory publications seems to date from the early 1990s. It could be that we are in the early phases of this transition.

This chapter was not intended to be a complete survey of the state of complexity theory in the social sciences. Rather, the intent was to provide some examples of the problems that have surfaced in some studies to date. My point is to reestablish that the lack of appropriate mental metaphors of chaotic phenomenon is an impediment to progress. These mental models are controlled, to a large extent, by the paradigm that is established for the conduct of the physical sciences. The lack of a powerful controlling paradigm, with its associated powerful, applicable mental metaphors, has resulted in a great deal of stray scientific thinking. A chaos paradigm will need to be developed in order to serve as an exemplar of "normal science," as a form of quality control, in order to provide a basis for weeding out the bizarre to maintain, and learning from, good science.

MANIFESTATIONS OF CHAOS IN AN

ECONOMIC THEORY OF THE ORGANIZATION

VOLUME II

by

Alan D. Zimm

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CHAPTER XV

GLOBAL IMPLICATIONS AND CONCLUSIONS; SUGGESTIONS FOR FUTURE RESEARCH

The first concern will be to evaluate if the purpose of this dissertation has been supported.

The purpose of this dissertation was to clearly state and substantiate the argument that the social sciences are undergoing a broad battle between scientific methodologies, triggered by the failure of the Scientific Method to develop social science knowledge. I believe that this overall hypothesis has been substantiated.

In particular, there were four key goals that were to be investigated:

1. *Human behavior based on simple principles can result in complex or chaotic behavior.* This has been shown though both the simulation of the organization, and the Organization Logistic equation.

2. *Complex or chaotic behavior cannot be correctly analyzed with the Scientific Method.* This was demonstrated by examples.

3a. *The social science community has tried to use the Scientific Method to develop social science theory, and has failed.* This was shown, using the field of

Public Administration as the primary example, but also through examples in other fields in the social sciences.

3b. *This failure can be viewed in sociological terms. The inability of the Scientific Method to deal with complex or chaotic systems has resulted in predictable and identifiable behaviors in the social science community.*

4. *The Scientific Method can be likened to a mental model in human cognitive processes. The violation of a human mental model can cause certain behaviors. These behaviors can also be identified in the social sciences today.*

The connection between these the behaviors predicted and the behaviors observed is clearly very high. It would not be expected (if human behavior did indeed exhibit deterministic chaos and extreme sensitivity to initial conditions) that all of the fields of social science would exhibit all of the predicted characteristic behaviors; however, it was not necessary to search very hard for examples. Certainly an in-depth examination of the behaviors on a field-by-field basis would be interesting and is recommended for future work. However, as a demonstration of feasibility, there has been enough of the predicted behaviors that fall in line with the predictions to give the proposition credibility.

Changing the fundamental worldview of social science, away from expectations of deterministic causality to one of complex behaviors grounded in simple principles of behavior, will allow us to better learn fundamental truths about human behavior. The new paradigm for the social sciences will look more like meteorology: the underlying principles will be appreciated and the general ranges of behavior understood, but predictions will have to be approached in a different way. Longer-term predictions will be recognized as subject to chaotic variation. Individual point predictions will not be possible, but it will be possible to change the boundaries of behavior or the propensity of a system (like an organization) to occupy certain states.

In this context, treatments will change behavior by adjusting the underlying principles that are the source of the behavior. Continual sampling and re-sampling of system behavior will be required to track variations and adjust path-dependent behavior. Variability and non-repeatability of results will be recognized as a normal condition, and will not trigger rejection of a hypothesis.

Individual human behavior is subject to wide variation. However, the behavior of large numbers of humans may be more predictable, in a similar way as the Law of Large Numbers in statistics points out that large samples of a system will tend to converge on the true underlying distribution of results.

A New Way of Approaching Problems

According to Durham (1997, p. 4): "Chaos is a fundamentally different way of viewing reality; it is a type of behavior that has characteristics in common with both order and randomness, but is not either." The social scientific community needs to come to grips with this reality. It augurs an entirely new worldview for understanding the social sciences, a new paradigm for gaining knowledge of social systems.

A new paradigm will provide new exemplars for gaining knowledge and a new foundation for training the next generation of scientists. Katz and Kahn (1978, p. 186) observed: "What scientists seek reflects their frame of reference, and each discipline teaches its own."

Harvey and Reed (1996, p. 295) relate that "despite three decades of development in the physical sciences, the social sciences are only now coming to grips with deterministic chaos and its worldview." They cite three different explanations for this. The first comes from the conceptualization of a development hierarchy in the sciences, where mathematics and physics and astronomy are at the top rung of this hierarchy as the most mature of the sciences, while the social sciences are at the bottom. "After all, they do not possess the mathematical or technical sophistication of the older sciences and are not prepared, therefore, to take advantage of their most recent innovations."

The second explanation is that the development of chaos theory has come mainly from the mathematicians, and there are only a handful of applied mathematicians working in the social sciences that have addressed the fundamentals of the new science.

Katz and Kahn (1978, p. 186) go on to offer:

> A third and final interpretation of the slow reception of chaos theory into the ranks of the social sciences underscored the fact

> that in the last thirty years science itself has become déclassé in many circles. The cultural revolution of the sixties identified science with the repressive and dehumanizing tendencies of modernism. Influenced by this radical humanism, many social scientists rejected quantification with a vengeance and have opted for a hermeneutic method in their research. In the process, they have steadily retreated into the deconstruction of texts, or into the study of postmodern and postindustrial *mentalites*.

To this I offer a fourth interpretation: that the natural cognitive models of the human brain make it difficult for human not trained in mathematics to build appropriate mental models of complex, chaotic, or stochastic processes. The failure of the social sciences to discover laws or principles of human behavior caused social scientists to challenge the classical paradigm. The weakening of the prevailing paradigm allowed fringe philosophies of science, previously effectively contained by the discipline of the prevailing classical paradigm, into a position to challenge the mainstream. In the absence of an alternative mental model that could deal with chaotic behavior of social systems, these challenges essentially imploded, offering the alternative that there was no alternative, no truth, no standards.

Durham (1997, pp. 13-14) included two figures entitled "Pre-Chaos Worldview" and "Generalized Post-Chaos Worldview," which she attributes to an unpublished paper by DeBlois. I have modified these figures to reflect what has been learned in this research (Figures 49 and 50).

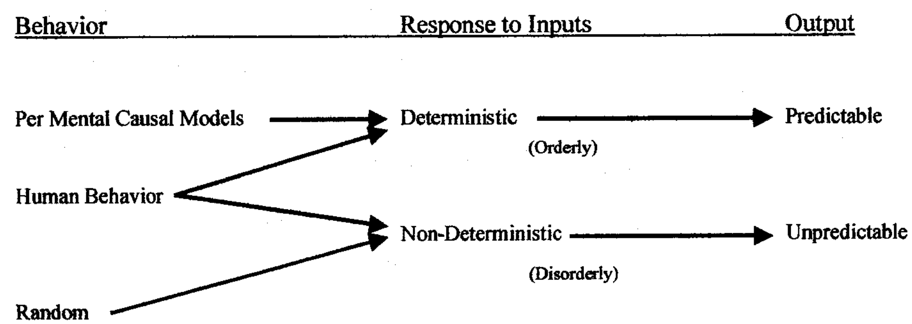

FIGURE 49

PRE-CHAOS WORLDVIEW

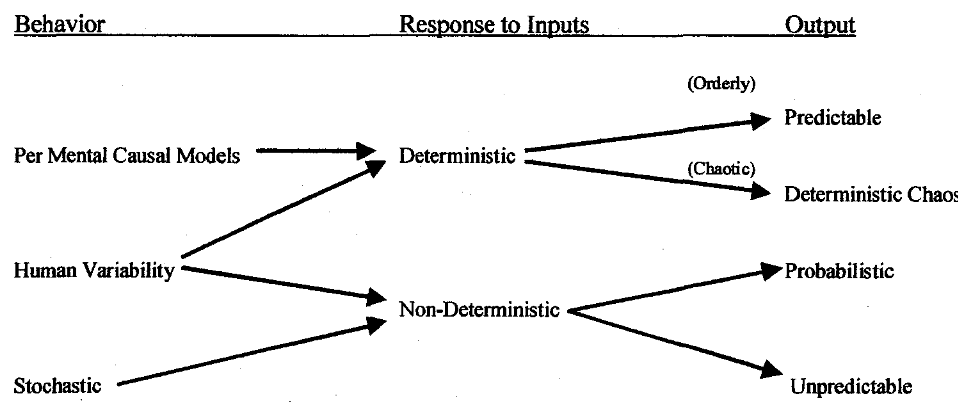

FIGURE 50

GENERALIZED POST-CHAOS WORLDVIEW

In Figure 49, the diagram shows how classical thinking has segregated deterministic and non-deterministic thinking. Certain things are believed deterministic, and we have developed mental models for them. We believe that we can predict the output of events by these models. However, under this belief system there is also the realm that is unpredictable. Random behavior is considered unpredictable; human behavior falls into this same category.

Figure 50 postulates a generalized post-chaos worldview. We still have our causal scientific models with their deterministic predictability. However, we also know now that there are deterministic models that can give deterministic chaos. For human behavior, we should recognize that there are regions that give deterministic behavior as well as regimes of non-deterministic behavior. Human behavior can thus range from predictable, to chaotic, to probabilistic, to unpredictable, to bounded. We understand that all of this complex behavior can be generated from a set of relatively simple laws or principles.

The difference between "before" and "after" consists of a fundamental expansion of human understanding. In "before" we segregated out a large part of human behavior as unpredictable and beyond human understanding. In "after" we extend human understanding a bit further. We recognize that some things are unpredictable, but that set is much smaller, shrunken by the creation of the new category of deterministic chaos that may give us understandings of processes, limits, and boundaries, if not exact predictions.

Kiel and Elliott (1996, p. 2) provide an idea of the significance of this new worldview. They state:

> The emerging paradigm of chaos thus has profound implications for the previously dominant Newtonian view of the mechanistic and predictable universe. While a Newtonian universe was founded on stability and order, chaos theory teaches that instability and disorder are not only widespread in nature, but essential to the evolution of complexity in the universe.

I believe that chaos is not only essential to the evolution of the universe, but that it is an essential component of any organization, providing a mechanism that contributes to the organization's adaptability to adapt to changing circumstances.

Elliott and Kiel (1996, p. 2) go on to add some additional thoughts that I believe are valid:

> The social realm is clearly nonlinear, where instability and unpredictability are inherent, and where cause and effect are often a puzzling maze. The obvious fact that social systems are historical and temporal systems also stresses the potential value of chaos theory to the social sciences. Social systems are typified by changing relationships between variables.

Clearly, there is much that is new and different when one approaches the problem from the viewpoint of a different paradigm.

Implications to the Science of Economics

There are implications of this work on the science of economics. In particular, the Organization Logistic equation is a way of modeling an environment with multiple organizations, each with their own costs and productivity and reinvestment ratios, competing in the production and sales of a single product. The

Organization Logistic equation has been shown to be related to May's Logistic equation and the Lotka-Volterra equations, which can generate deterministic chaos in certain regions. To my knowledge, this is the first demonstration of a representation of this type. It certainly merits additional attention from the economics community.

Relatively Simple Laws of Behavior Can Generate Complex Results

In the discussion of chaos, it has been shown that simple equations can return complex results. In experiments with artificial intelligence, very complex behavior has been simulated by the application of only a few rules.

There are two rather famous examples of this. First is the experiment with "boids." Scientists were interested in the flocking behavior of bird. As related by Waldrop (1992, p. 241), a computer program was created populated with "boids" who followed three simple rules. The rules were to "maintain a minimum distance between yourself and other boids," "match velocity with the closest boids," and "move toward the perceived center of mass of boids in its neighborhood."

The rules were few and simple, yet when animated on a computer screen, the behavior of a flock of boids replicated observed behavior of real birds. What was remarkable was that none of the rules said, "form a flock," but it happened.

Another experiment replicated the schooling behavior of fish with a similar set of instructions.

The inescapable conclusion is that a relatively small set of simple rules can result in very complex behavior. As Wheatley (1992, p. 11) stated (in one of the things she did get right): "Scientists now understand that order and conformity and shape are created not by complex controls, but by the presence of a few guiding formulae or principles." Fundamentally, William of Occam's and Einstein's observations apply: "What can be done with fewer [assumptions] is done in vain with more"; "our experience until now justifies our belief that nature is the realization of the simplest mathematical ideas that are reasonable."

This is reinforced in other observations of the real world. For example, Peters and Waterman (1996, pp. 508-509) talk about how excellent organizations are defined by a small number of governing values, rather than thick manuals of rules and regulations. Speaking of some of the luminaries of the business world, they found that

> . . . Carlson doesn't blush when he talks about values. Neither did Watson—he said that values are really all there is. They lived by their values, these men—Marriott, Ray Kroc, Bill Hewlett and Dave Packard, Levi Strauss, James Cash Penney, Robert Wood Johnson. They *believed* in the customer. They *believed* in granting autonomy; room to perform. They *believed* in open doors, in quality.

That some of these organizations later fell into difficulties (and some regenerated themselves, several times over) is not a nullification of Peters' and Waterman's observation, but rather a commentary on the non-linear nature of human activity, with perhaps a little of the Fractional Penny Effect thrown in.

There is evidence that an approach, assuming that fundamental principles of behavior govern some aspects of performance, can make significant changes in the performance of organizations. According to Caulkin (1995, p. 33), Knowledge Based Development Ltd., a United Kingdom management consultancy, has employed knowledge of how complex systems interact on the level of basic organizational design to advance productivity. "Productivity reportedly doubled at the construction yard, and the chemical company that had already sweated blood to get the cost of a plant down to economic levels took out a further 30 percent by working to the same principles."

Elliott and Kiel (1996, p. 3) addressed this issue when they wrote:

> Clearly, the fundamental gap between the clear success of knowledge acquisition in the natural sciences versus the rather minimal successes in understanding the dynamics of the social realm is the inherent nonlinearity, instability, and uncertainty of social systems behavior. . . . Social scientists have long argued that this relative knowledge gap was due to the relative complexity of the phenomena examined by the two scientific cultures. Yet chaos theory teaches that the "gap" between the two sciences may have been largely artificial. . . . Chaos theory seems to represent a promising means for a convergence of the sciences which will serve to enhance understanding of both natural and social phenomena.

There are further implications to the idea that simple rules can result in complex behavior, which I will address later.

A Small Sampling Can Generate Non-Intuitive Results

I discussed in the previous section on chaos where combat simulators operating in a region of chaos could return non-intuitive results, i.e., if you reinforced the winning side and then ran the simulation again, that side would then lose. This type of effect can also be seen in other situations where the system is operating in a bounded chaotic region around an attractor. The treatment in the experiment may well move the attractor, indicating a positive relationship between the treatment and the desired (expected) effect.

If, however, the movement of the attractor is small relative to the chaos boundaries, there will be a region of overlap, such as that shown in a Venn diagram in the intersection between two areas. Thus, the chance exists that a small sampling of the results—say, a "before" and "after" case, two samples—could result in a non-monotonicity. This probability would be high if the movement of the stable point is relatively small relative to the dimensions of the boundaries. The area of intersection would be large, increasing the probabilities that the treatment might be right, but, just by chance, the "after" sample moved in an unexpected direction relative to the "before" case.

Just applying this insight casts an entirely different light on to much of the progress in many fields of the social sciences. One example can be drawn from the literature on the interactions between personality and organizational behavior. Weiss and Adler (1984, p. 1) state: "Personality differences, once fundamental to the study of work motivation, attitudes, and leadership are now assigned only secondary roles in most theories of organizational behavior." As additional substantiation they quote Mitchell's statement in his 1979 Annual Review of Organizational Behavior:

"Personality variables probably control only a minor percentage of variance in behavior when compared to situational factors."

The original idea, that personality was an important factor in organizational behavior, is a common-sense proposition intuitively obvious to front-line managers dealing with such issues on a daily basis. However, as research was conducted, there were cases when the results of the experimentation contradicted the thesis. According to the popular interpretation of Popper's doctrine of nullification, all it takes is a single negative result to nullify the hypothesis, so scientists employing classical science methodologies would be driven to reject that personality had a major role in organizational behavior if there were even a few studies with contradictory or indeterminate results. From a chaos perspective this may be a wrong conclusion. Single point samples can only indicate the presence of a chaotic region, not define its boundaries or behavior.

Because of the nature of personality research, any investigation, or process of recording data, will have different impacts on different personality types, and will influence the overall study to a greater or lesser extent depending on the process in which the data was collected. The fact that their personality is being studied influences different personality types differently, just as some people can easily make a speech before a camera while others freeze and become dysfunctional.

Consider the further implications from this further quotation from Weiss and Adler (1984, pp. 2-3) regarding the state of personality research:

> Historically, personality research on organizational behavior has suffered from inadequate conceptual development and poor methodology and these factors have conspired to give personality a bad name. It is simply premature and unproductive to make any general normative statements about restricting the role of personality in organizational research. Adequate tests of the usefulness of personality must be conducted with theory based, operationally precise efforts, and past and current research on personality and OB has failed to do this.

It is cheering to read their reference to the need for theory-based research; however, consider the implications of conducting such research under the aegis of the wrong paradigm. Remember, the idea of a paradigm extends to the establishment of what constitutes "normal science"; it establishes the rules for what is good research and what is bad. I would contend that what is considered bad research under the current paradigm might be useful research in populating and bounding an uncertainty region for chaos-paradigm research. The implications of looking at social science under a different paradigm is that all previous research must be reexamined under a new microscope, using new principles, looking for different connectors, judged against different standards.

Methodological Implications

Many social science problems may not be amenable to classical statistical inference testing. Rather, social science problems should be sensitive to the existence of deterministic chaos or complex behavior. Future treatments in the social sciences must be attuned to the movement of points of stability and boundaries of results, rather than expecting a treatment to generate a single response vector.

Kellert (1993, p. 44) states: “As chaos sets in, we encounter the inadequacies of our methods, not the inadequacy of our laws.”

He goes on to discuss the methodological implication of chaos to the realm of classical physics:

> Newtonian mechanics breaks down at high speeds, small sizes, and now the transition to chaos as well. But the boundary of the applicability of classical physics does not have to be redrawn because of the failure of some fundamental laws. For it is not that the equations of motion must now be written in new ways or that basic concepts like mass or energy need to be redefined. Recall that a premier chaotic system like that of Lorenz is based upon a set of equations describing scrupulously Newtonian behavior.

The laws of classical physics remain, only they are bounded in their realm of applicability—only in certain regions do they give deterministic results. In other regions (like the three-body problem), they may give chaotic results. The laws may be correct—we just need to develop better methodologies for understanding their limits.

Assume for a moment that Organizational Behavior in humans is chaotic, and then think of the consequences inherent in the following quotation from Bowditch’s and Buono’s (1994, pp. 39-40) chapter entitled “The Research Process in Organizational Behavior”:

> One of the ways in which people attempt to simplify the world is to break down the complex sets of information that surround them into manageable portions. Within the field of Organizational Behavior (OB), this process often takes the form of propositions about human behavior and its causes. . . . As members of organizations, it *is* important to understand what individuals *believe* to be true about organizational behaviors. However, it is

> also critical to know the extent to which such beliefs are supported by empirical research. . . . First, such knowledge can contribute to effective organizational problem solving as compared with simple trial-and-error efforts. By studying research and analysis methods one can develop a greater capacity for understanding and *applying* the research reported by others. Moreover, an understanding of the whys and hows of research and data analysis can also facilitate the ability to assess the validity of research claims and evaluate the soundness of the various theories dealing with organizational behaviors. . . . Organizational researchers, on the other hand, will want to know as specifically as possible *why* the change was effective, what *caused* the change, or if it is impossible to identify what caused the change, what is *related* to the change.

If social science should be considered as problems in chaos rather than problems in classical science, it has profound impact on how we go about doing the business of science in the social world, and how we consider the results of past analyses. Consider two things that chaos theory teaches us: first, that relatively simple "laws of behavior" can return complex results; second, that an experiment with only two sample points could return results that contradict a particular thesis, when in actuality the thesis is correct but the variability due to chaos.

Chaos does not always imply that our sampling requirements will be astronomical. Some techniques have been investigated to ameliorate this problem. For example, Cheng and Tong (1994, pp. 1-22) relate:

> By studying systematically the orthogonal projections, in a particular sense associated with a (random) time series admitting a possibly chaotic skeleton . . . we describe a geometric characterization of the notion of embedded dimension within a statistical framework. The question of sample size requirement in the statistical estimation of the said dimension is addressed heuristically, ending with a pleasant surprise: the curse of

> dimensionality may be lifted in the excessively stringent case. . . . Once a model has been fitted, we may examine its skeleton (=signal) with the aim of detecting chaos by evaluating its correlation dimension, Lyapanov spectrum, etc. Fortunately the curse of dimensionality does not necessarily apply here. In short, we prefer to place chaos detection within the context of signal extraction, the signal being the skeleton which may be a limit point, a limit cycle or a strange attractor.

In other words, for certain problems it was possible to obtain information on the signal (which might, in a social science context, correspond to the "law of behavior"), and other characteristics of the chaos field such as limit point, limit cycle, and strange attractor, with a considerably smaller sample from the signal than would be expected—the "pleasant surprise" is that a model of the signal can be fitted that can give us a great deal of information about the signal itself, in spite of its chaotic tendencies.

Whether this technique would be applicable to social science problems is unknown. However, the study of chaos is in its infancy; if the universe is run on a set of simple laws or relationships generating complex behavior, it could very well be that the analysis of that behavior can be done using much simpler techniques and better tools than we have applied to date. As an example, I can remember that when I went to graduate school in Operations Research in the early 1980s, we studied linear optimization in its theoretical form only, because only the most simplistic problems could actually be solved. Now there are computer programs that can solve linear optimization problems consisting of hundreds of equations, variables, and constraints, and the programs can run on a personal computer.

One of the things that Kuhn observed in the transition from one paradigm to another is that, initially, all problems were seen as huge and complex. Eventually there emerges that simplifying assumption that puts all the complexity of data in context and makes it orderly and manageable. I would suspect that this pattern holds here. If enough smart people work on this problem, the future should yield to us techniques that make problems in chaos and complexity much more accessible.

Is this really possible, especially considering the butterfly effect that makes all long-term forecasts fly away from each other? Yao and Tong (1994, p. 57) offered the following:

> It is well known that deterministic chaos is characterized by the sensitivity to initial conditions. However, it is increasingly recognized that a purely deterministic system rarely exists in reality because stochastic noise is ubiquitous; even the computer generation of time series using a purely deterministic chaotic map cannot be free from rounding errors. Accordingly, it is more pertinent to replace the dynamics by the transition probabilities from states to states. Indeed, Cheng and Tong (1994) has shown how a deterministic dynamical system which admits a compact attractor can, in a noisy environment, give rise to an ergodic stochastic system. A convenient framework for this stochastic system is the Markov chain over general state space. As a result, nonlinear autoregressive models emerge quite naturally as a realization of this framework for the study of noisy chaos.

Now, we may not know a great deal about the mathematics of chaos, but Markov chains are something familiar and understandable that we can hug close to our chests. Complex problems in the mathematics of chaos can sometimes (so far) be reduced to other, less complicated techniques; this offers hope that complex problems in the (chaotic) social sciences might also be so reducible. However, in discussing economics (which I include as a social science), LeBaron (1994, pp. 137-138), commenting on the chaos connection between economic theories and empirical results, stated that "the theorist operating in this area will need to use new techniques, since old standards of estimating and diagnostic testing may fail." New tools will need to be developed and understood.

There are indications that some of the intimidating problems of deterministic chaos might not be totally impenetrable. Smith (1994, p. 87) wrote: "Variability and unpredictability are two of the hallmarks of deterministic chaos; it is our aim here to show how the first can be exploited to reduce the second."

Much social science data comes in the form of time series. There have been various attempts to develop universal algorithms that can look at time series data and predict its future. Certainly such a process would go far towards furthering our understanding of many social science phenomena; indeed, developing an underlying equation for observed behavior can be helpful in developing "laws of behavior." Unfortunately, there are limits to what can be done with predicting algorithms. A. S. Weigend (1994, p. 157) wrote in his article, "Paradigm Change in Prediction":

> Can we hope for the discovery of a universal forecasting algorithm that will predict everything about all time series? The answer is emphatically "no!" Even for completely deterministic systems, there are strong bounds on what can be known. The search for a universal time series algorithm is related to Hilbert's vision of reducing all mathematics to a set of axioms and a decision procedure to test the truth of assertions based on the axioms (*Entscheidungsproblem*); this culminating dream of mathematical research was dramatically dashed by Godel (1931) and then by Turing.

For Turing machines, the problem was that it was impossible for the algorithm to know when to halt; without that, there can be no universal forecasting machine/algorithm. If the "butterfly effect" is indeed operative in social systems, one would expect that any attempts to employ classical science modeling of organizations would meet with mixed results. In particular, statistical tests would not be able to explain a great deal.

That appears to be what happened in one large study conducted by Bozeman and Bretschneider (1994), where they built a large and elaborate model to test whether the "core" approach or "dimensional" approach to public-private organization comparisons were more appropriate. Predictably, their answers were inconclusive, which they decided to turn into a positive by asserting that the two approaches were not "mutually exclusive" but instead were "complementary alternatives."

The detailed findings were not persuasive. They reported that an "examination of the detailed results, equation by equation, demonstrates considerable variation in explanatory power. The best model explains over 53 percent of the variance . . . while the worst model explains less than 1 percent. . . ." In a hard science application, a model that only explains 53% of the variance would be considered a failure—and that appears to be the best that the authors were able to attain, yet in their conclusions they declared a measure of success.

If the butterfly effect were in force, one would expect wide variation in explanatory powers in the models simply because of the wide variations in results caused by microscopic differences in initial conditions. This appears to be in effect here, although the authors are insensitive to the possibility. Like the scientists measuring the weight of caloric, they were ready to declare victory in the familiar terms of their paradigm, rather than consider that something else was going on.

I believe that the great methodological implications apply to case study research. As I discussed earlier regarding the studies performed regarding dissertation research in the field of Public Administration, most Public Administration dissertations are case studies, particularly those of DPA students with considerable practical experience. Under a chaos paradigm, case study research will become simultaneously less important and more important. Case study research will become less important because of sensitivity to initial conditions and the wide variation of potential results makes it less likely that any conclusions would be universally applicable. However, case study research should be valuable in identifying the variables that should be considered in formulating a principle. Also, meta-analysis of case study research can be the means to discover generally applicable laws of human behavior. As such, I would suggest that the future for case study research lies along a path similar to critical research, i.e., a detailed examination of motivations and forces that caused movement within that system and which, when analyzed in concert with many other such analyses, can lead to universal generalizations of human behavior.

Kellert (1993, p. 45) offers a thought that can conclude this discussion of methodology: "Chaos theory issues a methodological challenge and not a metaphysical one."

Chaotic Systems are Unpredictable Except in Terms of Long-Term Trends and Occasionally in the Very Short Term; the Best Comparison Metaphor will be with Weather Forecasting

Gregerson and Sailor (1993, p. 777) argue:

> Some social behavior is hard to predict because it is, in a sense, unpredictable and the underlying social systems are inherently chaotic. This means that there exist social entities such as individuals, groups, organizations, or institutions, with virtually identical initial internal states, embedded in virtually identical environments, which can exhibit totally different behaviors even though their behavior is governed by the same set of rules or "laws."

I believe that the existence of deterministic chaos in social systems makes point prediction of cause and effect to be nearly impossible. However, the study of meteorology provides an appropriate metaphor. In meteorology, we have a good understanding of most of the basic processes that create the weather; we cannot, however, exactly predict the weather much further out than a few days. But we do know that the seasons are coming, and that the weather will in general behave in a certain way during the summer and adhere to certain limitations and boundaries during the winter, and to expect certain weather behavior during the transition. We have been able to predict, for example, that El Nino will cause a wetter winter in one area, and dryer in another. If we could do so well in predicting the behavior of organizations, it would represent a major step forward.

In the social sciences, I believe that our attention should be directed to attaining an understanding of the basic processes, and understanding the boundaries within which human systems are limited. We should learn the limitations of deterministic chaos, and where the systems transition to equilibrium or periodic behavior, so that we could, if desired, induce those conditions.

There May Just Be "Laws of Human Behavior"

I have alluded before to a characteristic of deterministic chaos systems where very simple rules can generate complex behavior. We have observed complex behavior in human systems, and always attributed that behavior to complex systems. I believe that this research reinforces the possibility that relatively simple laws of human behavior are generating the complex results we have seen.

Consider for a moment the possibility that there are laws of human behavior. As we have seen with the 'boids, we know that very complex behavior can be generated by relatively simple governing principles. We know, according to Kellert (1993, p. xi) that chaotic systems "can appear in the context of exceedingly simple and entirely Newtownian equations of motion," so there is the possibility that these simple laws manifest themselves in very complex patterns of behavior.

We would understand, then, from this perspective that we have to apply different analytical approaches and viewpoints to examining problems.

Instead of looking at single case studies and deriving point solutions, we have to perform meta-analysis of a range of situations and collect a great deal more data, so that we can (for example) determine the characteristics of the system's attractor or points of stability. Instead of looking to influence a single effect by implementing a treatment, we would then look to changing underlying principles of the operation of the system in order to change the location of the attractor or points of stability. We would understand that changing an attractor does not guarantee that every subsequent result will be in the favorable direction, but rather that the central tendency of the system will have been altered. It is like a ship steering a zig-zag course to confuse enemy submarines: a change in the base course does not mean that the ship will always steer the desired direction, but rather that the ship will, in the aggregate, make progress on the new course.

However, that has not happened. Instead, we appear to have examined the social sciences from the wrong viewpoint. We have expected that if we apply the right treatment, if we employ the right underlying principle of behavior, we will deterministically get the desired effect. If the desired effect was not achieved, well, then obviously, the treatment was wrong. So, the early pioneers of the social sciences proposed principles and laws of human behavior. Cases were found where these laws did not seem to hold; so, influenced by a scientific paradigm (with a little meddling from misinterpreters of Popper) that states that a single contradictory case disproves the principle, the principles were jettisoned. So many principles were proposed and jettisoned that, after a while, people began to question the very existence of principles.

My own belief is that principles of human behavior exist. I have seen them work too many times. In 20 years in the Navy, I saw good leaders who, by choice or by inclination, followed understandable principles of leadership: they took care of their people, they listened to them, they paid attention to detail, they were accessible, they placed a premium on good communications up and down the chain of command, they praised in public and chastised in private. I saw too many bad leaders that violated one or more of these principles; after some experience, I could see exactly where such poor leaders went wrong, and in what way they should change. Some principles of good military leadership have persisted from before the times of the Roman Legions.

There are also the principles of war—certainly war should be considered a social science, and not a branch of physics—that have shown enduring worth. The principles of war have not been jettisoned just because there have been some cases where a victor violated a principle. Why, then, do we insist on jettisoning other principles of human behavior because of one counterexample?

It may be that the future of the social sciences lie with this new perspective. This suggests that principles and laws of human behavior need to be reexamined from a new perspective and evaluated using different criteria.

There are skeptics. The skeptics have much to buttress their views, because there have been other "revolutions" that have proven to be less than revolutionary. Kellert (1993, pp. ix-x) is not particularly a skeptic, but he does sound a note of caution:

> The central insight of chaos theory—that systems governed by mathematically simple equations can exhibit elaborately complex, indeed unpredictable, behavior—is rightly seen as new and important. But sometimes there may be a temptation for researchers to hype their results, to make chaos theory sound too interesting, as if it will revolutionize our thinking not just about the physical world but about art and economics and religion as well. Considering that quantum mechanics was at least as revolutionary more than sixty years ago and that its cultural effects are still quite difficult to pin down, any grand claims for the implications of chaos theory may be setting us up for disappointment.

However, even while trying to contain his expectations, Kellert (1993, pp. ix-x) cannot fully discount the possibilities. He goes on to say: "Nonetheless, the new conceptual approaches and experimental techniques used in chaos theory raise important philosophical questions about the meaning of unpredictability manifested by the simple models studied and the nature of the scientific understanding they provide." There is much work to be done.

Revisiting Kuhn and Paradigms

In earlier chapters, I discussed Thomas Kuhn's theory that the advance of science consisted of stages where particular paradigms or *Weltanschuung* existed. Anomalies were discovered, there was a period of revolutionary science characterized by competing paradigms, and eventually the emergence of a new ruling paradigm. I also discussed in some length the criticism of Kuhn's thesis. In particular, there were questions regarding the historical accuracy of the process Kuhn described, in that there appeared to be many counterexamples of progress made without going through Kuhn's ideal evolution from paradigm to paradigm.

It would be interesting to reconsider some of Kuhn's ideas from a historical paradigm that would include a complexity and chaos viewpoint. In that view, it would be recognized that there were principles of historical development, that—because events are inherently chaotic—apply and yet can result in very complex behavior as history unfolds. From this viewpoint, Kuhn's ideas might hold as basic principles, with the exceptions being the inevitable result of deterministic chaos, which indicates that how they evolve in practice is path-dependent and sensitive to initial conditions.

I offer this as having potential application to the arguments presented in Chapters 4 through 6 in this dissertation. Unfortunately, while it would be fascinating to explore the idea in more depth, this also must be added to the "proposed additional work" bin. The conclusion that can be offered, however, is that the idea of guiding overarching principles that generate complex behavior has many potential applications, not the least of which would be investigating the possibility that there exists behavioral and political principles that guide the path of development of the history of mankind.

Summary

At this point, I would like to offer a summary of the points made in this dissertation, which I believe constitute (in whatever small way) do constitute an advance in our knowledge in Public Administration, Organizational Theory, Economics, and the social sciences in general.

1. The social sciences are currently undergoing a paradigm shift in methodology. The evidence offered to support this is:

- The controversy (and, to many, rejection) of the current paradigm, both in epistemology and in practice;
- The emergence of many other competing paradigms, with values that contradict those of the classical paradigm;
- The battle between these paradigms over fundamental values;
- The inability of these paradigms to communicate, based on differences in terminology and language use. This symptom has been noticed in other paradigm competitions; and

- The inability of the current paradigm to police the discipline and eliminate contenders that are ascientific, i.e., mystic, irrational, or without substantiating evidence.

2. One of the reasons for the tenacity of the classical paradigm in the face of decades of failure is that its methodology closely corresponds to natural human cognitive processes.

3. The rejection of the classical paradigm (and by implication the rejection of natural cognitive processes) is a contributing reason why many of the competing paradigms are so different, and reject such fundamental ideas such as the very existence of "truth," and rejection that there can be relative "quality" in ideas.

4. The classical paradigm is wrong for the social sciences. The classical scientific paradigm has failed to explain empirical evidence of the behavior of social systems.

5. A better paradigm for the social sciences is the chaos and complexity paradigm. The implications of this change in paradigm remain to be discovered.

A computer model was built simulating population changes in an organization in a financial environment with transaction costs. Organizations depicted in this model demonstrated extreme sensitivity to initial conditions, non-monotonic behavior, aperiodic oscillations with fractals, stable levels of division of the market, and seemingly spontaneous organizational failure. These results have significant implications to the study of economics and organizational theory.

6. The extreme sensitivity to initial conditions shown in the model would put to question the validity of an important assumption in neoclassical economics, that is, that price curves can be estimated by continuous functions, and that the methods of the calculus are appropriate for investigating problems in economic theory.

7. Because of changing background conditions, deterministic social systems will be very noisy and can show dramatic changes in underlying results structures. As such, the possibility exists that such systems can be depicted using other, simpler mathematical tools, such as Markov chains.

8. Social systems, in this case organizations in a competitive environment, may have an inherent "chaos content" based on their organizational characteristics. The chaos content of individual organization might be a positive factor in allowing adaptation of the organization to changing environmental conditions; a large amount of chaos in the system as a whole can make the system unstable, resulting in dramatic changes in the relationships between organizations, including the possibility of organizational failure.

Suggestions for Future Research

Suggested areas for additional research have been mentioned throughout this paper. This chapter will serve to consolidate them; in effect, it is a short summary of the areas that deserve consideration for future work.

The Model of Organizational Growth and the Organization Logistic Equation

The model of organizational growth presented here deserves additional scrutiny. First, it needs to be understood better. My objective in this work was to see if it was chaotic, and I believe that has been established.

Different decision making heuristics could be inserted in the model, both from a behavioralist's and an economist's perspective, to explore the effects of different decision methodologies.

Certainly the variables in the model could be empirically quantified.

I originally designed the model to handle two types of organizations under the New Institutional Economic's transaction cost scheme: an organization where transaction costs were so low that all production could be outsourced, and a organization where all production was best conducted internal to the organization. In this research I concentrated solely on the second type of organization. There is room to research the first type.

In addition, I looked at only three competing organizations. The model could be easily expanded to look at a greater number of organizations.

The price curve in the model was a simple linear decreasing returns price model. All the organizations were subject to the same price curve. The model is set up to handle more complex price curves that could be different for each organization. So, the model could be used to explore the effects of positive returns in price curves, or different-but-competing products, such as the competition between VHS and Beta for the home video player market.

The Organization Logistic equation is, I believe, unique. I was not able to discover any other scholars who have come to a similar derivation. The implications of the equation could either be trivial or could be broad. Additional attention from the economics community would be worthwhile. In particular, matching the Organization Logistic equation with real data would be valuable.

Chaos in Organizations

The idea of a "quantity of chaos" in a social system is potentially useful. This aspect of the model could be explored further, especially with regards to organizational failure. I also believe that the analogy with the chaos inherent in heart rate is useful: a degree of chaos in an organization may be useful in making the organization more responsive to outside conditions, while too much would be disturbing to order-loving humans and could be a cause of organizational failure. I am reminded of another medical analogy: the drug strychnine, in small doses, can act as a cure for certain nerve disorders; in larger doses, it is deadly.

This suggests a world of questions: does organization size (or rate of change, or organizational architecture, or leadership, or a host of other possibilities) change the chaotic behavior of organizations? What is the effect of the environment on the content of chaos in organizations?

Another area for investigation is the idea that chaotic systems have regions of deterministic behavior, regions of periodic behavior, and regions of chaotic behavior. How can we identify the boundaries of such regions in real-life human systems? Can we control our systems sufficiently—and how do we control them—to be deterministic when we want them to be deterministic, and chaotic when we want them to be chaotic? Can the degree of chaos or level of chaotic content in a system be measured, and then controlled?

Throughout this dissertation I have given little attention to one of the prime characteristics of a chaotic system: internal feedback. The feedback mechanisms in organizations can contribute to the amount and nature of the chaos in organizations. Research into the nature of feedback systems in organizations would certainly be a fruitful area of research.

I was also fascinated by the fact that introducing additional organizations into a competitive environment appeared to increase the chances of organizational failure in the model. That seems intuitive—more competition surely means more failures. The behavior of the organizations seemed as if some measure of chaos was transferred from one organization to another. Is this a reasonable or useful characterization? What are the implications?

Chaos in Human Systems

As mentioned earlier, when describing the decision making heuristics in the model, we have not studied human responses to chaotic behavior from a chaos worldview. How do humans cope with deterministic chaos in human systems? Is learning possible? Is there an optimal decision making scheme?

In history, we attribute the success of great men and women to some inherent character trait: their virtue, or their grasp of military theory, or their leadership. How does this perception change when we view the world as a social system under the domain of deterministic chaos? Could it be that human greatness consists of a string of good fortune, like going to Las Vegas and rolling sevens fifty times in a row, or is there something else? If indeed simple principles can result in complex behavior, than what simple principles contributed to the greatness of our historical figures? Perhaps it was this grasp of these central principles that lead to greatness?

There may be simple human principles or laws that guide human behavior and result in our rich variety of behavior. The social sciences generally began in searching for such principles, but rejected them due to the strictures of the classical Newtonian paradigm. I believe that the search should be renewed. In addition, there is the possibility that these human principles are not fixed, but fuzzy in nature. That appears reasonable, considering the number of variables that are considered in human decision processes and other human cognitive processes that would tend to have decisionmaking criteria fuzzy rather than distinct.

The idea of chaos as the governing paradigm for social science research opens a huge field of potential research. An early step that must be taken will be to develop the necessary tools. Just as mathematicians have developed specific statistical tools for use in social science research, help from mathematicians will be needed to develop the necessary tools to characterize, measure, and differentiate chaos in social systems. The first and obvious research target would be how to measure the boundaries of chaotic systems with a reasonable number of samples. Then, when a treatment is applied, tools must be developed to allow determination with confidence that these boundaries have shifted.

In mathematics of chaos, I believe that a new and interesting field will be in investigating the dynamic changes inherent in chaotic systems when parameters change. In most mathematical investigations of chaos, the attractors are fixed, parameters are fixed, and variables change. In social systems, the underlying environment is constantly shifting so, as shown in the results of the model of the organization, attractors may shift and the character of the patterns that they generate may change; in effect, the parameters change as well as the variables. For example, taking the standard logistics equation

$$X_{i+1} = sX_i(1-X_i)$$

The usual practice is to study the behavior of the system with s fixed. However, I suspect that in social systems, the equivalent of the fixed parameter s also varies dynamically.

I suspect that attractors move in social systems. Perhaps our objective in future treatments of social problems is to move the attractor in the desired direction.

Paradigms and Past (Rejected) Work

In the early chapters, I discussed the ideas of paradigms as proposed by Kuhn. Kuhn's ideas have been generally rejected because of the large number of counterexamples that other scholars have found, situations where there are no paradigms, or where change occurred without going through the paradigm shift process outlined by Kuhn.

These rejections are based on classical Newtonian criteria for judgment. However, what if history is viewed not through a classical lens, but through a deterministic chaos lens? Perhaps the image that would emerge is that there are such things as principles, or perhaps, more generally, principles of change in human systems. The underlying principles may be simple, but when they are combined they result in complex, chaotic behavior. This complex behavior may be reflected in the result that at times history followed Kuhn's prescription to the letter, while at other times considerable variation was exhibited. A re-evaluation of Kuhn's ideas through this new lens may be appropriate.

For that matter, much of the way that we view the development of history, and even such things as political theory and other social theory, need to be reexamined under a new scientific methodology.

I mentioned previously that early social scientists searched for laws of behavior, and that most of their work has been discredited. I would suggest that the criterion under which that research was discredited was improper. If we look at things from a fundamental grounding in chaos and complexity theory, it could very well be that much of this work has been discarded inappropriately. A fruitful field of future research will be to exhume some of the rejected ideas of the past and measure them against the new criterion. Perhaps in the past we have been right, it is only that we just thought we were wrong.

The hypothesis of this dissertation implies that many of the developments in the social sciences have their origins in dysfunctional mental models that have induced by the failure of the Scientific Method to produce social science knowledge. I have provided an outline and introductory justification for this thesis. Additional work would certainly be in order to examine the details of the hypothesis in the context of individual fields in the social sciences.

Conclusion

This chapter provided some initial suggestions for additional work. It is rather inadequate in both scope and detail. I would suggest that, if the idea of chaos in social systems was presented to several good graduate students (say, a mix of economists, social scientists, mathematicians, and one good parliamentarian) in a brainstorming session, a flood of additional ideas would come forth.

I believe that this research could result in the opening of several entirely new fields of investigation. The implications are broad and as yet ill defined. What I do feel comfortable in asserting, however, is that much work remains to be done.

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APPENDIX

PROGRAM CODE

Note: Be advised that some lines of code are longer that 80 spaces, and so there will be some wrap-around of lines when constrained to 80 spaces per line in the word processor program.

This is the final version of the code.

```
REM          PROGRAM DPAM01.BAS
REM  New DPA program:  Economic Theory of the Organization
REM  last modified 2/8/02
REM  this program generates 2 firms

DEFINT F-Z

DIM WI(3), WE(3), SI(3), SE!(3), ML(3), MS(3)
DIM WID(3), WED(3), SID(3), MLD(3), MSD(3)
DIM FIRED(3), TDROP(3), CAPITAL!(3), PLOWBK!(3)
DIM GROWSTAT(3), NOER!(3), TAX!(3), PDROP(3)
DIM WIDGETS!(3), WIDGETS(3), FRAQWID!(3), HIREFIRE(3), COST!(3)
DIM PROFITL!(3), FAILED(3), LCARRYO!(3), DSIZE!(3), LOSS!(3)
DIM PRICE1!(3, 3), SLOPE!(3, 3), PPOINT(3, 2), PEAKCAP!(3), MPROD!(10)
DIM PWI(3), PWID(3), PSI(3), PSID(3), PML(3), PMLD(3), NEEDPX!(3), PSHORTF!(3)
DIM GFUND!(3), PLAST!(3), PRICENW!(3), OPROD!(10), POPMAX(3), POP%(3)


REM  BEGIN program --  initialize variables



REM number of cycles to execute
MAXTURNS = 1000

REM number of firms in this run
FIRMS = 2


REM Global factors applying to all firms
GGGGGGGGGGGGGGGGGGGGGGGGGGGGGGG

REM transaction cost
TXCOST! = 2

REM initial profit per widget

REM Cost of capital per cycle
CAPCOST! = .02
```

```
REM transaction hiring cost
TXHIRE! = .25

REM number of workers + management supported by 1 S
SUPPORT! = 10

REM number of ML that can be supported by one Management Staff
MSTAFF! = 10

REM number of workers supported by one Management Line
MLINE! = 10

REM Noer factors indexed as 1=growth, 2=no growth profit, 3=unprofitable
NOER!(1) = 1
NOER!(2) = 1
NOER!(3) = 1

REM initial demand price, 3 DIFFERENT companies, 3 POINTS
PRICE1!(1, 1) = 3
PRICE1!(1, 2) = 3
PRICE1!(1, 3) = 3
PRICE1!(2, 1) = 3
PRICE1!(2, 2) = 3
PRICE1!(2, 3) = 3
PRICE1!(3, 1) = 3
PRICE1!(3, 2) = 3
PRICE1!(3, 3) = 3
REM initial price offered - beginning of the curve
PRICENW!(1) = 3
PRICENW!(2) = 3
PRICENW!(3) = 3
REM slope of price curve; put in - sign if negative
SLOPE!(1, 1) = -.005
SLOPE!(1, 2) = -.005
SLOPE!(1, 3) = -.005
SLOPE!(2, 1) = -.005
SLOPE!(2, 2) = -.005
SLOPE!(2, 3) = -.005
SLOPE!(3, 1) = -.005
SLOPE!(3, 2) = -.005
SLOPE!(3, 3) = -.005
REM inflection points of price curve; looks at (2) first
PPOINT(1, 1) = 0
PPOINT(1, 2) = 0
PPOINT(2, 1) = 0
PPOINT(2, 2) = 0
PPOINT(3, 1) = 0
PPOINT(3, 2) = 0
REM set first turn price at beginning of curve
PRICENW!(1) = 3
```

```
PRICENW!(2) = 3
PRICENW!(3) = 3


REM Correction factor for staff management; one turn queue
MSCORR!(1) = 1
MSCORR!(2) = 1
MSCORR!(3) = 1


REM number of consecutive turns of dropping profits to trigger downsizing
TRIGFIRE = 3

REM value of over / under production when line management imbalance
OPROD!(1) = .9
OPROD!(2) = 1.7
OPROD!(3) = 2.4
OPROD!(4) = 3!
OPROD!(5) = 3.5
OPROD!(6) = 3.9
OPROD!(7) = 4.2
OPROD!(8) = 4.4
OPROD!(9) = 4.5
OPROD!(10) = 4.55
MPROD!(1) = .55
MPROD!(2) = 1.2
MPROD!(3) = 1.95
MPROD!(4) = 2.8
MPROD!(5) = 3.75
MPROD!(6) = 4.8
MPROD!(7) = 5.95
MPROD!(8) = 7.2
MPROD!(9) = 8.55
MPROD!(10) = 10


REM cost of capital
COSTCAP! = .05

EM current population of firm
POP%(1) = 0
POP%(2) = 0
POP%(3) = 0

REM needed profit to stay competitive against risk-free capital
NEEDPX!(1) = 0
NEEDPX!(2) = 0
NEEDPX!(3) = 0

REM end Global factors applying to all firms END
GEGEGEEGEGGEGGGEGGGEGGEGGEGG
```

```
REM BEGIN firm specific descriptions
FFFFFFFFFFFFFFFFFFFFFFFFFFFFFFFFFFFFFFFF

REM  initial available capital
CAPITAL!(1) = 50
CAPITAL!(2) = 50
CAPITAL!(3) = 50

REM  initially, all firms are in "growth" (1) stage; 2=profit no growth
REM                                   3=unprofitable
GROWSTAT(1) = 1
GROWSTAT(2) = 1
GROWSTAT(3) = 1

REM this variable indicates if firms is 1=hiring, 2=P-NG, 3=downsizing
REM simulation begins with all firms in hiring mode
HIREFIRE(1) = 1
HIREFIRE(2) = 1
HIREFIRE(3) = 1

REM Taxation factor - represents taxes, dividends, incentives, other   REM outlays as a fraction of
profits
TAX!(1) = .5
TAX!(2) = .5
TAX!(3) = .5

REM Plowback factor: fraction of profits plowed back into growth
PLOWBK!(1) = .25
PLOWBK!(2) = .25
PLOWBK!(3) = .25

REM flag indicating 1=downsize occurred this cycle; 0=no downsizing occurred
FIRED(1) = 0
FIRED(2) = 0
FIRED(3) = 0

LOSS!(1) = 0
LOSS!(2) = 0
LOSS!(3) = 0


REM consecutive cycles firm not growing
PDROP(1) = 0
PDROP(2) = 0
PDROP(3) = 0

REM amount of product made during the cycle
WIDGETS(1) = 0
WIDGETS(2) = 0
WIDGETS(3) = 0
```

```
REM profit achieved last cycle
PROFITL!(1) = 0
PROFITL!(2) = 0
PROFITL!(3) = 0

REM initialize running total of funds for internal growth
GFUND!(1) = 30!
GFUND!(2) = 30.0005
GFUND!(3) = 30


REM initial beginning with one line management
ML(1) = 1
ML(2) = 1
ML(3) = 1

REM initial beginning with no staff management
MS(1) = 0
MS(2) = 0
MS(3) = 0

REM  initial beginning with two internal workers
WI(1) = 2
WI(2) = 2
WI(3) = 2

REM  initial - no firm has failed (failed = 1)
FAILED(1) = 0
FAILED(2) = 0
FAILED(3) = 0

REM  initialize loss carryover
LCARRYO!(1) = 0
LCARRYO!(2) = 0
LCARRYO!(3) = 0

REM  initialize downsizing fraction when firing needed
DSIZE!(1) = .08
DSIZE!(2) = .08
DSIZE!(3) = .08
REM end firm specific descriptions end
FFFFFFFFFFFFFFFFFFFFFFFFFFFFFFFFFendF
```

```
REM Begin program ++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++

REM First record capitalization of each company
PEAKCAP!(1) = CAPITAL!(1)
PEAKCAP!(2) = CAPITAL!(2)
PEAKCAP!(3) = CAPITAL!(3)

REM Calculate company size where first SI is hired
FIRSTSI = SUPPORT! / TXCOST!
FIRSTML = MSTAFF! / TXCOST!

REM determine if company is 1=internal growth or 2=contractor
A = 1 + (1 / SUPPORT!) + (1 / MSTAFF!) 'cost of internal production
B = TXCOST! + (1 / MSTAFF!)          'cost of contracting
IF A < B THEN TYPECOMP = 1 ELSE TYPECOMP = 2
IF TYPECOMP = 1 GOTO 14
WI(1) = 0
WI(2) = 0
WI(3) = 0

REM print out time 0 variables

POP%(FIRM) = WI(FIRM) + SI(FIRM) + ML(FIRM) + MS(FIRM)
Q! = 0

14 TIME = 0
OPEN "A100.OUT" FOR APPEND AS #1
REM WRITE #1, "TIME", "POP", "Pmax", "MS", "MSD", "ML", "MLD", "SI", "SID", "SE!",
"WI", "WID", "WE", "PROFITL", "GFUND", "P SHORT", "NEDPX", "GROW", "H/F",
"WIDGETS!", "PRICENW", "COST", "COST/WID", "PROD!", "PRODCML!", "CFNOW!",
"EFFN", "PRODUCT!", "LCARRYO", "Fail"
REM WRITE #1, TIME, POP%(1), POPMAX(1), PMS(1), PMSD(1), PML(1), PMLD(1), PSI(1),
PSID(1), SE!(1), PWI(1), PWID(1), WE(1), PROFITL!(1), GFUND!(1), PSHORTF!(1),
NEEDPX!(1), GROWSTAT(1), HIREFIRE(1), WIDGETS!(1), PRICENW!(1), COST!(1), Q!,
PROD!, PRODCML!, CFNOW!, EFFN!, PRODUCT!, LCARRYO!(1), FAILED(1)
WRITE #1, "FIRMS=", FIRMS, "GFUND=", GFUND!(1), GFUND!(2), GFUND!(3)
CLOSE #1
```

```
REM  begin time cycles  TIME TIME TIME TIME TIME TIME TIME TIME TIME

15 FOR TIME = 1 TO MAXTURNS

PRINT TIME;

    FOR FIRM = 1 TO FIRMS

    WE(FIRM) = 0

    REM skip this firm if it has failed
    IF FAILED(FIRM) = 1 GOTO 750

    REM is the firm downsizing?  If yes, go to firing section
    IF HIREFIRE(FIRM) > 2 GOTO 200

    REM calculate number of available hires
    HIRENR = .4 * INT(GFUND!(FIRM))


REM hire WI

    REM skip if firm is a contracting organization
    IF TYPECOMP = 2 GOTO 25
    GOTO 26

REM hire management for contracting organization

25    MLD(FIRM) = HIRENR
      HIRENR = 0
      GOTO 28

REM end hire management for contracting org; goto hire staff management


REM     hire internal workers

26  IF HIREFIRE(FIRM) = 2 GOTO 27 ' if static, do not hire WI
    WID(FIRM) = HIRENR

    HIRENR = 0
```

```
REM  hire line management

27  NEEDCAP = WI(FIRM) + WID(FIRM)
    CURCAP = ML(FIRM) * MLINE!

    DELTA = NEEDCAP - CURCAP
    IF DELTA < 1 GOTO 28

    DELTAML = .1 * DELTA
    REMAINDR = DELTA - (10 * DELTAML)
    IF REMAINDR > 4 THEN DELTAML = DELTAML + 1

    MLD(FIRM) = DELTAML

REM  hire staff management

28   NEEDCAP = ML(FIRM) + MLD(FIRM)
     CURCAP = MS(FIRM) * MSTAFF!

    DELTA = NEEDCAP - CURCAP
    IF DELTA < 1 GOTO 29

    DELTAMS = .1 * DELTA
    REMAINDR = DELTA - (10 * DELTAMS)
    IF REMAINDR > 4 THEN DELTAMS = DELTAMS + 1

    MSD(FIRM) = DELTAMS
    IF TYPECOMP = 2 GOTO 300

REM  hire support staff

29  NEEDCAP = ML(FIRM) + MLD(FIRM) + MS(FIRM) + MSD(FIRM) + WI(FIRM) +
    WID(FIRM)
    CURCAP = SI(FIRM) * SUPPORT!

    DELTA = NEEDCAP - CURCAP
    IF DELTA < 1 GOTO 300

    DELTASI = .1 * DELTA
    REMAINDR = DELTA - (10 * DELTASI)
    SID(FIRM) = DELTASI
    IF REMAINDR > 4 THEN SID(FIRM) = SID(FIRM) + 1
```

```
REM determine amount of external support required

300  REM  number that the current SI can support
     SUPNR = SI(FIRM) * SUPPORT!

     REM  amount of workers needing additional support
     SUPNR! = (WI(FIRM) + ML(FIRM) + MS(FIRM)) - SUPNR

     IF SUPNR! < 1 THEN SE!(FIRM) = 0: GOTO 200
     SE!(FIRM) = SUPNR! / SUPPORT!



REM  begin firing section

     REM  skip if not firing
200  IF HIREFIRE(FIRM) < 3 GOTO 600



REM first do contractor
     IF TYPECOMP = 1 GOTO 330
          IF HIREFIRE(FIRM) = 2 GOTO 310' if static growth, check MS only
          IF ML(FIRM) < 5 THEN MLD(FIRM) = -1: GOTO 310

          REM first reduce firings to scale for managers only
          MLD(FIRM) = .1 * LOSS!(FIRM)

          REM check for firing Management Staff
310        NEEDMS = INT(ML(FIRM) / MSTAFF!)
          IF NEEDMS < MS(FIRM) THEN MSD(FIRM) = NEEDMS - MS(FIRM)

REM END of contractor firing
GOTO 600



330 ' begin internal organization firing


     REM determine downsize number
     FIRE = PSHORTF!(FIRM)
     X% = ABS(FIRE)
     IF X% > 0 THEN FIRED(FIRM) = 1  ' set "fired" flag
```

```
    REM don't fire too much if organization is small
    IF WI(FIRM) < 5 THEN WID(FIRM) = -1: GOTO 340
    IF WI(FIRM) < -FIRE THEN WID(FIRM) = -.5 * WID(FIRM): GOTO 340

    REM if organization large, fire IAW losses
    WID(FIRM) = FIRE


340 ' fire ML if not supported by WI population

    REM don't fire the last manager
    IF ML(FIRM) < 2 GOTO 360


    REM calculate how many are needed
    NEEDED = (INT(WI(FIRM) + WID(FIRM)) / MLINE!)
    IF NEEDED < 1 THEN NEEDED = 1
    IF NEEDED < ML(FIRM) THEN MLD(FIRM) = NEEDED - ML(FIRM)
    IF MLD(FIRM) < 0 THEN FIRED(FIRM) = 1


360 ' fire MS if not supported by ML population

    REM calculate how many are needed
    NEEDED = INT(ML(FIRM) / MSTAFF!)
    IF NEEDED < MS(FIRM) THEN MSD(FIRM) = NEEDED - MS(FIRM)
    IF MSD(FIRM) < 0 THEN FIRED(FIRM) = 1


REM  fire internal support staff
    X% = WI(FIRM) + WID(FIRM) + ML(FIRM) + MS(FIRM) + MLD(FIRM) + MSD(FIRM)
    X% = X% / SUPPORT!
    IF SI(FIRM) > X% THEN SID(FIRM) = X% - SI(FIRM)
    IF SID(FIRM) < 0 THEN FIRED(FIRM) = 1


600  '  begin production section

    REM  hire outside production if management capacity exists
    REM  and if profits outside production would be profitable

    REM  skip external production ifit would not be profitable
    IF PRICENW!(FIRM) < TXCOST! GOTO 700

    REM  skip if losing money
    IF GROWSTAT(FIRM) = 3 GOTO 700
```

```
     REM  calculate capacity for outside production
     OUTSOURC = (MLINE! * ML(FIRM)) - WI(FIRM)
     IF OUTSOURC < 1 THEN OUTSOURC = 0
     WE(FIRM) = OUTSOURC


700  ' Calculate PRODUCTION

REM note that the following code is only one version of the penalty functions that were tried;    other
versions also provided chaotic results

      PROD! = WE(FIRM) + WI(FIRM)

      REM adjust factor for LINE management efficiency

      PRO! = WE(FIRM) + WI(FIRM) ' unaffected production
      MAN! = ML(FIRM) * MLINE!   ' capacity of line management


REM management and production in balance
     IF PRO! = MAN! THEN EPROD! = 0: UAPROD! = PROD!: GOTO 701

     IF PRO! < MAN! GOTO 702


REM Production greater than management capacity
     MORE = PRO! - MAN!
     UAPROD! = MAN!
     EXMORE = 0
     IF MORE > 10 THEN EXMORE = MORE - 10: MORE = 10
     IF EXMORE < 0 THEN EXMORE = 0
     EPROD! = OPROD!(MORE) + (.2 * EXMORE)

     GOTO 701


REM too much management, asses penalty function
702    UAPROD! = PRO!
       DELTAP = 10 + PRO! - MAN!
       UAPROD! = PRO! - DELTAP
       IF DELTAP > 10 THEN DELTAP = 10
       IF DELTAP < 1 THEN DELTAP = 1
       EPROD! = MPROD!(DELTAP)
```

```
REM ********************************************
REM  The following is an example of an alternate
penalty function for management imbalance that
was used in substitute to the code above.
It places a greater penalty on having too many
Line managers.


REM Production greater than management capacity
    MORE = PRO! - MAN!
    UAPROD! = MAN!
    EXMORE = 0
    IF MORE > 10 THEN EXMORE = MORE - 10: MORE = 10
    IF EXMORE < 0 THEN EXMORE = 0
    EPROD! = OPROD!(MORE) + (EXMORE * .2)
    GOTO 701


REM too much management, asses penalty function
702  UAPROD! = PRO!
     DELTAP = 10 + PRO! - MAN!
     UAPROD! = PRO! - DELATP
     IF DELTAP > 10 THEN DELTAP = 10
     IF DELTAP < 1 THEN DELTAP = 1
     EPROD! = MPROD!(DELTAP)

REM end of substitute penalty function code
REM ********************************************

701    PRODCML! = UAPROD! + EPROD!
       IF PRODCML! < 0 THEN PRODCML! = 0



REM adjust factor for STAFF management efficiency

    NEED! = ML(FIRM)
    AVAIL! = MS(FIRM) * MSTAFF!
    DELTA! = ABS(NEED! - AVAIL!)

    IF NEED! = AVAIL! THEN CF! = 1: GOTO 707 ' in perfect balance

    CFX! = .05 * DELTA!
    IF MS(FIRM) = 0 GOTO 704

    CF! = 1 - (CFX! / MS(FIRM))
    GOTO 707
```

```
704    ' no MS
        CF! = 1 - CFX!


707 '  place in carryover file
        CFNOW! = CF!


REM  end section on staff management efficiency

      PRODUCT! = PRODCML! * CFNOW!

REM adjust production for NOER factor

715    EFFN! = NOER!(HIREFIRE(FIRM))

        PRODUCT! = PRODUCT! * EFFN!


REM calculate final production and NEW carryover

      REM  add in production carried over from last cycle
      WIDGETS!(FIRM) = PRODUCT! + FRAQWID!(FIRM)

      REM re-zero carryover
      FRAQWID!(FIRM) = 0

      REM  calculate new fractional carryover
      WIDGETS(FIRM) = INT(WIDGETS!(FIRM))
      FRAQWID!(FIRM) = WIDGETS!(FIRM) - WIDGETS(FIRM)


REM calculate total cost of production and all other actions

      REM cost of internal organization
      COST!(FIRM) = WI(FIRM) + SI(FIRM) + ML(FIRM) + MS(FIRM)

      REM cost of contracting
      COST!(FIRM) = COST!(FIRM) + ((WE(FIRM) + SE!(FIRM)) * TXCOST!)

      REM cost of hiring
      COST!(FIRM) = COST!(FIRM) + ((WID(FIRM) + SID(FIRM) + MLD(FIRM) +
      MSD(FIRM))
      * TXHIRE!)

REM if expenses are greater than capital, deduct loan costs at CAPCOST! rate

      IF COST!(FIRM) > CAPITAL!(FIRM) THEN CAPITAL!(FIRM) = CAPITAL!(FIRM) -
      ((COST!(FIRM) - CAPITAL!(FIRM)) * CAPCOST!)

750 NEXT FIRM
```

```
REM at this point all firms have produced
REM now calculate total production and price

UNITS = WIDGETS(1) + WIDGETS(2) + WIDGETS(3)


FOR FIRM = 1 TO FIRMS


REM skip this firm if it has failed
     IF FAILED(FIRM) = 1 GOTO 900


REM calculate income


     REM Calculate region on the price curve and associated price
     IF UNITS < PPOINT(FIRM, 2) GOTO 765

     X% = UNITS - PPOINT(FIRM, 2)
     PRICENW!(FIRM) = PRICE1!(FIRM, 3) + X% * SLOPE!(FIRM, 3)
     GOTO 770

765   IF UNITS < PPOINT(FIRM, 1) GOTO 767
      X% = UNITS - PPOINT(FIRM, 1)
      PRICENW!(FIRM) = PRICE1!(FIRM, 2) + X% * SLOPE!(FIRM, 2)
      GOTO 770

767   X% = UNITS
      PRICENW!(FIRM) = PRICE1!(FIRM, 1) + X% * SLOPE!(FIRM, 1)



770   INCOME! = WIDGETS(FIRM) * PRICENW!(FIRM)

REM calculate profit
     PROFIT! = INCOME! - COST!(FIRM)

REM if a loss, record loss for later use in determining how many to fire
     IF PROFIT! < 0 THEN LOSS!(FIRM) = PROFIT!
     IF PROFIT! < 0 AND LOSS!(FIRM) > -1 THEN LOSS!(FIRM) = -1


REM do not apply taxes if loss; put in loss carry function

     TAXNOW! = TAX!(FIRM)
```

```
REM divide between case where profits made & no profits
     IF PROFIT! > 0 GOTO 804

     REM case where negative profit
     TAXNOW! = 0
     LCARRYO!(FIRM) = LCARRYO!(FIRM) + PROFIT!
     NET! = 0
     GOTO 810

     REM positive profit
     REM divide between positive taxable and not taxable

804  IF (PROFIT! + LCARRYO!(FIRM)) < 0 GOTO 805

     REM taxable
     NET! = PROFIT! + LCARRYO!(FIRM)
     LCARRYO!(FIRM) = 0
     GOTO 810

     REM not taxable

805  NET! = 0
     LCARRYO!(FIRM) = LCARRYO!(FIRM) + PROFIT!


810  CAPITAL!(FIRM) = CAPITAL!(FIRM) + PROFIT! - (NET! * TAXNOW!)
IF CAPITAL!(FIRM) > PEAKCAP!(FIRM) THEN PEAKCAP!(FIRM) = CAPITAL!(FIRM)



REM establish growth status of company

820  IF PROFIT! >= PROFITL!(FIRM) THEN GROWSTAT(FIRM) = 1 ' profitable & growing
     IF PROFIT! > 0 AND PROFIT! < PROFITL!(FIRM) THEN GROWSTAT(FIRM) = 2 '
     profitable, not growing

     REM establish return on capital profit criteria
     NEEDPX!(FIRM) = COSTCAP! * (ML(FIRM) + MS(FIRM) + SI(FIRM) + WI(FIRM))

REM insufficient earnings to meet return on capital criteria
     IF PROFIT! < NEEDPX!(FIRM) THEN GROWSTAT(FIRM) = 4

     IF TIME < 4 THEN GROWSTAT(FIRM) = 1

     IF GROWSTAT(FIRM) = 2 THEN PDROP = PDROP + 1 ELSE PDROP = 0

REM establish hiring status of company

830    HIREFIRE(FIRM) = GROWSTAT(FIRM)
```

```
REM fire if extended dropping profits
     IF GROWSTAT(FIRM) = 1 GOTO 890
     IF GROWSTAT(FIRM) = 2 AND PDROP <= TRIGFIRE GOTO 890

REM static - no growth, dropping profits but profitable, PROFIT < NEEDPX!
     HIREFIRE(FIRM) = 3



REM save the initial setting in the "P" variables to print at end

890  POP%(FIRM) = MS(FIRM) + ML(FIRM) + SI(FIRM) + WI(FIRM)
     IF POPMAX(FIRM) < POP%(FIRM) THEN POPMAX(FIRM) = POP%(FIRM)


PWI(FIRM) = WI(FIRM)
PWID(FIRM) = WID(FIRM)
WI(FIRM) = WI(FIRM) + WID(FIRM)
IF WID(FIRM) > 0 THEN GFUND!(FIRM) = GFUND!(FIRM) - WID(FIRM)


PSI(FIRM) = SI(FIRM)
PSID(FIRM) = SID(FIRM)
SI(FIRM) = SI(FIRM) + SID(FIRM)
IF SID(FIRM) > 0 THEN GFUND!(FIRM) = GFUND!(FIRM) - SID(FIRM)

PML(FIRM) = ML(FIRM)
PMLD(FIRM) = MLD(FIRM)
ML(FIRM) = ML(FIRM) + MLD(FIRM)
IF MLD(FIRM) > 0 THEN GFUND!(FRM) = GFUND!(FIRM) - MLD(FIRM)

PMS(FIRM) = MS(FIRM)
PMSD(FIRM) = MSD(FIRM)
MS(FIRM) = MS(FIRM) + MSD(FIRM)
IF MSD(FIRM) > 0 THEN GFUND!(FIRM) = GFUND!(FIRM) - MSD(FIRM)
```

```
IF GFUND!(FIRM) < 0 THEN GFUND!(FIRM) = 0

WID(FIRM) = 0
WED(FIRM) = 0
SID(FIRM) = 0
MLD(FIRM) = 0
MSD(FIRM) = 0


    PROFITL!(FIRM) = PROFIT!

    REM calculate profit shortfall from target

    PSHORTF!(FIRM) = PROFIT! - NEEDPX!(FIRM)
    IF PSHORTF!(FIRM) < 0 AND PSHORTF!(FIRM) > -1 THEN PSHORTF!(FIRM) = -1


    IF HIREFIRE(FIRM) = 3 THEN GFUND!(FIRM) = 0: GOTO 850
    GFUND!(FIRM) = GFUND!(FIRM) + (PROFIT! * PLOWBK!(FIRM))
    IF GFUND!(FIRM) < 0 THEN GFUND!(FIRM) = 0



REM check if any firm has failed

850 IF T < 4 GOTO 900 ' first four turns "free"

REM failed if organization size drops by 80%
IF POP%(FIRM) < (.2 * POPMAX(FIRM)) THEN FAILED(FIRM) = 1

IF FAILED(FIRM) = 0 GOTO 900

REM firm has failed, zero out population

WI(FIRM) = 0
ML(FIRM) = 0
MS(FIRM) = 0
SI(FIRM) = 0
POP%(FIRM) = 0


900 NEXT FIRM


REM print out the results

X! = COST!(1)
Y! = WIDGETS!(1)
IF Y! = 0 THEN Y! = 1

Z! = X! / Y!
```

```
OPEN "A100.OUT" FOR APPEND AS #1

REM full printout of one firm
REM IF TIME = 1 OR TIME = 30 OR TIME = 60 OR TIME = 90 THEN WRITE #1, "TIME",
"POP", "Pmax", "MS", "MSD", "ML", "MLD", "SI", "SID", "SE", "WI", "WID", "WE",
"PROFITL", "GFUND", "P SHORT", "NEEDPX", "GROW", "H/F", "WIDGETS!", "PRICENW",
"COST", "COST/WGT", "PROD!", "PRODCML!", "CFNOW!", "EFFN", "PRODUCT",
"LCARRYO", "Fail"
REM WRITE #1, TIME, POP%(1), POPMAX(1), PMS(1), PMSD(1), PML(1), PMLD(1), PSI(1),
PSID(1), SE!(1), PWI(1), PWID(1), WE(1), PROFITL!(1), GFUND!(1), PSHORTF!(1),
NEEDPX!(1), GROWSTAT(1), HIREFIRE(1), WIDGETS!(1), PRICENW!(1), COST!(1), Z!,
PROD!, PRODCML!, CFNOW!, EFFN!, PRODUCT!, LCARRYO!(1), FAILED(1)


REM print out populations of 2 firms
WRITE #1, POP%(1), POP%(2)


CLOSE #1



NEXT TIME
```