Revision: May 2, 2006

RSVP to Sociology:
Semantics, Syntactics, Empirics,
and the Theory/Method Interface

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May 21, 2006

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Preface

A cybermodel (or hypermodel) is a large-scale system theory that often contains both microsocial and macrosocial elements, emphasizes causal interconnections among variables, and uses many of the traditional thought patterns of functionalism including routine searches for circular causation. One observes among theories of this genre a continuity that belies the persistent claim that the social sciences do not accumulate knowledge. Theories of any form, and associated methods, tend to evolve in similar ways, and they generally evolve toward increased complexity, not toward parsimony. The most highly developed multidimensional theories, because of their complexity, often outrun available methods for testing hypotheses; the situation exemplifies Ogburn's cultural lag principle. Therefore I discuss strategies for minimizing this lag, including taxonomic checklists, expert systems, chaos theory, social simulations, and the method of comparative narratives. Given the complexity of highly developed cybermodels, such models should consciously, consistently, and carefully avoid the generation of non-testable hypotheses and unanswerable questions. The most elaborate and abstract ecological cybermodels almost invariably force social scientists toward the Comtean emphasis on synthesis of and generalization across disciplines, implying that recruitment and training of social scientists must be highly selective and demanding.
Preliminaries and Predilections

(1) The perennial centrality of the systemic mode or, The Elders paradox [note 1]: Functionalism may make you blind, but it cannot give you AIDS or an unwanted pregnancy

This paper is dedicated to Joycelyn Elders, former Surgeon General of the United States. Apparently Elders was dismissed from her job in the Clinton administration largely because she argued that (1) masturbation is a widespread albeit widely condemned practice, and that (2) we should acknowledge this anomaly and try to entice the few who have not yet discovered masturbation into taking advantage of it. A major advantage, she suggested, is that the practice could help to prevent sexually-transmitted disease, such as AIDS.

With a similarly logical and compelling perspective, the present paper argues that functionalism—also known as the systemic mode—is a widespread albeit widely condemned practice, and that we should acknowledge this anomaly and try to entice the few who have not yet discovered functionalism into taking advantage of it; it makes for good mental health, and it may even have physical benignancies. The analogy does fail at certain points: Masturbation, for instance, cannot be done inadvertently or subconsciously, except under the rarest of circumstances. We find in the present instance, however, that among scholars who work at the very center of several contemporary theoretical movements, there are those who openly condemn the allegedly illicit practices of functionalism while nevertheless indulging themselves in them wholeheartedly and apparently unawares. Verily, we cannot condemn these scholars as they themselves wantonly have condemned their brethren, for they know not what they do (Harris, 1980).

Another task of this paper, then, is to make inadvertent, subconscious, functionalist system theory explicit, to show that this form of social-science theory does indeed have continuity right up to the present moment. Joycelyn Elders made the same assumption, *mutatis mutandis.*
(1.1)

Working theoretical strategies, to Berger and Zelditch (1993:14),

... are important factors in growth ..., [but] the working strategy does not grow if it does not produce theories. Merton's [functionalist] paradigm has been evidently difficult to realize. For example, it has proved difficult to develop a theory within which one could determine the positive, negative, and net functions of a structure. But theoretical growth depends on using strategies to formulate theories. Thus, despite Alexander's “neofunctionalism,” Merton's paradigm of functionalism does not seem to have grown at all unless one takes its abandonment as a kind of growth.

On the other hand, perhaps what we really have witnessed is the abandonment of the terminology of functionalism, as opposed to the substance of it, because the former has simply fallen out of fashion. [note 2] If this is true, then we must become alert to the possibility that we may find the same fine savory wine under a new label, and perhaps many new labels.

In any case, I propose the following not-unfriendly syllogism. It is a perfectly logical syllogism, despite the fact that its conclusion is false. Its conclusion is false because at least one of its premises is false.

Major premise, from Berger and Zelditch: If a theory is functionalistic, then it has not grown.

Minor premise: In the Berger-Zelditch volume, the first example of “the growth of a program,” involving status characteristics theory and provided by David Wagner in collaboration with Berger (1993) himself, is undeniably functionalistic.

Therefore, Wagner and Berger describe a program that has not grown, and the major thesis of the Berger-Zelditch book is contradicted.

In my estimation the conclusion is false because the first premise is false—functionalistic theories, as I propose to show, are flourishing. Assuming that nobody wishes to dispute the
claim that status characteristics theory is also growing apace, my sole responsibility in isolating the first premise as the lone culprit is to demonstrate that the minor premise is correct. This is easy: We merely let Wagner and Berger speak for themselves.

Wagner and Berger (1993:31) describe Lenski's status crystallization theory as a “core idea” of status characteristics theory. They say that “... inconsistencies are assumed to create tension and anxiety” and that “actors can reduce the tension by eliminating (e.g., ignoring or hiding) inconsistent statuses. If they cannot reduce the inconsistency, then they may become isolated and be prone to various types of coping behaviors (e.g., political radicalism.)” Presumably in cases involving political radicalism, status inconsistencies themselves may be changed directly, through feedback; therefore, this is a theory that at least potentially explains a structure, status inconsistency, with reference to its consequences, and it is therefore functionalistic. “Ignoring or hiding” inconsistencies, on the other hand, involves a range of indirect adaptations, as often occurs in the Freudian functionalism of psychological adjustment. In subsequent paragraphs, Wagner and Berger speculate about additional ways in which people may adapt to perceived status discrepancies, in their own situations or among those with whom they interact. These various implications of the Lenski-Wagner-Berger theory may not be true, but the logic of the theory is precisely Mertonian; and, since the theory addresses the consequences of tension and anxiety, it is also precisely Malinowskian. It does not matter that Malinowski and various Freudians did not agree about, say, the parricidal crisis, and that one side or the other had to be essentially wrong; Malinowski and his Freudian opponents were all nonetheless card-carrying functionalists.

Nor does it matter that tension and anxiety, in Lenski's earliest formulation of status-discrepancy theory, may not have a clear relationship to the survival of social entities. If we focus on contemporary work by Lenski and collaborators (Lenski et al., 1991:105), we find a strong emphasis on survival: for instance, the claim that “... if there ever were [hunting-gathering] societies that used women extensively in hunting, they probably did not survive because of low birth rates.” The entire theoretical edifice of the Lenski, Nolan, and Lenski textbook (1995) is built on ecological and evolutionary hypotheses that often overlap Darwinian functionalism. And another theoretical tradition, based on event-history studies and strongly represented within the Stanford sociology department, has turned up many instances in which contemporary organizations have their lives considerably shortened if they contain maladaptive structural features. If these features reduce survivorship, and if social selection acts against these features—and by definition it does [note 3]—then once again we find ourselves within the Mertonian camp: We explain structures with reference to their consequences.
Having reminded ourselves about the essential elements of Merton's functionalist paradigm, we need no further interpretation of the following remarks, also from Wagner and Berger. In each instance it is relatively easy to see the structural feature, the consequences flowing from the structure, and how the consequences themselves may feed back to modify the structure (Wagner and Berger, 1993:37; final italics mine):

The status value view of justice was developed originally to challenge an earlier view called *equity theory* ... Equity theory focuses on the exchange, or consummatory, value of rewards; it assumes that evaluations of justice and injustice are based on comparisons of one actor's ratio of the values of investments to rewards with that of the second actor ... If the ratios are equal, the situation is regarded as equitable and stable; *if the ratios are unequal, the situation is inequitable and subject to pressures to reduce the inequity.*

The “status value view” introduces new dimensions into this argument [cf. Geschwender], but in so doing it does not depart from the Mertonian paradigm: Social structures, or other aspects of human culture, again explained with reference to their consequences.

Wagner and Berger (1993:41) say that status characteristics theory has engendered a major program of applications and intervention research ... by E. G. Cohen and her associates ... The initial status characteristics theory was used to describe the interracial interaction disability experienced by black students ... Subsequently, an intervention was developed to help reduce the inequality in power and prestige these students faced.

Regarding this example I am prepared to argue that, if we assume that evaluation research leads to a redesign of intervention strategies and to additional experiments, then by definition intervention and evaluation programs comply with and exemplify the Merton paradigm.

Notice, finally, that similar formulations are found throughout the remainder of the Wagner-Berger article, and indeed throughout the remainder of the Berger-Zelditch book. In conclusion, while I carry no brief for the use of traditional functionalist terminology, I strongly suggest that we recognize that the functionalist logic, in its essential Mertonian manifestation, has been the keystone of a large part of our best work, contemporary and/or historical.
In Coleman's (1990:259-60) seminal opus on rational choice theory we find, with regard to functionalism, the same paradoxical attitude:

In the preceding examination of the use of norms by sets of actors, I have avoided using the term “function,” although it would be natural to have written, for example, that “sets of persons develop norms not only to serve a protective function against actions that impose negative externalities, but also to perform positive functions for them.” I have avoided using the term because of the confusion surrounding its use in social theory. In particular, radical versions of functional analysis have purported to explain the existence of a phenomenon by its function. In this context that would mean explaining the emergence of a norm by the functions it serves for the set of actors who hold it.

Coleman does not provide citations to “radical” functionalists, nor to any comparable practitioners of self-abuse. However, radical or not and self-acknowledged or not, functionalists generally subscribe to the principle of multicausal explanation, and cannot typically be accused of trying to explain structures solely with reference to the conscious interests of actors (Harris, 1979).

Coleman continues:

It should be clear, however, that the functions a norm serves for those who hold it ... are not sufficient as an explanation of its emergence or continued existence. ... In the explanation of the emergence of norms given in this book, that is only the first of two necessary conditions. The condition under which those interests will be realized, to be examined in the next chapter, is the second half of the explanation.

When we read the next chapter, however, we see that there is no substantial departure from the functionalist paradigm: The functionalist terminology could readily have been adopted, for instance, in Coleman's discussion of gossip—i.e., the functions of gossip in creating and/or maintaining norms. Continuing from Coleman:
The common tendency of ... “functional analyses” to explain a phenomenon solely by its function is the principal failing of functional analysis ... For a theorist to go beyond this, to examine how a phenomenon has come into existence, requires going from the macrosocial level down to the level of actors, thus abandoning the paradigm of functional analysis for a paradigm that, like the one used in this book, contains actors and a theory of action.

But the social-selection processes of Lenski, Nolan, and Lenski (1995), not to mention the typical event-history analysis focussing on organizational survival, explain the “realization” (i.e., the emergence) of social norms without abandoning the functionalist paradigm and without “going ... to the level of actors.”[note 4]

Again, we must conclude that Coleman's approach to theory has substantial continuity with traditional functionalism, i.e., system theory.

(2) “Accidentally on purpose”
or, When are consequences unanticipated?

One of the central arguments of this book is that it would be advantageous to organize all social-science knowledge under a small set of headings. The headings will be derived by cross-tabulating two acronyms, POET and ADA. POET stands for population, social organization, environment, and technology. ADA stands for accumulation of inventions, discoveries, and natural innovations; diffusion of these phenomena; and human adaptation to them. POET sums up a traditional, largely materialist perspective among human ecologists, and ADA performs a similar service for social scientists who study technology in the style of William F. Ogburn. In order to subsume all social-science knowledge under these rubrics, we must show (among other things) that traditional human ecology and Ogburnian analysis do not have any special hostility toward social psychology, or toward subjectivism, or toward a highly qualified materialism.

KAP (knowledge, attitude, practice) studies, famous among demographers, introduce cognition and affectivity into the population component of POET by ADA. Therefore, nothing about the cross-classification of these acronyms is hostile toward any brand of social psychology—nor, for that matter, toward efforts of sociobiologists, let's say, to find genetic factors that influence behavior. With Bruce Mayhew, however, we must recognize that it is often untenable to explain behavior by suggesting that people do things because they wish to do them. Wishes are subtle. The child's accusatory shout—“accidentally on purpose”—implying that what appears to be an accident actually has a devious purpose, tells
us that motivation may be hidden deeply and that Mayhew therefore may occasionally be wrong. And a handy reversal of the childish insight creates the realization that explicitly purposeful behavior often leads inadvertently to horrendous accidents and that much of life, in the words of Howard Becker's (1994) Brazilian informants, “fôi por acaso”—it occurs by chance. In any case, there is nothing about contemporary interpretationist sociology that cannot be subsumed under the rubrics of POET by ADA, and I would argue that virtually all socio-psychological theories evolve through the several stages defined above.

POET by ADA, then, encourages us to make pictures of very large networks. Cybermodels span any combination of cells \( a \) through \( el \) of my cross-classification; assume an infinitude of logical possibilities. Over the years, social scientists move around like the peripatetic scholars of old, more or less unpredictably, across this indefinitely large number of possibilities. We range from micro-level to macro-level analyses, from the relative absolutes of cultural materialism to the absolute relativism of radical postmodernism, from verbal formulations to mathematical logic and back again. Novick (1988:250), discussing historians, has some interesting ideas about the cyclical nature of these movements:

... during the interwar years, [there was] a widespread questioning of the founding program of the American historical profession: the scientific and detached search for impartial, objective historical truth. ... [The] interwar criticism of the objectivist posture was a moment in a philosophical debate that went back to Aristotle and Protagoras.

And the debate arguably has been cycling back and forth (and up and down, and left and right) since the days of Aristotle and Protagoras. I suspect that it will continue to do so. Incidentally, I have not overlooked the fact that POET by ADA does not say anything directly about “scientific and detached” searches, nor about the “objectivist posture.” I hypothesize, however, that any cybermodel evolving from stage 1 toward stage 4 will tend to have these laudable properties; take a look at the American historical profession since World War II.

Finally, a brief discussion of my own biases: Ever since Mayhew (REF) and Harris (REF) argued that sociologists have a penchant for mind over matter, I’ve been searching actively for examples of matter over mind: Following is a list of my favorite examples. Everything on this list is simply sound and fury signifying nothing if one wishes to know the unknowable, to resolve the materialist-idealista conundrum in its larger dimensions. Stated abstractly, this conundrum does not generate falsifiable hypotheses. We find falsifiable hypotheses only in specific instances—the hypothesis, say, that early hominids of Lucy’s ilk
had developed elaborate biomechanics involving bi-pedal locomotion *before* there was much cranial development, vs. the opposite assertion. In this instance, it is possible at least to imagine that the evidence would favor one of these hypotheses over the other. The same applies to my other examples, many of which may place too much weight on material factors.

[Cont’d]

[Material factors and their impact]

*Prehistorical*

Lucy: body and brain
Bruce Smith: inadvertent evolution, plants and animals
Harris and Ross
Desmond Morris
man the hunter and woman the gatherer, in LLN
global warming and its causes

*Historical*

market dynamics of spinning and weaving
Crumley on climate
steam power and electrical power: W.S. Thompson
accidental inventions: Faraday and the dynamo
latent functions of witchcraft
Farb on the horse among Plains Indians

*Contemporary*

Japanese youth: parallel processors
s-functions, i-functions, and vestigial structures
Phillips Cutright, Rose Frisch
*Time* and the “girth of a nation”
Busch scheduling
Perrow: unanticipated interactions
snowmobiles among the Innuit
impact of AIDS on sexual behavior (Olivier, 1996)

*Miscellaneous*

many examples in Volti]
(3) Shunning the frumously unanswerable

(3.1) My name is legion

Physicists cannot tarry for long in quotidian worlds before they hear questions that they cannot answer. For instance: If “big bang” theory is now reconstructing events that occurred during the first few seconds of the existence of the universe, then what transpired during the immediately preceding seconds? If we know that the universe is expanding rapidly toward its peripheries, then what do find beyond the peripheries? If a channeling-scanning microscope enables us to “see” atoms manipulated on a sort of tabletop, what are the constituents of the constituents of the constituents of these tiny entities? While the typical physicist may wander the quotidian spheres from time to time, she does not devote much energy to grappling with these issues, primarily because issues of a more or less answerable genre have a ubiquitous presence. And judging by James Watson's attitude, as expressed in The Double Helix (1980), biologists have the same stubborn insistence: Who has time for a bull session on the meaning of life, when there is a new batch of x-ray diffraction experiments to be interpreted?

A major problem with social scientists is that we do not routinely eschew the unanswerable. We do not insist upon a demonstration of the falsifiability of everything that currently passes for knowledge in our discipline. We do not have the proper skepticism. In a recent book by Collins (1992) we encounter a classic series of non-falsifiable assertions, said to be of Durkheimian origin. What Collins tries to show is that social contracts must be based on a “precontractual solidarity” throughout society (Collins, 1992:23), a solidarity that has something to do with religion (1992:35):

... there is one reality that does have all the characteristics that people attribute to the divine. It is not nature, nor is it metaphysical. It is society itself. For society is a force far greater than any individual. It brought us to life, and it can kill us. It has tremendous power over us. Everyone depends upon it in innumerable ways. We use tools and skills we did not invent; we speak a language passed on to us from others. Virtually our whole material and symbolic world is given to us from society. The institutions we inhabit—our form of family life, economy, politics, whatever they may be—came from the accumulated practices of others, in short, from society. This is the fundamental truth that religion expresses. God is a symbol of society.
If there are circumstances under which these assertions would be falsified, I should like to become acquainted with them. My fear is that the child's game of “mad-libs” readily creates hypotheses of equal plausibility and equal non-falsifiability. For instance:

There is one reality that does have all the characteristics that people attribute to the divine. It is not psychological, nor is it metaphysical. It is the sun itself. For the sun is a force far greater than any individual. It brought us to life, and it can kill us. It has tremendous power over us. Everyone depends upon it in innumerable ways. ... Virtually our whole material, mental, and spiritual existence is sustained by the energy of the sun. The institutions we inhabit—our form of family life, economy, politics, whatever they may be—came from the accumulated energy of the universe, in short, from the sun and all that it has created. This is the fundamental truth that religion expresses. God is a symbol of the sun.

So: The gods, at times, are the polytheistic creation of anthropomorphic thought. But, with equal plausibility, the modern God is a creation of heliomorphism: She is monotheistic, warm, nurturant; she is regal, monarchical, and often wears a corona; she creates continuous cycles of renewal—and yet, it is thought that she may someday change catastrophically into a state of nothingness.

Finally, we have a non-falsifiable assertion from the gospel according to John: In the beginning, just prior to the “big bang”—here I paraphrase—was the Word; and the Word was with God; and the Word was God. God, then, is a symbol of language.

(3.2) Back to orientations: Controlling the chaos of academic journals

What is truly unanswerable is the conundrum of “orientations” and their relative importance. Yet, we devote prodigious energy to the quest.

Robert Merton realized long ago that when social scientists appear to be arguing about theories, they are often arguing about orientations. Merton's (1957:87-88) definition of an orientation remains satisfactory:

Much of what is described in textbooks as sociological theory consists of general orientations ... Such orientations involve broad postulates which indicate types of variables which are somehow to be taken into account rather than specifying determinate relationships between particular variables. ... Such
general orientations may be paraphrased as saying in effect that the investigator ignores this *order of fact* at his peril. They do not set forth specific hypotheses.

... [Such] general theoretic outlooks ... constitute only the point of departure for the theorist. It is his task to develop specific, interrelated hypotheses by reformulating empirical generalizations in the light of these generic orientations.

Years ago, as an undergraduate who had just begun studying sociology seriously, I labored under the misapprehension that it makes sense to read an entire issue of the *American Journal of Sociology* from cover to cover. Now, in my inexorably emergent role as a Nestor of the discipline, I know that this was a truly sophomoric attitude. When was the last time you read a given issue of a general social science journal from front to back, non-stop? We find it difficult to do so because we encounter too many paradigm shifts along the way, we get caught up in what my graduate-school mentors used to call “sociology's crazy quilt,” the entire process would become disorienting, and we would probably remember very little from the experience. The more one thinks about such an adventure, of course, the more enticing it becomes and the more it sounds like our future lives on the earth-computer known as the Internet; but this—for better or worse—is not the way we currently work, for it does not fall within the linear constraints of print culture.

We select; editors and authors select for us, as do many others. What we really must do, then, is to pay far more attention to the many processes of selection. *How* do we select, and how does the culture of this discipline help us to select, or hinder us from making more rational selections? For instance, what is the source of the taxonomies that inform the editors of *Contemporary Sociology*, taxonomies that enable these editors to classify books? What is the source of the taxonomy of *Sociological Abstracts*—that is, whence did it diffuse?—and what will happen when we use the electronic version of *SA* to compile our own personal journals and books—if these be the right names?

Earle Eubank began this sort of inquiry during the 'thirties, and Walter Wallace has continued it up to the present time. But we have not yet made adequate progress, primarily because the media have been changing fast and, as we Ogburnians would predict, outpacing us.
(4) Synthesize and generalize: The Comtean style

In the 1850's, when Charles Darwin and Alfred Wallace were preparing, independently, to revolutionize biology by creating the biological sciences essentially as we find them today, they received a major inspiration from two sociologists: Malthus and, largely by way of Malthus, Adam Smith (Himmelfarb, 1960:xxiii; Jones, 1989; Rosser, 1992). Since sociology subsumes biology—or, at least, should attempt to do so [note 5]—it would be appropriate for us to adopt precisely the same attitude toward this sub-discipline that Smith and Malthus—not to mention Comte—would have adopted toward Darwin and Wallace. When we stop mucking about in that quotidian limbo known as the doldrums (Bainbridge et al., 1994:408,431), we will promulgate biosociology as a subdiscipline that will countervail against sociobiology; we will follow the suggestion of Charles Perrow, in Normal Accidents (1984), that if we wish to understand high-tech accidents we must understand, among many topics, the basics of several fields of engineering; we will follow the suggestion of the “strong program” of the sociology of science, realizing that if we wish to understand physicists we must understand physics (Cole, 1992:74-77); we will follow the suggestion of Van den Berghe (1978:10-15) that if we wish to understand human culture, we must master at least one foreign language and a corresponding body of literature. We will stop worrying over the question whether our curricular offerings are comparable to those of political science, economics, psychology, etc.—since we subsume these disciplines, our curriculum may well have unique features. And we may have to stop reaching routinely below the 90th percentile on assorted selection criteria if we wish to work with students who have the necessary background and motivation for mastering and integrating the verbal, the quantitative, the humanistic, the scientific, the theoretical, and the applied.
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Sociological theory := Cybermodels by POET by ADA

Abstract

A cybermodel (or hypermodel) is a large-scale system theory that often contains both microsocial and macrosocial elements, emphasizes causal interconnections among variables, and uses many of the traditional thought patterns of systemic analysis including routine searches for circular causation. Theories and associated methods tend to evolve in similar ways, and they generally evolve toward increased complexity, not toward parsimony. The most highly developed multidimensional theories, because of their complexity, often outrun available methods for testing hypotheses; the situation exemplifies Ogburn’s cultural lag principle. Promising but underutilized strategies for minimizing this lag, introduced in Part (1), include expert systems, taxonomic checklists and crosslists, chaos theory, computer (or partially computerized) simulations of social processes, and the method of comparative narratives. In Part (2) we discuss synergistic interrelationships among these strategies. We develop several examples including long-term macrosocial evolution, global warming, aviation and shipping accidents, logistic growth processes, the structure of small business firms, environmental pollution, computer purchase decisions, and counterfactual historical analysis. These examples consistently imply that the most elaborate and abstract ecological cybermodels almost invariably force social scientists toward the Comtean emphasis on synthesis of and generalization across disciplines.
(1) The Tilly paradox: Bigger structures, larger processes

(1.1) A tale of two textbooks

In Blalock’s (1979:470) textbook on statistics one finds an edifying discussion of the many problems encountered in trying to disentangle causal relationships among only three variables. After introducing a typical example, Blalock says that

of course most empirical situations are much more complex than this simple illustration suggests and more advanced techniques may be required, procedures that apply when the assumptions appropriate for ordinary least squares cannot be met. Indeed, since both arrows between two variables may be present or both absent or there may be single arrows in either direction, there will be \(4^3 = 64\) different possible diagrams for the three-variable case ...

[A] combination of a priori theoretical assumptions and empirical data will be needed to decide which of these possibilities are most plausible in a given instance.

If we add more variables and allow nonlinear relationships among them, the number of “possible diagrams” becomes indefinitely large. For instance, in supply-demand analysis one ordinarily begins with three variables, including the price range for a given commodity or service, the demand for it, and the supply of it. Supply and demand usually are taken to be nonlinear effects of price, and it may be desirable to break with convention and place them on the vertical, or \(Y\), axis of a graph in which price is the independent variable (Hughes-Hallett et al., 1997:450). Supply and demand curves tend to be highly unstable, especially in times of rapidly changing technology (Alcorn, 1997: Chapter 4). If an investigator were interested, say, in treating as a dependent variable the ratio of “producer surplus” to “consumer surplus” for a particular type of personal computer (Dowling, 1990:190-91; Kolman, Anton, and Averbach, 1992:751-56; SELFREF), she would be dealing, essentially, with a problem of infinite complexity.

This small set of observations has massive philosophical implications: Our most powerful methods imply a parsimonious world, but our most impressive theories oftentimes do not. Examining a second textbook (Lenski, Nolan, and Lenski, 1995; cf. Lenski, 1988), we see that the theoretical ideas currently presented to students involve an immense complexity that would quickly overwhelm our best methods. Make the transition from ordinary least squares to more sophisticated methods, and we still hardly know how to handle
the Lenski-Nolan-Lenski (1995:172; cf. Smith, 1995) causal model that shows the effects of
the shift from hunting-gathering to horticultural societies, and that has no fewer than fourteen
interacting variables—some of them are actually blocked or clustered combinations of related
variables. The causal arrows hypothesized among these variables, or sets of variables, imply
many instances of reciprocal effects: A “rising rate of innovation and new material products”
are thought to have reciprocal interactions with a list of processes including “increased size
of societies,” “increased division of labor,” “increased inequality,” and so forth. Dawkins
(1986) is probably correct in the observation that contemporary biology must concentrate on
creating theories about complexity, and it is likely that the same claim applies to the social
sciences. The “law of plenitude” negates the “law of parsimony”; since plenitude is presented
by nature and parsimony by humans, plenitude prevails (Wallerstein, 1991:13).

(1.2) Five adaptations to extreme complexity: An introduction

(1.2.1) An ESIE checklist

Identifying a theory may have a lot in common with identifying a disease. Nowadays,
when medical doctors try to identify a disease, they are likely to make use of machine
intelligence: They use a type of program called an expert system (ES). Mycin and Iliad (Ross,
1996), along with a number of databases currently available on the Internet, return medical
diagnoses. An ES called Prospector is said to have struck oil a few times. There are ES’s in
biology that identify animal and plant species with considerable accuracy. In a sense, both
Prospector and the taxonomy ES’s of biology have important features in common with
medical ES’s: They may be looked upon as ways of obtaining diagnoses and suggested
remedies. If you find an anticline with assorted surface properties and a certain environment,
drill. If it costs a lot to drill, and you’re risk-averse, improve your ES.

Social scientists need an ES for theory¹, starting perhaps with a “shell” program that
would learn by interacting with us, by figuring out, recording, and acting upon the processes
taking place inside our minds when we try to ascertain what sort of theory we have at the
moment, and what we should do next in order to strengthen it. A non-learning ES—the
Expert System Inference Engine (ESIE)—is currently available on the Internet (dean, n.d.).
Using the ESIE shell, we have begun to compile an admittedly crude database² that would
enable us to diagnose a theory in process and (eventually) to obtain ideas about directions in
which the theory may develop further. The following listing shows key elements of the
diagnostic part of the database:
goal is theory.type

legalanswers are y n *

question describe is “Does the theory attempt a detailed description of a social phenomenon?”

question causal is “Does the theory imply at least one simple (bivariate) causal relationship?”

question multivar is “Does the theory imply a multivariate set of causal or network relationships?”

question ordered is “Does the theory imply a multivariate causal process with intervening variables?”

question function is “Does the theory imply a process involving reciprocal causation or feedback?”

question A1 is “Does the theory deal primarily with innovations, inventions, or discoveries?”

question D is “Does the theory deal primarily with diffusion of POET processes?”

question A2 is “Does the theory deal primarily with adaptations to POET processes?”

question P is “Does the theory focus primarily on population variables?”

question O is “Does the theory focus primarily on organizational variables?”

question E is “Does the theory focus primarily on environmental variables?”

question T is “Does the theory focus primarily on technological variables?”

question global is “Are the theory’s variables selected from several or all POET categories?”

question deduct is “Does the theory have elaborate deductive content, little empirical content?”

if describe is n and causal is n and multivar is n and ordered is n and function is n then theory.type is non-sociological

if describe is y and causal is n and multivar is n and ordered is n and function is n then theory.type is descriptive.theory.type-1

if describe is n and causal is y and multivar is n and ordered is n and function is n then theory.type is analytical.induction

if describe is n and causal is n and multivar is y and ordered is n and function is n then theory.type is simple.network.model

if describe is n and causal is n and multivar is y and ordered is n and function is n then theory.type is axiomatic.theory
if describe is y and causal is n and multivar is y and ordered is n and function is n and deduct is n then theory.type is simple.network.model

if describe is y and causal is n and multivar is y and ordered is n and function is n if describe is y and causal is y and multivar is y and ordered is y and function is y and A1 is y then theory.type is functionalist.or.cybermodel.cell-b

if describe is y and causal is y and multivar is y and ordered is y and function is y and A1 is y then theory.type is complex.axiomatic.theory

if describe is n and causal is y and multivar is n and ordered is y and function is n then theory.type is time-series.causal.theory

if describe is n and causal is y and multivar is n and ordered is y and function is y then theory.type is functionalist.or.cybermodel.cell-a

if describe is y and causal is y and multivar is y and ordered is y and function is y then theory.type is protofunctionalist.type-4

if describe is y and causal is y and multivar is y and ordered is y and function is y then theory.type is functionalist.or.cybermodel.cell-b

if describe is y and causal is y and multivar is y and ordered is y and function is y then theory.type is functionalist.or.cybermodel.cell-c

if describe is y and causal is y and multivar is y and ordered is y and function is y then theory.type is functionalist.or.mosaic.cybermodel

if describe is y and causal is y and multivar is y and ordered is y and function is y then theory.type is functionalist.or.global.cybermodel

if describe is y and causal is y and multivar is y and ordered is y then theory.type is functionalist.or.global.cybermodel

if describe is y and causal is y and multivar is y and ordered is y then theory.type is functionalist.or.global.cybermodel

answer is "Type of theory: "theory.type
This database is both simple and complex. Simple, because it explores only a few basic dimensions of social theories: their descriptive properties, whether or not they invoke causation, whether or not they involve feedback, where they are placed in terms of basic ecological orientations, whether they emphasize deductive processes, etc. Complex, because it already generates a very lengthy list of diagnostic possibilities and recommended treatments. A realistic expert system for theory construction would have many dimensions, many questions, many questions contingent upon answers to preceding questions, responses that go beyond the simple binary level, and far more elaborate recommendations for further inquiry. But the current example gives the flavor. It also becomes clear as one reads this simple listing that the process of creating theories has self-reflexive properties: The first cluster of questions, from “describe” to “function,” contains more or less independent exogenous dichotomies with direct and indirect effects on the final diagnosis; the second cluster of questions, from “A1” to “A2,” involves intervening dichotomies that become relevant only within certain configurations of the first cluster; the “A1” to “A2” cluster may be correlated with the “P” to “T” cluster. And so forth. Eventually we arrive at the basic premise of the sociology of science: That it is possible to make theories about the making of theories. Notice, finally, that complete information about the “dependent variable”—i.e., our diagnosis of a particular form of theory—along with scores for all independent variables at the time of the final iteration of this program, would tell us next to nothing about the many iterations that may have preceded the ultimate diagnosis. In other words, if the nurturing of a theory involves elaborate “productions” and many feedback cycles, it is probably not highly amenable to induction: We cannot reconstruct the history of it. In conclusion, the process of creating cybermodels is itself a cybermodel, it cannot be completely understood, and iterative simulations sometimes tell us more about reality than does reality itself.

Suppose, for instance, that the user enters ESIE with a theory in mind based on analytic induction (Ragin, 1987:37; 1994:93-98). If he answers the first five questions n, y, n, n, n, he finds that the theory type is analytic induction; he receives a summary of the literature (and perhaps w leads) inspired by analytic induction, and perhaps a few detailed examples; he learns that subsequent research would, could, or should attempt to broaden the theory by invoking more complex causal arguments, by observing variables through time, and perhaps by discovering circular causal processes.

After the user had carried out such steps, she would return to the ES and receive another diagnosis. Suppose, again, that ESIE only needs answers to the first five questions, and that these are y, y, y, y, and n. The user would then be informed that she had developed a complex multivariate causal theory; recommended treatment would surely entail a search for reciprocal causation, and a first-rate ES might even suggest analogous situations, described
in the literature, in which reciprocal causation had been found. If such causal properties were discerned and ESIE invoked once again, the user would encounter a series of contingent or nested questions having to do with the crosstabulation of ADA and POET, to be introduced in a moment; for starters, one would expect to be asked whether any of the variables of the system were innovations subject to diffusion, and whether the feedback mechanisms involved adaptations to such innovations. Notice that if we did not have the benefit of nesting, so that all fourteen questions were necessary in order to obtain a diagnosis, the list of possible diagnoses would become very long. If a complex causal argument, involving feedback, were confined to a single cell of what we shall define as the ADA-by-POET crosslist, ESIE would so inform the user; appropriate treatment would involve broadening the inquiry in a way that would span several cells of the ADA-by-POET scheme (cf. Duncan, 1959; Duncan 1961).

Highly complex theories—the Malthusian theory of population, the Club of Rome or “limits to growth” world model, the Wharton model of the U.S. economy, the Rosenzweig-Parry (1994) model of global warming and its impact on agricultural productivity—would be described as functionalist cybermodels with either “mosaic” or “global” properties; the former ignore crucial cells of the ADA-by-POET paradigm, the latter essentially cover all of them. Treatment would involve the sorts of things that take place in an anatomy class whenever one encounters an especially interesting cadaver: Analyze components, contemplate the basic synthesis, find out what has been lacking over the years, design new hypotheses and new simulations, appreciate the elegance.

From time to time one discovers ways of improving an ES. For instance, the introduction of computers has produced a substantial amount of social research that generates cybermodels of immense complexity, but for which actual empirical data, at best, are minimal. Marwell and Oliver (1993) or Kim and Bearman (1997), who deal with social movements devoted to the provision of collective goods such as bridges, propose to model such processes by means of fixed probability density functions. The lognormal distribution, for instance, may represent the resources that members of interest groups contribute to some sort of common goal; contributions have an impact based on an assortment of “production functions” that are generally curvilinear and take many forms; the probability that a given individual will contribute toward a collective good may be influenced by the earlier contributions of others; all the above functions may have a particular configuration, say, within a given clique and a different configuration outside the clique; and there are many additional complexities. If these studies were to simulate random disturbances of their many distributions, and if they were to seek actual empirical data amenable to tests for conformity to the proposed density functions, they would become immensely more complex and perhaps more valuable. This effort, however, along with deductive models such as stable population
theory, stand alone as elaborate deductive cybermodels, and therefore we introduced into our ES a question that identifies such models and (eventually) provides diagnostics for them. Blau’s research (1977) on the social consequences of various demographic configurations is another case in point. Early on, my expert system described such theories as “functionalist or mosaic cybermodel”; the revised ES, however, reminds us that these theories have “elaborate deductive content, little empirical content.” Presumably, the diagnostics would suggest ways of enhancing the deductive sophistication of these fascinating cybermodels, broadening their empirical content, and so forth.

For the Rosenzweig-Parry example, which happens to be sociologically underdeveloped, the penultimate ES syllogism above returns the diagnosis “functionalist or mosaic cybermodel.” The treatment would involve introducing P, O, and T innovations and adaptations into subsequent studies; that is, a substantially stronger sociological component would transform the Rosenzweig-Parry simulation into a multidimensional functionalist theory of the type called a “global cybermodel.” A social scientist reading the following excerpts (Rosenzweig and Parry, 1994:134,135,138) should be able to return a diagnosis, treatment, and prognosis far more quickly than would my expert system—although checklist mnemonics remain essential as sensitizing concepts:

In each participating country, the agricultural scientists used the crop models to test possible responses to the worst climate-change scenario ... These adaptations included changes in planting date, variety and crop, and applications of irrigation and fertilizer. Irrigation simulations assumed automatic irrigation to field capacity ... and 100% irrigation efficiency. These optimistic assumptions imply that water supply for irrigation would be fully available at all locations under climate change conditions.

... There may be social or economic reasons why farmers are reluctant to implement adaptation measures, for example, increased fertilizer application and improved seed stocks [and irrigation] may be capital-intensive and/or not suited to indigenous agricultural strategies.

Climate change [toward global warming] was found to increase the disparities in cereal production between developed and developing countries. Whereas production in the developed world benefitted from climate change, production in developing nations declined. Adaptation at the farm-level did little to reduce the disparities, with the developing world suffering the losses.
Notice, however, that the Rosenzweig-Parry research provides clear instances in which POET processes occur according to the ADA time-series paradigm: First we observe global warming, apparently a partly man-made and partly natural innovation that diffuses, probably not uniformly, throughout the atmosphere; a huge variety of adaptive efforts then occurs in sectors such as agriculture and these efforts, over many years, generate variable levels of success; throughout this process, P/O/T innovations—another dimension of the misplaced sociology of Rosenzweig and Parry—may reduce atmospheric accumulations of carbon dioxide, methane, nitrous oxide, CFC’s, etc. In brief, we see here the early evolution of a major global cybermodel, one that describes an elaborate socioeconomic process having *longue durée*; for that reason, it is amenable to systematic research employing the sorts of block recursive causal models proposed by Blalock (1969:71-74).

In conclusion: If computers are clever enough to implement chess strategies that defeat all chess players except (for the moment) those of the caliber of Kasparov, then an ES should be able to elicit theoretical strategies in the mind of a social scientist that would be at least as powerful as, say, those once elicited by one’s dissertation committee.

(1.2.2) POET by ADA: A crosslist strategy

A few years ago we argued that sociological theories, as they grow in complexity, evolve through a predictable series of stages (SELFREF, 1986:128-33). At stage 1 we select a realm of discourse (e.g., “sexual harassment”), a conceptual apparatus, a basic orientation; we soon invoke something comparable to “Bloom’s taxonomy,” a checklist of infinitives that suggests tactics for further inquiry; we encounter Bridgman’s advocacy of a “no-holds-barred” attitude toward scientific inquiry—in essence, Bridgman seconds Bloom; or, we listen to the laity, arriving at claims such as that of Martinez (1994:261) that Nanci Griffith’s song “It’s a hard life ...” implies a plausible sociological theory; or, again relying on popular culture, we read an incisive bumper sticker asserting that “when momma ain’t happy, ain’ nobody happy.” Similarly, while exploring a book that exemplifies the best and the worst of postmodernism (Greeley, 1994), we discover fascinating things about, say, the sexual behavior of foot fetishists (Janus and Janus, 1993:123-24). At stage 2 of this evolution we apply more or less rigorous variants of Mill’s methods of causal inference—perhaps the boolean methods developed by Ragin (1987)—persuading ourselves that stage-1 phenomena do not occur randomly, that they have, let’s say, causal interconnections. For foot fetishists, we realize that this affliction occurs under discernable circumstances and that its effects on marital stability may be especially devastating. At stage 3 we introduce additional factors and typically begin sorting out the dynamics of complex “causal networks” (Hage and Meeker,
1988) with multiple causal agents. Finally, at stage 4, we realize that feedback—a special type of causal process assuming circular (i.e., reciprocal) causation—may be in evidence, presenting opportunities for sophisticated theorizing. Regarding foot fetishists, we surmise that past catastrophes would produce special efforts to suppress the affliction or to adapt to it more expeditiously, by means which we would then try to discern.

Because this theoretical evolution involves uninhibited exploratory behavior across large networks with circular interaction and, perhaps, adaptive feedback, it would be appropriate to refer to the resulting theories as cybermodels or hypermodels. Socio-ecological networks have a heterogeneous content: Some of them involve what engineers call “communication engineering”—to sociologists, weak ties; others, at an opposite extreme with perhaps many stops along the way, involve “power engineering” with strong causal interaction; some networks are very large—for instance, the Internet—while others are very small; some networks, candidates for the chaos processes of Perrow’s (1984) “normal accidents,” may become deadly, while others, such as the World Wide Web (w³), cause nothing more than occasional mild frustration or short-lived elation. Incidentally, the on-line helps for somebody’s network server recently informed us that w³ is not a hierarchy: There is no top, bottom, left, right, etc. This claim implies that the Parsonian notion of a cybernetic hierarchy is an oxymoron: Cybernetic structures tend to be far more complex than mere hierarchy.

It is possible to create a powerful mode of sociological analysis by exploring cybermodels within the dozen cells (a through el) of the following cross-tabulation of a pair of acronyms:

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ADA is an acronym for Accumulation of inventions, discoveries, and other (i.e., natural) innovations; Diffusion thereof; Adaptation thereto. ADA captures essential features of William F. Ogburn’s (1964; Lenski et al., 1995) impressive design for social science theory. POET, a standard term among ecologists, is an acronym for Population, Organization, Environment, and Technology. The cross-tabulation seems to have the following advantages:
(a) It is a mnemonic serving as a crosslist, and gives due honor to Ada Byron, daughter of a poet and mother of the software concept.

(b) It has some surprising dimensions: When we ask, e.g., how the ADA processes may take place within the natural environment, we realize that, for example, global warming may be as much a natural10 as a man-induced innovation (Dufour, 1994). The AIDS virus is an evolutionary innovation created by nature, it diffuses as a natural innovation highly dependent on social norms, and we adapt to it in one way or another (Lemonick, 1994). Various human genome projects trace the history of natural innovations involving the genetic composition of the human species—the famous knee, for instance, of the fossil known as Lucy, which permitted upright, bipedal locomotion and must have transformed completely the lives (and, eventually, the brains11) of these early hominids. By contrast, a rapid decline in mortality, or a high sex ratio brought about by prenatal sex selection of children (James, 1987a, 1987b), or a set of innovative names for the newly born (Lieberson and Mikelson, 1995), would be population innovations; stock market index futures and a host of more recent financial devices for assuming or “laying off” risk, or new adaptations by foot fetishists, or the sorts of “regional advantages” (Saxenian, 1994) unique to places like the Silicon Valley, are organizational inventions; the McDonnell Douglas MD-11 airliner began as a congeries of old and new technological innovations that has diffused widely, and that may interact with nature and social organization in catastrophic ways (see below). In brief, the POET-by-ADA mode permits us to see Ogburnian innovations within the technological realm and within areas other than technology.

(c) Human ecologists urge us to examine causal relations within and among the four POET categories (Duncan, 1959; Duncan, 1961; Duncan and Schnore, 1959). The indefinitely large number of causal models resulting from this strategy can be worked through the Ogburnian ADA process: All aspects of human population12, social organization, the environment, and technology were at one time inventions, discoveries, or natural innovations. They diffused. They generated adaptive efforts. They generally have a long and complicated history.

The POET-by-ADA crosslist forces us to think about detailed, sophisticated, realistic social scenarios. Recently, my “technology and society” students became involved in a discussion of a paper dealing with the effects of computer networks on social isolation (Kupfer, 1996). The paper is
inconclusive, and so were the students. When they began imagining detailed scenarios, however, they started having insights: What sort of social interaction would occur if we had a high resolution, large-screen, powerful computer, attached to the network and capable of responding to remotely-issued commands (all T factors) from a large number of students and faculty seated on a comfortable circular couch in a mood-lighted room (P, E), interacting in a non-competitive situation (if possible) where students are trying to learn, say, Minitab or Maple or Matlab, or trying to navigate the network (all O factors)? This sort of problem, with its readily envisioned specificities, they handle deftly.

(d) As we shall see below in a discussion of global warming, ADA implies time-series processes; this is an advantage whenever we wish to derive causal theories.

(1.2.3) Chaos theory

Chaos theory, as defined by Medio (1992:4), focusses on apparently “...stochastic behavior occurring in a deterministic system,” and it purports to explain such behavior. Chaos theory therefore raises a classic issue: Whether all changes in nature are ultimately deterministic, if we but knew all positions and motions of all particles at a given starting time. For problems of limited complexity, such as the behavior of two coupled pendula (the “double pendulum” experiment\(^{(1)} \)), the apparently stochastic disruptions of the motions of the pendula often turn out to be highly regular and predictable, with highly structured and symmetrical geometries (Korsch and Jodl, 1994: Chapter 5). Needless to say, this sort of deterministic explanation does not seem to exist in the social realm, and the inexorable intrusions of stochastic realities into social processes add substantially to the complexity of chaotic, holistic explanations. Yet, we find here a grand paradox: When one examines the work of chaos theorists, one notices that the major operating premise is that chaotic transformations throughout nature tend to be sudden, qualitative, binary, and holistic. This premise, one hopes, will serve to simplify matters that otherwise remain highly complex.

Benign premises sometimes lead to reckless promises. Briggs and Peat (1989:83), in a work dealing with chaos theory, appear to reconcile the tensions created by hypercomplexity, but their *modus operandi* is not entirely satisfactory:

... when scientists study complex systems, the notion of parts begins to break down so that quantification of such systems becomes impossible. So scientists
wanting to study dynamical systems have turned to another approach to measurement—qualitative mathematics. In the old quantitative mathematics the measurement of a system focuses on plotting how the quantity of one part of the system affects the quantities of other parts. By contrast, in qualitative measurement, plots show the shape of the system’s movement as a whole. In a qualitative mode, scientists don’t ask, How much of this part affects that part? Instead they ask, What does the whole look like as it moves and changes? How does one whole system compare to another?

Although the catastrophe of chaos theory appears to be at least as engaging and promising as it was back in the days when it was promulgated, under a different set of labels, by famous dialectical materialists such as Friedrich Engels ([1972]) and J.B.S. Haldane (1969), I have little enthusiasm for the Briggs and Peat suggestion that the “old quantitative mathematics” is now somehow passé. Nothing should be eliminated arbitrarily from the social science toolbox. Huge cybermodels do indeed behave chaotically, with unpredictable catastrophic transformations; but we readily capture many of these binary switches by means of established quantitative techniques—loglinear analysis, logistic regression, multiple discriminant analysis, and boolean analysis. René Thom’s topology of chaos no doubt has a role (Briggs and Peat, 1989:84), but it has no need to restrain competition.

In any case, chaos theory contains an important lesson: The dialectics of qualitative transformation are alive and well, and may have high utility as applied to the evolution of complexity.

(1.2.4) Fuzzy elephants: A didactic simulation

... a major thread running through earlier mathematical models in sociology has been borrowing ... construct-by-metaphor from all sorts of engineering and natural science disciplines. Jim Coleman’s seminal Introduction (1964) concentrated on ... processes ... developed earlier ... in operations research and ... chemical engineering ... I early proposed models of kinship systems and vacancy chains ... influenced by the solid state physics in which I was first trained.

—White (1997:54)

One could write a fascinating simulation that would capture a famous speculation by Darwin about a self-regulating process. The process contains many POET dimensions, and allegedly works as follows:
A large population of “spinsters” in a given community has acquired a large number of cats. The cats are accustomed to feeding on field mice, so that any increase in the cat population would tend to reduce (due to a stochastic impact, no doubt) the field mouse population. Since the mice feed on the larvae of bumblebees, a reduction of the mouse population would make for an increase in bumblebees, and the latter would soon become active in pollinating the local cash crop, a type of clover used as a livestock feed. As the huge clover harvests begin coming in, farmers in the area become prosperous and soon find themselves marrying members of the standing spinster population; the marriage rate increases sharply. On marrying, the spinsters tend to get rid of their cats, whereupon all the causal impulses reverse themselves, the farmers eventually go bankrupt, the divorce rate increases, the cats return, and so forth. If this simulation contained appropriate properties, the cyclical process could go on indefinitely.

It is a virtual certainty, however, that a realistic simulation would not have the necessary stability to make this process constantly self-replicating.

To illustrate the advantages and disadvantages of simulation, we have written a brief program using Minitab, a statistical package that includes many commands comparable to BASIC. This program creates a simulation of Stephen Jay Gould’s example involving the evolution of elephants, presented during a public lecture at William & Mary on 15 November 1994. It tells us something about the strengths and weakness of simulation, and about the sorts of questions raised by a given simulation. It shows the operation of negative feedback by means of mortality in the biological realm, and leads to the speculation that similar processes may occur in the social realm (Lenski, Nolan, and Lenski, 1995: Chapter 3, esp. 75-77). Logically, it illustrates the same process of negative feedback that we encounter in the Malthusian theory of population (1798 edition). As Gould claims, both Darwin and Wallace were inspired by Malthusian and Smithian negative feedback theories about socio-demographic processes, although one must recognize that, in an instance of reverse causation, Adam Smith received considerable inspiration from the great physiologist William Harvey.

In Gould’s example, elephants in Siberia had to adapt to an increasingly cold ice-age climate by evolving toward having thicker body hair, a.k.a. increased fuzziness.

The simulation language is Minitab, and it has considerable flexibility. Suggestion: Log into a PC that runs Minitab, write these commands into a file, and submit them to Minitab. Run experiments, as suggested below. Use the file output command if you wish to
store results of experiments. Here is the first set of Minitab commands, with explanatory comments following:

MTB > set c1
DATA> -10:10
DATA> end
MTB > name c1 'fuzz'

This is a scale of hair thickness and coverage. It ranges from -10 to 10, and it is called 'fuzz'.

MTB > pdf c1 c2;
SUBC> norm -5 1.
MTB > name c2 'pop'

These three commands create the experimental-stimulus variable, a normally distributed population of elephants with mean = -5 on the fuzziness scale, and standard deviation = 1; the pdf (probability density function) command merely asks that the normal distribution be created over the 'fuzz' variable. To run one type of experiment, adjust the first value after the word “norm”; this action changes the mean fuzziness of the elephant population at a given time. Notice that all is relative: An average population fuzziness of -5 means that we start this natural-selection process with a population that, on average, is 5 units below the optimum for fuzziness, which is a score of zero. This discrepancy may be due to genetic variation, a change in ambient temperature (as in Gould’s example), migration, or to a combination of these factors. If we wished to begin the simulation with a population favorably adapted to the environment, we would have to invoke factors such as genetic variations, changes of the environment, etc., in order to create a disturbance. That is, these factors would have to be written into the simulation.

If, therefore, we substitute 0 (zero) for -5, we have the baseline: the lowest possible mortality (according to a formula below) and therefore the best adaptation. In this simulation we arbitrarily hold the optimum fuzziness, with its relatively low mortality, at a score of 0 on 'fuzz'.

Question: What happens if we change the shape of the population—now normal with a standard deviation of 1.0—or the shape of 'mort', the death rate, soon to be defined? This is a question for which there is an infinitude of answers. And the selection process itself is likely to bring about changes in the shape of the population, as well as the size of it.
MTB > let c3=10+'fuzz'**2
MTB > name c3 'mort'

This is the death rate. It is set at 10 plus the square of fuzziness; it is exponentially higher for those elephants either above or below the optimum level of \( 'fuzz' \). The lowest death rate is 10 for those elephants with optimal fuzziness, and this can be taken as a crude death rate: number dead per thousand population per year. Notice, again, that a real death rate could have many forms other than this simple exponential function. *Question:* Do we now begin to understand why it is that biological functionalism claims that structures survive because they have survival value, and that these structures are known to have survival value because they survive?^{14}

MTB > let c4='mort'*'pop'
MTB > name c4 'fertdead'

Here we apply the death rate to the current population members. We named the product \( 'fertdead' \) to remind ourselves that an elephant does not have to die literally in order to be selected against: S/he merely has to fail to reproduce, either through lethal or reproductive selection. The \( 'fertdead' \) concept also reminds us that, if this population were to move through several generations, each new generation of elephants would change the shape of the population—perhaps with a tendency toward preserving normality and with a more highly adaptive level of mean fuzziness. It should be clear, also, that in a more realistic simulation we would have to make decisions about the impact of *lethal* selection on reproduction, because lethal selection, morbidity, etc., do not matter in natural selection unless they have an impact on reproduction.

MTB > sum 'fertdead' k1

\[
\text{SUM} = 36,000
\]

Now we sum up the harvest of mortality for the initial level of adaptation, with adaptation understood in terms of general fuzziness. Total mortality is relatively high whenever the mean fuzziness of the population is far from the optimum. When it is too high, and when it is too low, we see selection against the elephants that are furthest from the optimal fuzz levels. This process always acts by means of negative feedback: Any discrepancy between optimal and actual fuzz will select most strongly against the elephants furthest from the optimum.

For a series of experiments in which you change the “norm” subcommand above, examine the relationship between (1) the discrepancy between population fuzziness and
optimal fuzziness and (2) the harvest of mortality (and subfecundity), a.k.a. 'fertdead'. If there is a positive correlation, and if mortality selects against the elephants furthest from optimal fuzziness, and if this selection does indeed change the population mean for fuzziness (does it?), what is the sense in which this process constitutes negative feedback? How does this process relate to Malthus and Smith and to other sociological examples?

For instance: Suppose that we substitute 'auth' for 'fuzz' and 'acci' for 'mort', etc. Would this simulation then represent Perrow's claim (1984: Chapter 6) that command and control in the realm of maritime shipping (especially huge oil tankers) are maladaptive in their authoritarianism? Perrow implies that too little 'auth' would raise 'acci', and that too much 'auth' would have the same impact. Do we then predict an evolution toward optimal levels of authority in maritime shipping, at least in crowded environments such as the English Channel? What sort of evidence would be relevant to this question? What could we discover by reading NTSB reports about aviation accidents, especially those that seem to involve too much or too little authority on the part of, say, airliner captains?

MTB > desc ‘fuzz’ ‘pop’ ‘mort’ ‘fertdead’

<table>
<thead>
<tr>
<th>N</th>
<th>MEAN</th>
<th>MEDIAN</th>
<th>TRMEAN</th>
<th>STDEV</th>
<th>SEMEAN</th>
</tr>
</thead>
<tbody>
<tr>
<td>fuzz</td>
<td>21</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
<td>6.20</td>
</tr>
<tr>
<td>pop</td>
<td>21</td>
<td>0.0476</td>
<td>0.0000</td>
<td>0.0316</td>
<td>0.1083</td>
</tr>
<tr>
<td>mort</td>
<td>21</td>
<td>46.67</td>
<td>35.00</td>
<td>45.26</td>
<td>33.49</td>
</tr>
<tr>
<td>fertdead</td>
<td>21</td>
<td>1.714</td>
<td>0.000</td>
<td>1.160</td>
<td>3.924</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>MIN</th>
<th>MAX</th>
<th>Q1</th>
<th>Q3</th>
</tr>
</thead>
<tbody>
<tr>
<td>fuzz</td>
<td>-10.00</td>
<td>10.00</td>
<td>-5.50</td>
</tr>
<tr>
<td>pop</td>
<td>0.0000</td>
<td>0.3989</td>
<td>0.0000</td>
</tr>
<tr>
<td>mort</td>
<td>10.00</td>
<td>110.00</td>
<td>16.50</td>
</tr>
<tr>
<td>fertdead</td>
<td>0.000</td>
<td>13.963</td>
<td>0.000</td>
</tr>
</tbody>
</table>

MTB > print ‘fuzz’ ‘pop’ ‘mort’ ‘fertdead’ k1

<table>
<thead>
<tr>
<th>ROW</th>
<th>fuzz</th>
<th>pop</th>
<th>mort</th>
<th>fertdead</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>-10</td>
<td>0.000001</td>
<td>110</td>
<td>0.0002</td>
</tr>
<tr>
<td>2</td>
<td>-9</td>
<td>0.000134</td>
<td>91</td>
<td>0.0122</td>
</tr>
<tr>
<td>3</td>
<td>-8</td>
<td>0.004432</td>
<td>74</td>
<td>0.3280</td>
</tr>
<tr>
<td>4</td>
<td>-7</td>
<td>0.053991</td>
<td>59</td>
<td>3.1855</td>
</tr>
<tr>
<td>5</td>
<td>-6</td>
<td>0.241971</td>
<td>46</td>
<td>11.1307</td>
</tr>
<tr>
<td>6</td>
<td>-5</td>
<td>0.398942</td>
<td>35</td>
<td>13.9630</td>
</tr>
<tr>
<td>7</td>
<td>-4</td>
<td>0.241971</td>
<td>26</td>
<td>6.2912</td>
</tr>
<tr>
<td>8</td>
<td>-3</td>
<td>0.053991</td>
<td>19</td>
<td>1.0258</td>
</tr>
<tr>
<td>9</td>
<td>-2</td>
<td>0.004432</td>
<td>14</td>
<td>0.0620</td>
</tr>
<tr>
<td>10</td>
<td>-1</td>
<td>0.000134</td>
<td>11</td>
<td>0.0015</td>
</tr>
<tr>
<td>11</td>
<td>0</td>
<td>0.000001</td>
<td>10</td>
<td>0.0000</td>
</tr>
<tr>
<td>12</td>
<td>1</td>
<td>0.000000</td>
<td>11</td>
<td>0.0000</td>
</tr>
</tbody>
</table>
MTB > plot ‘pop’ ‘fuzz’

MTB > plot ‘mort’ ‘fuzz’
The preceding four commands produce descriptive statistics and graphic displays. Examining the list of scores for all four variables, we see that the highest number of dead elephants, indexed at 13.96, occurs among those closest to the current mean for fuzziness, which is -5. This large number of decedents occurs because there are more elephants within this segment of the population: Forty per cent of the elephants are close to the average for fuzziness. But the 24 per cent of the elephant population that is close to a score of -6 have higher mortality rates, and higher numbers of decedents, than do those elephants at an equal distance above the mean—and this is where natural selection begins to impinge. The latter, with fuzziness indexed at -4, are closer to the optimum, have a lower death rate, and produce fewer decedents. Similarly, elephants with fuzziness scores around -7 produce more decedents (indexed at 3.18) than those around -3, and so forth. This is how natural selection would create a new level of fuzziness in this population—or a new level of authoritarianism among ships’ captains. Presumably, as implied above, the fertility of surviving elephants would correct the distortion of normality brought about by this biased selection around the average level of fuzziness. Clearly, this particular simulation does not create the next generation; a more complete simulation would do so by subtracting decedents, adding the new generation, and moving into another generational cycle. A more complete simulation, that is, would allow the population to move further toward a new adaptation or equilibrium and would also allow both lethal and reproductive selection to operate as continuous processes.

“Counterfactual” history (Fogel, 1964; see Ogburn et al., 1946; SELFREF, 1993:179-89) provides a large range of hypotheses testable by means of simulated cybermodels. In general POET by ADA, as we shall see, suggests many opportunities for social simulations—beginning, perhaps, with artificial intelligence (Collins, 1992:155-84). A simulation by Rosenzweig and Parry (1994), discussed above, shows that changes of global climate over several decades could have a major impact on food cultivation throughout the world; their simulations, however, remain at least as incomplete as the simulation involving natural selection, saying little about the socio-ecological processes that influence how cultivators within various cultures adapt to new ecological conditions. For another example, see SELFREF (1998): In this instance a Maple simulation shows that plausible markets are selected by the same sort of dynamic that selects fuzzy elephants. Sociologists should attempt to understand such processes better by expanding existing simulated cybermodels. Among the major languages of “cybermodels by POET by ADA”—i.e., of neo-functionalism—one should find Pascal, C++, Simscript, artificial intelligence languages, Maple, Matlab, Minitab, and the better spreadsheets.
(1.2.5) Comparative narratives and large generalizations

Abell (1987: Chapter 1) defines *comparative narratives* as “... stories accounting for the occurrence of a given event.” The ultimate task of the narrator is to ask “... how characteristic (i.e., generalizable) the story is in accounting for the generation of similar or identical events.” In explaining “... two or more apparently identical or different social events ...,” one is involved in making a comparison of two or more narratives. Abell (1987: Chapters 5 and 6) has developed an algebra for writing narratives and an associated geometry for depicting them. These devices would have many potential applications, including database programs designed to select narratives according to various algebraic or geometric criteria. Abell (1987:87-88, 90) argues that a major role for comparative narratives is to help us discover intervening variables that explain a causal relationship identified through the “variable-centered method,” and to help us identify deviant cases; to Abell, traditional statistical methods and contemporary storytelling methods are tightly enlaced. As a minimum, then, Abell advocates stage-3 theories; however, his narratives may also contain circular-causation “cycles,” provided that these causal impulses do not occur simultaneously (Abell, 1987:53), and therefore the narratives may embody stage-four theories.

The following summary, based on a story written by the National Transportation Safety Board (NTSB), is beyond any doubt a narrative and it has applications comparative:

The direct cause of a recent aviation accident was an “inadvertent ... slat deployment” on China Eastern Flight 583, about 1,000 miles south of Alaska while en route from Beijing to Los Angeles. An appropriate cybermodel for accidents of this type—*nota bene*, this case is not entirely unique—would focus on the following ADA-by-POET issues, suggested by a detailed NTSB narrative (NTSB, 1993:54-55):

(a) the demographics and physical vulnerabilities (to non-controlled flight) of 235 passengers and 20 crewmembers: a population (P) factor;
(b) deficient flap/slat handle system design, operation, and training: an organizational/technological (O/T) interaction;
(c) diffusion of information to pilots regarding known problems with inadvertent slat extensions: an O process;
(d) the aerodynamics of an aircraft for which pitch control has been lost: an E/T interaction;
(e) an over-reaction by pilots to loss of pitch control, due to highly responsive MD-11 controls and inadequate training in their use: an O/T interaction;

(f) “deviant behavior” by passengers, who tend not to comply, while en route, with warnings about seatbelts; inappropriate use of seatbelt warnings by flight attendants: P/O/T interactions;

(g) the nature of trans-Pacific air routes, necessitating in this instance a long flight to an alternate airport, with dead and severely injured passengers on board: a complete P/O/E/T interaction;

(h) political and cultural differences between, say, the Civil Aviation Administration of China and analogous organizations in the U.S., as these differences impinge on reactions to serious accidents: a P/O/T interaction.

In this narrative it becomes clear that ADA-by-POET overlaps in many ways Perrow’s (1984:8) DEPOSE acronym, a checklist with considerable utility in interpreting a variety of system accidents. The ADA-by-POET paradigm enables us to elaborate on many such examples and, more importantly, to tie them together. The algebra and geometry for tying narratives together should be comparable to those suggested by Abell; as a minimum, we should adopt a database format similar to that already in use at the NTSB (1995:61-71) for coding “cause/factor assignments.” And remember, the NTSB is not merely classifying engineering stories: They are classifying complex ADA-by-POET narratives. Comparable efforts, emphasizing generalization across several independent narratives, appear in Licklider (1993) and Smith (1995). Such efforts should be written into expert systems.

Reading NTSB narratives or Licklider or Smith is like reading a series of short stories. According to an established “theory” (Day and Bauer, 1953), a good short story is a sort of literary research monograph: It sustains a single theme, it provides tightly organized information in support of its theme, and it does so convincingly. At first, a comparative narrative seems to behave like a short story. Soon, however—especially if we have a series of good narratives and know how to compare them—the sociological imagination takes over, metaphysical conceits proliferate (one of the best: Saxenian, 1994:134-41, drawing an analogy between the Silicon Valley and a Sun Workstation), and the comparative narratives come together like a lengthy, complicated, multidimensional novel. Barzun (1964:198) claims that this is precisely the sort of transformation that is the central accomplishment of scientific thought; he only derides the practice when we overdo it, thereby losing our aptitude for other forms of thought. Barzun may be right, but we should probably cultivate more seriously the bad habits that he claims we already possess.
Comparative narratives, then, produce novels. Novels (Mailer excepted) are a form of fiction, and fiction is largely a process of exploring counterfactuals. Counterfactuals are a favorite pastime of various historians, and many virtual-reality hypotheses—“what might have been” (McDonough, 1977:221-25)—have been proposed by them. It is clear that counterfactual analyses should often be made by means of simulations, with the latter informed, eventually, by expert systems. At some point the results begin to look less like a novel and more like social science theory.

(2) “Connect, always connect” (Koestler, 1964:230-33)

If you understand something in only one way, then you do not really understand it at all. This is because if something goes wrong you get stuck with a thought that just sits in your mind with nowhere to go. The secret of what anything means to us depends on how we have connected it to all the other things we know.

—Minsky (1996:234)

All of which inspires us to seek out some interrelationships. The five research strategies elaborated in this work—expert systems, taxonomic checklists and crosslists, chaos theory, social simulations, the method of comparative narratives—have various features in common: First, they have been inexplicably neglected, especially in the textbook literature on research methods and theoretical models; second, they seem to have utility in our efforts to deal with the complexities of social theory and the corresponding complexities of social reality; third, they interact with each other in ways that are mutually edifying and reinforcing. The first claim we will not try to establish at all; it is merely an impression, albeit a strong one. The second claim, regarding complexity, is the basic thesis of this inquiry. The final claim, at the moment, requires special attention: We propose to defend it in several different contexts.

(2.1) Taxonomic checklists and expert systems

During the decade of the ’sixties, with the development of high-speed, third-generation computers, it soon became possible to address various research questions that, earlier on, had remained beyond reach, unassailable, or at least highly intractable. Third-generation computers enabled us, for instance, to invert huge matrices of the form $X'X$, in which $X$ represents all the constants and independent variables included in a multiple-regression problem, each with a very large number of observations. Inverting such a
matrix, an essential step in finding solutions for regression equations, was extremely difficult if not impossible until adequate data-processing machinery began to appear. Similarly, the extraordinary information-processing capabilities of today’s computers, millions of which now interact within an expanding global network, will encourage us to organize social-science information, defined more broadly than ever, in ways that heretofore have been unattainable, perhaps inconceivable. One likely scenario toward the end of the ’nineties is that a large assortment of expert systems will make themselves available, and these systems will encourage us to develop elaborate taxonomies in the same way that earlier computer facilities encouraged the manipulation of large matrices. In a discussion of ES’s, Benfer, Brent, and Furbee (1991:9) refer to systematic taxonomies as “… deep hierarchies (those with many levels related by class inclusions),” and they believe that such hierarchies are likely to become an essential component of ES’s. ES’s based on tree diagrams, for instance, typically make use of taxonomies that impose exhaustiveness and mutual exclusiveness—essential features of taxonomic organization—at each of many branching points.

Inverted matrices and Internet magic, in brief, have a lot in common.

(2.2) Taxonomic checklists and chaos theory

In recent years discussions of taxonomies, expert systems, and chaos theory suggest that qualitative, Gestaltist ways of measuring and classifying social events may have arrived at a renaissance comparable to the famous “rediscovery of the primary group” that occurred several decades ago. Systematic taxonomies invariably place a large emphasis on hierarchically organized qualitative concepts treated as nominal variables (SELFREF, 1993: Chapter 7). In a work on the comparative method, Ragin (1987) argues that comparative analysis often generates hypotheses about combinations of causes; logical possibilities increase geometrically when one attempts to formulate such hypotheses, and in the absence of adequate taxonomic hierarchies and theoretical guidance one is likely to end up and remain lost. Ragin (1987:41) presents a relatively simple example in which a combination of land hunger and rapid commercialization, in various parts of the world, may be necessary and/or sufficient to bring about peasant revolts; additional causal factors would make the problem far more demanding. Taxonomies capable of accommodating such combinations of potential causes would have considerable complexity and utility.

Ragin (1987:43,47-9; cf. SELFREF, 1993:208-09) describes a way in which taxonomies may be evaluated and restructured by new findings. The taxonomic hierarchies deemed valid at a given moment are subject to falsifiability, a property shared with other
scientific formulations. Conceptual hierarchies typically become unstable because they cannot accommodate new findings; they may then change swiftly, like a slowly rotating kaleidoscope in which there is a sudden shift to an entirely new visual image. The logical processes are captured by the following syllogistic exercise. Suppose that in a simple taxonomic hierarchy, the categories el and m are subsumed under a broader category C. Dentists and Physicians, let’s say, are subsumed under Professional Personnel, as in the thesaurus of Sociological Abstracts. In the process of generating and accommodating new research, one might take advantage of the modus tollens by constructing a syllogism that would generate a prediction based on this hierarchy, as follows:

First premise: If (a) categories el and m are subsumed under category C, and if (b) variable x is related to variable y within category el, then variable x is related to variable y within category m;

Second premise: Variable x is not related to variable y within category m;

Therefore, categories el and m are not subsumed (or subsumable) under category C—clause (a) is false—and/or variable x is not related to variable y within category el—clause (b) is false.

What does it mean in this instance to say that at least one premise is false? Given that clause (b) of the first premise may have been well established through past research, it is clause (a) that becomes highly suspect. But clause (a) is largely a matter of definition, and declaring it to be false is merely a way of telling ourselves that our taxonomy needs to evolve, to be updated. If the relationship between x and y varies from category el to category m, and if this variation recurs for many other relationships, we conclude that the taxonomic distance between el and m, now subject to “creative tension,” should be increased: The internal dynamics of entities of type el and entities of type m are substantially different, and they should not be so closely linked taxonomically as is indicated by clause (a) of the first premise. And changes within taxonomic systems do indeed occur suddenly, as a rapid chaotic switch: New findings cause tension, creating internal contradictions, and then without warning there is a reorganization as all the colorful terminology tumbles into a new configuration.

Furthermore it is entirely possible that, in the preceding case, a powerful ES would inform us of the existence of a specific mechanism (Bunge, 1997) accounting for the observed nexus between x and y; logically, our next hypothesis would stipulate that such a mechanism is not present in instances where the x, y nexus does not exist.
The preceding syllogism, involving a comparison of two different types of professions, is already complex. However, if one were testing combinatorial, situational, or configurational hypotheses such as those of Ragin regarding causes of peasant revolts, then for every instance in which one mentioned $x$, a causal cluster, within the syllogism one would have to substitute the crucial combination of alleged causes, land hunger and rapid commercialization. (Larger combinations would become ever more demanding, all of which implies, again, that social theorists may need help from expert systems.) Once again $y$—in this instance the occurrence of peasant revolts—would be the dependent variable. The $x, y$ relationship would be examined within subcategories of $C$, which would define different types of agrarian societies. It should be carefully noted that, as Kuhn (1970) implies in what is actually a chaos theory about the evolution of scientific thought, when taxonomies collapse into new forms they do so suddenly, implosively, with little warning.17

(2.3) Taxonomic checklists and social simulations

(2.3.1) Stipulative mechanisms

If it is well designed and developed and permits experimentation, a given simulation produces results so quickly that it is almost inescapable that we develop adequate taxonomies for sorting out myriad results. The following list contains many examples of excellent simulations already available and in some instances highly developed. A few of these simulations we discuss briefly below, exploiting primarily their heuristic significance.

- logistic patterns of growth
- queueing theory
- sex preselection of offspring
- crime victimization, and adaptation to risk thereof
- salary determination in large bureaucracies
- traffic control: small-scale, grand-scale
- the AIDS epidemic
- world systems, greenhouse effect, etc.
- aviation simulations: including, e.g., MDM, and larger systems
- Bainbridge simulations (Bainbridge, 1985)
- railroads and waterways; other counterfactuals
- legislative re-districting
- aircraft takeoff distances
fuzzy elephants
chess games
dollar.bas; search algorithms, e.g., neural networks, TSP (travelling salesman problem)
Harris and Ross (1987:32-33) examples
purchase of computer system (Thesen and Travis, 1992:276-93)

The logistic curve, for instance, has inspired many simulations of growth and diffusion processes, oftentimes very successfully; yet, because of a few notorious instances in which “predictions” based on this large family of functions failed to materialize, the idea of modelling logistic processes has received bad press among social scientists. When one understands the basic mathematics of the logistic, however, one realizes that a negative reaction to a given application cannot be taken as a blanket condemnation, a permanent attain: There is, alas, an infinitude of logistic curves and an infinitude of potential applications of them, and rejection of an infinitude, necessarily out of hand, is a good definition of prejudice. A taxonomy of time-series models (SELFREF, 1986:56-58), combined with a taxonomy of applications or potential applications of these models, would help us to connect the right models with the right applications. In many instances a given logistic model would be capable of revealing essential features of a given growth process, perhaps more accurately than any competing model, quantitative or otherwise. For an illustration, examine the uses of logistic curves in Coleman, Katz, and Menzel (1966).

The actual or proposed simulations listed above have common elements. For instance, each of them strongly encourages us to stipulate specific mechanisms that move the simulation from its inputs to its outputs. It is true, however, that even in instances where it is possible to design differential equations—an excellent way of simulating time-series processes—investigators may fail “to specify the mechanism of a process” (Bunge, 1997:423). In the absence of such specificity, simulations usually make explicit use of probability distributions that capture the range of unknown or poorly understood mechanisms. If the mechanisms built into simulations do produce plausible results, however, they should be tested within the contexts provided by the real world, where the mechanisms are “both confirmable and falsifiable” (Bunge, 1997:420). Mechanisms that typically survive these tests are listed by Bunge (1997:447-450); as we said earlier, it would be wise to write such lists into the list processors of expert systems.
(2.3.2) Does everything grow logistically?

The following is a Maple simulation of a logistic diffusion process involving a new type of crop introduced into an ecological area that has a limited carrying capacity, a typically Ogburnian event involving diffusion of an innovation and adaptations to it. As we shall see, a limited carrying capacity is the central feature and the central power of the logistic, and critics of logistic modelling should realize that explaining a growth process constrained by, say, spatial or other environmental limits is usually far more realistic than the simplistic although commonplace practice of projecting a constant rate of increase into an indefinite future. The latter is a procedure that almost invariably produces erroneous results that are often taken too seriously. The logistic curve may not invariably do a better job of capturing the dynamics of a given growth process, but it has better prospects and, more important, it shows us the right directions to take as we try to make growth simulations more realistic: We try to add constraints that reflect demonstrable realities of the situation.

We assume that total available acreage is 10,000 acres. At the outset of this diffusion process there is a census indicating that 75 acres have been planted in the particular crop; a later census shows that, after 30 weeks of planting activity, 1,000 acres have been planted. We wish to project the diffusion of this crop over a period of 150 weeks, using the logistic curve.

We begin by setting up an ordinary differential equation for logistic growth. We then solve it, insert an initial value and an intermediate value from two periodic agricultural censuses, and plot the resultant growth pattern.

The value $Q$ is the amount of acreage allocated at a given time, $k$ is the growth rate, and $L$ is the carrying capacity—in this case, the maximum available acreage.

\[
d_{eq1} := \frac{dQ(t)}{dt} = kQ(L - Q)
\]

Expand the above expression:

\[
\frac{d}{dt}Q(t) = kQ^2 - kQ
\]

Here we insert a value from the initial census, showing acreage already devoted to the crop at the start of the diffusion process.
Now we obtain a general solution that captures the growth process, although it remains largely in symbolic form:

\[
\begin{align*}
L \\
\text{sol1} := Q(t) &= \frac{\text{L}}{1 + \frac{1}{75} \exp(-k L t) (L - 75)} \\
\end{align*}
\]

Notice that the expression containing epsilon is multiplied by \( L - 75 \) and divided by 75; in a moment, when a value is declared for \( L \), this calculation will become the constant for the solution \( \text{sol1} \).

Now, let's capture the righthand side of \( \text{sol1} \), for later use.

\[
\begin{align*}
L \\
\text{rh1} := \frac{\text{L}}{1 + \frac{1}{75} \exp(-k L t) (L - 75)} \\
\end{align*}
\]

Declare the carrying capacity, i.e., the limiting value of acreage available within this environment:

\[
L := 10000
\]

Check the solution again, with appropriate substitutions.

\[
\begin{align*}
/ & \quad 10000 / \\
\text{sol1} = & \left| Q(t) \right| = \frac{\text{10000}}{1 + \frac{397}{3} \exp(-10000 k t)} \bigg| \\
\end{align*}
\]

Substitute the value for acreage planted, \( Q(t) \), from the second census, and solve for the growth rate \( k \). It should be easy to find this combination of values, \( Q(t)=1000 \) at time \( t=30 \), on the plot below.

\[
\begin{align*}
10000 \\
1000 = \frac{\text{10000}}{1 + \frac{397}{3} \exp(-300000 k)} \\
\end{align*}
\]

Now we are prepared to solve for the growth rate:
Simplifying:

\[-5k := 0.896031382 \times 10^{-5}\]

Now let's take another look at results:

\[
\frac{10000}{1 + \frac{397}{3} \exp\left(-0.0896031382 t\right)}
\]

Isolate the righthand expression, \(rh1\):

\[
\frac{10000}{1 + \frac{397}{3} \exp\left(-0.0896031382 t\right)}
\]

Figure 1 shows \(rh1\) as it changes over time, revealing the essential form of this particular diffusion process. The plot shows the anticipated growth trajectory. If time is given in weeks, acreage allocated to this crop reaches its maximum in about three years.

If one were to ask what the logistic curve would project regarding the time when, say, 3,000 acres will have been planted in the new crop, we see that this event would occur at about the forty-fifth week.

This simulation has several advantages. First, it is based on a differential equation that necessitates empirical data at two census points. Second, the logistic formula certainly would conform to the data in many instances; when it does not, we know the relevant questions pertaining to \(L\), or to the census data, or to the initial presumption of a logistic model. Finally, a given logistic function, selected from an infinitude of possibilities, is highly parsimonious—unless, of course, we combine several such functions, summing their derivatives as a way of capturing the growth dynamics of several related situations, as in the case of a crop diffusion process that may differ among different classes of cultivators. Here, the need for taxonomic sophistication becomes paramount. Coleman, Katz, and Menzel (1966) might suggest that crop diffusion would be influenced by friendship ties among
farmers, by their rates of geographical and social mobility, by demographic factors such as age, etc.

Anton and Kolman (1982: Chapter 15) use logistic functions to develop simulations of depreciation processes, marginal analysis in economics, the effects of new drugs in the treatment of disease, various processes involving plant and animal ecology, the spread of epidemics, the effects of advertising, the spread of rumor, and the dynamics of learning processes. Similar applications are found in Dowling (1990). Each of these applications raises large issues pertaining to human ecology, i.e., the interaction of demographics, social organization, the environment, and technology. For instance, one could ask whether declining marginal utility is a major growth inhibitor in our example of diffusion of a new type of crop, whether climate has an appreciable impact on the distribution of feedlots, whether the use of various combinations of drug therapies for AIDS patients diffuses according to a discernable function, whether advertising and mass media campaigns have had an impact on the practice of safe sex or on demand for abortions, whether the new food grains of the Green Revolution have permitted the development of natural enemies that depreciate the value of these grains, etc.

If one were to experiment with the structural features of the crop-diffusion simulation—perhaps by changing the census inputs—one would realize that the infinite number of logistic curves always have certain properties: For instance, they are always s-shaped, and they always have their most rapid acceleration at the time when the growth process has reached half the distance to $L$. In a social diffusion process, the halfway point has distinctive features: It is the point where the adopters of, say, a new technology may have a maximum impact in stimulating the remaining non-adopters to acquire the technology, i.e., the product $0.5 \times 0.5$ is substantially larger than the earlier $0.2 \times 0.8$ and somewhat larger than the later $0.7 \times 0.3$, etc. If, on the other hand, little interaction occurs between adopters and non-adopters, then the logistic curve is likely to be inappropriate and a linear diffusion process is more likely to occur. In our own research, we wish to determine whether the diffusion of home personal computers (Bureau of the Census, 1991) is logistic or linear. If the former, one would surmise that adopters stimulate non-adopters to purchase PC’s. If the process is linear, it is more likely that such interactions do not occur and that new PC purchases result from advertising, price changes, etc. In this same project, I’ll also focus on diffusion of use patterns for home PC’s, comparing boys and girls (or other taxonomic categories) as to their readiness to adopt and use programs such as spreadsheets. One must remember, then, that diffusion curves, whether logistic in form or not, vary substantially according to time and space. And if it is true that all POET entities were at one time innovations, it follows that all such entities underwent some sort of diffusion process that we may be able to discern.
Models of the diffusion of the HIV, for instance, would be especially fascinating and illuminating.

Finally, experimentation will show that a small change in the census data—say, 700 acres planted after thirty weeks rather than 1,000 acres—would lead us to project a substantial delay in the arrival of the time when 3,000 acres have been planted in the new crop: a small cause greatly amplified.

In any given instance the logistic growth model, despite all its faults, foibles, and frailties, may be the best theoretical formulation available. In some cases—e.g., those in which state legislatures grapple with scores of simulations of new re-districting schemes—we are forced by the politics of the situation to make choices that go beyond merely doing nothing, and we must make these choices by means of the best available methods and the best available information. Faultiness and folly may pervade our more powerful theoretical models, but these impediments must be mastered, or at least tolerated.

(2.4) Taxonomic checklists and the method of comparative narratives

Ragin’s work (1987; 1994) on comparative methods implies a central role for formal taxonomies in the development of social theory.

In one of Ragin’s many examples (1987:43) dealing with peasant revolts, we encounter a simple taxonomy that evolves in order to accommodate new findings. In so doing, it exemplifies the logic of the example above involving health professionals. Ragin’s procedures also recall the evolving taxonomies of analytical induction. As Ragin continues his presentation, in fact, one realizes that he often echoes the basic strategies established many years ago by analytical inductionists. “Rather than conclude that there are no invariant relationships,” he says, “the investigator may suspect that there are different types of national revolts and that different sets of causes are relevant to each type.” This is precisely the language by which analytical induction developed and deployed its various taxonomic systems and research hypotheses. Cressey (1971), for instance, would have continued to sound like the quintessential analytical inductionist if he had used exactly these same phrases to summarize one of his own noteworthy studies: “Rather than conclude that there are no invariant relationships, the investigator may suspect that there are different types of embezzlement and that different sets of causes are relevant to each type.” The paraphrase would also have served in the case of Lindesmith’s (1957) research on opiate addiction.

Ragin (1987:47-49) next develops ideas about ways in which adequate taxonomies would help us find contextual effects, i.e., instances in which a causal relationship exists only within certain social, demographic, or ecological (i.e., POET) categories that ideally would
be identified by means of a working taxonomy subject to revision due to new findings. He cites research by Allford (1963), for instance, on the relationship between social class and support for political parties, pointing out that this research focussed only on “... English-speaking democracies with single-member, simple-plurality electoral systems ... because the interpretation of the relationship between social class and party support is different in electoral systems that use proportional representation.” In other words, findings from previous studies have highlighted a taxonomic category in which the politics of social class take on certain properties.

Finally, one finds an edifying discussion in which Ragin (1987:169) claims that in boolean analysis, the central topic of his book, causation must always be a fundamental concern. Yet, this was not a necessary step in Ragin’s evaluation of Stapleton et al., whose research originally (and in the hands of Ragin) had to do with taxonomies of social organizations and led to an elaborate taxonomy of juvenile courts in the U.S. This taxonomy does indeed evolve further through application of Ragin’s boolean analysis, but in this instance the boolean analysis was not at all concerned with causal hypotheses. The focus was entirely taxonomic, for reasons that were entirely convincing.

Victory will be at hand when a book such as Chan and Pollard (1988) sees itself as dealing with elaborate, detailed, and evolving taxonomies, rather than as a mere collection of thesauri.

(2.5) The social contexts of chaos

(2.5.1) Basic orientations

The essential ideas of chaos theory are not entirely new. They have a long, involved, and illustrious history. Over the last two hundred years or so the most elaborate expressions of chaos concepts have been set forth in the writings of geologists and other naturalists who argued, especially during the early nineteenth century, in support of the “catastrophist” point of view as the capital achievement of their fledgling discipline. Early catastrophists in geology had the pre-Darwinian burden of challenging creationist constructs of a divine stability capable of giving way only to inexorable decline and fall. More recently, chaos concepts appear in the Marxian and (therefore) catastrophist philosophy of science advocated by J.B.S. Haldane (1969), and in the writings and therapies of the Gestaltist school of psychology. But in its intellectual history, first and foremost, catastrophism pervades the works of Friedrich Engels, especially those completed more or less independently of Marx and primarily after the latter’s death (Engels, [1972]). These concepts, hypotheses, and
theories have had their ups and downs: For instance, the recent revival of plate tectonics in geology has been in many ways a revitalization of a form of catastrophism that had fallen much earlier into disfavor (Stewart, 1990).

A few excerpts from these several sources will capture the essential styles and strategies of catastrophic or chaotic thought.

Medio (1992:4) gives a definition of chaos that leads to fascinating and fundamental philosophical questions. Chaos, he says, is “... stochastic behavior occurring in a deterministic system.” His definition raises a highly demanding traditional issue: whether all changes in nature could be shown to be deterministic if we but knew all positions and motions of all particles at a given starting time. On this question, philosophers, physicists, and others who have examined it tend to divide into two camps, which we may as well call the deterministic and the stochastic. Perfect determinism, although plausible and intellectually comfortable, does not appear to exist, primarily because whenever we try to demonstrate the presence of it something goes wrong either with the starting-point data or with the physics or, apparently, with the deductive processes. Alas, nature manages to come up with a host of surprises, and we end up labelling these as stochastic because we cannot fully explain them.

To me, one of the most compelling contributions to this dispute comes from avant-garde computer scientists who say that, when they create massive computers with high capabilities for parallel processing of hugely complex problems, the resultant machines act more and more like human beings, with a comparable, sometimes perverse intelligence: Among other fascinating behaviors, they present surprises, and they seem to have an aptitude for practical jokes. They present field-and-ground trickery reminiscent of the old story about floppy-eared rabbits (Barber and Fox, 1958) in which we learn that, if one does not watch carefully all parts of one’s machinery (rabbits, computers, quarks, crowds, etc.), the most fascinating surprises—earth-shattering discoveries, in the words of chaos—are likely to be missed. In its short history, the modern version of chaos theory has produced several instances in which anomalous findings were not missed. That of Lorenz (1993), for instance, an atmospheric scientist whose free-wheeling computer, after carrying out a stunningly long weather simulation, caused him to have major insights into ways in which tiny atmospheric events may have a major impact on big weather systems developing much later on. For want of a nail, a kingdom was lost; for want of light hail, Miami was tossed.

And the light hail could have been missing weeks or months ago in Madagascar.

Lorenz’s computer produced extraordinary results partly because it introduced rounding error into its calculations, and those who find the philosophical implications of his research to be convincing must have an a priori commitment to the idea that nature has at least the randomness of rounding errors, which in lengthy calculations such as Lorenz’s do
indeed produce big surprises. The elegant, state-of-the-arts, carefully engineered roulette wheel (of sorts) created some years ago to produce random numbers for the Rand Corporation would occasionally slip into a risky non-randomness, and it is arguable that these lapses occurred because of variations no larger than the tiniest rounding errors. But the mathematical program package called Maple, or any one of its several competitors, readily simulates the deterministic side of the argument (Char et al., 1992:21): “Maple’s primary mode of operation uses ‘exact numbers’—integers and rational numbers of any magnitude.” These numbers are different from the decimal expressions used by calculators and by standard programming languages such as Fortran, Pascal, or C. Decimal expressions have the advantage of using less memory, but “Maple’s results with exact numbers are free of rounding error that is typically unavoidable with fixed-precision floating point numbers.” Maple insists on calling a third a third, i.e. 1/3, and it will not introduce the distortions of .33333333 unless it is told to do so. Whether it could carry out Lorenz’s weather analyses in a reasonable amount of time, we do not know.

(2.5.2) Dollar.bas and boulangeries

We have produced designs so complicated that we cannot anticipate all the possible interactions of the inevitable failures ...

—(Perrow, 1984:11)

The same, of course, is true of successes.¹⁹

From time to time we use a fascinating program, written in BASIC, in order to test the speed of various computers. Called dollar.bas, the program determines the number of ways in which one could gather together one dollar, or perhaps more than one dollar, in coin. It begins by telling the computer that a dollar in coin must contain 0 through 100 pennies, and 0 through 20 nickels, and 0 through 10 dimes, and 0 through 4 quarters, and 0 through 2 half-dollars; these ranges are expanded for larger amounts and faster computers. It then tells the computer to examine each combination of the various types and numbers of coins, to add up the value of each combination, and to count all instances in which the value of a given combination equals one dollar or whatever larger amount may have been selected.

In order to find the 292 correct or successful instances, i.e., those combinations of coin that amount to one dollar, a computer must loop through and evaluate 101 * 21 * 11 * 5 * 3 = 349,965 equations representing each logically possible combination of coins; double that number if we allow silver dollars. For some computers, this task requires a substantial amount of time. Time requirements, then, may become a major consideration when we work
with complex inductive/deductive models, even an undemanding one such as dollar.bas. In real life, of course, this is not the way things happen because this is not the way we count out change; for one thing, we usually do not have the time to go through all the “combinatorics of social structure.” In a sense we use a theory, just as we ought to do when we design, say, traffic-control systems. First, we use a taxonomy that sorts available coins on the basis of value, usually placing them in separate piles inside a cash register. Then, we follow an algorithm that tells us to add coins together from largest to smallest until we have arrived at one dollar, being careful not to exceed one dollar. Usually this task can be accomplished by an efficient store clerk in less than the 20 seconds required by a Prime 9955 to run dollar.bas, but if the coins are incorrectly classified or if there is a shortage of the larger denominations or too few coins altogether, a store clerk will quickly get into the drudgery of PC time with a much higher error rate. In other words, if the ancient theory of efficient coin selection is not implemented, the strategy will not work effectively.

In dollar.bas, we might think of the five terms of the main equation as referring to each of a set of five structural features of social organizations or ecological systems, and we might think of the 292 correct instances as types of organizations or ecological entities that have relatively high survival prospects, while the remaining 349,673 (349,965 - 292) do not. Again paraphrasing Dawkins (1986:9): No matter how many ways there are of making a dollar, there are vastly more ways of making a non-dollar. Now the realities of social experimentation are such that we could not possibly work through the entire list of 349,965 organizational types in order to evaluate each of them for survivability, and it is at this juncture that we must find an algorithm, that we must know something about a priori prospects. If we had this knowledge, we might design and run actual social experiments; more likely, we would run simulations. If we became very good at this sort of activity, we could do things like preventing airplane accidents—in this instance, clearly, the challenge is to find, by means of comparative narratives and various theoretical devices, lethal combinations having low probability.

This brings us to the question of how best to design a boulangerie. In Montpellier, a medium-sized city located in the south of France, there is a neighborhood called the Quartier des Arceaux, centered on a major tourist attraction consisting of a large, ancient aqueduct surrounded by generally large commercial establishments and high-status residential areas. An adjacent neighborhood to the east, called Gambetta, has several major government buildings, a variety of commercial establishments, a large population whose antecedents are North African and West African, and several working-class residential districts. A third contiguous area, Figuerolles, is a working-class neighborhood with many small commercial establishments, a few government buildings, and a military training base; it is crosscut by
several high-speed auto routes. One of the more fascinating features of this environment is that, if one walks through any one of these three neighborhoods, one is likely to encounter several boulangeries of various sizes, shapes, and business philosophies. I would guess that, within a radius of a mile from the geographical center of the entire area there are probably a hundred boulangeries. Some of these establishments seem relatively new, and many seem very marginal with little prospect of survival; inevitably, one begins to speculate about why some boulangeries are more successful than others.

A merchant, or potential merchant, who wishes to establish a boulangerie must engage in a complex search protocol. He must answer many questions bearing on the survival prospects of his contemplated venture. In France, especially if the potential merchant is currently unemployed, there is a wide variety of government subsidies with which he should become familiar; he should probably take advantage of most of them, because there is evidence indicating that they raise one’s prospects of success. In carrying out these tasks he almost certainly will encounter, at some level of consciousness, a number of options far larger—although it is hard to believe—than the 349,965 equations of the dollar. Furthermore, evaluating each of the many options that make up the larger business plan will prove to be far more difficult than merely evaluating an equation with five clearly defined multiplicative terms, which often seems tough enough.

If an aspiring boulangère were asked to present a business plan, she probably would have to spell out intentions in at least the following realms:

- size and physical structure of the establishment
- neighborhood location
- source of products offered
- range of products offered
- quality of products offered
- prices
- personnel, working hours and conditions, etc.
- wages
- advertising

There is an indefinitely large number of options under each of these rubrics; these many options make possible an infinitude of ways in which one might design one’s proposed boulangerie. Given that the possible number of designs is indefinitely large, how does one decide upon a given business plan? First of all, one copies. Second, one uses “theories”
ranging from folk wisdom to educated guesses to the latest word from economics. Third, one hopes for good luck.

If one copies intelligently and uses a more or less valid set of theories, one’s efforts may explode into a chaotic outcome, a level of success perhaps without precedent: One hits not only a dollar in coin, one strikes a fortune. But an abject concession to good luck means that one does not see this decisionmaking process as a deterministic one, comparable to dollar.bas, and one discerns many ways of missing the goal. We see this process, almost inescapably, as an instance of “... stochastic behavior occurring in a deterministic system.” A large part of sociology, we believe, should devote itself to assessing the chaotic aspects of social entities comparable in their complexity to the average boulangerie. And we should always try to discern deterministic relationships that underlie and explain the various surprises we call stochastic.

(2.5.3) Hazardous high-tech

Perrow’s Normal Accidents (1984) appeared a few years before chaos theory became widely known, but virtually every disaster, actual or potential, described in this work involves a process of chaos, an extremely dangerous series of surprises that develops rapidly due to the interaction of several events that have never before occurred together, like those that make a given boulangerie an extraordinary success. The most rapid and dangerous development occurs when the several events that create a disaster have a property called “tight coupling” (Perrow, 1984:89-90), i.e., they interact closely so that causal processes redound immediately throughout the system, and even a small problem in one place quickly creates larger problems in a contiguous place. Consider a simple illustration: If many human bodies, pieces of luggage, food trays, and other paraphernalia already have become airborne inside a large aircraft that is wildly out of control, as in a recent Beijing-to-Los Angeles flight cited earlier, it is almost impossible even to begin a process of securing things or helping badly injured passengers; flight attendants, despite their admittedly minimal training, cannot secure anything while they and others in the cabin undergo constant bombardment by all sorts of heavy or hard-edged objects. A more subtle example: A few years ago an airliner crashed into a mountain because the pilot, very long of the torso, could not see the top of the number 7 on a digital display indicating the frequency of a nearby navigational radio transmitter that he wished to use to establish a course. Instead of tuning in, say, to 108.1, he had tuned in to 108.7, and this tiny error took him way off course.20 Perrow’s disasters, all involving modern high technology of high complexity such as nuclear power plants or Boeing 747’s, appear to
be stochastic, not deterministic: They have the unpredictability of a newly established boulangerie, not the easy predictability and no-rounding replicability of dollar.bas.

Toward the end of his book Perrow (1984: Chapter 9) presents a lengthy discussion in which, in essence, he tries to diagnose the major illnesses of a given type of high-tech system. For some of these systems, such as nuclear power plants, he regards the sudden maladies—perhaps wrongly—as being inherently incurable; in other cases, such as civil aviation, he believes that effective prevention, diagnosis, and treatment have already been implemented. Given the notion that disaster specialists should try to diagnose recurring (and therefore somewhat less surprising) disaster scenarios and to find treatments for them, we are led to the speculation that expert systems and social simulations should implement chaos models of disaster and then train us to be effective medical practitioners. The most impressive applications of ES’s and (arguably) simulation programs, after all, occur in the medical realm.

Indeed, there are many social analogies for the medical ES’s. For instance, Perrow discusses a recurring type of accident in which large cargo ships, traveling in opposite directions through narrow ship channels, suddenly turn toward each other and end up having a serious collision. These collisions often occur even after captains have agreed, through radio communication, on a procedure for passing one another. Aviators, encountering analogous situations, may know at least one of the sources of this particular affliction; ES’s and simulators that experiment with various treatments should be able to show us a way out. What occurs, in all likelihood, is this: If two aircraft (or ships) approach each other at an angle that is not far from ninety degrees (note: simulations would show how far) and if they appear to have entered a collision course, right-of-way rules (codified in the federal aviation regulations) provide that the aircraft to the other’s right, which we’ll call A, is allowed to proceed. The other aircraft, called B, must give way. (A is likely to have red lights showing on its left wingtip.) At this instant the pilot of B may have a problem: He must decide which way to turn. At first, he may have an inclination to turn left, but if he does so he may end up flying more or less parallel to A and too close, and this is not desirable. Therefore the pilot of B decides to turn right. The pilot of A may have become anxious during these moments because she notices indecisiveness and hesitancy on the part of the pilot of B. The pilot of A decides that she cannot trust B to yield the right of way; she then decides to make a turn herself. But she now encounters exactly the same problem that soon induces the pilot of B to turn right: If she turns right, she believes that she will end up flying alongside B, and she does not consider the pilot of B to be reliable. If she turns left she avoids this difficulty. When B turns right and A turns left, and they do so simultaneously because of the subtle miscues and misperceptions just described, a collision may result. (Pilots like to say that it is
better to be far from one’s destination and headed in the right direction, than close to one’s
destination and headed in the wrong direction. In the case at hand, the pilots are close and
the direction is wrong.)

In simulating and experimenting with this process, one should notice that it has all the
earmarks of an “escape panic” arising paradoxically out of rational calculation (Coleman,
several diagrams that seem to show precisely the interaction dynamic just described, although
he does not discuss the social dynamics of it in detail, nor does he touch on chaotic aspects
of it, nor on modes of treatment.

(2.5.4) Dumping trash, dumping computers, and dumping Beauregard

What was it that moved [Confederate General] Johnston? One careful student ... has
interpreted Johnston’s determination to fight, whatever the odds, as possibly the
decision of a desperate man attempting to regain his image as a successful
commander. [A] biographer ... has suggested that Johnston may have finally gotten
enough of Beauregard’s tendency to dictate ... It is interesting that neither of these
writers interprets Johnston’s decision to fight as a rational judgment ...
—James L. McDonough (1977:83)

Sudden, chaotic transformations such as diseases, accidents, or spectacularly
successful boulangeries appear to be qualitative, binary, either-or. Oftentimes these
transformations result from the impact, perhaps delayed, of small causal agents such as the
simple misperceptions and misapprehensions occurring in the example of aircraft or ship
collisions. In the following Minitab simulation we have an instance in which a manufacturer,
over a period of many weeks, dumps a pollutant into an environment with a limited capacity
for biodegradation of the pollutant. The problem is that a sudden, chaotic transformation of
the environment is likely to occur when the dumping rate exceeds the biodegradation rate.
Complexities arise due to the fact that the dumping rate is a nonlinear function of time, a
surmise that seems highly realistic although in a given instance we may find it difficult to
write an accurate equation for the dumping function. The logistic curve does not seem
plausible in this instance, because even though there is an environmental limit (the
biodegradation rate), there is little reason to suppose that this limit would produce negative
feedback as the dumping rate approached it. This sort of feedback is an essential dimension
of the logistic curve; in the present case, it might be a good idea to introduce it.
We examine two simulations, with only the first shown in detail. The second differs from the first in that the volume of material dumped will be reduced by exactly one half; however, the effects of this change are magnified—a common chaotic occurrence—and the environmental impact is far more benign than one might suppose.

In these simulations the constant $k_1$, supplied (along with $k_2$) by the experimeter, appears in the equation that defines $c_2$; the latter is named 'dumped' and gives the amount of waste dumped at each point in time. The dumping-rate constant for the first simulation is set at 4. The problem is to ascertain the time—a crossing point—when the rate of increase of dumping exceeds the rate at which the environment biodegrades this particular pollutant; the biodegradation rate is estimated at 30 units per week. In the first simulation the crossing point occurs at 25 weeks, at a time when 500 units are being dumped; presumably, previous dumping has been absorbed through biodegradation. The pollutant will now start to accumulate, and may interfere with the absorption process itself—an important system transformation. This simulation assumes that the biodegradation process will remain constant at 30 units per week, at least until the crossing point has arrived; this assumption may be unrealistic. Again, input commands are set in Times New Roman.

MTB > # set $k_1$=the constant for the dumping rate, e.g., 4
MTB > # set $k_2$=exponent for dumping rate, e.g., 3/2 or 1.5
MTB > # raise $k_2$ to 1.6 or 1.7, and see dramatic results
MTB >
MTB > set c1
DATA> 0:150
DATA> end
MTB > name c1 't'
MTB >
MTB > let c2=$k_1$\cdot('t'**($k_2$)) # amount of waste dumped
MTB > name c2 'dumped'
MTB >
MTB > let c3=$k_1$\cdot($k_2$)*('t'**($k_2-1$)) # derivative of 'dumped'
MTB > name c3 'deriv'
MTB >
MTB > print c1-c3

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<td>6840.13</td>
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<tr>
<td>31</td>
<td>6912.00</td>
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<td>32</td>
<td>6984.12</td>
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<td>33</td>
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<td>7201.99</td>
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<td>36</td>
<td>7275.11</td>
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<tr>
<td>37</td>
<td>7348.47</td>
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</table>

The amount of pollutant dumped each week increases according to the following curve:

```plaintext
MTB > plot 'dumped' 't'
```
The dumping rate increases each week, as follows:

MTB > plot 'deriv' 't'

MTB > print k1 k2
K1 4.00000
K2 1.50000

MTB > stop

A second experiment shows that if the constant k1 were reduced from 4 to 2, thereby reducing by half the weekly amount of dumping, 100 weeks would be required for the rate of increase of dumping to exceed 30 units per week, and by that time 2000 units per week could
be dumped. Clearly, the payoff for a linear reduction of the amount of dumping would be considerably magnified. And one assumes that, given the generally more benign conditions of the second simulation, it would be much easier for management to reduce the rate of increase of dumping to zero after 100 weeks had elapsed.

Searching by means of the POET paradigm, one finds many instances in which small causes produce highly magnified effects; we cannot accept the claim that the social sciences have tended to neglect these instances (SELREF, 1993: Chapter 4). Crumley and Marquardt (1987: Chapter 6), for example, suggest that during the last thousand years within the Bourgogne region of France, apparently minuscule changes of climate have produced a complex series of sudden crop switches. Kolman, Anton, and Averbach (1992: Chapter 4) show by means of simplex analysis that tiny changes of market conditions often lead manufacturers to seek “corner solutions” in which they undertake sudden, complete switches of their product lines and may decide to produce only a single commodity, at least for a time. In counterfactual historical analysis, major outcomes often appear to have resulted from small, chance events: In Fogel’s (1964) classic study of transportation in the U.S., it appears that there were no highly compelling reasons for relying primarily on railroads rather than a carefully designed combination of roadways and waterways; in many ways, the decision was essentially a coin flip. Many roadways and waterways were already in place, and a stronger commitment to them would have had an Ogburnian effect in giving impetus, say, to the development of small, efficient internal-combustion engines (SELREF, 1993:184). Similarly McDonough (1977:218-25), writing about the early years of the U.S. Civil War, argues that “at the Battle of Shiloh ... the possibilities for a totally different outcome were so real that they bear repeating. The Rebels were in fact so close to victory ... that one marvels that the Union army was able to escape disaster.” And he strongly suggests that if several small conditions had been slightly different—say, the personal tensions between Confederate Generals Johnston and Beauregard, or the tactical inflexibility and lack of imagination of the Confederate command, especially Beauregard—a major Union army would almost certainly have been obliterated at Shiloh and the entire course of the war could have been changed radically.

Finally, a more detailed illustration, using simulations: Thesen and Travis (1992:276-93) provide a case study in which a large firm makes decisions about the purchase of a computer system that serves many users. This example is highly sophisticated and shows a demanding and subtle application of simulation modelling. The problem was that the older system, on average, processed jobs too slowly, costing a large amount of money in employee overtime. There existed three possible solutions: upgrade the old system, buy a new system called B, or buy a more powerful (and more costly) new system called C, with the possibility
that the latter system might permit a slight reduction of the work force. (In this example there is much emphasis on profit-maximization—usually over the next five years—as the dependent variable, but one discerns occasional concern with, say, relocating employees displaced by automation.) Simulations of the three alternatives tend to favor option C, but the authors show by means of “sensitivity analysis”—a way of finding large effects from small causes—that a relaxation of certain assumptions will tend to shift results so as to favor option B. The most important assumption within the simulation favoring C was that the speed of computer processing would have no impact on the length of time required for employees to prepare data-processing jobs. The authors believe, however, that a faster processing time would indirectly reduce productivity by causing employees to use more time in preparing jobs—albeit perhaps more carefully. This latter speculation introduces an Ogburnian intervening variable that works in the same way as the internal-combustion engine in the example of waterways and roadways discussed above. In this case, however, it shifted the optimal decision from system C to system B, providing an excellent example of a binary, chaotic, Perrow-type switch resulting from the interaction of several POET variables. Other applications of sensitivity analysis for this problem did not have a similarly large impact, and did not reverse decisions. But once again, we see small causes having a large, qualitative impact.

Clearly, then, chaos theory places a strong emphasis on the qualitative, nominal dimensions of these holistic transformations, and often has a surprising hostility toward mathematical formulations (Briggs and Peat, 1989:83). But this attitude creates endless trouble, and we must learn, with John Stuart Mill, to combine the qualitative and the probabilistic and to think quantitatively about many aspects of chaos processes. Granted, the topology of chaos requires highly sophisticated mathematics that social scientists should attempt to understand; for the moment, however, we would argue that our most effective ways of combining the qualitative and the quantitative involve loglinear analysis, logistic regression, boolean comparative methods, and related techniques. In conclusion, then, we suggest that social scientists exploring chaos should make ample use of methods that model binary outcomes—e.g., loglinear analysis; that we develop an ability to discern those exquisitely complex causal processes, favored by comparative analysis, in which large configurations of causes bring about a binary effect, perhaps an event such as a serious accident or disaster; and that we also have an eye for the opposite sort of situation, in which a large causal configuration may contain several small, apparently simple causal agents that have a tripwire impact in bringing about highly magnified effects, often after a considerable delay. Nor should we overlook prospects for effective diagnosis and treatment, especially through use of expert systems and appropriate simulations. We are confident, for instance,
that almost all major aviation disasters, with their manifold POET dimensions, would be amenable to this sort of analysis. Research into “human factors ecology”—i.e., human factors research within a large POET context—would provide employment for large numbers of social researchers.
REFERENCES


1. Expert systems for statistics are already available. On the Internet (dean, n.d.), for instance, there is a program called Statcon, which suggests appropriate statistical methods for a given research problem (Andrews, n.d.). The excerpt below, from a recent conversation with Statcon, makes us slightly less apologetic about our own ES for theory. Replies to the ES’s queries follow the colons. The ES then makes a recommendation:

Is a distinction made between dependent and independent variables? (y/n): y
Is there more than one dependent variable? (y/n): y
Is there more than one independent variable? (y/n): y
Do you want to treat the relationships among the variables as additive? (y/n): n
Do you want to treat all the dependent variables as interval? (y/n): n

No appropriate test has been identified by the CONSULTANT. It is suggested that you consult an expert in the field to help define alternatives.

2. Our ES database makes reference to the POET acronym and the ADA acronym, which will be defined in detail in the next section. Provisionally, POET is an “ecological” model involving interactions among populations, organizations, environmental conditions, and technologies; ADA refers to the accumulation of natural innovations and human inventions and discoveries, the diffusion of them, and human adaptations to them. It sums up Ogburn’s (1964) approach to sociological analysis.

3. In this section and throughout this article, we discuss lists in great detail. It is noteworthy that many expert systems are written in LISP or Prolog, which perform essentially as list processors. They present checklists in order to obtain input, and they
process checklists in order to return diagnoses.

4. Ogburn, for instance, clearly believed that the most interesting research on ADA processes would involve technological innovations.

5. A statistical program such as Statcon should probably be incorporated into any effective theory consultant.

6. In the *Social science quarterly* for September, 1993, the first three articles deal with “discriminatory electoral practices, contextual effects, and a new double regression method for the courts.” Intensely methodological, these articles develop elaborate deductive models that tie together discrimination, micro- and macro-level social and demographic events, and regression methods that may elucidate these phenomena. Clearly, the articles present what may turn out to be a cybermodel.

7. If one were to transpose the ADA-by-POET diagram, it would appear as follows:

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<th>D</th>
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<tbody>
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The arrows serve to remind us that, in Ogburn’s view, the study of innovation, diffusion, and adaptation necessitates time-series data.

8. While each of these stages presents immense complexities, stage 3 may be
exceptionally demanding. In Lazarsfeld’s view (1958, 1967; cf. Bunge, 1997), the elaboration of causal models involves searches for additional antecedent and/or intervening variables. One must realize, however, that each new variable creates many new opportunities for interaction with other variables already included in a model. Given the fact that even a very small number of variables may be modeled causally in a large number of ways, the addition of several new variables will usually overwhelm us unless we invoke simplifying assumptions.

Simplifying assumptions sometimes turn out to be very subtle. For instance, Phillips (1979) argues that if detailed mass media reports of suicides tend to stimulate imitative suicides, then this relationship should be relatively strong in geographical areas that receive intense mass media coverage. He argues further that suicide that is preceded or accompanied by homicide tends to stimulate imitative homicide-suicide in the same areas of intense media coverage. The media, that is, act as an intervening variable or mechanism, and our taxonomies enable us to record which behaviors are highly imitative or replicative, and which are not. Bunge (1997) considers the clear specification of mechanisms, and testing of them, to be the central feature of all explanation. His equations 5a through 6 (Bunge 1997:430) sum up precisely what Lazarsfeld used to say about intervening variable analysis.

Similarly, Schwartz et al. (1994:389) argue that nongenetic family interaction is more powerful than genetic factors or shared environments in transmitting mental illness. If this form of interaction is indeed the mechanism or intervening variable, then filial illnesses should more or less replicate parental illnesses, and we should not observe merely a general tendency toward mental illness within the filial generation. It is not entirely clear, however, why social replications should be more exact than genetic replications.

Finally, the same sort of argument appears in a book by Dawkins (1986:99-100): If the 13- or 17-year cycles of cicadas evolved as an adaptation to predators, it is not surprising that these two numbers are primes. Cycles based on prime numbers become an intervening variable, and we find, perhaps, that predators have trouble adapting to such rhythmicity. Under the U.S. Constitution, the length of a Presidential term is not a prime (as it is in France), and the President is highly vulnerable to the two-year Congressional cycle. The six-year Senatorial cycle, on the other hand, appears to have a far smaller impact on Presidential power.

9. The *American college dictionary* defines cybernetics as “the scientific study of those
methods of control and communication which are common to living organisms and machines, esp. as applied to the analysis of the operations of machines such as computers.” Webster’s seventh new collegiate dictionary contains a similar definition, although it regards control systems as “automatic.”

If one understands “control” in a non-deterministic sense, these definitions capture the essence of the socio-ecological systems discussed throughout this work. When control occurs automatically, it almost certainly implies causal loops involving feedback, and all stage 4 social processes discussed herein have this feature.

In reading Coleman’s (1990:616-25) work on social theory, one realizes that “applied social research” in American sociology has evolved through something like the four stages just described. It is encouraging that, in discussing “the future direction of applied social research,” Coleman makes reference to the globalization of social research, its “... greater order of complexity ...,” and to the possibility that research at an international level will provide “feedback” for the international social system.


11. The “Nova” program called “The story of Lucy” claims that the ability to walk upright, not a larger brain, was a crucial factor in the early evolution of distinctive human behaviors.

12. The population dimension of POET subsumes an elaborate taxonomy concerning which we have a high degree of consensus: Formal demography studies changes in the size, distribution, and composition of (human) populations, and the components of such changes, which are always some combination of fertility, mortality, migration, and social mobility. The field of “population studies” deals with factors (from POET) that influence the vital processes and social mobility; and it also deals with the determinants and consequences (United Nations, 1953) of all seven demographic variables (size, distribution, composition, the vital processes, and social mobility).
Fertility itself is determined by the eleven factors comprising the standard Davis-Blake “intermediate” variables: the intercourse variables, the conception variables, and the gestation variables. There is no comparable taxonomic consensus for O, E, or T; the point, however, is to have a little imagination, asking an infinitude of questions such as: What has been the historical impact upon the intercourse variables of environmental innovations such as the HIV, and how have such impacts affected fertility? When such innovations diffuse, do they have comparable effects on fertility in various cultures? One also asks about, say, populations of organizations, populations of technologies such as snowmobiles among the Inuit, whether technologies create special opportunities for natural innovations as in the case of legionnaires’ disease, etc.

13. However, the starting values for this experiment differ considerably: One can change the length and weight of each pendulum, the initial position of each pendulum, the gravitational constant, etc. In my experience, the behavior of the pendula always has a replicable geometric structure. I don’t know of any strictly comparable situation in the social realm. The nearest examples, involving many social-science applications, would be the deterministic way in which Markov processes move toward their fixed points, or the deterministic way in which stable populations emerge under the Lotka conditions. In each instance, we have fixed geometries that emerge inexorably.

14. Markets that manage to sell all their commodities—i.e., efficient markets—survive because they have prices that seem reasonable to an equal number of suppliers and buyers. Efficient markets, in brief, occur where supply/demand curves intersect. Markets outside this intersection tend not to occur, primarily because they produce too many dissatisfied buyers and/or sellers. It is possible, however, to “analyze” non-occurring markets, as in the case where we calculate a buyers’ or producers’ surplus. There are many counterfactual markets: To paraphrase Richard Dawkins (1986:9), no matter how many ways there are of having a market, there are vastly more ways of having a non-market.

If we follow Dawkins’ thinking closely, we place ourselves at risk of creating apparent tautologies—but they are only apparent. Perhaps another biologist will help to clarify things (Ruibal, 1967:1):
The organisms of today are those that have succeeded in adapting and thus have survived the changes in their environment. The mechanisms that a biologist studies are *ipso facto* adaptations. Our study of these adaptations is merely our way of understanding the nature of the successful existence of organisms.

Ruibal appears to be close to asserting a tautology: That which survives has apparent survival value, and that which has survival value tends to survive. Social theorists often consider such tautologies unacceptable. We would agree, unless one is willing to do what biological functionalism does routinely: Compare surviving organisms against those that do not survive. In the case of non-existent markets, the only way in which one obtains supply/demand curves that cover such markets is by asking potential buyers or sellers why they would or would not participate in a given counterfactual market—and we are now on the fringes of “marketing research.” Or, we study other markets at other places and other times. Or we find records of dead markets, usually in old archives.

15. Assume that Abell meant to say “similar,” not “different.”

16. The POET acronym is also essentially synonymous with the PILOTS acronym presented by Bailey (1994:231). Several apparently unique features of PILOTS, such as level of information, level of living, and space, are characteristics of populations or organization. PILOTS, however, does not seem to give adequate attention to the natural environment. Alcorn (1997: Chapter 10) discusses several checklists—he calls them “systemic models”—including a “general categorical model,” the “PERSIA concept,” and an “anthropological model.” PERSIA is an acronym representing politics, economics, religion, social interaction, intellectual content, and aesthetic endeavors. From the human ecological point of view, PERSIA should be subsumed under the organizational rubrics of POET. Another checklist—one that tries to identify a specific element as the dependent variable—is the famous equation \( I = P \cdot A \cdot T \), due to Ehrlich (1995): It represents the environmental impact of population size multiplied by level of affluence multiplied by type of technology.
17. In virtually every instance in which we falsify a given hypothesis, we follow a pattern similar to that just described: The hypothesis arrives as a result of taxonomic analyses, and if the failure rate for similarly derived hypotheses is high, then we redesign the taxonomy. Jasso (1988), in apparent contrast, has a method whereby hypotheses flow from a mathematical *deus ex machina*, but one suspects that even in this instance the entire process could be expressed by means of natural language. Jasso, in fact, seems to agree with this claim in her discussion of natural languages, although she would insist that mathematical formulations, as a minimum, are more parsimonious than natural-language formulations. In a critique of Jasso’s paper, Turner (1989) claims that Jasso’s mathematics for deriving “predictions” is unclear, and that her method does not allow for falsification of such predictions. Turner may be right on the first claim; but regarding his second claim we believe, again, that a large number of false hypotheses, within Jasso’s framework, would explode verbal taxonomies (and the corresponding mathematical principles) in the usual way.

When they are doing their job, *taxonomy experts simulate chaotic narratives*; that is, they practice the sorts of changes just discussed, even when changes are not yet forced. (The italicized phrase above also provides a mnemonic for the topics of this paper.)

18. If, due to seasonality, some weeks have no planting activity, then adjustments must be made: Perhaps the data would consist of a moving average across only those weeks in which planting activity takes place.

19. Perrow’s theory of accidents is a “situational” theory, and this sort of theory has been used to explain all sorts of social phenomena—successes and failures, as in the present instance, but also crime, especially embezzlement, and a range of social problems such as divorce, drug use, mental illness, etc. Note, also, that Perrow (1984:75) presents an exercise involving combinatorial mathematics as applied to technological systems, and this exercise has much in common with the dollar.bas program discussed herein.

20. Several other causes, of course, interacted with this one. But this factor was a
necessary condition for the accident, as were several other factors. When a combination of causes is individually necessary and collectively sufficient for an accident, we have a Perrow-style interaction.
Theory and Method as Strategies of Search

Abstract

Modern technologies of electronic data processing may create highly significant, if not revolutionary, changes in the nature of social thought. High-speed computers with powerful software and complex network interconnections are essentially systems for conducting searches, and the essential nature of searches is the central focus of this paper. The paper argues that successful searches nowadays often lead to the creation of virtual realities (or simulations), that virtual realities lead to a renewed emphasis on classical experiments, and that classical experiments, having the forward-looking properties of virtual reality, flourish within an Ogburnian theoretical context.
No matter how many ways there are of being alive, there are vastly more ways of being dead.
—Richard Dawkins (1986, p. 9)

This paper continues a discussion that has to do with the strengths, weaknesses, and prospects of deterministic and stochastic representations of various social processes. A major thesis of the paper is that, first, due to the ubiquity of high-speed computers, social scientists are likely to become more heavily involved in the development of social simulations, perhaps virtual reality simulations such as those produced by the virtual reality modeling language (VRML); second, as they do so they will perceive the advantages of combining simulation research with traditional experimental research, thereby benefitting from the interaction of two powerful approaches to “forward modelling”; third, forward modelling will emphasize deterministic rather than stochastic processes, and will lead to a revival of the type of theory once associated with William F. Ogburn (1964). The latter prediction arises from the fact that Ogburnian theories begin with innovation; innovations, since they do not by definition have a history, support forward modelling. This evolutionary pattern already has occurred in other scientific disciplines, primarily because (1) stochastic models tend to become overwhelmingly complex and (2) simulations that insist on leaving relatively little to chance encourage scientists to identify clearly the gaps in knowledge, and to fill those gaps by developing greater theoretical acumen.

BLANK (1989) takes exception to my remarks (SELFREF, 1989b) about the importance of deterministic models, i.e., inductive/deductive processes that leave little to chance. “To me,” he says, “the presence of deterministic models in any social science discipline does not make the discipline superior to others with no deterministic models” (1989, p. 660). He then provides a brief explanation of the distinction between deterministic and non-deterministic (or stochastic) models:

Suppose the members of a homogeneous population of size N are exposed to risk, pi, of occurrence of a given event. A stochastic model for the number of events actually occurring in the population treats the number of events as a random variable with properties such as that its expected value is N*pi. A stochastic model for the outcome of an experiment in which a fair coin is tossed 10 times recognizes that the number of heads may be 0, 1, ..., or 10, each with a specified probability. A deterministic model for the outcome of the experiment would focus on the expected number of heads, namely 5. For this
reason, deterministic models are often referred to as expected-value models. In
demography, most of the population projection models are of this latter kind. I
consider it a healthy sign that recently demographers have begun to pay
increasing attention to the stochastic counterparts ... 

It is perhaps revealing that BLANK thinks of deterministic models as involving expected
values derived from stochastic distributions. In the present paper, expected values will have
*theoretical* origins and justifications. In any case, I feel compelled to point out, early on, that
I have not attempted any sort of survey to determine the prevalence of these two types of
deductive strategies in the various social science disciplines; nor, to my knowledge, has
BLANK. We seem to agree, however, that deterministic models have been readily available
in demography and human ecology, and that they are not so highly developed in other
disciplines. Our argument has to do primarily with the utility of these models wherever they
may occur.

In recent work of my own, based on Fogel’s (1964) research into the history of
railroads in the United States, I develop a spreadsheet program derived from several tables in
Fogel’s book. These tables contain likely outcomes, what could be considered plausible
“expected-value” data, analogous to the score of 5 for BLANK’s coin-tossing experiment. In
Fogel’s case, however, the data have to do with the prospects of substituting waterways for
railroads in the distribution of agricultural and manufactured products during the early
decades of the nineteenth century. For instance, the value of American farmlands as of 1890
(Fogel, 1964, p. 82) is a series of careful estimates based on an analysis of appropriate data;
it is not essential to Fogel’s argument that this set of estimates be moved through a large
number of values generated by a stochastic process. Similarly, estimates of the effects of new
canals on land values (Fogel, 1964, p. 99) are based on a plausible list of proposed (and
carefully designed) canals and their likely impact on the shipment of intraregional agricultural
products. If counterfactual history contains a cornucopia of plausible estimates, then surely
theoretical sociology must be similarly endowed.

(1) Constraining the Infinite

The most interesting features of the Fogel counterfactual analysis have to do with the
behavior of the model after various input data are taken to be fixed. What I imply, then, in
my argument with BLANK is that it is advantageous for a discipline to have complex
deductive models that will simulate reality by processing such input data, allowing only a
single outcome or a very narrow range of outcomes. In my work with Fogel’s data, for
instance, I have run experiments based on the Ogburnian hypothesis that expanded waterways would have given impetus to rapid construction of roadways, and that this latter change would have stimulated a more rapid development of the internal combustion engine, thereby sharply reducing the costs of transportation from farms to docks.\(^2\) I believe that demography and human ecology are blessed with a relatively large assortment of models in which deterministic processes predominate, as in Fogel’s work. The complete determination of the size, distribution, and composition of human populations by fertility, mortality, migration, and social mobility is a prime example, and it is an example that can be elaborated even further when we realize that fertility itself is completely determined by the eleven variables that comprise the widely used Davis-Blake (1956) list of “proximate” factors.\(^3\)

There is a classic SLAM traffic simulation (Pritsker, 1984, Table 6.34) that I should like to discuss in some detail, because it clarifies further the nature of the disagreement between BLANK and me. The traffic problem represented by this simulation is relatively simple: One lane of a two-lane road is closed because of a construction project, and the single remaining lane must accommodate traffic entering from either direction, with the two traffic streams allowed to alternate. Traffic lights are set up at both ends of the bottleneck. The red/green phases of the lights are set to a certain number of seconds, and they are timed in such a way that each light must be red while the other is green. There is also a phase in which both lights are red for a certain length of time in order to allow traffic to clear from the bottleneck before other cars enter from the opposite direction. The cars traveling in either direction are generated by stochastic processes that are likely to be dissimilar; at each end of the bottleneck, there is a certain probability per unit of time that another car will appear.

The essence of the problem is to set the timing of the red/green phases of the two lights (and the red/red period for both lights simultaneously) in such a way as to minimize the average waiting time for cars. This dependent variable is comparable to Fogel’s cost criterion for railroads versus waterways. Operating this simulation through 3600 seconds (one hour) requires only a few real-time seconds on a Prime 9955. In a typical run, average waiting time is about one minute for approximately 800 cars that enter the system over a hypothetical one-hour period.

Suppose, however, that in pursuit of verisimilitude we allow the model to grow in complexity as follows: (1) many additional roads, with something approaching a grid pattern, are introduced into the model; (2) many more traffic lights—perhaps scores of them—are added; (3) the processes generating the numbers of cars that enter the traffic pattern are made to vary by time of day, allowing for rush hours, times of minimal traffic, etc.; (4) qualitative or “chaotic” transformations such as gridlock, accidents, or stalled cars are allowed to occur; (5) realistic variations in the environment, such as hilly terrain or harsh
weather, are allowed to occur. A major consequence of these sorts of changes would be that the number of ways in which the program could perform—the number of potential simulation experiments—would become indefinitely large, and we would be compelled to seek ways of estimating many of these new dimensions accurately so that they could be set to a predetermined level.

Counts of the number of cars entering the system at various places and times would be a logical starting point. In addition, theories could be invoked about synchronization of traffic lights, about the ability of incentive lanes to attract high-occupancy vehicles, about ways of deterring frivolous travel, about the behavior of traffic in the presence of hills or bad weather, about ways in which gridlocks are resolved, about the strange traffic patterns that underlie “Braess’ paradox” (Liebman et al., 1986, p. 53), etc., and these theories, one hopes, would eliminate vast numbers of potential model settings.

The lessons learned from SLAM simulations can be illustrated in another way. From time to time I use a fascinating program, written in BASIC, in order to test the speed of the various computers with which I work. Called dollar.bas (and available to readers on request), the program determines how many ways there are of making a dollar in coin—for large machines the program can easily be expanded to larger amounts of coin. It begins by telling the computer that a dollar in coin must contain 0 through 100 pennies, and 0 through 20 nickels, and 0 through 10 dimes, and 0 through 4 quarters, and 0 through 2 half-dollars. It then tells the computer to examine each combination of the various types and numbers of coins, to add up the value of each combination, and to count all instances in which the value of a given combination is one dollar.

In running this program the computer must loop through and evaluate 101 * 21 * 11 * 5 * 3 = 349,965 equations, each with five terms, in order to find the 292 “correct” instances, i.e., those combinations that amount to one dollar. This is a deterministic task (no coins are flipped!), it is infinitesimally small compared to the expanded traffic program described above, and the answer is always the same. In real life, of course, this is not the way we count out change. In a sense we use a theory, just as we ought to do when we design traffic-control systems. In this instance, the theory eliminates a huge number of equations from the list of 349,965 logical possibilities. First, we use a taxonomy that sorts available coins on the basis of value, placing them in separate piles. Then, we follow an algorithm that tells us to add coins together from largest to smallest until we have arrived at one dollar, being careful not to exceed one dollar. Usually this task can be accomplished by an efficient store clerk in less than the 20 seconds required by, say, a Prime 9955 to run dollar.bas, but if the coins are incorrectly classified or if there is a shortage of the larger denominations or too few coins altogether, a store clerk will quickly get into the drudgery of PC time with a much higher
error rate. In other words, if the ancient theory of efficient coin selection is not implemented, the strategy will not work effectively.

Rules for counting coins have a lot in common with the rules of genetic algorithms. A genetic algorithm is a simulation that changes hypothetical chromosomes and thereby changes the adaptability of populations possessing them. These rules apply to lengthy strings of binary digits—the chromosomes—that generate high or low scores for survivability: There are rules for eliminating chromosomes (and the organisms or entities possessing them) by means of lethal and (therefore) reproductive selection, and there are rules for creating new chromosomes (and their carriers) by means of “crossover” and “mutation” (Michalewicz, 1994).

In dollar.bas, we might think of the five terms of the main equation as referring to each of a set of five structural features of social organizations or ecological systems—their chromosomes—and we might think of the 292 “correct” instances as types of organizations or ecological entities that have relatively high survival prospects, while the remaining 349,673 (349,965 - 292) do not—re-read the Dawkins epigram, which at least implies that social theories should regard vast numbers of social structures as untenable, e.g., markets with non-equilibrium prices. Now, the realities of social experimentation are such that we could not possibly work through the entire list of 349,965 organizational types in order to evaluate each of them for survivability, and it is at this juncture that we must find an algorithm, that we must know something about $a$ priori prospects. If we had this knowledge, we might design and run actual social experiments; more likely, we would run simulations. We would also be able to take advantage of methods such as linear programming, which introduce “constraints”—read theoretical constraints—that transform searchable sets of solutions from infinitude to a finite size. See the appendix for an illustration involving such a constraint.

The dollar.bas program, then, has the same interpretation as the SLAM traffic simulation. The SLAM simulation makes it clear that a truly complex traffic-control system along the lines of that now installed in Singapore would have to be based on plausibility considerations, on some sort of theoretical formulation that would eliminate the combinatorial explosions introduced by stochastic processes or introduced into any model by the general absence of algorithms based on theory. This kind of selectivity would move us toward the deterministic end of the deterministic-stochastic spectrum, and I believe that such a progression would be an important accomplishment for the social sciences.
In a sense, of course, BLANK is right: If 5 heads provide an interesting outcome, it might be worthwhile to see what would happen if the number of heads were allowed to vary between 0 and 10. My argument, in essence, is that we should not make this transition prematurely. I make this argument primarily because I believe that, in many instances, deterministic models require far more theoretical sophistication than their stochastic counterparts. Deterministic models often carry the implication that the fixed values have a special relationship to reality, that they replicate reality in some sense (Neelamkavil, 1987, ch. 4), that they have, as mathematicians like to say, isomorphism with reality. If one really believes that the properties of a Markov process (e.g., the transition probabilities) have a close connection with the realities of stability and change across many time periods, it is highly inspirational to watch one’s Markov chain arrive at its inherent “fixed points”—a deterministic outcome—as it moves into the future.

If we have good reason to believe that Fogel’s estimates of, say, farmland values were derived in a realistic way, why not say so and spell out, through deduction, the remote socioeconomic consequences of these estimates? If (to anticipate a later discussion) a highway engineer, circa 1925, had reason to believe that the elevations of a highway route across complex terrain should never exceed the alleged least-effort elevations of deer trails, why not say so, get about the construction tasks, and carry out the appropriate benefit/cost analyses? Similarly, if we have reason to believe that large categories of moves in chess can be more or less ruled out through a judicious and prudent appreciation of the game, why not say so, write the appropriate program, and make ready to challenge the grand masters and even Kasparov as current chess programs in fact are doing? In the latter instance, of course, an unwillingness to use one’s theoretical knowledge, an unwillingness to rule out implausible categories of moves, might lead one to turn loose one’s computer on a task that cannot be completed before the heat death of the solar system. In fact, there are many instances in which the combinatorial explosion rules out the use of complex stochastic models: There are far too many variations, and we do not have time to examine all of them. We must make deterministic choices, and we must try to develop theories sophisticated enough to enable us to make those choices.

There is a fascinating literature dealing with the problem of combinatorial cornucopias in various fields. Lenat (1983, p. 289), for instance, points out that

the earliest research on artificial intelligence quite naively but reasonably assumed that if you wanted to tell a program what to do, without telling it precisely how to do it, then you would have to employ some kind of random program generator, and follow it up with a test to see if the program was the
desired one. ... That is, computer scientists’ intuitions then were precisely in agreement with biologists’ today: the adequacy of random generate and test. Over the last twenty years, several painful research experiences have changed those computer science intuitions.

Lenat (1983, p. 293-98) develops the fascinating argument that the genetic material known as DNA may have evolved in such a way that it contains “heuristics” with information about “plausible changes.” If biologists could discover these alleged heuristics and state them in a natural language, they would constitute functionalist theories. More conventional biological theory, of course, regards genetic variation as a random process and natural selection as non-random (Dawkins, 1986, p. 41,312).

In a chapter dealing with classification, Foucault (1970, p. 140-41) shows a strong appreciation of combinatorial problems in the sciences, and in a classic work on conflict strategies (Schelling, 1963, p. 54-58) we have the uncomplicated and edifying example of how it is that people may use the theoretical paraphernalia of “tacit coordination” to find one another after being separated in a large department store. As it turns out, one can indeed find a needle in a haystack if one has a good theory about how needles behave when they have been inserted into haystacks under known circumstances. Furthermore, a sufficient number of blind men, studying elephants by means of appropriate instrumentation, high intelligence, and solid scientific procedure, could discover most of the things worth knowing about elephants. On the matter of finding needles in haystacks and defining large objects, carefully trained pilots know that it is a mistake to try to spot other aircraft by scanning the skies rapidly in broad sweeps; it is far better to scan small segments of the sky and to look toward places (e.g., standard airport approach or departure lanes) where one is likely to see another aircraft. That is, one should have a theory about how airplanes and their occupants behave. Recall that some of the earliest research on game theory had to do with ways of searching for German submarines during World War II—the problem was to apply pursuit algorithms that would defeat escape algorithms.

Over the last few years I’ve identified several instances, drawn in part from the realm of human ecology, in which it appears that largely random searches should be replaced, have been replaced, or should have been replaced by searches governed by theory:

(a) The search for sunken ships in the Eastern Mediterranean by maritime archaeologists.

(b) The search for a “magical frequency” and focal direction in radio scanning for intelligent life in outer space, or in scanning for comets and asteroids on a collision course with the earth.
(c) The search for an appropriate method of thawing frozen human embryos, in fertility clinics.

(d) The search for an effective version of the Shettles regime for controlling the sex of children.

(e) The search for a cure for syphilis; more recently, a cure for Brazilian purpuric fever.

(f) The process of rediscovering principles of chemistry through artificial intelligence.

(g) The search for effective medicinal plant and animal substances, by ethno-pharmacologists.

(h) In legal theory, the difficulty of proving a negative, which would require searching through infinite space and time.

(i) The search for a solution to the four-color problem in mathematics.

(j) The search for appropriate crosstabulations in Robert Textor’s cross-cultural summary.

(k) Using the GSS (General Social Survey) variable that codes astrological sign, picking up all the type I error, writing a book based on these “findings,” and getting rich.

(l) The IRS, searching for plausible targets for audits.

(m) R-SQUARE, once available in the Statistical Analysis System (SAS) as a way of searching for appropriate regression models.

(n) In commercial demographics, the search for individuals who have an unmet need or (induced) want for particular commodities.

I would have mentioned the increasingly active search for substances that permit relatively high-temperature superconductivity, except that for many years this search apparently was governed by a flawed theory regarding plausible substances: Ceramics with high superconductivity at low temperatures were once thought to be a poor prospect. Similarly, something like five years was required for the Virginia state legislature to reapportion its electoral districts on the basis of the 1980 census. This long delay was due to the fact that the legislature used an elaborate program for simulating a large number of reapportionment schemes, and the simulations were governed not by a general “normative” theory (other than the one-man/one-vote principle) but rather by the desire of individuals and/or coalitions to
gerrymander districts. The 1990 reapportionment requirements were implemented much more quickly.

In summary, I maintain that a study such as Fogel’s should not be done unless one has theories that produce plausible sets of counterfactual conditions.

(2) Theoretical demands and social simulations

My argument with BLANK can be illustrated in another way. In statistical hypothesis testing, we often choose between one-tailed and two-tailed tests. Two-tailed tests are analogous to BLANK’s example in which a fair coin is tossed 10 times and we observe the range of outcomes from 0 to 10 heads. In the case of one-tailed tests, however, we rule out a large set of potential outcomes of an experiment—half of them in the coin-flipping example—and we do so on the basis of theory. If, for instance, we wish to assess the prospects of deterring some form of deviance, and if the null hypothesis states that deterrence is inoperative in this case, our use of a one-tailed test would imply that we know *a priori*, from theory, that this particular form of deterrence is not at all likely to bring about an *increase* in the sort of deviance that it was designed to suppress. Whenever my students select a one-tailed test, it is always instructive to ask them how they justify the choice.

In fact, there are many instances in which statistical methods provide lessons about theory. When social scientists or scholars from other disciplines write regression equations, they place consequences or effects to the left of the equality sign and the various causes to the right; the presence of random stochastic disturbances is also captured by a rightward term, usually called an *e* term. On the other hand, when we draw graphics in order to represent causal hypotheses we ordinarily place the causal agent or agents toward the left side of the graphic, and the effects or consequences toward the right; and usually one finds a little box that contains an error term, *e*, for each effect. Despite these rhetorical inconsistencies and the grave limitations implied by the stochastic terms, regression models are a common way of explaining things in the social sciences, and they are heavily relied upon in other academic disciplines.

These inconsistent rhetorical practices cause confusion. When I talk about *forward* regression analysis (SELFREF, 1989a), I have in mind the graphic mode, with causation moving from left to right. *Reverse* models, which we typically use, work backwards through time from right to left, taking data about an effect and about its alleged causes and trying to determine whether each cause has had some discernable impact on the effect. The usual measure of this impact is called a regression coefficient. These coefficients almost invariably explain only a part of the observed variation in the effect, so that the coefficients, as I’ve
said, must be accompanied by an $e$ term that tells us something about the general relevance and efficacy of our list of causal agents.

In the case of forward regression processes, often used in simulations, we see a world that moves with time’s arrow—real time or virtual—from left to right rather than right to left: Instead of asking “What processes could have produced these results?” the simulations ask “What results would be produced if we were to set up such-and-such a set of processes?” Instead of asking How come?, we ask What if? We decide on the basis of the best available information what the important causes of a phenomenon (or several related phenomena) are likely to be, we write into a simulation program our understandings about these causes, and we do not necessarily leave anything to chance, i.e., we do not necessarily introduce an $e$ term.

In many instances these forward regression models, or simulations, are of such complexity that it would be extremely difficult, due to the combinatorial explosion of the list of possible causal patterns, to discern by means of reverse regression analysis how a given model works. This claim is readily proven: Using a programmable statistical package such as Minitab, generate a relatively complex, albeit commonplace, dataset with, say, nine or ten potential independent variables, a few of which will turn out to be irrelevant; square or take logarithms of a few of these terms in order to generate nonlinearities; create a few simple interactions; finally, allow this set of independent variables to produce scores on a dependent variable, and add a reasonable error term to the dependent variable. Then, ask the best statistician in your department to find the best specification for a regression analysis—that is, to find the model that generated the scores on your dependent variable. There are so many possible combinations of independent variables, nonlinearities, interactions, and error terms that the task of finding a good specification of the original model is likely to prove impossible. Here we encounter one of the major limitations of reverse models, and it is a major reason why they must contain a stochastic term.

SLAM simulations are typically forward processes, as are FIRM simulations, the Club of Rome world model (Forrester, 1971; Meadows et al., 1974), the various fertility models alluded to earlier, and the Fogel counterfactual analysis of the American railroads. Many elements of forward models, of course, may be (and should be) derived from earlier studies using reverse models, but a host of alternative sources of information, including sources outside the social science literature (Jasso, 1988, p. 9), may be invoked. An important part of the Fogel counterfactual analysis, for instance, was based on a perusal of engineering reports dealing with proposed waterways, on early studies of the feasibility of internal combustion engines, and so forth.
My favorite examples of the use of deterministic strategies in demography and human ecology are (1) the Club of Rome model, showing the development of a series of eco-catastrophes over the next century or so; (2) several excellent simulations that accompany the textbook by Bainbridge (1987); (3) Bongaarts and Potter’s (1983, p. 108-12) deterministic “decomposition procedure” that suggests, among other things, that the very processes of modernization that lead to greater use of contraception are also likely to lead to greater use of bottle feeding of infants, with the strong possibility that the latter practice, by raising the fecundity of mothers who no longer produce milk, will cancel any reductions in fertility; (4) a spreadsheet program designed to show what would happen to the sex ratio of newly born infants if there were a widespread practice of sex-preselection of children, brought about by new technologies; (5) a large part of the recent literature on “collective goods” (e.g., Heckathorn, 1996).

Many forward models, including several of those mentioned in the preceding paragraph, have been projected into the future, with results that are often erroneous. An important caveat for those who wish to make such projections is that one should never commit the rhetorical indiscretion of using the words “prediction” or even “forecast” when the appropriate term is projection. However, the most important way of falsifying the theory underlying a forward model is to show that the model’s projections rarely materialize, that its “postdictions” do not fit the historical record, or that its outcomes are better explained by some other set of hypotheses. But if these are the appropriate means of falsification, how do we test a counterfactual historical pattern that cannot be projected into the future and does not, by definition, fit the known past? In functionalist/systemic theory, in other words, how do we decide that alternative social structures—i.e., those allegedly eliminated by social selection—would not have worked well in terms of i-functions or s-functions (SELFREF) if they had been implemented? As we have seen, the counterfactual tradition of historical analysis, exemplified by the Fogel study, provides a way—perhaps not the only way—of answering this question. And, in general, we must be prepared to bring to this sort of research enterprise a large set of plausible inputs.

In conclusion, I paraphrase Dawkins: No matter how accurate a given regression specification may be, the corresponding simulation, informed by experimental research, is likely to be more accurate. Especially if the simulation has sufficient verisimilitude that it becomes an experiment.
(3) The killer’s in your coffee cup (and so is the floppy-eared rabbit)

As the infrastructure of the last century disintegrates ..., sociologists will work more closely with planners, architects, and engineers to develop new social infrastructure.
—Steele and Marshall (1996:4)

Ogburnian narratives are often safety, search, and rescue stories. We search for high-risk innovations within the technological realm, and perhaps also within the other three categories of the famous POET paradigm—the demographic, the organizational, and the natural. Furthermore, we search for innovations involving interactions among the POET categories. When we find dangerous interactions, especially of the sort identified by Perrow (1984), then the Ogburnian diffusion and adaptation processes are likely to involve safety measures and search-and-rescue operations. Ogburnian innovations, by definition, have no history, and it is therefore highly probable that we would study them by means of forward models—simulation, experimentation, and their interaction. The high-risk innovation known as RSA code, a technique for writing secret messages, is dangerous primarily because it involves “public encryption”—anybody can easily obtain the code for writing secret messages, perhaps to a bookie or a drug dealer—and because the code appears, so far, to be unbreakable. The FBI and NSA, believing that this innovation gives a huge advantage to their adversaries, search actively for ways of adapting to this perceived threat. The “clipper chip” was one of their (experimental) strategies.

The following narrative, describing a dangerous interaction, begins in my coffee cup. One morning a few weeks ago I was preparing to stir my café au lait with a necessarily hefty soup spoon. With the concave side of the spoon toward me, I slowly pushed it into the liquid, moving it slightly toward the far side of the cup. At first more or less subconsciously, and then consciously, I noticed that as the spoon was immersed in the liquid, vortices formed on either side of it. I soon realized that I was perhaps witnessing what aviators call wake turbulence, and I repeated the experiment several times in order to see whether the vortices behaved in the manner of vortices left in the wake of airplanes. They did. They rotated in the appropriate direction: Clockwise for vortices forming from the left edge (wingtip) of my spoon, and counterclockwise for those forming from the right edge; to me, these directions seem counterintuitive.7 When gravity pulls these wake vortices toward the ground, one assumes that the rotational direction, creating friction against the ground, would make them converge, collide, and perhaps cancel out one another. But in fact they move outward, away from each other, toward the edge of one’s coffee cup or, in the real world far from my café
au lait, toward an adjacent parallel runway. This behavior, too, appears to be counterintuitive.

Wake turbulence is deadly. The *Airman’s Information Manual* points out that the wake vortices of large aircraft “... can impose rolling moments exceeding the roll-control authority of the encountering aircraft.” In other words, if you are taking off or landing or even cruising behind a large aircraft, a vortex that is rotating, say, clockwise can roll your airplane to the right faster than you can roll it to the left. Until these forces were understood, they killed many pilots and passengers. And they have not been understood for many years, despite the fact that simulations similar to the coffee-cup phenomenon have surely been known for an indefinitely long period of time. The AIM, a little surprisingly, says that “initially” when pilots encountered wake vortices in flight, these were attributed to “prop wash.” It is hard to believe that wind-tunnel experiments (not to mention coffee-cup observations) would not have revealed this phenomenon early in the history of flight. In any case, wake vortices and their dangers should be subjected to an Ogburnian analysis in which one would study the process of discovery, the diffusion of information about the dangers, and the manner of adaptation to them. The same should be said of “microbursts,” a meteorological phenomenon that is also deadly, also counterintuitive, and only recently discovered, understood, and subjected to a range of adaptive efforts. Needless to say, this research agenda would emphasize simulation, experimentation, and their interaction.

(3.1) Pathfinders

What is the least-effort path for discovering dangers—factors that increase mortality and morbidity among humans or other organisms (e.g., the crazy-cow syndrome) or machines (e.g., computer viruses)—and the most effective ways of adapting to them? To answer this sort of question we need to think about paths in general, and then we need to think about specific paths, such as that of an asteroid or comet threatening to collide with the earth, or the paths whereby the NASA Spaceguard program would try to head off such a cataclysm (Harrois-Monin, 1996:35; NASA Ames ...).

NASA points out that “cosmic impacts are the only known natural disaster that could be avoided entirely by the appropriate application of space technology.” That is, there are no known paths for averting other types of natural disasters. Perhaps this inexorability is a major reason why natural disasters fascinate us, but one should nevertheless remember that the human species has succeeded magnificently in finding ways of averting death and morbidity, even in instances where the situation appeared hopeless. More broadly, as Moore (1972) has argued, we have had an endless series of jousts with the Four Horseman and their close
compatriot whose name is Repression, and these powerful foes, from time to time, do indeed finish a tournament on their collective backside. In Moore’s view, however, we have learned little about the paths to happiness: We know how to stave off misery much more effectively than we know how to achieve happiness.

Path selection has fascinating mathematical properties. Discussing partial derivatives, Hagle (1995:47) says “suppose \( f(x,y) \) is a function in two variables: We want to determine the rate of change for several points... [H]owever, it is not possible to calculate a single number that represents the rate of change at a particular point. This is because the rate of change of the function depends on the direction from which one approaches the base point.” Then he provides an illustration involving paths: “To illustrate the point, think of climbing to the summit of a steep mountain ridge. If you approach the summit along the ridge line the climb will not be too steep. If you approach the summit perpendicular to the ridge line the climb will be very steep.” Discussing essentially the same problem, Zipf (1949:2,6-7) says “... let the first example [of his book] be ... two towns, A and B, that are separated by an intervening mountain range,” and between which people wish to travel. For Zipf, the least-effort route may go over the top of the mountain, or it may involve a relatively tortured path that takes travelers around the mountain at a reasonable distance below the summit. Or, the communities may decide to dig a tunnel. “Here,” says Zipf, “we can see the enormous amount of work that could be saved ... by a tunnel of least distance through the base of the mountain; we can also see the enormous amount of work that it would take to construct such a tunnel ... When the probable cost in work of digging the tunnel is estimated to be less than the probable work of not having the tunnel, then ... the tunnel will be dug. The problem relates, therefore, to the probable amounts of work involved, as estimated by one or more persons. Naturally, these persons can have been mistaken ...”

The least-effort principle, then, is essentially a form of benefit/cost analysis. In the appendix of this paper, I have prepared an exercise involving a very simple, highly symmetrical mountain. The exercise shows why these analyses involve highly complex searches. Once we appreciate this complexity, we shall begin to understand some of the exotic algorithms used historically to find workable solutions, such as the practice whereby designers of the early U.S. highway system often relied on deer trails for least-effort solutions to highway route selection. As it turns out, deer may find least-effort (i.e., cost-minimizing) solutions as reliably as do our students when they trudge across campus, marking out various least-effort paths; and pathbuilders often follow student trails, for better or for worse.9 Once we have understood the complexities involved in trying to cross the simple mountain diagramed in the appendix, we shall be ready to appreciate Chandler’s (1979) effort to explain why least-effort solutions for railroad transportation were a
fundamental challenge for management, especially during the nineteenth century and especially in connection with selection of the best routes. We shall also understand, up to a point, how it is that a few airline pilots are able to assimilate huge amounts of information about weather in many locations, about air traffic along a given route, about the properties of a given aircraft, about situations prevailing at airports of origin and destination, and then routinely make flights that save, say, an average of $3000 in aviation fuel while also reducing flight time.

(3.2) Chandler, railroad experiments, and Liebling combinatorics

There are those who argue that we are losing our moral fiber because we no longer want to take risks with technologies. But ... the corporate and military risk-takers often turn out to be surprisingly risk-averse ... when it comes to risky social experiments. ... The risks that made our country great were not industrial risks ..., but social and political risks associated with democratic institutions, decentralized political structures, religious freedom and plurality, and universal suffrage.

—Perrow (1984:311)

University campuses provide many examples of what appear to be least-effort paths. The geometries of campus pathways become highly complex, especially on large campuses with many obstructions, uneven terrain, endless construction projects, etc. But campus logistics do not begin to approach the complexity of, say, the aviation route systems that cut across American or global airspace, the route systems of the inland and intercoastal waterways, the route systems established by U.S. railroads primarily during the nineteenth century, the elaborate networks created by the U.S. and Interstate highway systems or by the Internet, or the elaborate space/time zones created by the Greenwich longitude-latitude and mean-time system (Zerubavel, 1982). One should note, however, that there are strange anomalies when we compare these systems: For instance, one cannot ordinarily get anywhere on campus by walking a straight line or by taking great circle routes over symmetrical hills, but both options are available to aviators and ships’ captains. While railroads and highways, then, must absorb huge sunk costs before they can experiment with different ways of organizing trips, this is not the case for airways or sea lanes.

The sunk costs, however, did not discourage early railroad management from conducting elaborate experiments; many degrees of freedom remained for experimentation with this new technology, even after tracks were laid. Chandler (1979) has collected
documents that describe these experiments in detail, and the reader soon realizes that an extremely complex cybermodel such as a railroad system can only be understood by means of highly realistic experiments; simulations have limited utility, because so many variables, early in the research enterprise, are poorly understood. The same is true of aviation, despite the simplifications made possible by the greater flexibility of route selection. Constant weather and traffic variations by longitude, latitude, and altitude, among many other factors, make logistics extremely challenging for aviation, and the authors of aviation expert systems would grow wealthy if they fully understood (and could program, deterministically) the behavior of the aforementioned quintessential pilot who can quickly select a low-cost route from an indefinitely large set of potential routes; the process must be a little like playing Kasparov. The sort of experimentation advocated by Chandler, and the sort of expert system\(^\text{10}\) that such experimentation may make possible, should be a major preoccupation of human ecologists. In these instances, as Chandler realized, the engineering component does not overwhelm the sociological component any more than it does in the instance, say, of the IRS’ search for least-effort paths to lucrative tax audits, or Virginia Power’s search for effective ways to implement a two-hour rotating blackout strategy during times of excess energy demand. And many experimental projects fall indisputably within the realm of the social: for instance, commercial demographics, with the traditional emphasis on bootlegging theoretical issues into one’s research.

Chandler experiments are the ultimate simulation: They take place in real time and in natural social settings, they leave almost nothing to chance, they carefully measure virtually all crucial variables, they make definitive causal inferences, and they have a high degree of replicability; other simulations fall away from this ideal type for one reason or another. Further, the most impressive social-science theories probably occur in fields amenable to realistic experimentation: learning theory in psychology is a prime example. We constantly tell ourselves, however, that the realities of social research are such that it is often impossible to meet the demands of realistic experimentation. The setting is likely to be artificial, as in the use of aviation simulators instead of costly experimentation with real airplanes; it may be necessary to substitute virtual persons, generated by computers, for real persons; many of the variables introduced into simulations do not behave in predictable or understandable ways, and programmers must guess. In my estimation, while none of these problems is entirely inescapable, social scientists have given too much ground, have retreated far too hastily from fields in which it may be possible to sustain an experimental program in the classical sense. Many years ago Henshel (1972) addressed this reluctance, this pragmatic acquiescence (Mumford, 1950), implying that something along the lines of the Steele-Marshall strategy (see the epigram for this section) would enhance the prestige of the social-science disciplines
and provide ever more opportunities for experimentation with the “social infrastructure.” Elaborate simulations found in the current literature (e.g., Heckathorn, 1996), although entirely deductive, largely deterministic, and having the most minimal empirical support, inspire all sorts of ideas for experimentation. For instance, what would happen if the IRS set up a national lottery with hefty payoffs, in which player/taxpayers would make voluntary contributions (buying multiple, high-priced lottery tickets) toward eliminating the national debt, but would also be assured that their contributions would be returned if “free-riders” were so numerous that the lottery could not generate, say, at least $100 billion per year net profit?

Suppose the administration of my university were to take seriously A.J. Liebling’s claim that he was an excellent journalist because “I wrote faster than anybody who could write better, and better than anybody who could write faster.” By “take seriously,” this is what I mean: My colleague Professor X probably knows more math and French and social science than any of the many students who, compared to him, know more math or more French or more social science, or more math and French, or more French and social science, or more math and social science. Here, then, are six large categories of students whom Professor X would be able to teach a great deal, and who would also be able to teach him and their fellow students; everybody would get unique combinatorial benefits, and would also learn to teach, which is the best form of learning. Obviously, anybody who knows more math and French and social science than does Professor X should not take the class, or should teach it. But read Mary Douglas’ How institutions think, and you will see why none of this is going to happen; nor will it ever be thought about seriously, even by those enthusiastic for interdisciplinary studies. Thinking about it might lead to the discovery that old ceramics can still conduct, if they’re kept cool enough. Or warm enough.
REFERENCES


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Comparative analysis with qualitative variables: A loglinear view

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May 21, 2006

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Running head: Comparative analysis
Word count: ~6,500

printed: May 21, 2006
Comparative analysis with qualitative variables:
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Abstract

This article addresses a controversy about the utility of Mill’s methods. In part, the dispute focuses on questions raised decades ago by analytical inductionists, concerning sociological “laws” versus probabilistic assertions. Although analytical induction has a diminished role nowadays, it raised perennial questions about the bases of causal inference. Mill’s critics, I argue, should realize that Mill urged scholars to use the methods of agreement and difference in combination; this strategy, to Mill, appeared to generate valuable findings of a probabilistic nature, even in the presence of small N’s. Moreover, Mill was clearly aware of the possibility of testing interactive hypotheses. Critics often attempt to invalidate Mill’s methods in precisely the way in which any method may be invalidated: by placing powerful constraints, unrealistically “strong” assumptions, on the data. Ironically, Mill’s critics often demonstrate the strengths of his various methods, not their weaknesses. Finally, there is a logical progression from Mill’s methods toward modern methods such as loglinear analysis, a highly versatile set of techniques that may accommodate distinctive approaches to complexity such as chaos theory. Essential parts of the modern accouterment, including careful inspection of one’s data listings and partial tables, go far toward reconciling issues raised by this controversy.
Comparative analysis with qualitative variables:  
A loglinear view

This paper contains two sections. Section (1) presents a brief summary of Robinson’s (1951) formalization of the method of analytic induction; the major purpose of this summary is to remind readers of the way analytic induction used relatively simple logical forms, and of the way in which it broadened definitions of key terms, making them multidimensional, in order to reduce the frequency of deviant cases. Next, I try to show that recent discussions of Mill’s methods have been unduly disparaging primarily because these discussions have failed to realize that Mill’s approach, by taking us well beyond the pursuit of necessary conditions, was logically more demanding than analytic induction. Furthermore, Mill was fully aware of the possibility of interaction effects among three or more variables, of the possibility that findings regarding necessary and/or sufficient conditions might best be stated in probabilistic terms, and of the strategy of broadening one’s definition of causal agents and thereby creating unique causal configurations. Section (2) argues that one should apperceive not only the continuity from Mill to Pearson and Fisher with their predilection toward continuous variables and experimental methods (Willer and Willer, 1973:44-57); one should appreciate also the further continuity that leads to loglinear analysis and related methods, carries us to fringes of chaos theory, and then brings us full circle by invoking once again the importance, sometimes denied (Willer and Willer, 1973:34-35), of qualitative configurations of causes.

(1) Sufficiency and necessity, agreement and difference

(1.1) Analytic induction, once again briefly

Lieberson (1991:307) and Ragin (1994:93-98) recognize that the method of analytic induction relies heavily on the logic of Mill’s methods of difference and agreement. It would be helpful, then, despite the fact that analytic induction is not a central focus of this paper, to restate a small part of Robinson’s classic attempt to formalize the logic of analytic induction. Consider the following table (Robinson, 1951:815):
In this table, row C refers to instances in which a cause or condition is present, and row C' refers to instances in which the same cause or condition is absent. Column P contains instances in which a given effect occurs, and P' contains instances in which the same effect does not occur.

If all observations fell into the cells marked with X’s, we would conclude that “if and only if C occurs, then P occurs,” and C would be both necessary and sufficient for P. Robinson points out, however, that analytic inductionists actively sought and often found cases that would fall into the lower-left cell, cell (c), containing a zero in the table above. He inserted the zero because he believed that, in the typical instance, analytical inductionists would redefine C, the cause or condition, in such a way that the recalcitrant case(s) of cell (c) would be elevated to the upper-left cell, cell (a), in which the X presumably represents an adequate number of observations. In practice, C would become a multidimensional cluster of presumed causes—i.e., it would often contain a configuration of several causal factors—the creation of which would require a high level of semantical skill. Analytic induction, said Robinson (1951:815),

... consists in studying cases in the left column [of the table above] and then so defining C, the conditions, as to make all these cases fall in the upper cell of the column. ... A person practicing analytic induction ... would study cases only in the left column of the table ... [He or she] could not determine whether or not there were cases in the upper right cell, as indicated by the ? in that cell.

Robinson says that if one does not give adequate attention to cases falling into cell (b), then one cannot test the assertion that “if and only if C, then P.” If we focus only on the
leftward column, and assuming that we succeed in holding cell (c) at zero by means of the redefinition strategy, we conclude at best that “only if C, then P” or, more simply, “if P, then C”. That is, we conclude that C is a necessary condition for P. If several cases appear inadvertently in the question-mark cell, then there are instances in which C does not lead to P, and C cannot be a sufficient cause or condition for P. Again, Robinson (1951:815) spells out implications effectively. “This argument,” he says, “shows why the method of analytic induction ... cannot enable us to predict. It cannot because it gives us only the necessary and not the sufficient conditions for the phenomenon to be explained.” But Robinson then shows that analytic inductionists did not accept these shackles for long.

This discussion leads me to the following provisional conclusions:

First: Analytical inductionists made it clear that C often represents highly complex, multidimensional clusters of causes; therefore one must question Lieberson’s suggestion (1991:307,314; 1994:1225, 1233-34) that these simple causal inquiries confine us to single causes. Mill, as we shall see, would surely raise the same question; so would Savolainen (1994:1220-21).

Second: If analytic inductionists, at times, did confine themselves to searches for necessary conditions only, this may have been an unwise tactic. But remember: Robinson says (1951:816) that in actual studies the analytic inductionists did indeed concern themselves with the population of cell (b), the question-mark cell, and tried to reduce that population; in other words, they sought both necessary and sufficient conditions. In essence, as we shall see, this means that the analytic inductionists applied the method of difference combined with the method of agreement, and that Lieberson and others, by focussing on these methods, thereby have re-opened the debate regarding analytic induction. One of Robinson’s contributions to that debate was to point out that if an analytic inductionist accepts any cases in cell (c), while also tolerating cases in cell (b), she is no longer an analytic inductionist. She is doing what the rest of us do, seeking multivariate, probabilistic relationships that usually involve neither necessity nor sufficiency.

It has often occurred in the history of this discipline that scholars have tried to make causal inferences involving simple variables, often dichotomous, and relatively small numbers of cases. Savolainen (1994) and Lieberson (1991; 1994) discuss many examples. Despite the admonitions of general semantics against “two-valued logic,” the discipline has a lasting willingness to use binary, qualitative variables, either singly or in combination.12 Such variables are essential, for instance, to chaos theory. Other theoretical traditions have shown a comparable flexibility. A few years ago I argued (SELFREF, 1986:18-19) that, by definition, a functionalist seeking structural imperatives or prerequisites has a lot in common with analytic inductionists: He searches for necessary structures. I also argued that at the
moment when a functionalist starts to speak of structural *alternatives or equivalents*, he is no longer limiting the search to necessary structural conditions; functional equivalents are substitutable, like coffee and tea, guns and knives, projection and identification. Finally, I argued that since many functionalists had a willingness to abandon the search for necessary conditions, just as the pure analytic inductionists had a willingness to abandon the search for sufficient conditions, we should honor both traditions simultaneously by playing down both necessary and sufficient conditions and concentrating on a search for probabilistic relationships. As I show below, Mill makes—or at least implies—the same argument. And so does chaos theory.

Third and finally, an *obiter dictum*: When Lieberson (1991:315, 317-18) speaks of an aspiration toward “...explaining all the variance ...” and the prospects of measuring dependent variables by means of the “...ratio of odds ...,” we enter the exotic realm of loglinear analysis, logistic regression, and related procedures, and we must entertain the possibility that this relatively recent methodological armamentarium may turn out to be, in essence, Mill’s methods in modern format.

(1.2) *Exegesis*

Lieberson from time to time makes claims about Mill’s text, and I have difficulty in reconciling some of these claims with the text as I interpret it. These difficulties convince me that, if Mill were to read Lieberson’s writings on Mill, he would have trouble recognizing himself; Lieberson’s writings would not, as they say, act as a mirror. There are several instances:

- Lieberson claims (1991:308; 1994:1226) that Mill had misgivings about the applicability of his methods to social-science research. However, as Mill continues grappling with the central issues, he arrives at a highly optimistic view of the future of the social sciences and of the role of his methods, and more advanced methods, within them. I refer to more advanced methods because I believe that, as I try to show in a moment, any fair reading of Mill makes it clear that he anticipated the sorts of breakthroughs in statistical methodology that were about to be made by, say, Karl Pearson; he realized, in other words, that the social sciences have a compelling need for multivariate, probabilistic causal explanations with due allowance for feedback.

The method of difference, in Mill’s view, is the experimental method *par excellence*, and it is true that he is not optimistic about its applicability to social phenomena. At one point he asks us, for instance, to consider the relationship between protective tariffs (PT) and general economic prosperity (PR); the unit of analysis is the nation-state. We begin, says
Mill, by finding two nations alike in many ways, including their “natural advantages,” the “physical and moral” qualities of their people, and a large number of “habits, usages, opinions, laws, and institutions.” We then attempt, applying the method of difference, to ascertain whether PT is associated with PR while PT’ is associated with PR’. “But,” says Mill (1888, Book VI, Chapter VII, § 3, p. 610), “the supposition that two such instances can be met with is manifestly absurd. ... Two nations which agreed in everything except their commercial [tariff] policy would agree also in that.” That is, Mill assumes a priori that the independent variables have extreme multicollinearity: The problem, in essence, is held to be that once we have taken into account a lengthy list of exogenous factors (but surely not “everything”), we may predict with certitude whether or not a given nation will have a protective tariff. If economic prosperity, PR, were also strongly associated with the cluster of causes of which a protective tariff is now merely a highly predictable part, we would have a major finding. Such results would be cause for rejoicing—especially when the rest of the world discovered how powerful, incisive, and rewarding the practice of sociology had become. Alas, social realities do not often have this quasi-deterministic structure. Clearly, however, Mill believes that from time to time they do, and that from time to time it is appropriate to consider tightly coupled clusters of causes and consequences—precisely the sorts of necessary causal clusters that analytical induction tried to associate with particular effects such as embezzlement or opiate addiction. This is an essential point to keep in mind as we evaluate Lieberson’s critique of Mill’s methods.

With regard to protective tariffs, the method of difference may have limitations due to multicollinearity, as discussed above. The method of agreement, however, seems to have considerable utility in this instance. If we had reason to suspect, due to the extraordinary multicollinearities just described for several conceptually distinct variables, that PT and its associated cluster of causes invariably bring about PR, then it would be reasonable to use the method of agreement to see whether this causal sufficiency exists for a variety of nations and historical periods. Mill claims that we would have to make the “... luckiest hit ...” in order to find two nations “... which agree in no circumstance whatever, except in having a restrictive [tariff] system, and in being prosperous.” He does not believe that we would find such nations, but his pessimism arises solely from the fact that he has already assumed that PT is embedded in a fixed cluster of social characteristics, and of course these characteristics cannot vary in the way required by the method of agreement, while we hold constant for PT and check for PR; if PT is held constant, the characteristics are held constant. But if we intentionally hold constant for the entire causal cluster—a tactic routinely advocated by Mill—then it is entirely appropriate to use the method of agreement to associate PT (and its causes) with PR. If there were a host of background variables outside the realm of PT and its
associated cluster of causes, and if these background variables were rotated through a series of diverse values, and if PT and its associated cluster of causes never appeared without PR, we would have, once again, a major finding. Mill calls this result “trifling” because he does not realize the importance of discovering a tightly associated cluster or configuration of causes; he does not know about factor analysis. Nor does he know about Galton, or about Pearson, or about Goodman.

- Although Lieberson (1994:1235) faults Savolainen for his failure to cite Mill directly, Lieberson commits the same peccadillo, with results equally mischievous. He tells us, without citations (1994:1230), that “… the methods of agreement and difference are both outdated and inappropriate. They are outdated because they cannot cope with a probabilistic perspective … [The] rules of evidence used in the Mill methods are logically incompatible with a probabilistic viewpoint.” I call this claim, in context, a peccadillo because any reasonable reading of Mill shows that the methods of difference and agreement often lead to probabilistic assertions. Eventually, in fact, one realizes that the underlying cause of confusion is that Mill did not feel constrained to use small N’s. If N increases, he shows, the probabilistic character of conclusions becomes manifest and inescapable.

- Lieberson says (1994:1231) that although Savolainen has reminded us that Mill allows for causal structures of highest complexity, “[we] are not told where this argument of Mill is stated … As far as I can tell, Mill did not visualize interaction effects at all …” However, Mill strongly implies the existence of such effects. In the conventional definition provided by Lieberson (1991:312), interaction means that the influence of a given independent variable on Y is affected by the level of some other independent variable. Suppose that, following Mill’s “third canon” (1888, Book III, Chapter VIII, pages 284-85), we examine difference and agreement simultaneously and establish that C is necessary and/or sufficient for P under conditions Z₁ through Z₄, but that C has no impact on P within the fifth category of Z. We have four positive outcomes for five trials: This strikes me as precisely the probabilistic process developed by Mill in his presentation of the “method of residues,” and by definition it involves interaction. Notice that it is only when agreement and difference turn up negative or unexpected results, as in the context Z₅, that matters become especially interesting from the interaction standpoint. Things also become interesting from a chaos standpoint, because in this instance a unique cluster has a singular effect.
(1.3) Mill’s causes: occasionally necessary and sufficient, but not always

In this section I show that the logic involved in applying the methods of agreement and difference will lead us inexorably to commit the fallacies known as asserting the consequent and denying the antecedent. This problem, however, is chimical, and only occurs because it is often assumed that one wishes to establish only sufficient conditions and not necessary conditions; i.e., that one’s strategy is the opposite of that of analytic induction.

For clarity, it would be helpful at this stage to restate the method of difference and the method of agreement:

If an instance in which the phenomenon under investigation occurs, and an instance in which it does not occur, have every circumstance in common save one, that one occurring only in the former; the circumstance in which alone the two instances differ, is the effect, or the cause, or an indispensable part of the cause, of the phenomenon.

If two or more instances of the phenomenon under investigation have only one circumstance in common, the circumstance in which alone all the instances agree, is the cause (or effect) of the given phenomenon.

Now, for further orientation, examine again Robinson’s table reproduced above. If a given scholar were the sort of analytic inductionist who had evolved along lines suggested by Robinson, and were trying to establish both necessary and sufficient conditions, then by definition she would be testing the hypothesis that “if and only if C, then P” or, in a simpler symbology preferred by logicians, “iff C, then P.” This scholar would have to apply Mill’s methods, and she would be able to diagram Mill’s basic strategy by means of an appropriate combination of the following syllogisms. As her first option, she would be able to invoke the *modus ponens*:

\[
\begin{align*}
\text{Iff C, then P; } & \quad \text{(the hypothesis)} \\
\text{C occurs; } & \quad \text{(an observation)} \\
\hline
\text{P occurs. } & \quad \text{(an observable conclusion, if premises true)}
\end{align*}
\]
If the observation and conclusion were both true, this procedure would suggest that C is a sufficient condition for P. Or, our investigator could invoke the following argument, the *modus tollens*, in which the prime indicates absence:

\[
\begin{align*}
\text{Iff } C, \text{ then } P; & \quad \text{(the hypothesis)} \\
P' \text{ occurs;} & \quad \text{(an observation)} \\
& \quad \text{(therefore)} \\
C' \text{ occurs.} & \quad \text{(an observable conclusion, if premises true)}
\end{align*}
\]

Again, if observation and conclusion were true, this argument too would imply that C is sufficient to bring about P. Notice that either the *modus ponens* or the *modus tollens* will hold the population of Robinson’s cell (b) at zero, but that neither argument has any impact on cell (c), which is crucial for establishing necessary conditions.

However, since our hypothesis uses the “iff” expression, thereby asserting that C is also necessary for P—i.e., that Robinson’s cell (c) must also be held at zero—our investigator must use at least one of the following two arguments:

\[
\begin{align*}
\text{Iff } C, \text{ then } P; & \quad \text{(the hypothesis)} \\
P \text{ occurs;} & \quad \text{(an observation, requiring assertion of consequent)} \\
& \quad \text{(therefore)} \\
C \text{ occurs.} & \quad \text{(an observable conclusion, if premises true)}
\end{align*}
\]

\[
\begin{align*}
\text{Iff } C, \text{ then } P; & \quad \text{(the hypothesis)} \\
C' \text{ occurs;} & \quad \text{(an observation, requiring denial of antecedent)} \\
& \quad \text{(therefore)} \\
P' \text{ occurs.} & \quad \text{(an observable conclusion, if premises true)}
\end{align*}
\]

The first of these two arguments is usually alleged to commit the fallacy of asserting the consequent, but it is not a fallacious argument when we are testing for a necessary condition. The same should be said of the second argument. It is usually claimed that this argument commits the fallacy of denying the antecedent, but again it is not a fallacious argument if the hypothesis states that “iff C, then P,” including both sufficiency and necessity.

Now we arrive at an important set of conclusions. Mill’s method of difference combines the *modus ponens* with a denial of the antecedent, and it therefore attempts to establish both sufficient and necessary conditions. Interpreted in relation to Robinson’s table,
the method of difference tells us to move along both rows of the table, crossing all columns and covering all cells. The method of agreement, in contrast, repeats the modus ponens in many different contexts and must do so “two or more” times, perhaps many times, but it only attempts to establish sufficient conditions because it only covers Robinson’s first row. If we combine the two methods, as Mill suggests, we are attempting to establish both necessary and sufficient conditions in various contexts. Notice, also, that we could cover all four cells of the Robinson table by moving downward within each column of the table, using arguments that assert the consequent and arguments that deny it. If we design arguments, for two-by-two tables, that use only one column (as in the case of analytic induction) and one row, we must always neglect at least one cell.

Further, if all results are consistent with the hypothesis “iff C, then P,” then they must also be consistent with the hypothesis “iff P, then C”; Mill, therefore, occasionally reminds us that we cannot easily distinguish a cause from an effect. If something goes wrong with the combined method—that is, the relevant syllogisms do not invariably produce true conclusions—then the necessity or sufficiency of a given cause, or both together, become less probable as we move from one social context to another. In a moment, I’ll demonstrate this process with reference to hypothetical data provided by Lieberson.

(1.4) Probabilities and causal configurations

Examine Mill, 1888, Book III, Chapter X, page 311, § 1. Notice, first, the chapter title: Of plurality of causes, and of the intermixture of effects. Notice, secondly, in Mill’s table of contents, the title for section § 1: One effect may have several causes. Throughout this chapter one finds strong evidence that Mill did not believe that the power of his methods was vitiated by the existence of multiple causes for a given phenomenon; nor did he feel that his methods could not accommodate probabilistic events.

Regarding probability, Mill makes matters clear (1888, Book III, Chapter X, page 311-13, § 1 and § 2):

... it is not true that the same phenomenon is always produced by the same cause: the effect a may sometimes arise from A, sometimes from B. ... A and B may produce not a and b, but different portions of an effect a. ... One of the principal consequences of this fact of Plurality of Causes is, to render the first of the inductive methods, that of Agreement, uncertain ...
Uncertain, because the method of agreement, alone, cannot search for alternative causes, i.e., cannot test for the necessity of a given cause or condition. Mill continues:

This, therefore, is a characteristic imperfection of the Method of Agreement, from which imperfection the Method of Difference is free. [That is, the latter gets at the necessity of given causes.] ...

It is only when the instances, being indefinitely multiplied and varied, continue to suggest the same result, that this result acquires any high degree of independent value. If there are but two instances, ... the result is at most only a slight probability in favor of A [a given cause]. ...

After how great a multiplication, then, of varied instances, all agreeing in no other antecedent except A, is the supposition of a plurality of causes sufficiently rebutted, and the conclusion that a is connected with A divested of the characteristic imperfection, and reduced to a virtual certainty? This is a question which we can not be exempted from answering: but the consideration of it belongs to what is called the Theory of Probability ... It is seen, however, at once, that the conclusion does amount to a practical certainty after a sufficient number of instances, and that the method, therefore, is not radically vitiated by the characteristic imperfection.

If, of course, there is a certain probability, less than 1, that A is necessary and sufficient for a, then there is a certain probability, greater than zero, that it is not. Mill’s third canon, cited earlier, tells us that it is advantageous to combine the methods of difference and agreement so that we begin by seeking both necessary and sufficient conditions and perhaps finish by producing a certain probability for conditions neither necessary nor sufficient. To arrive at this sort of conclusion we need relatively large N’s—again, according to the third canon, “two or more” cases. With substantially more than two cases, the major consequence is that probabilities shift; they shift, one hopes, toward a better representation of reality.

Mill’s critics develop fascinating arguments, but these arguments typically rely on the removal of a sufficient number of degrees of freedom that Mill’s methods cannot properly work. For instance, Ragin (1987:41) asks us to consider a situation in which the combination of “land hunger” (H) and “rapid commercialization” (C) is sufficient (and perhaps necessary) to bring about “revolts” (R). Each of the three variables is dichotomous. Neither of the alleged causes, acting alone, is sufficient. Ragin claims that the method of difference would
fail in this case, provided that “... all instances of land hunger also are instances of rapid commercialization, but not the reverse.” The intention of this statement is to create a correlation, to create collinearity, between the two alleged causes. As I read the statement it means that, if we crosstabulate H and C, one of the non-diagonal cells will contain a zero as in the Robinson table. And as we saw above, Mill was fully aware of the existence of this sort of problem, although he was unduly pessimistic about our ability to accommodate it.

Let us, however, consider a more extreme case: that of a perfect relationship between the two alleged causes. H always implies C, and H' always implies C'. We soon run up against the usual problem, with which statisticians have more than enough familiarity: If a pair of independent variables have a perfect correlation, then one of them does not contain any information not included in the other. At least one of the variables has no utility as an independent predictor, and your computer, perhaps, will be the first to call this to your attention. It will produce an error message when it tries to invert the appropriate matrix.

Now, let’s see if Mill’s alarms are as compelling as Minitab’s. We begin by developing case studies according to the usual format, using Mill’s method of difference:

\[
\text{Iff } H, \text{ then } R; \quad \text{(the hypothesis)} \\
\text{H occurs;} \quad \text{(an observation)} \\
\quad \quad \text{R occurs.} \quad \text{(an observable conclusion, if premises true)} \\
\text{Iff } H', \text{ then } R'; \quad \text{(the hypothesis)} \\
\text{H' occurs;} \quad \text{(an observation)} \\
\quad \quad \text{R' occurs.} \quad \text{(an observable conclusion, if premises true)}
\]

These two syllogisms, by definition, apply the method of difference with regard to H. If, as Ragin assumes, the hypothesis is incorrect because land hunger and commercialization must occur together, must “coincide,” to bring about R, then neither of these causes, alone, is sufficient. Therefore we would expect the first argument above, apparently testing for the sufficiency of H alone, to be false. However, it will turn out to be true, because whenever we find an instance of H we must perforce introduce the collinear value, C; remember, they always occur together. The second argument, which apparently tests for the necessity of H, would also be true.

As results begin to accumulate we realize that although land hunger appears to be promising as both sufficient and necessary cause, it cannot be changed without a
corresponding change in rapid commercialization, and therefore we cannot meet one of the essential criteria of Mill’s method of difference: that all background factors must be held constant, as in an experiment. If we had begun our investigation by experimenting with rapid commercialization, we would have produced the following syllogistic representation of the method of difference:

\[
\text{Iff } C, \text{ then } R; \quad \text{(the hypothesis)}
\]
\[
C \text{ occurs;} \quad \text{(an observation)}
\]
\[
\text{———} \quad \text{(therefore)}
\]
\[
R \text{ occurs.} \quad \text{(an observable conclusion, if premises true)}
\]

\[
\text{Iff } C, \text{ then } R; \quad \text{(the hypothesis)}
\]
\[
C' \text{ occurs;} \quad \text{(an observation)}
\]
\[
\text{———} \quad \text{(therefore)}
\]
\[
R' \text{ occurs.} \quad \text{(an observable conclusion, if premises true)}
\]

Same method, same result: Both arguments would be true. But in fact, since H changes with C, this second set of syllogisms is precisely the same as the first. In both instances we must allow H and C to vary together, and we therefore fail to meet the strictures of the method of difference. Instead of having “... every circumstance in common save one,” the experimental and control cases would have every circumstance in common save at least two—the bane of any experiment.

If, however, we had by now discovered the collinearity of H and C, we again would have an important finding regarding the clustering tendencies of the causal agents themselves and we would be able to apply the method of difference as follows:

\[
\text{Iff } [H,C], \text{ then } R; \quad \text{(the hypothesis)}
\]
\[
[H,C] \text{ occurs;} \quad \text{(an observation)}
\]
\[
\text{———} \quad \text{(therefore)}
\]
\[
R \text{ occurs.} \quad \text{(an observable conclusion, if premises true)}
\]

\[
\text{Iff } [H,C], \text{ then } R; \quad \text{(the hypothesis)}
\]
\[
[H,C'] \text{ occurs;} \quad \text{(an observation)}
\]
\[
\text{———} \quad \text{(therefore)}
\]
R' occurs. (an observable conclusion, if premises true)

In Mill’s example discussed earlier, regarding protective tariffs (PT) and prosperity (PR), the same approach would be appropriate. If there were a cluster of causes (x, y, and z) associated with PT, we would eventually arrive at a test of the following pair of syllogisms embodying the method of difference. Three additional variables (l, m, and n) remind us that, when we are ready to invoke the method of agreement, we would wish to test these syllogisms further under a variety of circumstances:

Within l,m,n: If [x,y,z,PT], then PR; (the hypothesis)
Within l,m,n: [x,y,z,PT] occurs; (an observation)

PR occurs. (therefore)

Within l,m,n: If [x,y,z,PT], then PR; (the hypothesis)
Within l,m,n: [x,y,z,PT]' occurs; (an observation)

PR' occurs. (therefore)

It is unseemly to criticize Mill’s methods when we have violated their essential premises. And it is unwise not to see that if we have now identified clusters of causes, with the proviso that a cluster of causes may also be regarded as an effect, we are again on the fringes of chaos theory.

(1.5) Capturing Lieberson’s probabilities and interaction terms

Studies cited by Lieberson (1991:308) as examples of the use of Mill’s methods with small N’s would benefit if there were a way of expressing the probabilities and identifying the interactions whose existence one cannot deny. There is a way.

Table 1 contains data, with embellishments, from Lieberson’s Table 4 (1994:1235). For each of Lieberson’s six fictitious applications of Mill’s methods, A through F, there is a syllogism that expresses a hypothesis about the impact of $X_1$ on Y, describes a finding, and concludes with a prediction. The column called “Lieberson column Y” lists Lieberson’s
observations on Y: In some instances Y occurs, and in some it does not. If, as in case B, the
syllogism predicts that Y will not occur, but Lieberson’s hypothetical data show that Y has
occurred, then we have a case study that does not support the initial hypothesis. Accordingly,
I place a zero in both the “X_i nec” and “X_i suff” columns. For cases such as E, in which the
prediction agrees with Lieberson’s observation, I place a 1 in the appropriate column. Half
the time X_i is either necessary or sufficient for Y, and half the time it is not. If the data were
inserted into a table comparable to Robinson’s, half the cases would enter the X cells, and
half would not. Clearly, Lieberson’s hypothetical data for Y and X_i are not very exciting.

[TABLE 1 LOCATION]
The column called “Lieberson column X_2” lists Lieberson’s codes for a hypothetical
control variable. A code of -1 indicates that the level of this variable was low or absent; a 1
indicates the opposite. These scores, when multiplied by the data of the “X_i nec” and “X_i
suff” columns, identify (in the “product” column) the social contexts in which X_i has a strong
impact on Y. These contexts, defined according to X_2, are preceded by a minus sign
whenever X_2 is “low.” Among Lieberson’s set of six cases we observe that for A, E, and F
the initial hypothesis receives support when the level of the control variable is low or absent.
But again, this pattern occurs for only half the cases. And it raises an entirely new set of
issues, to which we now turn.

(2) Darwin needed Mendel, and Mill needed Goodman

As I examined Lieberson’s Table 4, it reminded me of the way in which, say, SPSS
would set up effect codes for three simple dichotomies. I began experimenting with the
SPSS\textsuperscript{18} crosstabs and loglinear routines, and eventually ended up with a simple SPSS job that
illustrates many of the issues dealt with in this paper. For those who may wish to experiment
with it, I shall reproduce and interpret the entire listing; following that, I discuss a small part
of the output.

```
DATA LIST LIST/Y X1 X2 WT1

DOCUMENT
LIEBERSON TABLE 4 INTERACTION HYPOTHESIS

set width=80

WEIGHT BY WT1

VALUE LABELS Y 1 'Y-HIGH' 2 'Y-LOW'/
X1 1 'X1-HIGH' 2 'X1-LOW'/
```
X2 1 'X2-HIGH' 2 'X2-LOW'

crosstabs variables=Y, X1, X2 (1,2)/
tables=Y by X1 by X2 /* Does X1 impact vary by X2?
options
statistics 1 2

BEGIN DATA
1 1 1 1 /* Y X1 X2
1 1 2 2 /* Y X1 X2' Lieberson case A
1 2 1 3 /* Y X1' X2  L. cases B, C
1 2 2 4 /* Y X1' X2'  L. case D
2 1 1 5 /* Y' X1 X2
2 1 2 6 /* Y' X1 X2'
2 2 1 7 /* Y' X1' X2
2 2 2 8 /* Y' X1' X2'  L. cases E, F
END DATA
/* col. 1 is Y, col. 2 is X1, col. 3 is X2,
/* col. 4 indicates N of cases (wt1)

LOGLINEAR Y (1,2) BY X1 (1,2) X2 (1,2)/
PRINT=FREQ DESIGN ESTIM/
DESIGN=

FINISH

As we examine the job listing, the following features should be highlighted:

(1) The input data are listed from left to right, in the order Y, X1, X2, and N-of-cases; the latter is called WT1. This procedure makes it clear that Lieberson’s data are extremely minimal. Each of his rows contains only one implied case, with N = 6. For the three-variable problem of my SPSS listing—assuming that each variable is dichotomous—there are eight logically possible rows; Lieberson’s data occupy only four of these rows. And since Lieberson actually assumes four variables, with the six cases assigned to five distinctive rows, he is asking us to ignore eleven of sixteen logically possible rows.

(2) For SPSS value labels the words “high” and “low” must be used in lieu of primes (’) to distinguish between the two values for a given variable.
(3) The crosstabs command produces two partial tables: The first shows the relationship between Y and X, with X coded high, and the second shows the same relationship with X coded low.

(4) For the lines following the BEGIN DATA command, I have inserted codes for the three variables, and N’s—i.e., the WT1 values—for each of the eight rows of data. The N’s, for the moment, range from 1 to 8, which facilitates our finding them in the output listing; the N’s should also be thought of as row numbers for the BEGIN DATA statement. I also provide comments that show where Lieberson’s cases occur. For instance, the expression “Lieberson case A” shows the row where Lieberson’s case A would be found. Remember: Lieberson has only one case in that row, not two as in my didactic listing.

(5) Loglinear commands then appear. I discuss them below.

First of all, we need to use the partial crosstabulations to get oriented. Table 2 suggests that, as Lazarsfeld (1958) showed many years ago, we would generate strong interaction—i.e., we would force the relationship between Y and X to vary within the subcategories of X—if we were to place large numbers of cases in the main diagonal of the first partial table, and large numbers of cases in the opposite diagonal of the second partial table. In other words, we would have to insert many cases into rows 1, 7, 4, and 6 of the BEGIN DATA statement. (In Table 2, remember, these row numbers appear temporarily as the cell frequencies, for ease of identification.) [TABLE 2 LOCATION]

This is precisely the set of circumstances that occurs in Lieberson’s example, except that his example is very attenuated. In effect, Lieberson inserts into the above SPSS job listing the following BEGIN DATA statement. Notice that the fourth column of the data matrix now contains Lieberson’s cases A through F.

BEGIN DATA
1 1 1 0 /* Y X1 X2
1 1 2 1 /* Y X1 X2’ Lieberson case A
1 2 1 2 /* Y’ X1 X2 L. cases B, C
1 2 2 1 /* Y’ X1’ X2’ L. case D
2 1 1 0 /* Y’ X1 X2
2 1 2 0 /* Y’ X1 X2’
2 2 1 0 /* Y’ X1’ X2
2 2 2 2 /* Y’ X1’ X2’ L. cases E, F
END DATA
SPSS will do this job, but not happily; it does not like all the zero weights. What happens in Lieberson’s partial tables—see Table 3 herein—is that the first of them collapses while the second, due to cases A, E, and F, may make a weak suggestion of interaction as discerned in my Table 1. But the first partial table collapses solely as a result of Lieberson’s arbitrary decision not to permit an opposite diagonal to appear at all within it. Any interaction will necessarily be attenuated. [TABLE 3 LOCATION]

To me, the clear lesson in all this is that scholars who use variants of Mill’s methods with small N’s should pay close attention to what is, in essence, their BEGIN DATA statement. If one were testing an interaction hypothesis, for instance, one would expect to find many cases in opposing diagonals of the partial tables, perhaps as in the following statement. Again, the fourth column of the data matrix contains the new data:

```
BEGIN DATA
  1 1 1 3 /" Y X1 X2
  1 1 2 1 /" Y X1 X2'  Lieberson case A
  1 2 1 0 /" Y X1' X2  L. cases B, C
  1 2 2 2 /" Y X1' X2'  L. case D
  2 1 1 0 /" Y' X1 X2
  2 1 2 2 /" Y' X1 X2'
  2 2 1 3 /" Y' X1' X2
  2 2 2 0 /" Y' X1' X2'  L. cases E, F
END DATA
```

Given the context of this discussion, eleven cases may appear to be exorbitant; but the results (Table 4), as one would expect, are far more interesting. They show that Lieberson (1991:315, 317-18) expresses a sound intuition when he invokes what appear to be loglinear tenets, suggesting that we should aspire toward “... explaining all the variance ...” and that we should measure dependent variables by means of the “... ratio of odds ...” In the loglinear commands near the end of the SPSS job listing, the expression “DESIGN =” is a default request for the saturated model, i.e., one that gives Y as a function of its own marginal distribution, each of the X variables independently, and the interaction between them. Such models are inherently non-parsimonious and therefore unsatisfactory: For the data of Table 4, the loglinear analysis provides four log-odds coefficients that do indeed “explain all the variance” in Y by reproducing Table 4, but one obtains the same insight merely by reading the four columns that comprise the two partial tables. Little, if anything, is gained: One does not find a more parsimonious way of expressing relationships. On the other hand, when a researcher examines results it is entirely possible that s/he will perceive more readily the
meaning of four $z$-values, from a saturated logit model, than the meaning of several tabular columns with their various frequencies. [TABLE 4 LOCATION]

Halli and Rao (1992:128), however, recommend that all loglinear analyses begin with the saturated form because it “... will give a good idea of which interactions are good candidates for deletion in a more parsimonious treatment of the data.” This evolution from the saturated model toward simpler formulations is indispensable for problems involving a relatively large number of variables and perhaps several interactions, and it also forces us to use the probability notions that lie at the heart of Mill’s methods: Non-saturated loglinear models often have many degrees of freedom, and do not ordinarily have deterministic outcomes. For the attenuated data and saturated model of Table 4, the loglinear coefficient showing the interaction effects of the two $X$ variables is relatively large, it is independent of the individual effects of the $X$ variables, and it is statistically significant ($z=2.12$).

(2.1) Contexts of determinism

While it is true, as claimed above, that non-saturated loglinear models do not ordinarily have deterministic outcomes, we do from time to time come across precisely the situation displayed in Table 4: The partial tables contain distributions of empty cells similar to those sought by analytic inductionists and to those still pursued by some contemporary versions$^{19}$ of comparative analysis. For the upper facet of Table 4, we observe a relationship that involves necessity and sufficiency, taking either $X_1$ or $Y$ to be the causal factor—as Mill reminds us, we often do not know the direction of a given causal nexus. And we have compelling statistical evidence leading us toward and corroborating this finding: For the partial table in which $X_2$ is high, the $\phi$ (or $\phi^2$) coefficient for $Y$ and $X_1$ is unity. For the lower facet of Table 4 there is only a single missing cell, and the $\phi$ coefficient does not clearly reflect it. Similarly, if we are not sure about the direction of causation, interpretations become difficult. In interpreting the partial table for which $X_2$ is low we would use, say, the modus ponens combined with assertion of the consequent in order to test for the sufficiency and necessity of $X_1$ as a cause of $Y$:

Iff $X_1$-LOW, then $Y$-HIGH; \hspace{1cm} \text{(the hypothesis)}

$X_1$-LOW occurs; 

\hspace{1cm} \text{(an observation)}

\hspace{1cm} \text{(therefore)}

$Y$-HIGH occurs. \hspace{1cm} \text{(an observable conclusion, if premises true)}
If X-LOW, then Y-HIGH; (the hypothesis)
Y-HIGH occurs; (an observation, requiring assertion of consequent)
__________ (therefore)
X-LOW occurs. (an observable conclusion, if premises true)

The first syllogism, for this particular partial table, generates a true conclusion, and the second does not; therefore we have evidence that X-LOW is a sufficient but not necessary condition for Y-HIGH.

I believe that we should routinely scrutinize partial tables, in the manner just illustrated, for any instance in which loglinear analysis uncovers a significant (i.e., probabilistic) causal structure and also informs us, as does SPSS, that a given multivariate table contains a number of empty cells. Clearly, the task of examining a lengthy series of syllogisms as complex as those given above would be—to say the least—cumbersome. When methods become cumbersome, however, our usual adaptation is to write and implement the appropriate software, and such a strategy appears to be feasible in this instance. Regarding empty cells, SPSS currently issues a warning (#3211) saying that

On at least one case, the value of the weight variable was zero, negative, or missing. Such cases are invisible to statistical procedures and graphs which need positively weighted cases, but remain on the file and are processed by non-statistical facilities such as LIST and SAVE.

But such cases are clearly not invisible if one examines partial tables separately (while simultaneously carrying out a more sophisticated procedure such as loglinear analysis), in the traditional fashion. Furthermore, it would be appropriate to modify current software so that in instances where cell frequencies were appropriately distributed, users would receive, along with the warning reproduced above, a message saying, for instance, that “for the partial table in which X2 = 1, the Y, X1 relationship involves necessary and sufficient conditions.”20 In any case, the use of loglinear modelling, $\phi$ coefficients, and empty cells, along with an array of edifying messages from one’s statistical software, strike me as a much simpler way of searching for either deterministic relationships (i.e., necessity and/or sufficiency) or probabilistic relationships than the dauntingly complex protocols of Ragin’s (1987) boolean analysis. A critic of this paper points out that “most actual tables are not easily translated into if-then statements because the expected zero cells are not found.” But if, on the other hand, they are found ...
Several colleagues have pointed out that it is an oxymoron to speak of the *probability* of a necessary and/or sufficient condition. One reader was “bothered by the ... claim that it is possible to have probabilistic necessary or sufficient conditions” while another asks “how can you talk about the probability that a condition is neither necessary nor sufficient? If you observe a SINGLE exception, then that alone is proof that the condition is neither necessary nor sufficient.” In my estimation this absolute standard is overly demanding because it would preclude opportunities for the sorts of insights that had pivotal significance for Mill, i.e., those involving interaction effects in which necessity and sufficiency may sometimes appear; further, it would deny us the opportunity to have the sorts of insights that remain central to theoretical traditions known variously as configurationism, situationism, or catastrophism (Perrow, 1984: Chapter 3). This opportunity is clearly before us: Suppose that one has followed Mill’s advice, and has examined the relationship between Y and X not merely for two partial tables but in a very large number and variety of contexts, situations, configurations; suppose further that in about a third of these instances one sees cell frequencies that suggest both necessity and sufficiency. Why would it not be appropriate and edifying to say that the probability is about one-third that Y and X are related by necessity and sufficiency, and then describe the contexts in which this determinism appears to exist? This is precisely the strategy advocated by Mouzelis (1995:71-74) when, in a discussion of Elias’ hypothesis about a connection between Durkheimian interdependence (organic solidarity) and the “civilizing process,” he tells us (twice) that

... the interesting problem generated by Elias’ substantive work is not whether a universal linkage can or cannot be established between interdependence and self-regulation; the really interesting problem is to explore the complex conditions where social differentiation and interdependence are linked to civilizing processes, and the conditions where they are not.

Mouzelis consistently argues that any “universal linkage” tends to be either “trivial or wrong”; the phrase recurs many times throughout his book. The same argument appears in critiques of traditional functionalism, where it has been pointed out occasionally that functional “prerequisites” by definition were necessary conditions thought to have universal linkages with essential outcomes. For instance (SELFREF, 1986:24), “... it is possible that (as Marxists imply) certain ‘economic systems’ become dysfunctional at certain levels of social development, [so that] merely asserting that an economic system [of some sort] is a prerequisite would hardly encourage one to undertake the appropriate Marxian analysis.”
Mouzelis also discusses, in a later context (1995:133-34), the prospects of counterfactual historical analysis. He comes close to making the essential assertion that, in demonstrating the plausibility of a counterfactual hypothesis, it usually suffices to show that while a counterfactual social structure evidently was not necessary in order to bring about outcomes that occurred historically for other reasons, the counterfactual would probably have been sufficient if it had appeared. This is the essence, for instance, of Fogel’s (1964) counterfactual analysis of waterways in the United States, as an unnecessary but potentially sufficient alternative to railroads.

(2.2) Context, configuration, chaos: A short list of FAQ’s

(2.2.1) What is chaos theory, and how does it relate to the social sciences?

Chaos is an “irregular oscillatory process” (Brown, 1995:8). That is, it involves periodic, recurring changes, sometimes better described as transformations of system states. These changes are brought about by nonlinear factors operating within a deterministic causal system. In many instances chaotic processes appear to involve random changes, but the processes involved are nevertheless governed by differential equations with deterministic properties (Hallinan, 1997:7). An essential feature of many chaotic systems is high sensitivity to initial conditions, i.e., to the starting values assigned to the variables of a given chaotic process, and this high sensitivity accounts for a large part of the apparently random behavior. Hallinan (1997:8) also points out that chaotic systems sometimes have high sensitivity to exogenous environmental conditions, and these conditions appear to involve the qualitative distinctions prominent in cross-national and cross-cultural comparisons—again, echoes of Mill.

In relating chaos theory to the social sciences, one should probably begin by learning about applications of the theory to problems of the natural sciences. Classic studies of weather (Brown, 1995:10-21; Korsch and Yodl, 1994:2-3,270-74; Nicolaides and Walkington, 1996:144; Sparrow, 1982), double pendula (Korsch and Yodl, 1994:Chapter 5), and logistic patterns of growth (Brown, 1995:10-21; Kolman et al., 1992:797-800) should give the essential flavor of this intellectual endeavor.

Focussing on the famous Lorenz equations for weather dynamics,21 we prepared a Maple experiment using differential equations and associated procedures written by Nicolaides and Walkington (1996:144) but using parameter values suggested by Sparrow (1982:2) and Brown (1995:11,14). When parameters are held within specified ranges, the result is invariably a time-series pattern of weather variations that has the unmistakable
butterfly form, with combinations of the three output values jumping back and forth, in what is presumably a “wildly erratic” pattern (Hallinan, 1997:7), between two “attractors” that involve substantially different weather configurations.

It is indeed delightful to activate the appropriate equations and watch this pattern—or the comparable pattern for double pendula or a logistic growth process—unfolding on one’s computer screen. As the time series moves through its various values, we realize that the “unpredictability” of the models means that if we run the program for a long time, all the available spaces making up, say, a butterfly image eventually will be filled in by the myriad values for the output (Brown, 1995:14). Yet, the form remains essentially and predictably that of a butterfly with the observed phenomenon making an occasional, characteristic, bilateral jump from one wing to the other. It is noteworthy that this sort of jump, this “wildly erratic” behavior, was not alien to the philosophy of Friedrich Engels or J.B.S. Haldane; I hesitate, of course, to say that chaos theory is a revival of dialectical materialism.

(2.2.2) How do chaos concepts relate to the comparative methods discussed in this article?

When one examines the “butterfly” geometry of Lorenz’ equations, one sees a pattern reminiscent of a crosstabulation where all cases fall into a single diagonal: Butterflies have bilateral symmetry, and so does a square-matrix crosstabulation in which, as in a few instances discussed above, all cases occur along or close to a single diagonal. The metaphor provides a unique insight.

Brown shows (1995:41-47) that it is possible to use methods comparable to regression analysis in order to find parameters, or estimates, that describe the behavior of chaotic social processes, but he is not optimistic. His misgivings arise primarily from the fact that time-series observations in the social sciences are typically too few and far between (Brown, 1995:46). By contrast, the Lorenz equations can be made to generate thousands of observations for the three crucial variables, with a time lapse perhaps amounting to only a few minutes between each pair of data points. More importantly, meteorologists do in fact obtain data pertaining to the real world that correspond precisely, in their frequency, with the data generated by the appropriate simulation models.

On the other hand, when Brown (1995:51-62) discusses “catastrophe” the prospects of estimation begin to look more promising. A catastrophe, for a phenomenon modelled on the butterfly pattern, would occur if the butterfly were suddenly to cause itself to yaw, say, in a leftward direction, permitting the “attractor” of its right wing to move a considerable distance to an entirely new equilibrium while the left wing remains stationary. Here we
encounter the sort of change that is of central importance in comparative studies of human societies. In an example drawn from election research, Brown derives parameter estimates for voter attitudes toward Reagan and Carter around the time of the 1980 election. Although the methodological details may vary, I see few important differences between this study and the logit modelling conducted by Yamaguchi (1991: Chapter 3) in order to find parameters that influence “two-way transitions” involving time-series variations in “personal efficacy.” To me, it appears that Yamaguchi’s major burden is to show us how to apply Mill’s methods to time-series data.
REFERENCES


SELFREF. 1986.


### Table 1. Multivariate data and Mill’s methods: A Lieberson Table Recast

<table>
<thead>
<tr>
<th>Cases: Syllogisms</th>
<th>Lieberson column</th>
<th>nec: suff: Lieberson column</th>
<th>product ((4)+(5))</th>
<th>Y: (X_2) *(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
</tr>
<tr>
<td>A</td>
<td>Iff (X_1), then (Y); (X_1) occurs;</td>
<td>(Y) occurs</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>B</td>
<td>Iff (X_1), then (Y); (X_1)' occurs;</td>
<td>(Y)' occurs</td>
<td>Y occurs</td>
<td>0</td>
</tr>
<tr>
<td>C</td>
<td>Iff (X_1), then (Y); (X_1)' occurs;</td>
<td>(Y)' occurs</td>
<td>Y occurs</td>
<td>0</td>
</tr>
<tr>
<td>D</td>
<td>Iff (X_1), then (Y); (X_1)' occurs;</td>
<td>(Y)' occurs</td>
<td>Y occurs</td>
<td>0</td>
</tr>
<tr>
<td>E</td>
<td>Iff (X_1), then (Y); (X_1)' occurs;</td>
<td>(Y)' occurs</td>
<td>Y' occurs</td>
<td>1</td>
</tr>
<tr>
<td>F</td>
<td>Iff (X_1), then (Y); (X_1)' occurs;</td>
<td>(Y)' occurs</td>
<td>Y' occurs</td>
<td>1</td>
</tr>
</tbody>
</table>
\( Y' \) occurs. \( Y' \) occurs 1 0 -1 -1

Note: For explanation of each column, see text.
Table 2. Partial tables: Orientation

---

**Y by X1**
Controlling for...
X2 Value = 1  X2-HIGH

<table>
<thead>
<tr>
<th>X1</th>
<th>Page 1 of 1</th>
</tr>
</thead>
<tbody>
<tr>
<td>Count</td>
<td></td>
</tr>
<tr>
<td>Col Pct</td>
<td>X1-HIGH</td>
</tr>
<tr>
<td></td>
<td>Row</td>
</tr>
<tr>
<td>Y</td>
<td></td>
</tr>
<tr>
<td>----------</td>
<td>-------------</td>
</tr>
<tr>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>Y-HIGH</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
</tr>
<tr>
<td>2</td>
<td>5</td>
</tr>
<tr>
<td>Y-LOW</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
</tr>
<tr>
<td>Column</td>
<td></td>
</tr>
<tr>
<td>Total</td>
<td></td>
</tr>
</tbody>
</table>

**Y by X1**
Controlling for...
X2 Value = 2  X2-LOW

<table>
<thead>
<tr>
<th>X1</th>
<th>Page 1 of 1</th>
</tr>
</thead>
<tbody>
<tr>
<td>Count</td>
<td></td>
</tr>
<tr>
<td>Col Pct</td>
<td>X1-HIGH</td>
</tr>
<tr>
<td></td>
<td>Row</td>
</tr>
<tr>
<td>Y</td>
<td></td>
</tr>
<tr>
<td>----------</td>
<td>-------------</td>
</tr>
<tr>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>Y-HIGH</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
</tr>
<tr>
<td>2</td>
<td>6</td>
</tr>
<tr>
<td>Y-LOW</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
</tr>
<tr>
<td>Column</td>
<td></td>
</tr>
<tr>
<td>Total</td>
<td></td>
</tr>
</tbody>
</table>

---

Note: Cell values are identifiers, not frequencies. See text.
Table 3. Partial tables: Lieberson’s data

<table>
<thead>
<tr>
<th>Y by X1</th>
</tr>
</thead>
<tbody>
<tr>
<td>Controlling for..</td>
</tr>
<tr>
<td>X2 Value = 1 X2-HIGH</td>
</tr>
</tbody>
</table>

```
X1                      Page 1 of 1
Count | Col Pct | X1-LOW |
      |         |        |
      | Row     | 2      |
Y      | Total   |
-------|--------+
1      | 2      | 2      |
Y-HIGH | 100.0  | 100.0  |
       +--------+
Column 2 2
Total   100.0 100.0
```

Y by X1
Controlling for..        
X2 Value = 2 X2-LOW

```
X1                      Page 1 of 1
Count | Col Pct | X1-HIGH X1-LOW |
      |         |               |
      | Row     | 1 2 Total |
Y      |         |        |
-------|--------+
1      | 1      | 1 2      |
Y-HIGH | 100.0  | 33.3 50.0|
       +--------+
2      | 2      | 2      |
Y-LOW  | 66.7   | 50.0   |
       +--------+
Column 1 3 4
Total   25.0 75.0 100.0
```
Table 4. Partial tables: Larger N, stronger conclusions

<table>
<thead>
<tr>
<th>X1</th>
<th>Page 1 of 1</th>
</tr>
</thead>
<tbody>
<tr>
<td>Count</td>
<td></td>
</tr>
<tr>
<td>Col Pct</td>
<td>X1-HIGH</td>
</tr>
<tr>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Y by X1
Controlling for...
X2 Value = 1  X2-HIGH

| Y | ---------+----- ---+--------+ |
|---|--------+----- ---+--------+ |
| 1 | 3 | 1 | 3 | |
| Y-HIGH | 100.0 | | 50.0 | |
| 2 | | 3 | 3 | |
| Y-LOW | | 100.0 | 50.0 | |
| Column | 3 | 3 | 6 | |
| Total | 50.0 | 50.0 | 100.0 | |

Y by X1
Controlling for...
X2 Value = 2  X2-LOW

<table>
<thead>
<tr>
<th>X1</th>
<th>Page 1 of 1</th>
</tr>
</thead>
<tbody>
<tr>
<td>Count</td>
<td></td>
</tr>
<tr>
<td>Col Pct</td>
<td>X1-HIGH</td>
</tr>
<tr>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Y | ---------+----- ---+--------+ |
|---|--------+----- ---+--------+ |
| 1 | 1 | 2 | 3 | |
| Y-HIGH | 33.3 | 100.0 | 60.0 | |
| 2 | 2 | | 2 | |
| Y-LOW | 66.7 | | 40.0 | |
| Column | 3 | 2 | 5 | |
| Total | 60.0 | 40.0 | 100.0 | |
ENDNOTES
Theory/Method Interaction and Serendipity: A Minimalist Presentation of Loglinear Methods with Applications to Sex-Role Theory

Abstract

This paper presents didactical exercises designed to show that (1) loglinear analysis is a highly efficient, effective, and parsimonious method for revealing relationships among qualitative and quantitative variables; (2) the method has wide applicability in areas such as sex-role theory, where there are many hypotheses about causal interactions among qualitative variables; (3) those who criticize loglinear methods often end by inadvertently revealing its advantages; (4) the method is now widely used, and should be fully incorporated into undergraduate and postgraduate social-science curricula.
A few years ago, Collins (1984:357) wrote that “log-linear analysis ... gives us an array of numerical coefficients that mean nothing except from the point of view of the statistical analysis proposed.” It has never been clear to me what he meant by the phrase “point of view of the statistical analysis.” If he meant to suggest that this point of view does not give sufficient play to theory, then he was, I think, clearly mistaken. Contemporary social-science theory has revitalized qualitative thought patterns, and various theories/methods such as chaos modeling, sophisticated comparative analyses (e.g., Ragin, 1987), and loglinear analysis elucidate the qualitative/quantitative interface in ways that are unprecedented.

The following example, presented in Tables I and II below, uses data from Halli and Rao (1992: Chapter 6); the data appeared originally in Taylor and Chappell (1980). First of all, notice that this “array of numerical coefficients” is nowhere near so forbidding as Collins implies. Granted, a lot of work has already been done; nevertheless, these two simple tables tell an important theoretical story, and they do so parsimoniously and understandably.

Earlier phases of the analysis inform us that it is appropriate to consider academic performance, called GRADES, to be a function of CLASS, EFFORT, and IQ independently, with all variables measured as dichotomous sets of categories. Furthermore, there are independent interactions between class and the other two causal agents, such that effort, for instance, will have a different impact for each subcategory of class. It is unlikely that traditional percentage crosstabulations could have arrived, convincingly, at these conclusions. Try it: You will indeed be lost in an array of numbers, with little prospect of rationally eliminating competing causal models, i.e., those that entertain other possible interactions, those that reject at least one of the three main effects, etc.

How large is each of these independent effects? To answer this question we have to read the tables, bearing in mind that since loglinear analysis, for the most part, tries to determine the odds for given events (such as having high grades), we will need to focus on odds ratios, just as gamblers do. Gamblers look for things that influence the odds, say, on a given horse or a given state-lottery game; we look for things that influence the odds on social events. (Statisticians traditionally have been inspired by gamblers.)

SPSS presents these two tables in the sequence given at the end of this paper, but Table II logically precedes Table I. The “Estimates for Parameters” tell us which causal agents are significant—each has a “Z-Value” outside the range -2 to +2; in addition, we
obtain “Coeff” values that enable us to arrive at the “EXP. count” column of Table I. The computations for this step are tedious, as are those involved in arriving at the coefficients themselves (Collins, in a sense, has already told us this), but it is this column that is truly important in interpreting results. It tells us in detail what our causal model predicts, explains, and implies about academic performance.

Take a look at the rows in Table I that I’ve underlined and named 1a, 1b, etc. If we take the ratio of “expected” percentages for the two rows called 1a, and then the two rows called 1b, we have the following odds for high grades:

For CLASS=MIDDLE, EFFORT=HIGH, IQ=HIGH: 72/28=2.57

For CLASS=MIDDLE, EFFORT=LOW, IQ=HIGH: 66.26/33.74=1.96

In this comparison, we see that effort has relatively little impact on academic performance for middle-class students of high intelligence. Weak effort lowers the odds on academic success from 2.57 to 1.96. By “relatively little impact,” I mean that the difference between these two odds ratios, 2.57 and 1.96, is much smaller than the difference found in other contexts, in which effort has a much larger impact. Interaction effects tell us where to look for these other contexts.

The interactions, perhaps, are more interesting theoretically than the main effects. In this example, the loglinear analysis has informed us (for Parameter 5) that an interaction exists between class and effort. This means that the effect of effort varies by class, and/or vice versa. Consider, then, the following odds ratios, involving the rows numbered 2a and 2b:

For CLASS=LOWER, EFFORT=HIGH, IQ=HIGH: 71.35/28.65=2.49

For CLASS=LOWER, EFFORT=LOW, IQ=HIGH: 32.23/67.77=.48

For bright, middle-class students (as we saw in the preceding paragraph), effort is largely inconsequential; even if they don’t try very hard, they tend to do all right. For bright, lower-class students, however, we now see that effort is crucial; if they don’t try hard, it shows up in their academic evaluations. This finding would be of significance in many theoretical contexts. For instance, symbolic interactionists might argue that, within the higher strata, students are propelled toward success by self-fulfilling prophecies—they are expected to do well—and effort matters relatively little (Rosenthal and Jacobson, 1968;
Michener, DeLamater, and Schwartz, 1986:241-42). Notice that I arrived at this theory after examining the loglinear results; this is entirely legitimate, and we can see a similar serendipity at work if we examine the history of analysis of variance.

Analysis of variance succeeded, in large part, because it gave Darwinists and geneticists a way of expressing and extending their theories (Porter, 1986:316). Although one hears caveats about allowing the methodological tail to wag the theoretical dog, it is arguable that statistical models have little value unless they create insight—say, by encouraging precisely such reversals (Koestler, 1964:191-99). And the tail/dog metaphor, containing its own contradiction, instructs us: Show me a dog that can cause himself to be wagged by his own tail, and I’ll show you how to generate a sensation among Darwinists and geneticists, not to mention physicists.

So let tails wag, and see what happens. In my current research, I’m using CPS data (October 1984 and October 1989) to find out how young boys and girls use home PC’s. So far, it appears that sex/gender, for both surveys, has an appreciable net impact on PC use. As I experimented with loglinear models, it occurred to me that sex/gender may have a larger impact on home PC use nowadays than it did back in 1984. In brief, I allowed theoretical bodies to wag, realizing that Ogburnian analysis strongly implies that early adopters of new technologies are likely to be innovative also in the realm of social behavior (Abrahamson, 1990:184-85). In the present case, early adopters of PC’s may have been more avant-garde in their willingness to challenge traditional sex/gender roles. And when I add race/ethnicity to this analysis, I’ll be able to test an interaction hypothesis proposed recently by a Massachusetts politician who said that she had had problems due to being black, and problems due to being a woman; but her major problems had resulted, in her opinion, from being a black woman.

Suppose we decide that two-variable interactions are enticing, and we want more of a good thing. We carry out a series of plausible loglinear analyses and identify the most plausible model among them; it suggests that the odds for some social process, called A, are influenced by three dichotomies and a single continuous variable; call these latter B, C, D, and X. Notice that under the logic developed above it is entirely possible that we would discern a three-variable interaction or two. Say, for instance, that the effects of B on A vary according to the subcategories of C and D.

Assume that C and D are dichotomies; when they are crosstabulated, they produce four subcategories. Flesh out these subcategories. Suppose that A is defined as
“familism,” i.e., heavy involvement with spouses, partners, children, in-laws, and so forth; B is defined as high v. low occupational status; C is sex/gender, and D is race/ethnicity. Now the interaction may embody a highly sophisticated hypothesis, viz., that the impact of occupational status on familism is likely to vary among black males, black females, white males, and white females. One could readily arrive at this hypothesis without loglinear analysis, and one could even test it by means of traditional crosstabulations (Author self-reference, 1977). But the test would not be convincing, and we would miss additional complexities, e.g., the role of X, other unanticipated interactions, the joint and independent impact of B, C, D, and X, etc. More importantly, if tail and dog do indeed interact as I suggest, we would impair our ability to use the concept of second-order interactions as a perceptual filter for studying the literature and for arriving at new hypotheses. In my view, most class time in theory/methods courses and seminars should be devoted to mental experiments of the sort just illustrated, with an odds ratio, for example, taken as a function of B, C, D, X, and their theoretically plausible interactions.

This is important: The sex-role literature, for instance, contains many ideas about how social processes vary by race/ethnicity and sex/gender combined (e.g., Uehara, 1994). Once one has a clear concept of the meaning of a multivariate loglinear model with a second-order (or higher) interaction, one is prepared to move through the literature and identify a host of intriguing hypotheses that meet the conditions of the model. Without the model, we are likely to miss many good prospects. And I would argue that it is the qualitative character of loglinear analysis that facilitates use of this perceptual filter. The sociological imagination is minimally stimulated when we ask, say, “What happens when age is multiplied by income?” It is much more intriguing to ask “How does this process work among black males, black females ...?” and so forth. This is one reason why the “quant-qual” distinction is at least as nonsensical in sociology as it is in chemistry.

Once one grasps the model as form, one finds that content becomes ubiquitous and hypotheses proliferate—hypotheses that go way beyond allegedly commonsensical assertions. In a book about sex roles, we find the following remarks (Stockard and Johnson, 1980:54):

No matter how mental illness is defined ..., married women have higher rates of mental disorder than married men. By contrast, most studies of people who have never married find that single men have higher rates of mental troubles than single women. ... While married people of both sexes have less mental illness than single people, the difference between the mental health of single and married people is much greater for men than it
is for women ... It appears that marriage does considerably more for men’s mental health than it does for women’s.

The findings concerning mental health differences between married men and women also apply to differences in physical health.

This is a classic $n^{th}$-order interaction hypothesis: The impact of marriage on mental health, it suggests, varies by sex. Presumably, physical health follows the same pattern. If one were to use the HEALTH variable from the General Social Survey, in which respondents evaluate their own general state of health, one could test the same sort of hypothesis with a subjective sense of well-being as the dependent variable. And the next logical step would be to invoke race and/or class as additional causal agents, potentially interactive.

The central idea, then, is that a given loglinear model works as a *deus ex machina*: It cranks out proposed solutions or potential insights. The model leads us to ask good questions. Using the idea of second-order interaction, we can take advantage of the fact that qualities such as sex/gender and race/ethnicity, due to their centrality in contemporary societies, have a wide range of potential interactions with a wide range of contextual variables. For instance, take the second-order interaction model and change the meaning of D: Let it now represent “tradition-directed” professions v. “change-oriented” professions. Suppose we decide that lawyers are tradition-directed, and social-science professors are change-oriented. We then rephrase the hypothesis stated earlier: The impact of occupational status on familism is likely to vary among male lawyers, female lawyers, male social-science professors, and female social-science professors.

How would it vary? My guess is that in comparing male and female social-science professors, we would find relatively little difference in the relationship between occupational status and familism; in comparing male and female lawyers, we would find a large difference in the relationship between occupational status and familism. Again, it appears to me that when we exercise the sociological imagination, we propagate such hypotheses the way turtles lay eggs.

iii

In a personal communication, a critic contends that the problem with loglinear analysis is not that it is atheoretical, but that

... in using it one is locked into the particular theory the analyst was using. The coefficients cannot be interpreted except from the point of view of that
particular model. One of the advantages of older cross-tabulation methods and their descendents was that if you didn’t like the theory the author of an article was using, you had the data available so that you could reanalyze it yourself.

This claim is untenable. All one typically can do in reanalyzing a published three-variable crosstabulation is to collect various marginal values and then, say, change the dependent variable. One could readily carry out the same steps using the “OBS. count” column of Table I. In loglinear analysis, however, we have an additional major advantage: If the author has done the necessary homework we should have evidence that many competing models have been eliminated on the basis of their inability to meet standard statistical criteria. In traditional crosstabulation, a systematic procedure for eliminating large numbers of competing models has never been available.26

The same critic continues:

In addition, the loglinear convention for giving only the odds ratios makes it impossible to dig out descriptive information. Suppose you wanted to report what percentage of working class students get good grades? The old cross-tabs would show it; the loglinear coefficients hide it. The older methods were informative in ways that the subsequent methods are not.

Again, untenable. Although they provide information that does not exist for crosstabulations, the loglinear coefficients should not be our only foci. Table I shows that the observed ratio of high to low grades for lower class students with high effort and high intelligence is 40/16, which translates into percentages as 71.43/28.57. Seventy-one percent, then, of the highly motivated, bright, working-class students have high grades. This is precisely what we would learn by reading percentages from the appropriate partial crosstabulation table. If you don’t ask your loglinear software for percentages, you don’t see them; actually, the same applies to crosstabs.27

But for those who remain strongly partial to percentage interpretations, here is the truly good news: Loglinear coefficients—e.g., any one of the six coefficients listed in Table II—do indeed have a clear percentage interpretation, one that is far more generalizable and parsimonious than disparate interpretations of the many percentages based on partial crosstabulations. A given loglinear coefficient, acting alone, will always raise or lower odds by a fixed percentage. Suppose, for instance, that we have two groups, A and B, and that both groups are low on effort. Suppose further that a loglinear
analysis tells us that (1) the odds favoring high grades for group A are 1.7, and for group B, 1.2; (2) the loglinear coefficient for effort, acting alone, is .24; (3) effort does not interact with other causal agents. If we could raise the level of effort for both groups from low to high, we would predict (or project) a 62 per cent increase (Halli and Rao, 1992: Table 6.4, row 3, column 3) for each group in the odds for high grades, regardless of the fact that the initial odds were unequal. We would expect the odds for group A to change from 1.7 to 2.75; for group B, from 1.2 to 1.94. High effort, in other words, would invariably suggest the same prediction, regardless of initial odds. This is a very systematic finding.

No amount of perusal of a large set of partial tables would lead to this sort of statement.

iv

If Collins anticipated that loglinear analysis would fall of its own computational complexity, he must be disappointed. Applying a single keyword—“loglinear analysis”—to the literature summarized over the last several years by Sociological Abstracts and ERIC, one found, circa January 1995, slightly more than 100 items that either discuss the method itself or present intriguing results in terms of it; by June 1996, this sum had increased to 148. Many additional papers make use of loglinear analysis without mentioning it in abstracts. Clearly, if one wishes to keep up with contemporary sociology, one must learn, as a minimum, to comprehend discussions based on loglinear analyses and related methods.

And one should definitely shun the following remarks by Denzin (1989:74): “It must be noted that ‘measurement-minded’ sociologists follow fads and fashions in the use of one statistical method and then another.” Denzin’s illustrations include factor analysis, multivariate analysis, path analysis, loglinear analysis, LISREL, etc. Is this a series of frivolous fads and fashions, or a logical progression indicating that there is an important process of cumulation in the methods realm, a process that reveals our perennial, inexorable pursuit of multivariate causal explanations of social phenomena of high complexity?
1. As understood in this paper, Ogburnian innovations do not occur solely within the technological realm. They may occur in any of the four areas of the POET paradigm, a classic definition of human ecology that includes demographic (population), organizational, and environmental factors as they interact with each other and with technology. Thus, the introduction of a “free flight” system by the FAA, allowing airline pilots far greater latitude in selecting routes and airspeeds, would be an organizational innovation.

2. Martin (1992:197-98) discusses the impact of early, rapid railroad expansion in improving the quality of tracks, which had been dangerously brittle.

3. There is, no doubt, a stage in the exploration of any given field during which we must tolerate many bivariate relationships that, in truth, explain very little, leaving to chance most of the variation of an alleged effect. The standard-format bivariate tables found in Herrnstein and Murray (1994)—the first appearing on page 32—serve as an example, as do the many standard-format bivariate tables in which Zipf (1949:386-415) suggested that the P/D relationship—population size, say, of a pair of cities divided by the distance between them—may have an impact on many forms of social interaction. Again, it is arguable that chance predominated in virtually all these instances.

4. The same argument has been made about ways in which nature selects for or against various complex chemical compounds.

5. In fact, a number of these examples were identified by the “Nova” series on PBS.

6. Blalock (1979, p. 470) shows that in a three-variable regression problem involving X, Y, and Z, there are 64 relatively simple linear causal diagrams that could potentially represent the interaction among these variables. He assumes that for each pair of variables, say X and Y, either there is no causation, or X influences Y linearly, or Y influences X linearly, or there is reciprocal causation. The same four conditions apply for the other two pairs of variables. It is arguable that, in the typical instance, the model selected by a researcher is not adequately tested against the remaining 63 potential models—although R-SQUARE in SAS aspires toward such extensive testing. If we added enough variables and allowed for nonlinearities, however, anybody trying to read R-SQUARE’s output would be overwhelmed.

7. To find a floppy-eared rabbit (Barber and Fox, 1958), one must look closely: The soup spoon represents the rabbit’s cranium, and the vortices look like downward flopping, neatly rolled-up
rabbit ears.

8. Wildavsky (1988) develops a stimulating argument to the effect that safety measures often cannot be justified on benefit/cost grounds.

9. At my university there was once a grassy knoll that attracted a pathway. Eventually, the path wore away the grass, and erosion began to occur. To control erosion, the university decided to install an elaborate brick staircase, with large brick walls on either side of it. Pedestrians, however, found this arrangement unattractive, and they created yazoo (parallel) paths on either side of the underutilized brick staircase. The yazoo paths, predictably, killed grass and started erosion again, and one of the yazoos has now been replaced with a gravel path. Whether the gravel path will attract traffic from the opposite yazoo as well as from the staircase, and also contain erosion, remains to be seen.

Incidentally, a cursory examination of campus paths will convince one that any given formal path will usually make additional informal paths appear to be rational. However, for an example of an “irrational” yazoo path, examine Braess’ paradox in Liebman (1986:53).

10. Postmodernism tells us that we tend to ignore the human subjects of social research. Asking respondents to provide the vast amount of information necessary for writing expert systems, while we take copious notes, surely resolves this issue—although we definitely need to think about how profits, if any, will be allocated.

11. A brief discussion of chaos theory appears in the final section of this paper. See also Hallinan (1997).

12. This article does not deal in detail with the issue of levels of measurement. However, the extremely widespread diffusion of S. S. Stevens’ distinctions among nominal, ordinal, interval, and ratio levels of measurement seems to be an adequate response to the semanticist’s conundrum of “Aristotelian” logic. Selection among levels of measurement is ordinarily treated as an empirical matter, subject to appropriate tests.

13. In personal correspondence Lieberson says that his 1994 reference should have been to Mill’s book 6, not book 4. My discussion assumes, then, that Lieberson intended to cite chapter VII of book 6. This chapter bears a title as follows: “Of the chemical, or experimental, method in the social sciences.” See “Erratum,” Social Forces 73, June 1995, inside back cover.
14. In a later section, the logic of this example will be demonstrated by means of syllogisms.

15. But here is a sample of what he does know—about complex causes, about multivariate analysis, about feedback, and about several related issues:

For a discussion of multiple causation of social phenomena and the need for multiple and partial causal analysis, see Book VI, Chapter VII, § 4.

For a discussion of the “inverse method,” which Mill attributes to Comte, which would involve social scientists in an attempt to predict (i.e., to “postdict”) past events, and which therefore involves the logic of multivariate analysis and the need for multiple and partial causal analysis, see Book VI, Chapter IX, § 1.

Important insights into multiple causation with feedback appear in Book VI, Chapter IX, § 2; and Chapter X, § 3.

For a discussion of the behavior of “desire for wealth” as a causal agent subject to contextual variations, see Book VI, Chapter IX, § 3. A similar discussion of “intensity of competition” is found in the section immediately following.

To see how a careful study of the history of “corn laws” might enable us to develop what appear to be leading indicators of the future performance of an economy, see Book VI, Chapter IX, § 6.

To see why Mill admired Buckle’s attitude toward social statistics, see Book VI, Chapter XI, § 1.

16. A referee for this article has reminded me that, although interaction among variables may imply probabilistic relationships, the probabilistic properties of assertions may also arise from sources such as random measurement error, or from the direct (non-interactive) effects of extraneous variables.

17. Notice that even though any given syllogism in the following series uses the “iff” expression, at least two syllogistic arguments must be applied in order to test the hypothesis of necessity and sufficiency. Thinking about these matters reflexively, we realize that if two syllogisms have been employed, then they are individually necessary and collectively sufficient to corroborate an “iff” expression.

18. A referee for this article asks why one would “prefer” SPSS for this type of analysis. My reason is that, although I occasionally use SAS’ categorical analysis packages, I happen to have greater familiarity with the loglinear routines of SPSS. Many packages, no doubt, would be adequate. As far as I know, however, none of the available packages implements Mills’ methods. In a moment, I argue that it would be helpful for them to do so.
19. See, for instance, Ragin’s (1987:45) discussion of Marvin Harris’ efforts to find highly probable (if not deterministic) cross-cultural causal patterns.

20. This phraseology is similar to that found in Textor (1967:37) where a “special verb-phrase,” written by computer, describes two-by-two tables having at least one zero cell. For example (Textor, 1967: paragraph 148/451):

   CULTURES WHERE THE INCIDENCE OF PERSONAL CRIME IS HIGH IN ALL CASES ARE THOSE WHERE TOTEMISM IS UNIMPORTANT OR ABSENT. CULTURES WHERE THE INCIDENCE OF PERSONAL CRIME IS LOW IN ALL CASES ARE THOSE WHERE TOTEMISM IS PRESENT.

See SELFREF (1993) for a further discussion of cross-cultural (HRAF) data.

21. There are three differential equations in this meteorological problem. The first measures the rate of convection circulation, the second measures change of the horizontal temperature gradient, and the third measures change of the vertical temperature gradient. See Sparrow (1982:2). Details and results of this experiment are available on request.

22. In essence, loglinear analysis is an effective implementation of Mill’s (1888: Book III, Chapter VIII) methods of difference and agreement. The method of difference informs us that, say, effort has an impact on grades. The method of agreement asks whether this effect occurs in a variety of social contexts. If there is an interaction, as in the present instance, agreement does not occur: effort does not have the same impact in the lower strata as in the higher strata. Paradoxically, when we fail to meet Mill’s criterion of agreement we are likely to be in the presence of a far more interesting result, from a theoretical standpoint. For a recent attempt to evaluate and improve upon Mill’s methods, see Ragin (1987).

23. In event-history analysis, loglinear methods model events that occur or do not occur through time intervals, e.g., marrying, becoming unemployed, migrating, dying. See Yamaguchi (1991).

24. South and Spitze (1994) test a similar hypothesis about the impact of marital status on the relationship between sex/gender and housework. Again, the most interesting theoretical question asks why marital status would have an impact on this relationship. The interaction of race and sex in relation to attitudes toward abortion is examined in Lynxwiler and Gay (1994) and in Dugger (1991).
25. Thus far, however, I have not found in the literature a discussion of ways in which one would test for reciprocal causation, i.e., feedback, involving two or more categorical variables.

26. Blalock (1979:234-36) presents a way of testing for the statistical significance of a “difference of differences of proportions.” In this instance he is dealing with ways in which the relationship between two dichotomies may vary within the subcategories of a third dichotomy. If a new variable were added, however, we presumably would have to deal with a difference of differences of proportions. And if any variable had more than two subcategories, we would soon be lost in complexity.

27. There is a procedure related to loglinear analysis in which the dependent variable is a proportion; multiply by 100, and it becomes a percentage. One problem with this approach, however, is that it tends to produce expected proportions that fall outside the range 0 to 1. I therefore prefer odds ratios, but chacun son goût. Theory is not precise: If one’s theory anticipates a high probability for some event, and one obtains a “probability” of 1.03, one’s theory should not be rejected.
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Table I. Observed, Expected Frequencies...

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<td>IQ</td>
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<td>40.00 (62.50)</td>
<td>42.40 (66.26)</td>
</tr>
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</table>

1a

| CLASS    | LOWER |                   |                   |
| EFFORT   | HIGH  |                   |                   |
| IQ       | HIGH  | 40.00 (71.43)     | 39.95 (71.35)     |
| IQ       | LOW   | 6.00 (15.79)      | 6.05 (15.91)      |
| EFFORT   | LOW   |                   |                   |
| IQ       | HIGH  | 18.00 (32.14)     | 18.05 (32.23)     |

2a

| IQ       | LOW   | 2.00 ( 3.57)      | 1.95 ( 3.49)      |

1b

| GRADES   | LOW   |                   |                   |
| CLASS    | MIDDLE |                   |                   |
| EFFORT   | HIGH  |                   |                   |
| IQ       | HIGH  | 20.00 (25.00)     | 22.40 (28.00)     |
| IQ       | LOW   | 24.00 (37.50)     | 21.60 (33.74)     |
| EFFORT   | LOW   |                   |                   |
| IQ       | HIGH  | 24.00 (37.50)     | 21.60 (33.74)     |
| IQ       | LOW   | 12.00 (33.33)     | 14.40 (40.01)     |
| CLASS    | LOWER |                   |                   |
| EFFORT   | HIGH  |                   |                   |
| IQ       | HIGH  | 16.00 (28.57)     | 16.05 (28.65)     |
| IQ       | LOW   | 32.00 (84.21)     | 31.95 (84.09)     |
| EFFORT   | LOW   |                   |                   |
| IQ       | HIGH  | 38.00 (67.86)     | 37.95 (67.77)     |
| IQ       | LOW   | 54.00 (96.43)     | 54.05 (96.51)     |

2b
Table II. Estimates for Parameters

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Quantification: Three Slightly Advanced Techniques

Abstract

This article uses special graphic techniques along with Minitab programming language in order to clarify complex statistical techniques such as regression analysis, loglinear analysis, and various applications of calculus.
(1) The aviation chase-around chart: A unique graphic for representing multiple regression equations with any number of variables

Statisticians (and their textbooks) sometimes talk about the arithmetic mean of a distribution as if it were the balance point of a seesaw; this is a graphic, “right-brain” technique of teaching that is both charming and edifying. Recently I discovered a graphic technique in aviation that enables pilots to carry out a task critical to safety—balancing the load of an airplane properly—by recognizing that an airplane has several features in common with seesaws. The location of an aircraft's center of gravity must be known at all times, and when loads are placed forward or aft of an initial CG the changes of it must be traced. What is unique about the standard technique is that it inadvertently involves the manipulation of weighted means. Pilots who may know little about statistics are able to use relatively simple graphic methods that “recalculate” CG's with considerable precision, as weighted means. In class, I now use aircraft weight-and-balance concepts as an extension of the traditional seesaw analogy.

Similarly, we who teach statistics are accustomed to the use of scatter diagrams to represent regression relationships between two interval-level variables, Y and X. The scatter diagram works well on a two-dimensional surface such as a blackboard or a piece of paper. When we try to depict multivariate relationships, however, we encounter a serious problem: It is difficult to draw, on a two-dimensional surface, a three-dimensional relationship representing Y as a function of X and Z. The typical textbook uses perspective methods to present a three-dimensional plane on a two-dimensional page (Blalock, 1979:454; Elifson, Runyon, and Haber, 1990:266; Hanushek and Jackson, 1977:26), but the results are usually only minimally helpful for students. Using a similar improvisation, I sometimes stand at the blackboard, draw standard Y and X axes, and tell students to imagine a Z-axis extending straight out from the blackboard; then I rotate a large piece of cardboard—a regression plane—through this three-dimensional volume in order to represent a regression equation giving Y as a function of X and Z. Up to a point this works, but students often feel that the Z-axis—not to mention the others—goes right past them.

FIGURE 1 ABOUT HERE

This brings us to the aviation “chase-around chart,” a truly sophisticated graphic device that does not seem to have caught the attention of social scientists knowledgeable
about graphic techniques (Tufte, 1983; Wilkinson, 1990). Figure 1 presents such a chart (Federal Aviation Administration, 1988: Appendix 3, Figure 31). It enables pilots to ascertain how long a runway they need in order to get an aircraft off the ground, and how much additional distance they may need in order to clear the first significant obstacle. The “ASSOCIATED CONDITIONS” of the graph should be considered as background constants, and the “TAKE-OFF SPEED” box is essentially irrelevant to this discussion. The “EXAMPLE” involves a particular aircraft that is trying to take off under the conditions specified. Its “scores” on the independent variables (IV's) are: 15 degrees celsius for ambient temperature, an airport altitude that rounds off to 6000 feet, gross weight at 2950 pounds, and headwind at 9.5 knots. The airplane therefore requires 1375 feet of runway to get airborne; if there is a fifty-foot obstacle near the end of the runway, the aircraft requires a total of 2300 feet to get airborne and clear this obstacle.

Again, what is significant about this diagram is the fact that it is used by pilots who may know little about the behavior of multiple regression equations. Obviously pilots have to understand the logic of such equations—in this instance it is a matter of life and death. To find a solution for the aircraft used in the example, one takes the first score, ambient temperature, and moves up the graph (following the arrow) until one intersects the second score, airport altitude. These two IV's alone would require a takeoff roll of about 1500 feet—just move across the graph until intersecting the dependent variable (DV), takeoff distance, on the rightward axis. However, an adjustment must now be made for the third variable, weight, by intersecting the first “REFERENCE LINE” and then moving downward along the appropriate slope of the weight variable. For the example used in the graph, weight does not require any downward adjustment of takeoff distance, because this particular airplane is loaded to maximum gross weight, 2950 pounds. It is not particularly helpful that the weight axis does not increase from left to right, as in conventional Cartesian coordinates.

The horizontal series of arrows continues across the graph until it intersects the next reference line. Then it moves along a downward slope for the wind component. Since the example involves a ten-knot headwind, the takeoff roll is reduced by a short distance represented by the small downward-sloping arrow on the left side of the wind facet of the graph, between zero and ten knots. The arrows again become horizontal and move across to the adjustment for obstacle clearance, which in this instance would increase the required takeoff distance to 2300 feet.

Using Minitab, I prepared a simple simulation of takeoff distances for 100 aircraft. The required takeoff distance is determined by the same five variables as in Figure 1. I allowed the regression equation to operate without error variance, but in the future I plan
to develop simulations that will show more realistically how much error of prediction would be needed to produce cases in which aircraft do not have adequate runway. For now, however, I wish to explore the ways in which output from this simulation provides a clear graphic representation of what goes on in a six-variable multivariate process.

I modified the original chase-around chart by treating ambient temperature and airport altitude in separate segments (not shown), avoiding the intersecting display for these two variables as shown on the chase-around chart.\(^1\) I reversed the direction of the axis for aircraft weight. The five IV’s have chance relationships with each other, except that ambient temperature is given as a function of altitude (elevation of a given airport): The rule of thumb among pilots is that the temperature lapse rate is 2 degrees celsius per thousand feet of altitude. I estimated appropriate regression coefficients through visual inspection of the original chase-around chart. Following is the complete regression equation, first in non-standardized form and then with all IV’s standardized while takeoff distance remains in non-standardized values. The standardized coefficients are listed in a tabular format.

In non-standardized form:
\[
\text{DIST} = (10.0 \cdot \text{OAT}) + (.100 \cdot \text{ALT}) + (.80 \cdot \text{WT}) - (15.0 \cdot \text{WND}) + (10.0 \cdot \text{OBST})
\]

With IV’s standardized:
\[
\text{DIST} = 2319 + (34.4 \cdot \text{OAT}) + (67.4 \cdot \text{ALT}) + (131 \cdot \text{WT}) - (56.2 \cdot \text{WND}) + (72.8 \cdot \text{OBST})
\]

The resultant regression graphic, then, looks very much like the chase-around chart. However, only a part of my chart is depicted in Figure 2, because if I were to include all five facets the graphic would be too wide for a journal format. The facets for weight and headwind relate to each other in precisely the same way as do the remaining facets of the complete graphic.

**FIGURE 2 ABOUT HERE**

Figure 2 shows the partial slope coefficients relating aircraft weight and headwind velocity to takeoff distance. Lines representing these slopes have the correct angles, but unlike a standard scatter diagram they do not have an origin at the Y-intercept; they rotate around the intersection of the two means. Therefore, in following a given airplane through the regression equation one has to be prepared to draw regression slopes parallel
to those provided, just as is done in the case of the chase-around chart. My chart is actually simpler than the aviation chart: I provide only one slope line, while the chase-around chart, involving curvilinearity and interaction among IV's, provides several distinctive slope lines for each segment of the graphic. Curvilinearity and interaction are nicely captured by the logic of the chase-around diagram, but I do not wish to introduce these sorts of complications into my own simulation at present.

Toward the bottom of each segment of Figure 2 we can identify the case that has the lowest score on the DV; it is located toward the lower left corner of the weight segment, and toward the lower right corner of the headwind segment. The real magic of this sort of graphic is revealed when we trace a given case through it. For this specific airplane, numerical scores are as given in Table 1. If readers wish to substitute social-science variables for the six aviation variables, this is readily accomplished.2

TABLE 1 ABOUT HERE

By the time we are ready to make the adjustment for weight, takeoff distance for our hypothetical aircraft has been set to 2345 feet by the first two variables (and the mean). A horizontal series of arrows moves onto the weight segment from the left, intersecting the DV at the previously established value, 2345 feet. It continues across the segment until it intersects a reference line, the standardized mean (zero) for weight. A line parallel to the partial regression slope is then extended toward the lower left corner of the segment, until it arrives at the z-score (for weight) for the aircraft of interest. This z-score is -2.27, and it lowers the takeoff distance by 297 feet, bringing it down from 2345 feet to 2048 feet. After we complete the adjustment for weight, the new value, 2048 feet, is carried across to the headwind segment of the graph. There, the same sort of adjustment involves extending a parallel slope toward the bottom rightward corner of the segment, where the aircraft of interest displays a z-score of 1.28 for headwind. The new takeoff distance is 1976 feet, and this value is carried across to the final segment of the graph where it is adjusted for obstacle height. The resultant takeoff distance for this aircraft is 1923 feet, with allowance for rounding error, QED.
(2) How to ransack complex log-linear tables without cross-tabulating one's brain synapses

... log-linear analysis ... gives us an array of numerical coefficients that mean nothing except from the point of view of the statistical analysis proposed.

—Collins (1984:357)

... the reader is unfortunately left, for the time being, to the villainy of SAS and SPSS when it comes to actually using log-linear analysis.

—Halli and Rao (1992:120)

Collins, as we shall see, is mistaken; Halli and Rao, however, make it clear why Collins' remark has a surface plausibility. Over the next several pages, I shall try to give sociological meaning—venturing way beyond “nothing”—to the many numerical coefficients generated by loglinear methods.

(2.1) Numerical examples drawn from the literature

Gilbert's (1981) book is generally elementary, and it provides several numerical examples. One of these examples (Gilbert, 1981:65-67) shows that even when one deals with only three dichotomous variables, the number of loglinear models that might capture the interactions among them is very large. In this relatively simple example, there are 18 possible models; with additional variables, things become unmanageable. This proliferation, of course, is not surprising: In multiple regression analysis there are no less than 64 different models that might account for the relationships among three continuous variables, X, Y, and Z; and there would be an infinitude of models if we allowed nonlinear interactions (Blalock, 1979:470,n.3).

Gilbert (1981:58-9) points out that with so many models to choose from in a three-variable problem “... and considerably more for tables of greater dimensionality, it is clear that some way of selecting the most appropriate one is a necessity.” The best way to reduce these large numbers is to do what we usually do: Select a given variable and treat it as dependent on the others. In Gilbert's example the three variables are these: (1) attitude about whether or not doctors should give certain kinds of advice; (2)
occupational class; (3) sex. It appears reasonable to regard the first of these variables as dependent on the other two. I have also dichotomized the DV, which I shall call ADVICE.

Now we find that the most complex (“saturated”) model has a maximum of four effects, as follows:

From Gilbert's model 1:
- ADVICE as influenced by its own statistical distribution
- ADVICE as influenced by CLASS
- ADVICE as influenced by SEX
- ADVICE as influenced by CLASS by SEX

There are only two IV's; a simple table of binomial coefficients tells us that there are two possible effects involving a single IV, and one possible effect involving the interaction of the two IV's. If this model turns out to be overly complex or otherwise inappropriate, we start peeling away effects from the bottom of the list toward the top—Gilbert goes from top to bottom. Instead of 18 potentially valid models, we now have the following:

Gilbert's model 9:
- ADVICE as influenced by its own distribution
- ADVICE as influenced by CLASS

Gilbert's model 11:
- ADVICE as influenced by its own distribution
- ADVICE as influenced by SEX

Gilbert's model 16:
- ADVICE as influenced by its own distribution

It is interesting that Gilbert decides that the most plausible model among his list of 18 is model 8, which implies that ADVICE is related to SEX, and that CLASS is predictable on the basis of its own distribution. However, the latter is of no interest to us if we merely wish to explain ADVICE as a DV. Of the four models listed above—1, 9, 11, and 16—it is number 11 that appears to be the most plausible; Gilbert's data, however, do not involve strong relationships.

TABLE 2 ABOUT HERE
We select model 11 by means of one of the standard criteria: The behavior of chi-square values (Table 2). Usually, chi-square tells us whether a set of observations depart significantly from the null hypothesis, which usually claims that two variables are unrelated. In loglinear analysis, maximum likelihood estimation (MLE) is used to produce chi-square values which tell us whether our data depart significantly from the various models that we might wish to test. A "good" model—one that is not rejected—therefore has a low (non-significant) value for chi-square. Our old ways of thinking about chi-square are still valid, but the logic of decisionmaking is now reversed: We do not decide whether to reject a null hypothesis; rather, we decide whether to reject a theoretical model. In the above instance we would probably conclude, for reasons that will soon become evident, that model 11 is the most plausible: ADVICE is probably influenced by sex, but not by class or by the interaction of sex and class.

The following SPSS program, of minimal villainy, replicates essential parts of a slightly more complex problem, as developed by Swafford (1980):

```
DATA LIST LIST/ROWID TIME EDUC REGION JOBS WT1

DOCUMENT


WEIGHT BY WT1

VALUE LABELS TIME 1 '1946' 2 '1963'/
    EDUC 1 'GRADE' 2 'HIGH' 3 'COLL'/
    REGION 1 'SOUTH' 2 'OTHER'/
    JOBS 0 'YES' 1 'NO'

LOGLINEAR JOBS (0,1) BY TIME(1,2) EDUC(1,3) REGION(1,2)/
PRINT=FREQ DESIGN ESTIM/
DESIGN=JOBS/
DESIGN/
    DESIGN=JOBS, JOBS BY TIME, JOBS BY EDUC, JOBS BY REGION,
    JOBS BY TIME BY EDUC, JOBS BY TIME BY REGION,
    JOBS BY EDUC BY REGION/
DESIGN=JOBS, JOBS BY TIME, JOBS BY EDUC, JOBS BY REGION/
```
As in Gilbert's example, one is able to experiment with weights, or frequencies, as one gets more competent with loglinear methods. In such experiments it should be possible to anticipate results more or less accurately.
The DESIGN statements, above, correspond to the ten models analyzed by Swafford for his Table 3; the order of presentation is slightly different. In Swafford's table the letter J refers to the DV, herein called JOBS; this variable distinguishes between agreeing and disagreeing that blacks should have equal job opportunities. To understand how the best of these models (Swafford's number 10) behaves, we will need to give some attention, in a few moments, to columns (15) through (17) of Swafford's Table 1.

In column (8) of Swafford's Table 3, we see how one would ask SPSS to evaluate the various models in which JOBS is treated as a DV. Of the various ways of representing loglinear models, I find the SPSS method to be the most understandable. Other popular methods are found in columns (2) and (7), same table.

Swafford claims that his Table 3 represents a “backward selection procedure” (Swafford, 1980:669), and the claim makes sense. However, I prefer a slightly different ordering of the models, as in (my) Table 3. (I also include Swafford's column (6), which presents chi-square values of the sort used earlier.) Model 1 is the saturated model because it hypothesizes that the DV, JOBS, is a function of every possible factor: Its own independent distribution, the effects of time, education, and region, the interaction of these three variables taken two at a time, and the overall impact of the interaction of time, education, and region considered together. This model is not satisfactory, however, because it is too busy: It is not parsimonious. One of the simpler models may be better. To decide on such a model, it is appropriate again to use chi-square tests.

**TABLE 3 ABOUT HERE**

The situation is fairly clear: Models 1 and 2 have low chi-square values (lacking asterisks), and we conclude that these models represent the data well. However, each of these models is complex, and it might be possible to arrive at a more parsimonious version. Model 3 apparently strips away too much—it has a relatively high chi-square value—and so we must re-introduce the various interaction terms, one at a time; here, we depart (as does Swafford) from the “backward selection” process. We see now that models 4 through 7 also produce significantly high chi-square values, and they too are rejected. Swafford's model 10 is apparently the most plausible, because it produces a relatively low chi-square value and has a minimal number of effects. It appears, then, that attitudes about job equality for black people can be predicted and explained (in part) by reference to time, education, and
region separately, and to the interaction of time with education, and time with region. That is, TIME has a different impact on JOBS depending on EDUC (or vice versa), and TIME also has a different impact on JOBS depending on REGION (or vice versa). This interpretation goes way beyond “nothing,” as perceived by Collins. One could spin all sorts of speculative stories from it, with the proviso that the main plot retain its stability. Imagination makes nothingness fascinating.

In Swafford's Table 1, columns (15) through (17), we see some of the loglinear properties of model 10. We shall try to understand these properties by interpreting further the SPSS output for this model.

(2.2) Swafford's model 10: Detailed interpretation

In the SPSS appendix we see how SPSS codes the scores for the DV, JOBS, and for the three independent variables. The eight PARAMETER codes correspond to model 10 only, and the other models are coded differently. The large matrix of ones, negative ones, and zeros involves “effect coding,” and these scores have very much the same role as the scale scores that are multiplied by regression coefficients in familiar regression equations; they are akin to standard dummy coding (Kerlinger and Pedhazur, 1973:117-25). Notice that when we see JOBS BY EDUC in the EFFECT NAME column, the effect takes two columns, 3 and 4. This is because education has three categories, and they are coded so that grade-school education is defined by PARAMETER 3, while high school education is defined by PARAMETER 4. College education is the “reference category,” in the terminology of dummy-variable analysis. Respondents coded 3 on education—the college educated—receive a score of -1 when we use effect coding: This -1 is used for the reference category.

The first row of the DESIGN MATRIX shows effect coding for those who had FACTOR scores of 0 1 1 1 in the original data: These respondents, of whom there are 52, supported the concept of equal job opportunity; they were interviewed in 1946, they had grade school educations, and they lived in the south.

The SPSS appendix provides observations and other information for the 24 cells of the large table with which we are working. Model 10 predicts that 54.53 individuals should fall into the cell for which JOBS is coded YES, interviews were conducted in 1946, etc.; this frequency is also found in Swafford's Table 1, column (16). Other expected counts (EXP. COUNT) are found in Swafford's columns (16) and (17), Table 1. The likelihood ratio chi-square for this model, \( \chi^2 = 8.3 \), is found just after the list of frequencies. Because it is significant at the .08 level, this chi-square is
nearly high enough for us to reject model 10; but if we require the usual .05 level, we shall decide not to reject it.

In the next segment of the SPSS output, we have coefficients that enable us to estimate the odds of agreeing versus disagreeing, on the equal-opportunity item, for any given combination of scores on the IV's. The loglinear coefficients (in the COEFF. column) have many features in common with regression coefficients; each PARAMETER has at least one Z-VALUE that exceeds 2.0, which implies that the chi-square technique for selecting a plausible model must have had a degree of validity. To interpret the COEFF. column more clearly, however, we need to do a few manipulations.

(2.3) Minitab manipulations

The following Minitab program, listed along with its output, obtains some of Swafford's additional results. These results show how loglinear coefficients, also known as logits, enable us to predict odds on DV's—in this case, agreeing vs. disagreeing on the JOBS item. The Minitab program had to be written specially for this paper, because as far as I know none of the loglinear software currently available carries out these steps. The program itself contains several explanatory notes.

In line 104 of the Minitab listing, we see the effect codes provided by SPSS; these are the actual scores received on all variables by respondents who were interviewed in 1946, were college educated, and lived in the south. Here we can easily become confused, because the effect codes are not likely to be the same as the original codes used for the four variables in question; the translation is found in the SPSS appendix, where the DESIGN MATRIX is listed. The italicized lines 126 and 127 of this appendix provide the effect codings that enable us to compare, in the Minitab program, southerners against non-southerners, while holding constant for time, education, and the interaction terms.

The coefficients associated with the eight effects of model 10 are inserted into lines 109 through 116 of the Minitab listing. These coefficients are found in the ESTIMATES FOR PARAMETERS section of the SPSS output, and are analogous to regression coefficients. As in the case of regression analysis, we obtain predicted scores for the DV by multiplying scores by their coefficients. This is done in line 120 of the Minitab program; for reasons explained by Knoke and Burke (1980:24-27), results are multiplied by 2. When all these effects are added together, we obtain recognizable loglinear output: the logarithm of the odds that this particular type of
respondent supported equal job opportunity for blacks. Transforming the logarithm into actual odds, we see that these odds are .8, and this result corresponds to what we observe in the EXP. COUNT & PCT. segment of the SPSS appendix; the expected counts are derived from this regression-like procedure.

At line 158 of the Minitab listing, we begin exactly the same process for college-educated non-southerners interviewed in 1946.

```
100MTB > # FOLLOWING ARE RESULTS FOR 1946, COLLEGE, SOUTH:
  MTB >
  MTB > NAME C1 'PARAM'
  MTB > SET C1
  DATA> 1 1 -1 -1 1 -1 -1 1
105DATA> END
  MTB >
  MTB > NAME C2 'COEFF'
  MTB > SET C2
  DATA> .321346 [Coefficient for JOBS]
  DATA> -.455680 [Coefficient for JOBS BY TIME]
  DATA> -.344070 [Coefficient for JOBS BY EDUC_1]
  DATA> -.000893 [Coefficient for JOBS BY EDUC_2]
  DATA> -.226303 [Coefficient for JOBS BY REGION]
  DATA> .105408 [Coefficient for JOBS BY TIME BY EDUC_1]
115DATA> -.065779 [Coefficient for JOBS BY TIME BY EDUC_2]
  DATA> -.059111 [Coefficient for JOBS BY TIME BY REGION]
  DATA> END
  MTB >
  MTB > NAME C3 'PROD'
120MTB > LET C3=C1*2*C2
  MTB > # NOTE THAT COEFFICIENTS MUST BE DOUBLED
  MTB >
  MTB > SUM (C3) K1
  SUM = -0.22883
125MTB > # THIS IS -.23, FROM TABLE 1,
  MTB > # COL. 13, SWAFFORD (1980)
  MTB > EXPO K1 K2
  ANSWER = 0.7955
  MTB > PRINT K2
130K2 0.795465
  MTB > # THIS IS THE ANTILOG OF -.23,
  MTB > # I.E. ODDS FOUND IN COL. 12, SWAFFORD (1980)
  MTB >
  MTB > PRINT C1-C3
135
  ROW PARAM COEFF PROD
  1 1 0.321346 0.642692
```
MTB >
MTB > LET K10=41.2/51.8
MTB > # PREDICTED ODDS FOR THOSE CODED 1 3 1 IN I.V.'S
150MTB > PRINT K10
   K10  0.795367
MTB > # THIS IS THE .79 FROM ABOVE
MTB > LOGE K10 K11
   ANSWER =  -0.2290
155MTB > # THIS IS THE LOG OF THE ODDS RATIO FOR THOSE
MTB > # CODED 1 3 1--IT IS -.23, AS DERIVED ABOVE
MTB >
MTB > # FOLLOWING ARE RESULTS FOR 1946, COLLEGE, NON-SOUTH:
160MTB > NAME C1 'PARAM'
MTB > SET C1
DATA>
DATA> 1 1 -1 -1 -1 -1 -1 -1
DATA> END
165MTB >
MTB > NAME C2 'COEFF'
MTB > SET C2
DATA> .321346
DATA> -.455680
170DATA> -.344070
DATA> -.000893
DATA> -.226303
DATA> .105408
DATA> -.065779
175DATA> -.059111
DATA> END
MTB >
MTB > NAME C3 'PROD'
MTB > LET C3=C1*2*C2
180MTB > # NOTE THAT COEFFICIENTS MUST BE DOUBLED
MTB >
MTB > SUM (C3) K1
   SUM  =  0.91283
MTB > # THIS IS .91, FROM TABLE 1,
185MTB > # COL. 13, SWAFFORD (1980)
MTB > EXPO K1 K2
For southerners with a college education in 1946, the logit (logarithm of the odds) for agreeing with the JOBS item is -.23, and the actual odds are the antilog of this value, which is .8. The .8 equals the predicted agree/disagree ratio for these respondents; in the italicized rows of the EXP. COUNT column of the SPSS appendix, we verify that these odds are 41.2/51.8=.8.

For non-southerners who were college-educated in 1946, the logit for agreeing with the JOBS item is .91, and the odds or antilog is 2.49. The 2.49 equals the predicted agree/disagree ratio for such non-southerners; in the EXP. COUNT column of the SPSS appendix, we verify that these odds are 204.8/82.2=2.49.

In 1946, then, the REGION variable had a large impact among the college-educated, shifting the odds from .8 to 2.49. For 1963 we anticipate a similar effect, but it is likely to be slightly different due to the fact that, as we said in
interpreting our causal model, “... TIME has a different impact on JOBS depending on EDUC (or vice versa), and TIME also has a different impact on JOBS depending on REGION (or vice versa).” If we examine the odds for college-educated respondents interviewed in 1963, and compare them as to region, we find the following odds ratios based on the EXP. COUNT column. It will be convenient to repeat the two odds ratios discussed above:

FOR 1946, COLLEGE, NON-SOUTH: 204.8/82.2=2.49  
FOR 1946, COLLEGE, SOUTH: 41.2/51.8=.8  
FOR 1963, COLLEGE, NON-SOUTH: 195.31/13.69=14.27  
FOR 1963, COLLEGE, SOUTH: 60.69/8.31=7.30

There is still a gap between southerners and non-southerners, but the 1963 gap, 14.27/7.3 is smaller than the 1946 gap, 2.49/.8. This result is consistent with the following interpretation: Both southern and non-southern respondents—at least among the well-educated—are more supportive of equal job opportunity in 1963 than in 1946, and the increase of such support has been more rapid for southerners than for non-southerners. It would be interesting to measure these same trends among those at the low or intermediate educational levels.

Many such comparisons and interpretations are possible, even with this relatively simple set of variables. Standard graphic techniques would enable us to compare various ratios. Here, for instance, is a Minitab histogram comparing two of the groups mentioned above. The zeros and ones are not really “midpoints.” They serve to remind us of the original coding for the DV: Zero indicates support for equal job opportunity for blacks.

<table>
<thead>
<tr>
<th>Midpoint</th>
<th>Count</th>
</tr>
</thead>
<tbody>
<tr>
<td>0</td>
<td>61</td>
</tr>
<tr>
<td>1</td>
<td>8</td>
</tr>
</tbody>
</table>

Sets of four histograms could be used to make comparisons involving interactions.
(2.4) Abortion and the single woman: Sources of opposition

Using data from the 1988-90 General Social Survey (GSS), I carried out a loglinear analysis of factors that may influence attitudes regarding legal abortions for unmarried women. From the 4390 respondents for the three years, I selected 2789 who had answered “yes” or “no” to a question worded as follows: “Please tell me whether or not you think it should be possible for a pregnant woman to obtain a legal abortion if she is not married and does not want to marry the man.” Four variables were defined:

AB: Based on responses to the question above; coded 1 for those who answered “yes” and 0 for those who answered “no”;
FEMALE: Coded 1 for females and 0 for males;
BLACK: Coded 1 for blacks and 0 for all others;
REL: Coded 1 for respondents who attend religious services with high frequency, 0 otherwise.

Following are the SPSS loglinear commands:

```
LOGLINEAR AB (0,1) BY BLACK (0,1) FEMALE (0,1)
   REL (0,1)/
   PRINT=ALL/
   DESIGN/ /* SATURATED MODEL, MODEL 1
   DESIGN= /* START BACKWARD ELIMINATION, MODEL 2
      AB,
      AB BY BLACK,
      AB BY FEMALE,
      AB BY REL,
      AB BY BLACK BY FEMALE,
      AB BY BLACK BY REL,
      AB BY FEMALE BY REL/
   DESIGN= /* BACKWARD ELIMINATION, MODEL 3
      AB,
      AB BY BLACK,
      AB BY FEMALE,
      AB BY REL/
   DESIGN= /* BACKWARD ELIMINATION, MODEL 4
      AB/
   DESIGN= /* BACKWARD ELIMINATION, MODEL 5
      AB,
      AB BY BLACK,
      AB BY FEMALE,
      AB BY REL,
      AB BY BLACK BY FEMALE/ /* 1 WITH 2
   DESIGN= /* BACKWARD ELIMINATION, MODEL 6
```
AB,
AB BY BLACK,
AB BY FEMALE,
AB BY REL,
AB BY BLACK BY REL/ */ 1 WITH 3
DESIGN= /* BACKWARD ELIMINATION, MODEL 7
AB,
AB BY BLACK,
AB BY FEMALE,
AB BY REL,
AB BY BLACK BY FEMALE BY REL/ /* 2 WITH 3
DESIGN= /* BACKWARD ELIMINATION, MODEL 8
AB,
AB BY BLACK,
AB BY FEMALE,
AB BY REL,
AB BY BLACK BY FEMALE, /* 1 WITH 2
AB BY BLACK BY REL/ /* 1 WITH 3
DESIGN= /* BACKWARD ELIMINATION, MODEL 9
AB,
AB BY BLACK,
AB BY FEMALE,
AB BY REL,
AB BY BLACK BY FEMALE, /* 1 WITH 2
AB BY FEMALE BY REL/ /* 2 WITH 3
DESIGN= /* BACKWARD ELIMINATION, MODEL 10
AB,
AB BY BLACK,
AB BY FEMALE,
AB BY REL,
AB BY BLACK BY REL, /* 1 WITH 3
AB BY FEMALE BY REL/ /* 2 WITH 3
FINISH

These ten models parallel those presented by Swafford (1980) in his Table 3, and I shall therefore present my results (Table 4) using essentially the same format. Model 6 appears to be the most promising: It is more parsimonious than the other plausible (i.e., non-rejectable) models, and it has a low chi-square value. It appears initially, then, that attitudes about abortion are influenced by race, sex, and religiosity, and by the interaction of race and religiosity. The loglinear coefficients for the last two terms are especially strong, with statistically significant z-values: The coefficients are -.23 for religiosity and -.13 for race by religiosity.

TABLE 4 ABOUT HERE
As before, the Minitab program will help us to interpret these results:

```
MTB > # FOLLOWING ARE RESULTS FOR BLACK=YES, FEMALE=YES,
MTB > # REL=HIGH:
MTB >
MTB > NAME C1 'PARAM'
MTB > SET C1
DATA> 1 -1 -1 -1 1
DATA> END
MTB >
MTB > NAME C2 'COEFF'
MTB > SET C2
DATA> .2396
DATA> .0104
DATA> -.0226
DATA> -.2267
DATA> -.1275
DATA> END
MTB >
MTB > NAME C3 'PROD'
MTB > LET C3=C1*2*C2
MTB > # NOTE THAT COEFFICIENTS MUST BE DOUBLED
MTB >
MTB > SUM (C3) K1
   SUM = 0.70200
MTB > # THIS IS THE LOGIT
MTB >
MTB > EXPO K1 K2
   ANSWER = 2.0178
MTB > PRINT K2
K2  2.0178
MTB > # THIS IS THE ANTILOG OF THE ABOVE LOGIT
MTB > # I.E. ODDS FOUND IN EXP. COUNT COLUMN
MTB >
MTB > PRINT C1-C3
   ROW  PARAM  COEFF  PROD
     1     1  0.2396  0.4792
     2    -1  0.0104 -0.0208
     3    -1 -0.0226  0.0452
     4    -1 -0.2267  0.4534
     5     1 -0.1275 -0.2550
MTB >
MTB > LET K10=60.85/30.15
MTB > # PREDICTED ODDS FOR THOSE CODED 1 1 1 IN I.V.'S
MTB > PRINT K10
```
K10 2.01824
MTB > # THIS IS THE ANTILOG FROM ABOVE
MTB > LOGE K10 K11
    ANSWER = 0.7022
MTB > # THIS IS THE LOG OF THE ODDS RATIO FOR THOSE
MTB > # CODED 1 1 1, AS DERIVED ABOVE
MTB >
MTB > # FOLLOWING ARE RESULTS FOR BLACK=YES, FEMALE=YES,
MTB > # REL=LOW:
MTB >
MTB > NAME C1 'PARAM'
MTB > SET C1
DATA> 1 -1 -1 1 -1
DATA> END
MTB >
MTB > NAME C2 'COEFF'
MTB > SET C2
DATA>
DATA> .2396
DATA> .0104
DATA> -.0226
DATA> -.2267
DATA> -.1275
DATA> END
MTB >
MTB > NAME C3 'PROD'
MTB > LET C3=C1*2*C2
MTB > # NOTE THAT COEFFICIENTS MUST BE DOUBLED
MTB >
MTB > SUM (C3) K1
    SUM = 0.30520
MTB > # THIS IS THE LOGIT
MTB >
MTB > EXPO K1 K2
    ANSWER = 1.3569
MTB > PRINT K2
K2 1.35690
MTB > # THIS IS THE ANTILOG OF THE ABOVE LOGIT
MTB > # I.E. ODDS FOUND IN EXP. COUNT COLUMN
MTB >
MTB > PRINT C1-C3

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MTB >
MTB > LET K10=74.26/54.74
MTB > # PREDICTED ODDS FOR THOSE CODED 1 1 0 IN I.V.'S
MTB > PRINT K10
K10 = 1.35659
MTB > # THIS IS THE ANTILOG FROM ABOVE
MTB > LOGE K10 K11
RESULT = 0.3050
MTB > # THIS IS THE LOG OF THE ODDS RATIO FOR THOSE
MTB > # CODED 1 1 0, AS DERIVED ABOVE
MTB > STOP

For black females who are high on religiosity, the logit (logarithm of the odds) for opposing abortion is .70, and the actual odds are the antilog of this value, which is 2.02. The 2.02 equals the predicted NO/YES ratio for such women; in the EXP. COUNT column of the SPSS output, these odds are 60.85/30.15=2.02. [ratio 1]

For black females who are low on religiosity, the logit (again: logarithm of the odds) for opposing abortion is .31, and the actual odds are the antilog of this value, which is 1.36. The 1.36 equals the predicted NO/YES ratio for such women; in the EXP. COUNT column of the SPSS output, we see that these odds are 74.26/54.74=1.36. [ratio 2] Among black females, then, high religiosity increases the ratio of NO responses to YES responses.

The following NO/YES ratios show the predicted impact of religiosity among white females:

For BLACK=0, FEMALE=1, REL=1: NO/YES=423/121=3.5 [ratio 3]
For BLACK=0, FEMALE=1, REL=0: NO/YES=387/455=.85 [ratio 4]

Again, high religiosity among white females increases the ratio of NO to YES responses. Similar calculations would show the impact of religiosity among white males or black males. One could also show, for instance, the impact of race while sex and religiosity are held constant, but it turns out that race and sex, acting alone, do not have a statistically significant impact on this particular attitude toward abortion.

There is, however, an important interaction, and this interaction does involve race. The interaction is such that the impact of race varies when religiosity (along with sex) is held constant—or vice versa. For instance:
Among males who are high on religiosity, being black tends to reduce opposition to abortion. Among males who are low on religiosity, being black tends to increase opposition to abortion. Sociologically, it is this sort of interaction that is most intriguing. What is it that occurs in the social situations of many highly religious black men that apparently creates in them a relatively strong concern about abortion rights for unmarried females? Why—to reverse the question—are religiously active white men more likely to oppose abortion rights? Perhaps there is a problem with our definition of religiosity: High frequency of participation in religious activities may imply fundamentalism among whites more strongly than among blacks. Using the GSS, we could readily experiment with various definitions of religiosity, and we could also experiment with other aspects of abortion. The GSS asks, for instance, about legal access to abortion among women who have “a very low income.” Loglinear methods, of course, have not yet answered these questions, and may not be able to do so; without the methods, however, it is entirely possible that we would not have arrived at the questions. Similar inquiries, of course, would have to be raised with regard to female respondents.

Halli and Rao (1992: Chapter 6) present a clever method for selecting the best loglinear model from a set of several candidates; along with the use of significance tests and considerations of parsimony, their approach involves comparison of the chi-square values generated by various models. In interpreting results, however, they might benefit from the use of techniques outlined above. In a loglinear analysis of the impact of social class, motivation (“effort”), and IQ on grades, Halli and Rao (1992:131) say that the best-fitting model suggests that “... social class has the most substantial effect on the odds of getting high grades ...,” and that “... the only circumstance in which the odds are favorable for lower-class students getting high grades (i.e., odds greater than 1) is when they show both high effort and high IQ.” But since the model involves complex effects including interactions between class and effort, and between class and IQ, it would be much more edifying to create comparisons such as those made above. What, for instance, are the academic prospects for lower-class students who are bright but under-motivated, as compared with middle-class students with the same characteristics:
We see that poorly motivated, bright, middle-class students are about four times more likely to achieve high grades than poorly motivated, bright, working-class students. One hopes that working-class students of high ability will be clever enough to realize that they have limited opportunities for slacking off.

**3) Calculus quicker**

Minitab programs are written in a variant of BASIC, and all students should probably learn some basic BASIC. The first program below, involving differentiation, has only 22 lines—unless I miscounted. I have appended another program, with about the same number of lines, that produces a normal curve—ubiquitous in Stat I. When my students add together the (leftward) Y values for this function, they are in essence doing integral calculus by finite means, and they are also learning the source of the table of areas under the normal curve, also ubiquitous in Stat I. That is, they understand and learn to use this table by doing poor-man's integration.

**3.1) First program: Differentiation**

In a medium-sized city, the traffic division of the police department issues 2000 speeding tickets per year; the standard fine for speeding is $150. The traffic division has been persuaded that 2000 is too high a rate of deviance, and they now wish to deter a substantial number of these offenders. Simultaneously, the division wishes to maximize its revenues from speeding tickets, because these revenues pay the bills. How does the division achieve both of these apparently contradictory goals simultaneously?

The sociologist working for the division has evidence from research that, in cities of this type and size, every increase of $25 (k1) in the standard speeding fine will deter approximately 200 (k2) additional potential offenders, especially if the new fine has high “salience.” The sociologist does not necessarily believe these findings—they are essentially regression weights, which we never fully trust—but she's willing to experiment.
The above paragraphs would describe any situation where one is trying “to charge what the traffic will bear,” with the proviso that the traffic, for political reasons, must be reduced. For instance, substitute “Urbanna, Virginia” for “medium-sized city,” and grapple with the problem of trying to reduce vehicles per hour entering the city during the annual oyster festival. The goal is to reduce traffic while maximizing revenue.

Think of other examples in which the goal is to reduce the rate of some sort of deviant behavior while simultaneously maximizing revenue on the assumption that each instance of deviance adds to revenue. (Start by listing instances in which deviance does indeed add to somebody’s revenue.)

NB: Before running this program, set k1 and k2. These constants may have some sensitivities to range, but I don’t yet know what they are. Don’t stray too far from the values suggested above.

100MTB > exec 'speed'
      MTB >
      MTB > erase c1-c21
      MTB >
      MTB > set c1
105DATA> 0:400/8
      DATA> end
      MTB > name c1 'newfine'
      MTB >
      MTB > # (1) Calculate the number of potential offenders who will be
110MTB > # deterred by the increased fine:
      MTB >
      MTB > let c2=k2*('newfine'-150)/k1
      MTB >
      MTB > let c3=((k2/k1)*'newfine')-((k2/k1)*150)
115MTB > name c3 'newdeter'
      MTB >
      MTB > # (2) Calculate the new total number of offenders:
      MTB >
      MTB > let c4=2000-c3
120MTB >
      MTB > let c5=2000-((k2/k1)*'newfine')-((k2/k1)*150)
      MTB > name c5 'offnders'
      MTB >
      MTB > # (3) Then, by substitution:
125MTB >
      MTB > let c11='newfine'*c5
      MTB >
      MTB > let c12='newfine'*((2000-((k2/k1)*'newfine') & CONT>  -((k2/k1)*150)))
MTB >
MTB > let c13=(2000*'new fine')-((k2/k1)*'new fine'**2) &
MTB > +((k2/k1)*150*'new fine')
MTB > # The above equation is differentiable--see below.
MTB > name c13 'revenue'

MTB > plot 'revenue' 'new fine'

---
0 80 160 240 320 400

---
0+ *
150 - *
150 - *
150 - *
150 - *
100000+ *
145 - **
145 - *
145 - *
145 - *
140 - ***
140 - **
140 - **
140 - **
300000+ *******

MTB > plot 'offnders' 'newfine'

160
   _ **
3000+   ***
   -        ***
   offnders-   ***
165
   -        ****
   -
2000+   ***
   -
170
   -
   -
1000+   ***
   -
   -
175
   -
   -
   0+   **

+---------------------------------------------+-------
|                       newfine                 |
180
| 0   | 80 | 160 | 240 | 320 | 400 |
+---------------------------------------------+-------

MTB >
MTB > # (4) Obtain the derivative of 'revenue' as a function of
MTB > # 'newfine', set it to zero, and obtain the new fine:

185MTB >
MTB > let c21=2000+((k2/k1)*150)-(2*(k2/k1)*'newfine')
MTB > name c21 'deriv'
MTB >
MTB > # (5) Calculate the new fine:

190MTB >
MTB > let k3=(2000+((k2/k1)*150))/(2*(k2/k1))
MTB >
MTB > # (6) Calculate the new level of revenue:
MTB >

195MTB > let k4=k3*(2000-((k2/k1)*(2000+((k2/k1)*150))/(2*(k2/k1))))&
CONT> -((k2/k1)*150))
MTB >
MTB > # (7) Calculate the new number of offenders:
MTB >

200MTB > let k5=k4/k3
MTB >
MTB > # k3 is the new fine, k4 is the new level of revenue, k5 is
MTB > # the new number of offenders
MTB >

205MTB > print c1 c3 c5 c13 c21 k1-k5
K1  25.0000
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Now, suppose that the sociologist explores the model in which $k_1=25$ and $k_2=200$, and sets the speeding fine at $200$. Assume that it turns out that, because the actual regression coefficient for deterrence is proportional to $k_2/k_1=200/15$, not $200/25$, she has made an error. That is, the deterrent is more powerful than she realizes on the basis of her perusal of past research, and it only takes a $15$ increase in the fine to deter an additional 200 potential offenders. What would be the consequences of this error, in terms of the number of offenders and the total revenue? How could the error be repaired?

Here, our sociologist has to go to work and become an interpretationist: She has to decide whether the substantial deterrent impact of the recent increase of the speeding fine is such that, even though she must accept disappointingly low revenues from traffic tickets, it might be possible to parlay the successes of the deterrence program into more generous support from sources other than ticket revenues. Or, she could go back to the old fine. How much additional revenue could she obtain by this latter strategy? What would be the costs of this strategy? What is to be done?

### (3.2) Second program: Integration

100MTB > # Declare k4 = interval for x, if not given below
MTB > let k4=1
MTB >
MTB > erase c1-c32
MTB >
105MTB > let k2=7 # standard deviation
MTB >
MTB > set c1
DATA> 0:50/k4
DATA> end
110MTB >
MTB > name c1 'x'
MTB >
MTB > let k1=1/(k2*sqrt(2*k100))
MTB > let c2=((c1-25)**2)/(2*(k2**2))
115MTB > expo c2 c3
MTB > let c4=1/c3
MTB >
MTB > let c5=k1*c4
MTB >
120MTB > name c5 'y'
MTB >
MTB > plot 'y' 'x'

_                     _                     _
125 0.060+            130 0.040+            135 0.020+            140 0.000+
         *****           * * *              * * *              * * *          ******
         |                  |                  |                  |                  ********
  Y       |                  |                  |                  |                  +------------------------------
_                     _                     _                     _
  0        10         20         30         40         50

145MTB > sum 'y' k3
      SUM =  0.99973
MTB > let k5=k3/k4
MTB >
MTB > # print c1-c5
150MTB > # print k1-k5 k99-k100
MTB > # print 'x' 'y'
MTB >
MTB > copy 'x' 'y' c31 c32;
SUBC> use 'x' 25:32.
155MTB > sum c32 k11
      SUM =  0.38671
MTB > # print c31 c32
MTB > let k12=k11*k4
This final convergence illustrates processes based on integration, and it also shows students the source of the statement that, say, “34 per cent of the area under the normal curve lies between the mean and one standard deviation,” or that “5 per cent of the area under the normal curve lies above 1.65 standard deviations,” etc. It is essential that students know the meaning of these sorts of statements when they test hypotheses, and it is possible that an understanding of simple integration would help them interpret such statements correctly. Students can cause the constant k12 to converge on the value .3413 by calibrating the X-axis more finely, i.e., by breaking it up into smaller segments. The constant defined in the first two lines of the program, k4, determines the number of segments into which the X-axis is divided. If this constant is set, say, to k4=.1, then k12 becomes .3459, which is much closer to its theoretical value, .3413. The constant k4 could be reduced in small decrements.

In brief: A good theoretical chemist is a better chef than a hash slinger, *ceteris paribus*. 

NOTES

1 The first segment of the chase-around chart actually uses the three-dimensions-in-perspective technique of the textbook citations given earlier. Temperature provides the X-axis, takeoff distance provides the vertical Y-axis, and airport altitude could be represented on a Z-axis extending upward from the surface of the graph. The upward-sloping lines of this segment should be thought of as the underside of a regression plane—although this “plane” is concave upward. The curved line beginning at the “ISA” label represents the fact that, while both high temperature and high altitude bring about an increase in takeoff distance, these two IV's are inversely related to each other. In my opinion, the graphic would serve aviators better if the first segment were subdivided into two separate segments that would behave in the manner of the third, fourth, and final segments. This is my procedure for Figure 2, and mixing the two techniques seems unwarranted.

2 Blau and Blau (1982:124), for instance, develop a six-variable equation that gives violent crime rates for cities as a function of population size, general income inequality, socioeconomic inequality by race, percentage of the population divorced, and percentage black. If one wished to experiment with a negative coefficient, percentage black could be changed to percentage having majority racial background.

3 Swafford's tables are reproduced in the appendices of this article.

4 When we designate a given variable as dependent on several others, the number of logically possible models proliferates fast. For instance, if we wished to evaluate the dependence of Y on four other variables, A through D, we would have the following series of testable models. Swafford's use of “backward selection” would not help us in this instance: It is better to list from the least complex to the most complex models.

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<th>Symbols</th>
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<td>Y YA</td>
</tr>
<tr>
<td>3</td>
<td>Y YB</td>
</tr>
<tr>
<td>4</td>
<td>Y YC</td>
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</table>
We are not yet finished, but already we see an emergent pattern. A table of binomial coefficients would tell us that there is only one model in which A through D have no effect, four models (2 through 5) in which only one of the IV's has an effect, six models (6 through 11) in which two of the IV's have an effect, and so forth. We now have sixteen testable models, but none involves interaction among the four IV's.

The same table of binomial coefficients tells us that, given four IV's, there are six ways in which these IV's may interact two at a time:

(AB)
(BC)
(CD)
(AC)
(BD)
(AD)

A given model could contain any combination of the six bivariate interactions. The binomial table tells us that there are $1 + 6 + 15 + 20 + 15 + 6 + 1 = 64$ ways in which such combinations could be selected. Here are a few:

17 (none)
Because these models are hierarchic, the more complex terms must subsume their components. Model 80, then, should really be written as follows:

\[
\begin{align*}
\text{80} & \quad Y \ YA \ YB \ YC \ YD \ Y(AB) \ Y(BC) \ Y(CD) \ Y(AC) \\
& \quad Y(BD) \ Y(AD)
\end{align*}
\]

There may also be interactions involving three IV's. Again, the binomial table tells us that there are four ways in which three out of four IV's could interact: (ABC) (BCD) (ACD) (ABD). There are \(1+4+6+4+1=16\) combinations of these four ways:

\[
\begin{align*}
\text{81} & \quad \text{(none)} \\
\text{82} & \quad \text{(ABC)} \\
\text{83} & \quad \text{(BCD)} \\
\text{84} & \quad \text{(ACD)} \\
\text{85} & \quad \text{(ABD)} \\
\text{86} & \quad \text{(ABC) (BCD)} \\
\text{87} & \quad \text{(BCD) (ACD)} \\
& \quad \\
& \quad \\
\text{96} & \quad \text{(ABC) (BCD) (ACD) (ABD)}
\end{align*}
\]
Again, model 96 (along with others) would have to subsume simpler terms, all of which are found in model 80:

\[
\begin{align*}
96 & \quad Y Y A Y B Y C Y D Y(AB) Y(BC) Y(CD) Y(AC) Y(BD) Y(AD) \\
& \quad Y(ABC) Y(BCD) Y(ACD) Y(ABD)
\end{align*}
\]

Finally, model 97 is the saturated model, permitting an (ABCD) interaction. Given that models 17 and 16 are identical, as are models 81 and 80, we conclude that there are 95 possible ways to capture the impact of four IV's on a DV. There are really more than this: For instance, model 82 would have to subsume all its components in terms of A, B, and C, but it may or may not subsume YD. If YD were thought to be essential to the specification, it would be appropriate to include it.

When there are three IV's, as in Swafford's example, the procedure just followed will generate sixteen models. Swafford only considers ten of these instances, but this is because he eliminates \textit{a priori} the three possibilities in which only one of the three IV's has an impact, along with the three possibilities in which two of the three IV's have an impact. If one were to find support for Swafford's model 4 (J JT JE JR), it might be appropriate to make additional tests in order to assess the behavior of each IV independently of the other two.
REFERENCES


Table 1. A superbly short takeoff, single aircraft

Standard scores for five IV's (Minitab simulation):
-1.46591  1.15241  -2.27038  1.28129  -0.72981

Required takeoff distance (Minitab simulation):
1923.42 feet

Calculations through each of five segments of (complete) Figure 1, based on the regression equation given above:

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<th>(1) variable</th>
<th>(2) slope</th>
<th>(3) z-score</th>
<th>(4) product (2)*(3)</th>
<th>(5) adjustment for each independent variable</th>
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Table 2. An example drawn from Gilbert (1981)

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<th>Prob.</th>
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<tr>
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Table 3. An example drawn from Swafford (1980)

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Note: Asterisks indicate that alpha < .01, leading to rejection of model.
Table 4. Attitude toward abortion as a function of sex, race, and religiosity

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<th>Prob.:</th>
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</table>

Note: A=AB, F=FEMALE, B=BLACK, R=REL (see text); asterisks designate plausible (non-rejectable) models.

==============================================
SPSS APPENDICES:
(1) JOBS by TIME, EDUCATION, and REGION

| CORRESPONDENCE BETWEEN EFFECTS AND COLUMNS OF DESIGN/MODEL 10 |
|-------------------------|-------------------------|-------------------------|
| STARTING COLUMN | ENDING COLUMN | EFFECT NAME |
| 1 | 1 | JOBS |
| 2 | 2 | JOBS BY TIME |
| 3 | 4 | JOBS BY EDUC |
| 5 | 5 | JOBS BY REGION |
| 6 | 7 | JOBS BY TIME BY EDUC |
| 8 | 8 | JOBS BY TIME BY REGION |

---

DESIGN MATRIX

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Page 193 of 199
### Observed, Expected Frequencies and Residuals

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**Goodness-of-Fit Test Statistics**

- Likelihood Ratio Chi Square: $8.28522$, DF = 4, P = 0.082
- Pearson Chi Square: $8.52126$, DF = 4, P = 0.074
### ESTIMATES FOR PARAMETERS

#### JOBS

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#### JOBS BY TIME

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<th>Std. Err.</th>
<th>Z-Value</th>
<th>Lower 95 CI</th>
<th>Upper 95 CI</th>
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#### JOBS BY EDUC

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#### JOBS BY REGION

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#### JOBS BY TIME BY EDUC

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<tbody>
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#### JOBS BY TIME BY REGION

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(2) ABORTION ATTITUDE by RACE, SEX, and RELIGIOSITY

OBSERVED, EXPECTED FREQUENCIES AND RESIDUALS

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<td>30.15 (33.14)</td>
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</tbody>
</table>
NOTES: REFERENCES AND GENERAL BIBLIOGRAPHY:


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Page 29: describes a piece-rate pay system whereby drivers were paid 27 per cent of the company’s price for hauling a load; Page 33: describes a company that demanded that drivers observe the (then) 55 mph speed limit, and even used tachographs to enforce this; Time pressure: Pages 44, 47, 48-49, 62, 69, 70; Page 70: “Some [drivers] exceeded the speed limit almost constantly, often at gross levels …, and did so at night when it is difficult to spot police. … The paradox here is that speeding had the effect of reducing pay while inviting expensive citations by the police, and accumulating several of these could result in license suspension. Speeding also was especially dangerous in the type of trucks PetroHaul operated …”; Page 155: drivers often believe that police harass them for unfair reasons, e.g., working for a small company or being owner-operators themselves; Page 160: O. talks about speeders going up to 80 mph; Page 163: “… drivers for nonunion companies do not have the protections afforded their unionized counterparts and therefore are less able to avoid having to operate illegally … In effect [due to their poor safety records, etc.], the organization of work in the competitive [nonunion] sector tends, over a period of time, to lock the driver into both that sector and its second-rate companies.”; does this apply in aviation?


