WHAT'S WRONG WITH THE SOCIAL SCIENCES?
The Perils of the Postmodern

Michael A. Faia
College of William & Mary
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For Caitlin, Josephine, Gusty, and Pancho

Et si je connais, moi, une fleur unique au monde,
qui n'existe nulle part, sauf dans ma planète,
et qu'un petit mouton peut anéantir d'un seul coup,
comme ça, un matin, sans se rendre compte de ce qu'il fait,
ce n'est pas important ça!

—Antoine de Saint-Exupéry
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Introduction

The contention of this book is that the contemporary social sciences in America have the qualities of a 1936 Cadillac V-12 sedan making a belated trip to the repair shop: The classic lines, the elegance, the power; the smooth, sonorous forward thrust usually evident, but momentarily interrupted by an assortment of minor failures and afflictions, as yet undiagnosed, brought about by many years of not entirely benign neglect, by a shortage of replacement parts, and by bad luck; the unhappy proprietors vaguely conscious of flashier models whizzing past wildly, their half-crazed occupants oblivious of the nature of real workmanship, real quality, real substance, real inspiration, real art; and finally the hope, the faith, that a few new wires here and there, a better thermostat that no longer permits the cooling system to overheat, a little grease pumped into the strange sounding transmission, a few carefully sewn patches over nicely tucked but tattered upholstery—and once again we take to the highways, once again we have a touring car that gives us, and gives our passengers, free and easy access to exotic places that we do not ordinarily expect to see. As far as I'm concerned there never will be a trade-in: The new models do not impress me, I love the old Caddy, and I'm willing to ante up in order to get her fixed.

By 1936, the year of my vintage Cadillac, market segmentation had not yet been discovered (or invented). Detroit had not yet begun producing cars designed mainly for the female market, let alone cars for white female yuppies earning $70,000 or more per year, or cars for rural retired elderly gentlemen of Latin-American extraction and with a need for high motility in a harsh climate, or cars for wives of young black blue-collar aristocrats with strong Republican proclivities, an overweight mortgage, and two kids in private schools. The 1936 Cadillac was classy, and it was also androgynous: It combined the best of the masculine with the best of the feminine. Achieving such a combination is one of the central intentions of this book, which originally was to have been entitled Androgynous Sociology.

Chapters 1 and 2 argue that, for mysterious reasons, a vociferous anti-science movement has developed in recent years among students of society. While the size, structure, and sources of this movement remain obscure—although I think feminist theory and qualitative methodology have given it impetus—it has attracted the support of several influential scholars within the social science disciplines. The movement may be an attempted adaptation to a contemporary state of affairs in which, according to a recent President of the American Sociological Association, the field finds itself “in the doldrums.” These opening
chapters anticipate remaining parts of the book by arguing (1) that the social sciences already have achieved successes that rival those of the natural sciences, and that the current round of self-criticism, bordering on self-flagellation, is therefore an inexplicable failure of nerve; (2) that anti-science, arising in part from math phobia, is an indirect and unjustifiable attack on quantitative structuralism; (3) that advocates of anti-science wish to focus primarily on questions that lie outside the realm of science, or that constitute the preliminaries of scientific inquiry, or that constitute only one aspect of scientific inquiry; (4) that advocates of anti-science misconstrue the similarities and differences between scientific and non-scientific thought patterns, and do not appreciate the interactions and complementarities of these thought patterns; (5) that the anti-science movement wrongly asserts an ineluctable tension between science and “interpretationism,” the latter exemplified by scholars such as Foucault, with their strong nominalist and logocentric tendencies; and (6) that the embattled advocates of interpretationism and structuralism should declare an immediate and open-ended truce.

Chapter 3, derived from my own brief history of functionalist theory (Faia, 1986: appendix), asserts that in recent years we have made a shibboleth of the belief that social science theories are inescapably invaded, pervaded, and distorted by one's values, ideology, social class background, current class situation, age, sex, race, and so forth; in brief, by one's “biases.” Sometimes the process is alleged to operate in reverse: One's theoretical commitments, for instance, are said to influence one's ideology. Because shibboleths tend to terminate thought, hardly anybody bothers to define and distinguish among the central terms of the discourse, such as theory, values, ideology, class background, gender. Nor is there strong concern about testing the shibboleth as if it were just another hypothesis, which it is.

I have found an intriguing way of illustrating my argument, presented mainly in Chapter 3, about bias. This illustration uses the idea of triangulation, an idea that is to be of central importance throughout this book. When seismologists try to find the epicenter of an earthquake, they use triangulation. A given seismographic observation station, analyzing the various wave series set in motion through the earth's crust by an earthquake, ascertains the distance from the station to the epicenter; the direction cannot be ascertained. A circle is then drawn around the station, with a radius equal to this measured distance, and it is assumed that the circle, at some point, must touch the epicenter. If a second station follows the same procedure, the two circles usually will intersect at two points, one of which is the epicenter. A third station, swinging a similar arc, then decides between these two intersections. If the process does not work, then presumably somebody's measurements are off; or, something may be wrong with theories about earthquake foci, epicenters, seismic wave behavior, etc.¹

Now, go to your blackboard. Draw a diagram of this elaborate search process. You will see that three seismographic stations do indeed enable us to find an
epicenter. Then, erase one of the stations, along with the circle drawn around it. Using information from the other two stations, and retaining your knowledge of the location of the epicenter, try to determine the position of the third seismographic station. Voilà! It is harder to find than Graham Greene’s third man; in fact, one cannot find it. My (null) hypothesis, then, derived from geology, is that the perspectives adopted as we interpret the real world, as we try to locate what we consider the major social epicenters, will not necessarily tell our readers anything about our own location, our own presuppositions, even if readers have an epicenter and two snitches.

There are excellent social scientists who have repeated the truism about large and inescapable biases so many times that they now accept it as gospel, virtually without reservation, and my fear is that the habit tends to induce give-up-itis, or at least to contribute to the failure of nerve mentioned above. Why should we insist that our science has any special merit, when it is as fallible as any other system of thought? In Alan Sica’s (1988:257) sad claim—the sadness is mitigated by the fact that Sica is probably wrong—social scientists do not know much more about human fertility than do the more militant members of pro-choice or pro-life movements.

Scientists are people who have special skills in semantics, syntactics, and empirics (Hepp, 1956). In Chapter 4 we argue that although social scientists are relatively weak in the area of semantics—the discipline of definition and measurement—we are surprisingly sophisticated in the realms of syntactics and empirics. We have considerable skill in the use of deductive models of all sorts from Aristotelian syllogisms to the stable population model to modern statistics to kinship networks to the propositional calculi of learning theory. And in the realm of empirics, social scientists know a great deal about how to observe the world, about how to register social phenomena such as fertility, mortality, migration, marriage, divorce, crime, contagious disease, abortion, and economic activity, about how to conduct social surveys of all varieties from censuses to political “exit polls,” and about how to make plausible estimates, say, of the North American native population of 1492 (Thornton, 1987).

Among our most highly developed deductive models are those which enable us to make causal inferences, and this chapter explores the issues raised by modern scholars who suggest that the pursuit of causal explanation in the social sciences ought to be played down, if not abandoned.

In Chapter 5, we discuss “reverse” regression models and “forward” regression models—the latter often called simulations—as ways of testing causal ideas. For instance, ideas about ways in which the sex preselection of children might influence the sex ratio, ideas about monetary rewards and their allocation in bureaucratic settings such as the modern university, or the idea that it is possible to design a highly efficient, “optimal” traffic-control system for a central city. We then develop the argument that one of the major advantages of the American federated system, and perhaps of the new commonwealth that has
replaced the Soviet Union, is that these entities may provide many jurisdictions willing to carry out their own unique experiments. Modern nations and commonwealths should be conducting systematic social science experiments in large numbers, and we should be introducing the results of these experiments into our best social theories while simultaneously seeking opportunities to conduct “social engineering” (Henshel, 1976). We are not yet very good at social engineering, but we are much better than those who currently have the responsibility for it (Dye, 1990).

Chapter 6 argues that the best social science research of the future will make use of biosociological and cultural materialist perspectives within a functionalist/systemic theoretical framework, with appropriate attention to experimentation, to simulations that tie together the past, present, and future, and to interpretationism. This chapter tries to clinch a central argument: The most promising works of the social sciences excel primarily because they triangulate. They do so by combining a satisfactory conceptual scheme, appropriate empirics, and appropriate syntactics; by making sure that the syntactics of a given hypothesis are capable of predicting a fairly wide range of testable consequences; by using an assortment of instruments and methods to test a given hypothesis and its consequences; by invoking existing knowledge from several related scientific fields; by presenting findings in a way that exemplifies the skillful use of several different symbol systems—verbal systems, quantitative systems, those that are both verbal and quantitative (e.g., programming languages), and those that are neither (e.g., graphics, motion pictures).

The major purpose of chapter 7 is to evaluate the semantics of the social sciences by ascertaining whether the Lazarsfeld generalization-specification model, also known as the “elaboration model,” has been routinely and effectively used by social science researchers. If the Lazarsfeld model were widely exploited, then highly abstract assertions—e.g., “factors of type X tend to have a causal impact on variables of type Y”—would be tested for their applicability to a broad range of specific instances of both X and Y. For example, the literature discusses a number of situations in which deterrence allegedly increases conformity; generalizations have been derived from these specifications, and the generalizations seem to produce new specifications.

Using the Sociological Abstracts thesaurus of sociological concepts, in combination with online searches of the social science literature, this chapter assesses the extent to which researchers use systematic taxonomies and exploit them through application of generalization-specification strategies. Briefly, the chapter raises questions such as the following: First, do social science researchers and teachers, in a popular metaphor, tend “to mix apples and oranges”—and perhaps an occasional persimmon? Secondly, when they do so, do they arrive at generalizations applicable to broad categories? Thirdly, do they then test new hypotheses about narrowly delimited instances of these broad categories?
Chapter 7 concludes that, despite SA's listing of "broader" and "narrower" terms in relation to a given thesaurus entry, there is little evidence of the use of highly structured taxonomies in the social science literature.

When I speak of the structure of a taxonomy, I definitely intend the implication that the concepts of a scientific discipline, as Durkheim suggests, should have a sort of social organization. Durkheim would surely agree that a single concept has a lot in common with a single personality: It is an abstraction that cannot be clearly understood until it is related to the structures that surround it. The chapter concludes further that in creating workable taxonomies we would do well to begin with the limited classification schemes already available in fields such as "ecological demography" (Namboodiri, 1988), and that it would be wise for us to develop, early on, a taxonomy of time-series processes; the chapter then outlines preliminary steps toward both these objectives. The final section suggests that social science data of the future, gathered primarily by people who are not themselves social scientists, will come to us largely by way of telephone lines, optical cable, and satellite transmissions, and that we need to try to influence the organization of these masses of data.
NOTES (Introduction)

(1) I understand that it is also possible to find a “shadow zone,” 180 degrees away from an epicenter, where seismic waves suddenly disappear. Spencer (1910:83) gives a similar example: While standing by a lakeside in the moonlight, one sees “... flashes from the sides of separate wavelets ...,” but one's “... position in relation to certain wavelets brings into view their reflections of the moon's light, while it keeps out of view the like reflections from all other wavelets.”

(2) For instance, one would alternate between personal interviews and mailed questionnaires; among surveys and laboratory experiments and library investigations; between exploratory methods and approaches that are highly structured in their use of multivariate causal models, feedback, time-series data, etc. For a discussion of the relationship between theoretical and methodological aspects of research, see Faia (1986:128-41).

(3) When we extend the scope of a generalization, we are in essence applying Mill's method of agreement. In a paper written some years ago (Faia, 1968:117-8) I argued that while we make substantial use of Mill's method of concomitant variation (correlation) and his method of difference, we neglect the method of agreement. This claim is close to one of the central theses of this book: That while we have high skill in the realms of empirics and syntactics, we remain weak on the forms of conceptualization that support the method of agreement.
REFERENCES (Introduction)


Chapter 1
WHAT'S WRONG WITH THE SOCIAL SCIENCES?

The current controversy about science versus the humanities and the arts is ... quite absurd. The assumption seems to be that the advancement of science and especially of social science must be at the expense of the other intellectual, artistic, and religious pursuits of man. This is a preposterous assumption. It implies that human knowledge is a finite quantity so that any increase in one department must be at the expense of some other. Actually, the advancement of science can only liberate, stimulate, and advance also the arts.

—George Lundberg (1947:14-15)

As a field for cultivation, literature starts with two advantages: it deals with common experience, couched usually in common language, and it has a long tradition of being looked at and talked about. The corollary is that when the man of science finds it difficult or impossible to read Dickens, the deficiency is in him or in his training; but when the humanist finds it difficult or impossible to talk about science, the deficiency is in him, in his training, and also in the present state of the subject.

—Jacques Barzun (1964:26)

When scientists deal with each other, they should never think about tactics. Or, at worst, they should think about tactics only in the way a happily married couple does.

—Anonymous

Pain is a perception, not a reality.

—Shirley MacLaine, in Time magazine,
Pearl Harbor Day, 1987
Introduction

Lundberg's lively little volume arrived at the bookstalls nearly half a century ago. It was standard reading for us undergraduates of the early postwar years, and given what we knew about famine, disease, war, depression, genocide, and political repression, the book made big promises. I don't know whether the social sciences have delivered on these promises, but I do know that we have established a credible basis for continuing the effort, more or less unimpeded and with an appropriate Lundbergian optimism. I have many colleagues, however, who do not share this assessment, and they have launched an attack—sometimes subtly and sometimes not so subtly—against the established positivist premises of the social sciences. In my opinion these colleagues are basically wrong, perhaps disastrously wrong, and if they succeed in destroying the social sciences they will find themselves enjoying the spoils of a Carthaginian victory.

Perhaps, however, the forlorn victors will be able to deny the reality of their disappointment: The world according to MacLaine is a free-lance filibuster constructed on a simplistic denial of Barrington Moore's (1973) universal forms of human misery. Or, perhaps I'm simply wrong. I am told that I exaggerate, and that the anti-science critique is not all that deadly. But on the other hand, it is possible that the victors will eventually stumble upon a fundamental truth: Subcranial pain falls mainly on MacLaine, while the realities of pain fall upon the rest of us like inexorable Malthusian checks. In any case, and with due respect for Jacques Barzun, I would be satisfied if the anti-science philosophers were to learn the language of science in the same way that most of us learn the language of Dickens: We use it—with at least a modicum of skill.

(1) The dialectics of disenchantment

... 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.

—Peter Novick (1988:250)

There is, then, nothing new under the sun. However:

Karen Winkler's early aperçu on antipositivist opinion (26 June 1985) should not surprise anybody when it claims (with George Lundberg) that the social sciences have ties to the humanities. The natural sciences have similar ties, and these derive largely from the fact that all the sciences have their origins in the humane disciplines. This historical reality negates the deplorable tendency among Winkler's interviewees to suppose an irreconcilable conflict between the thought
patterns of the humanities and those of the sciences. As Arthur Koestler (1964) argued in *The Act of Creation* many years ago, the thoughtways of the sage, the artist, and the humorist, although not identical, have large areas of overlap. The disastrous confrontational atmosphere created by Winkler and her interviewees is degraded further by the absurd claims that “the discipline of economics is impervious” to non-scientific thought and that “the mainstream of sociology still almost unconsciously accepts the natural-science model.” If we tend to do things unconsciously we will surely find it difficult to “interpret” what we do—a point to which I shall return. For now, suffice it to say that if there is an “arbitrary boundary” between the social sciences and the humanities, then surely Winkler's brand of journalism—and the anti-science school is big on journalism—helps to produce and perpetuate it. But intellectual boundaries, I believe, have no objective existence; they are akin to the boundaries that create “periodization” in history. In fact, the “scientific” and “artistic” hemispheres of the human brain are joined together objectively by a structure called the *corpus callosum*, the ideational equivalent of which we desperately need.

“Economic man,” that quintessential human machine created centuries ago by the earliest economists, has been suspect for as long as he has existed, but Winkler and her interviewees decided that it might be fun to trot him out once again. Nobody who has studied sex or violence or non-answers to Donald McCloskey's question “If you're so smart, why aren't you rich?” has failed to notice that the “means-end, cost-benefit model is far from covering all aspects of human activity and experience.” Social scientists who study capital punishment, or who experiment with ways of punishing men who beat women, have known for many decades that deterrence—a simple principle based on the cost-benefit model—cannot be relied upon as a means of social control. And when students of teenage fertility discover adolescents who believe that the withdrawal method of birth control cannot fail, or that they are too young to get pregnant, or that they can only get pregnant just before or just after menstruation, or that they can avoid pregnancy by standing during sexual intercourse and perhaps (both?) facing north, we are in the presence not of rationality but of poetry. Ironically, Economic Man has been converting himself lately into a handy straw man, despite the fact that very few contemporary social scientists have ever tried to prop him up. Like Joan Rivers, we only prop up a dead man when we're desperate.

In American intellectual history it is not unprecedented for economists to make large departures from the means-ends, cost-benefit model. The famous “Phillips school,” for instance, believed that the staunchest supporters of slavery in the United States continued to favor the institution despite its irrationality from a cost-effectiveness standpoint. To Phillips and his followers slavery persisted largely because its advocates believed it to be a morally superior form of socio-ecological organization (Fogel and Engerman, 1974:II,176). The Phillips school no longer prevails, but one does wish that its willingness to treat the means-ends model as a mere hypothesis, rather than as a normative goal toward
which society ought to strive, were impressed on several unreflective economists currently in the employ of the American embassy in Mexico City, who seem to believe that it is divinely preordained that the central valley of Mexico be transformed into a replica of the San Joaquin valley of California. Don't bet on it.

Remember, too, that one woman's rationality is another woman's madness, even when the women in question happen to be social scientists. Discussing the San Joaquin model as applied to Mexico, De Janvry and Vandeman (1987:107) conclude that

For Third World countries under the imperative to rapidly increase food production and reduce rural poverty and slow down urban migration, the U.S. model is of little relevance. Nevertheless, increasing food production through large-scale farming, international transfers of technology, and a dominant agribusiness is clearly a tempting way to satisfy such pressing needs. ... The Third World increasingly is adopting this approach. Yet because they lack the labor absorption capacity and off-farm income sources that characterized the U.S. development, these countries are finding the social costs of the U.S. model simply unbearable. ... If rural development policy is dominated, as it is in the United States, by large landholders' lobbies, its purpose will be agricultural development couched in the rhetoric of rural development.

De Janvry and Vandeman (1987:98-104) also point out that agribusinesses do not emerge solely through the operation of market forces. They are highly subsidized. Beyond that, there is a possibility that the “unbearable” costs of agricultural technology may fall more heavily on women than on men (Harriss and Watson, 1987:100-101); among native Americans of the far north, the unbearable costs of the highly efficient snowmobile impose themselves without regard to gender (Georges, 1993).

In any case, McCloskey is wrong. Social scientists—excepting those assigned to the U.S. embassy in Mexico City—are clearly capable of evaluating the cost-benefit model scientifically. It is ironic that a book devoted largely to a critique of positivism, empirical investigation, quantitative methods, the rationality model, and their alleged dominance in the social sciences (Sica, 1988:20, 40, 78-9, 94-5, 118, 136-41, 259), ends by concluding that both Weber and Pareto gave large play to irrational elements in human behavior. It is untenable to argue, as Sica does (1988:262), that “...human rationality is a relatively rare event,...” and it is equally untenable to argue that social scientists have had little aptitude for the irrational.

And even though McCloskey may be justified in accusing many economists of relying too heavily on formal tests of statistical significance (1985: Chapter 9), one must point out that a major controversy over statistical significance occurred
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in sociology a generation ago (Morrison and Henkel, 1970) and was more or less resolved in favor of the notion that statistical significance is merely one means to the attainment of “substantive” significance (Selvin, 1957; Walster and Cleary, 1970). One of my students (Flippo, 1987) recently completed a master's thesis in which she was interested primarily in whether high educational attainment tends to delay sexual initiation for a longer time among nonwhite women than among white women. Her idea was that ambitious minority women develop a higher aptitude for deferred gratification than do ambitious majority women. I don't recall whether the observed difference in sexual initiation—age at first intercourse—between the two groups was statistically significant, but I do know that it was not substantively significant.

What bothers me about McCloskey is that he does not acknowledge these processes of intellectual evolution, and he therefore does not see the adaptive character of social science knowledge.

Historians, whether “scientific” or not, always have had strong ties to the humanities, and many universities still cannot decide whether to classify the history department within the humanities or the social sciences. It is not surprising that traditional historians often express their misgivings about cliometrics and other exotic forms of scientific history. But cliometrics is here to stay, probably because historians perforce must deal more and more with historical epochs in which human beings had long since established themselves as a “data-generating animal.” The media have arrived, and we cannot ignore the message. Historians should welcome the opportunity to triangulate among speculative history, text analysis, the intensive case-study method, and scientific, quantitative, data-oriented history. Anybody who believes, with Randall Collins (1984:331), that historians are better off when they do not permit “inroads from the statistical side,” should take a close look at Fogel and Engerman (1974).

Winkler quotes Alan Sica to the effect that various historical circumstances have contributed to the latest outburst of antipositivism. Sica's speculation about the impact of NSF funding policies would have far greater validity, however, if it were tested by cliometricians with a talent for deriving and assessing its empirical consequences. Sica implies that Congress must have expressed great faith in the scientific value of the social sciences back in the golden days of the 'fifties and 'sixties, that social scientists who had the benefit of Congressional largesse must have shared a similar perspective, and that the recent reduction of funding has somehow undermined the scientific faith among these erstwhile advocates and exemplars of a pure social science. I suspect, on the contrary, that the attitude of Congressmen and of “scientific” social scientists has changed very little, and that the challenge to positivism has occurred primarily because the antipositivists in academia have discovered that less money for their colleagues means less power. 3 The Reagan/Bush administrations did not create a revolution in our minds; they created a revolution in our budgets. On the other hand, perhaps Sica is right. Perhaps diminishing public support, involving explicit
disparagement of sociology by the president of the United States, has created a crisis of confidence in which the discipline finds itself “in the doldrums,” a prospect taken seriously by a recent president of the American Sociological Association—this is not a healthy form of presidential accord. If we actually have allowed Reaganite anti-intellectualism to undermine our confidence in ourselves as scientists, then I say that our behavior is approximately as rational as that of the adolescent lovers mentioned earlier.

(1.1) Predictability, vulcanology, seismology, and (especially) meteorology

The cases presented here ... give evidence of the fact that social science research can be effective. ... an extreme view of the ineffectiveness of social science research, it would now seem, is untenable.

—Bernard Barber (1987:154)

The antipositivist's claim that social scientists cannot make predictions is nonsense, and anybody who plans to abandon scientific thoughtways because of this alleged defect is conducting a most inauspicious withdrawal—I cannot seem to escape adolescent techniques. Social scientists have become so good at predicting electoral results that we allegedly keep people away from polling places. Using an extraordinarily powerful theory known as the stable population model, we can predict (among other things) the future burden of social security; if the Social Security Administration did not have the benefit of our predictions (or “projections,” as demographers prefer to say) the results could well be disastrous. We can predict, again within a small margin of error, the number of limited wars that will be fought by, say, the end of the current millennium. Social scientists predicted, correctly, that the peace settlements following World War I would create instability. During World War II we predicted that racial desegregation would have a generally benign impact on American society. Perhaps our greatest success in the realm of social engineering was the desegregation of the American military, a process strongly urged by social scientists despite the widespread belief, both within and without the military, that it would weaken America's warmaking capability. A few years ago a number of us predicted the recent resurgence of capital punishment in the United States, and we also had a few things to say about the prospects of reducing violence through this means. I do not claim that we can predict everything with complete accuracy, but I do claim that our ability to make predictions is at least as impressive as that of the vulcanologists working a few years ago in the vicinity of Mount St. Helen's, and more recently in Colombia (Marlowe, 1993). Decision theory, learning theory, the stable population model: These scientific devices, and their ability to anticipate the future, are comparable to much of what is found in the mighty arsenals of the natural sciences.
A few social science success stories have been so impressive and are now so completely taken for granted that they seem to be imbued in the very nature of things and we no longer receive due credit for them. Take, for instance, the matter of human mortality. Available data on the death rates of, say, 150 or more years ago suggest that these rates were highly unstable and unpredictable: They could readily vary from 30 per thousand annually to perhaps 200 per thousand annually during famines, plagues, and wars, and these variations were often not anticipated. Nowadays we can say with nearly complete certainty that the death rate of the United States throughout the 'nineties will be very close to 9.5 or 10 per thousand annually, as it has been for several decades. One of the reasons why I cherish this particular example is that it is arguable that, during the nineteenth century, large reductions in mortality as well as the stabilization of death rates were brought about not primarily by advances in the medical sciences but rather by improvements in transportation, communication, environmental sanitation, and public education (McKeown, 1988); that is, by inventions that took place in the realm of human ecology, broadly defined, and that were dependent on social diffusion. In brief, these changes were the work of people who could well be called applied social scientists.

For those contemporaries who have the courage to practice applied sociology (Whyte, 1986), the ability to predict and explain social phenomena becomes crucial: Enough mistakes, and one's job disappears.

As Seymour M. Lipset correctly points out (1979:5-7), we have had some notorious failures of prediction—for instance, our failure to anticipate the "stagflation" of the 'seventies, or our failure to predict changes in human fertility. In 1992 he added to his list the highly debatable allegation (Lenski, Lenski, and Nolan, 1991:261) that social scientists failed to anticipate the disintegration of various communist regimes. But for every demonstrable failure there are several impressive successes, and we can even provide instances in which prototypical social scientists such as the authors of The Federalist Papers have designed and correctly predicted the future behavior of elaborate social systems (Bellah et al., 1985:254-55; Diggins, 1990). If social scientists were more concerned about winning for themselves the prestige of the natural sciences (or, better yet, the prestige of the Founding Fathers), instead of slinking off to the English department for this afternoon's pomo brownbag, they would probably have many more opportunities, eventually, for designing and testing social systems or large elements of them. A few of our colleagues in West Germany (Rehberg, 1985; Stehr, 1986; Tenbruck, 1984) seem to believe that our past successes have produced the sort of anti-sociology backlash that gives some of us a sense of being in the doldrums, but I predict that we will rise above this petty affliction with minimal effort. I believe that what Tenbruck is saying about the social sciences is largely the sort of thing that economists might say about TV sets (Cook and Campbell, 1979:313): If manufacturers sold lots of TV sets last year,
they will not do so well this year because they have satisfied the high levels of demand that existed last year.

This year's low returns, in other words, may be an indication of last year's successes—or the last century's successes, to return to the mortality example cited above. And even regarding prediction, social scientists had as many successes last year as did vulcanologists, seismologists, or meteorologists—few of whom find themselves in the doldrums.

(1.2) Molecular mysteries and habits of the quark

Another inappropriate but highly predictable twist taken by the antipositivist argument (Alexander, 1988:80) is that a science of social behavior is impossible because men and women behave more subtly than do molecules. Molecules, says one of Winkler's interviewees, do not lie. While I know little about the veracity of assorted chemical compounds (except for tea leaves and sheep entrails), I do know that anybody who studies molecules is impressed by the fact that, as one scientist phrased it, nature is more subtle by many times than the puny ability of the human mind to comprehend it. And this generalization applies to physics just as clearly as it applies to the biological and social sciences, despite Dawkins' untenable claim (1986:2) that physics studies “simple things.” Any good physicist—say, a specialist in quantum dynamics—knows that her field is infinitely complex (von Baeyer, 1992), and if you try to tell such a colleague that the infinite complexities of your life are greater than the infinite complexities of hers, she is likely to conclude that you are mentally deranged.

As for biology, there are viruses that seem to have read every textbook ever published on molecular biology and a small set of manuscripts not yet published, submitted, or conceived. When Dawkins writes about his own field, which is biology, he speaks compellingly: Biology, especially at the level of molecular genetics, evolutionary theory, and their interrelationships, is a science of complexity. One cannot find anything in Habits of the Heart that makes its subject matter more complex than that of The Blind Watchmaker.5

To those who point out that there is nothing in the universe more complex than the human heart and the human mind, one can only reply that the complexity of human heads, hearts, hands, and health is to a large but indeterminate degree a matter of molecular biology.

In short, the world is very impressive; human thought, as I shall suggest in Chapter 2, is not much more impressive than computer thought. Nature presents infinite complexity in all its dimensions. The subtlety of nature tends to flamboozle all scientists, not just social scientists, and we do not find here any unique disqualification of the social sciences—even the great Michel Foucault, as we shall see, was mistaken on this question.

Winkler recounts Renato Rosaldo's experiences among the Ilongots, apparently trying to show that Rosaldo's emotional make-up contributes to the
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quality of his scholarly work. Her presentation in this case undercuts the anti-science stance, because one cannot see the alleged relationship between Rosaldo's sad experience of losing his wife and his excellent hypothesis that exchange theory may help us to explain (and "postdict," if not predict) head-hunting among the Ilongots. This is a typical social science hypothesis and it is testable regardless of whether one is capable, in Rosaldo's words, of getting "close to the emotional force involved." The emotions of the subjects are likely to be irrelevant to the explanatory tasks of the scientist. As Marvin Harris argues (1979:333-36), it does not matter what was going through the minds of the victims of Aztec sacrifice as these unfortunates were being dragged up the temple steps to the slaughter. (Harris, to be sure, overstates the case: Would that we could have interviewed some of the potential victims!) I believe that we could readily assemble a panel of anthropologists who could outline a procedure that would enable us to decide whether Rosaldo's hypothesis fits reality. In selecting this panel, we would be wise not to pay any special attention to the emotional make-up of its members. Selection by expertise rather than emotion or other irrelevancies is the hallmark of science and one of the major reasons why it progresses—at least in the sense that it can make decisions about hypotheses. And this, of course, is a good definition of scientific progress.

Rosaldo reminds me of George Bush: He has strong feelings, but once he has called this to our attention a few times it tends to lose its force, and its relevance.

It is puzzling to me that Rosaldo somehow has come to be associated with the antipositivist critique, because his writings on the topic insist that social scientists continue pursuing scientific knowledge according to the standard positivist maxims. He tells us (1986), for instance, that Le Roy Ladurie's studies of fourteenth-century French village life may have been impaired by the fact that the major source of data was the "inquisition register" of Bishop Fournier; many of the respondents may have slanted their answers out of fear for their lives and/or salvation. Fournier, it appears, was no Torquemada—but he wasn't Charles Booth or George Gallup either. Rosaldo makes similar claims about the classic studies of the Nuer by Evans-Pritchard, who worked under the auspices not of the heavenly powers but rather the earthly powers of the majestic Government of the Anglo-Egyptian Sudan (1986:88).

The cardinal rule of positivism, of course, is that we must strive for objectivity. Rosaldo is merely insisting that we abide by this maxim.

In any case, it may well be that an anthropologist whose personal experiences and emotions are comparable to those of Rosaldo is more likely to arrive at a particular hypothesis, and it is for this reason that we should not act as if the emotionality of science were somehow incompatible with insight and objectivity. The scientist who discovered a volcano on one of Jupiter's moons a few years ago speaks as if her emotional constitution—at least her persistence in poring over data—had a lot to do with the discovery, and I believe her argument is at least as compelling as that of Rosaldo. In Watson's The Double Helix (1968), one discerns
the strong emotions involved in the competitive search for DNA. Guillemin and Schally, who received the Nobel prize for their work on the ways in which the brain controls the endocrine system, were swept along (or perhaps impeded) by intense emotions (Meites, 1977). Again, Koestler (1964:101-267) shows that emotional experiences, dreams, accidents, coincidences, serendipity, and the like have always been at the center of scientific procedure and progress, and I believe that social scientists know this as well as anybody. In Hammond's view (1964:3), the best sociologists have a “feel” for the tactics and strategy of science, and Becker claims (1987:25-26) that neither natural scientists nor social scientists work with total impersonality and objectivity. Again, Sica's (1988) insistence that social scientists place too much emphasis on human rationality is undercut by these sorts of considerations.

On the other hand, when we evaluate the contribution of emotion to the sciences, we must remember that powerful emotions underlie the astronomy of Ptolemy, the biology of Lysenko, and the psychogenetics of Arthur Jensen. Science is the process of getting experts to agree about things regardless of their strongest emotions. The emotions that lead us to pursue the beauties of Jupiter's moons do not dictate a strong emphasis on emic explanation, and they do not dictate an abandonment of the central thoughtways of science. Incidentally, it is contradictory to argue, as do some of Winkler's interviewees, that a social science is impossible because the scientist is part of his subject and that the social sciences suffer because so many of us remain emotionally detached from the subject. It is interesting that Barzun (1964:87) considers detachment to be the most civilized aspect of science, an extraordinary accomplishment for the “restless rampaging beast” that spends most of its time trying to whip nature into shape.

Finally, Winkler's article wrongly asserts an ineluctable tension between science and interpretation, implying that scholars who opt for interpretation must necessarily declare war on science. On the contrary, it is entirely possible to study interpretation, the meanings of symbols and of human experience generally, from a scientific standpoint. There is, for instance, a highly developed positivist psychology of human perception and cognition. When scholars begin speaking about interpretive styles of investigation, however, we often encounter a distressing tendency that is dangerously exacerbated by the Winklerian world-view: The problem is that interpretations—either those of the observer or the observed—are taken as the ultimate reality. But social scientists who contribute to the debunking function, who (with Collins, 1984) take the null hypothesis seriously, have reason to believe that most interpretations, most of the time, are wrong. It is arguable, in fact, that this assertion is the central premise of all the sciences, applying therefore even to the finest etic interpretations. And if we cannot muck through with the best, what, pray tell, do we make of the rest?

Mortimer Adler (1985: Chapter 1) maintains that the separation of consciousness from its objects, the separation of “individualist” and “holistic”
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perspectives (James, 1984), is a serious philosophical error, and Marvin Harris has argued in several works (1979; Harris and Ross, 1987) that if we rely heavily on our own untested interpretations of social behavior or (worse yet) on the interpretations provided by “knowledgeable” interviewees, we will inevitably go astray. Harris (1979:246-53) shows that if we accept emic explanations of the significance of sacred cows, we are assured of learning next to nothing about the social and ecological dynamics of this custom. Similarly, if we rely on a cross-section of Gallup Poll interviewees for insight into the structure and functioning of American society, we will learn almost nothing about the social science principles that informed the authors of *The Federalist Papers*, about whether or not America has a ruling class, about the alleged indispensability of “waste” and “waste-makers” to the American economy, or about the extraordinary features of Southern slavery discerned by Fogel and Engerman (1974). Just ask your relatives, friends, and neighbors about the origins and functions of the incest taboo, comparing their replies against what anthropologists have theorized about this topic, and you will see my point. Or, in a more contemporary example, find a dean of faculty, ask him whether sex discrimination in faculty salaries exists at your institution, and compare his reply against the sort of reality discernable by an appropriate regression equation. I do not accuse the dean of malfeasance: As I implied above, if social scientists can do things without conscious awareness or intent, so can everybody else. The dean may simply not know what is going on.

It may or may not be true that “if everybody is somebody, nobody is anybody.” I am strongly convinced, however, that if every interpretation is taken to be real, then nothing is real.6 And if everyman's interpretation is capable of informing me, and I do my best to assimilate everyman's interpretation, then my own interpretation must be unassailable and I have achieved the superiority of the town gossip. Immediately, then, we are at the fringes of the politics of solipsism, and the consensus-creating practices of science become a mere imposition, an intrusion into the do-it-yourself reality of each of us. Obnoxious little books like Harry Browne's *How I Found Freedom in an Unfree World* (1973) become our scriptures, and our great commandment, standing alone as an icon of cultural illiteracy, is “Know thyself—and nothing more.” Tune in on Browne's discussion of the merits of tax evasion, and compare his view of taxpayers with that suggested below in Section (6).

Some years ago I believed, with Theodore Roszak (1969), that anti-science is an inexorable feature of the “counterculture.” Nowadays it occurs to me that anti-science may be the philosophical foundation of the “me generation,” and I do my best to suppress the fear that the me generation is merely the latest manifestation of the counterculture.

Science, on the contrary, is integrative, regarding intellectual property as communal in its creation and ownership. It insists upon cooperation. It insists that similarities among things—and among people—are more important than
differences. It insists that ego-tripping give way to humility and to an attitude of reverence toward nature. It insists upon its own intermingling with the many non-scientific thoughtways that strengthen it and are strengthened by it. It is ironical that if we aging scientists ever try to bring back the idealism that lived so briefly in our students of the 'sixties, we shall probably have to do so in the name of science. The most promising young radicals I ever knew referred to themselves as the HEADS—they were Human Ecology And Demography Students, they didn't smoke all that much, and they were the wave of the future. And although I do not believe that mathematical competency automatically makes one scientific, I can only hope that Barzun (1964:30) is correct in the claim that “the statistical outlook prevails and establishes an equality that dwarfs the individual.” Back in the 'sixties I tried to impress this argument on the Students for a Democratic Society (SDS), but at the time it appeared that they were too individualistic to accept it, not yet having discovered the likes of Althusser and Poulantzas (James, 1984:79-145). As we shall see in the next chapter, however, an intellectual discipline such as statistics is probably not loaded with the ideological biases that are often fashionably claimed nowadays for such disciplines.

(1.3) Rosaldo revisited: How would the catcher in the rye have felt?

I believe that there are fruitful possibilities ... for sociological theorists to shift their reflexive analytical focus from metatheoretical foundational concerns to practical-moral ones.


I'm very happy for Rosaldo.
I'm delighted that it is possible for an American anthropologist to live to full adulthood, to become a prominent scholar, to marry, have children, travel across the world many times—and never encounter the “rage in grief” until the awful day when his wife, a beloved friend and colleague, falls over a precipice to her death. I was born in 1938, and I was filled with grief and rage by the time I was three years old. By the spring of 1942—those sad, unforgettable days—I knew that an incomprehensible catastrophe had occurred at a place with a beautiful and mysterious name, Pearl Harbor; I knew that my grandfather, who early one winter morning had given me the exotic, surrealistic experience of walking across Santa Monica Boulevard for the first time, was dead; I knew that my parents were breaking up, that my mother was losing her father and her husband at the same time. And I had my counterpart of Rosaldo's headhunting: I knew that the world is flat, that bullets travel infinitely in a straight line parallel to the flat earth, that machine guns never run out of bullets, and that therefore all I had to do, to be safe, was to acquire such a weapon, press the trigger, and spin round and round.
I also knew that I had acquired a “twenty-five dollar United States war bomb” from a very motherly woman named Kate Smith, and that if my machine gun did not work—no problem: My very small pilot would throw open the canopy of his very small airplane, raise my very small twenty-five dollar United States war bomb, and drop it directly on General Tojo.

Which brings me to my current theoretical problem, the AIDS epidemic. I want to know how to model this epidemic: Is some sort of mathematical function appropriate (Brookmeyer, 1991), should I try to develop a simulation (Whicker and Sigelman, 1991: Chapter 6), or should I try to find more data, say, on sexual conduct comparable to the data provided by the National Survey of Family Growth (NSFG) or the General Social Survey (GSS)? Should I use all these approaches at once, and triangulate among them? Am I missing the legs of a larger triangle? In any case, my own endless experience of the rage in grief and (why not?) the grief in rage—and here I must allude to Holden Caulfield, the dialectical opposite of Toe Joe the Ogre—will further my understanding of the AIDS epidemic to precisely the same degree that Rosaldo’s “use of personal experience” has furthered my comprehension of headhunting.

One final thought: In the age of AIDS, those who believe in giving full play to their passions should contact the contemporary counterpart of Kate Smith, and get some investments in place.

(2) The trouble with feminist theory

When Susan Bordo (1987:261) argues that Western intellectual history over the last several centuries has produced a “Cartesian masculinization of thought” and that this masculinization results from “the seventeenth-century flight from the feminine,” she implies that there is something macho about the sciences, including the social sciences; presumably, there is something inherently feminine about an identifiable set of preceding centuries. Other advocates and exemplars of “feminist theory” develop the machismo argument more explicitly. Marlene Mackie (1988:1-2), for instance, claims that “... the linchpin of sexism in sociology is its methodology”—referring to those aspects of methodology that purport to be scientific. She laments “... the use of masculine research style ...” and refers (1988:4) to a “machismo element” in sociological research that leads to “exclusion of women through choice of research topics.” She tells us (1988:11) that a content analysis of the research literature has revealed that male-authored articles are more likely than female-authored articles to be “masculine oriented”—specifically, 90 per cent versus 71 per cent. (Interestingly, this comparison suggests that fewer than a third of the female authors in sociology write something other than masculine-oriented work; it also leads us to question Mackie's “exclusion of women” remark.) Mackie (1988:12) then arrives at a recurrent claim of feminist theory: That although we often view gender as an important characteristic of individuals, we fail to recognize its significance as a
"principle of organization"—a claim to which I shall return. Finally, Margaret Andersen (1987:247) tells us that “... critiques of the scientific method should be a primary concern in feminist revisions of social science courses,” and Susan Weisskopf (1978:277) informs us that quantitatively oriented psychologists and sociologists have not succeeded in controlling research biases; instead, they have managed to “... lose touch with the individuals behind the statistics”—another theme that recurs often in works of feminist theory and in the antipositivist critique generally.

In trying to assess whatever support has been adduced for these arguments, I usually find many instances in which scientific method has strengthened the feminist perspective, rather than subverting it. Longino and Doell (1987:165,175), for instance, while focussing on “masculine bias” in the social sciences, point out that

What the study of contemporary hunting and gathering societies should teach us is that, short of stepping into a time machine, any speculation regarding the behavior and social organization of early humans remains just that. This leaves framework choice subject to influences such as the speculator’s preconceived and culturally determined ideas of what human beings are. The distance between evidence and hypothesis remains an invitation to further theorizing or, as some would have it, storytelling.

Longino and Doell believe that masculine bias cavalierly fills this gap by telling the story of man the hunter and woman the gatherer. But the central tenet of scientific method is that one does not fill gaps with unsubstantiated guesses; rather, one follows the rules of scientific procedure in order to identify gaps in knowledge and arrive at an appropriate means of closing them. There is no evidence that the “masculine” style of scientific procedure has been remiss in these responsibilities (Lenski, Lenski, and Nolan, 1991:105). In fact, the sorts of scientific attitudes that feminist theory regards as male-oriented are probably associated with more than the usual degree of skepticism about scientific claims, including claims about the earliest sex roles. The many anthropologists and sociologists who laid to rest the various unilinear or parallelistic theories of social evolution were convinced that the earliest human origins, as Sumner once said, are lost in obscurity, and that dogmatic assertions about them therefore cannot be entertained. There is little enthusiasm, today, for Tylor’s theory of the incest taboo, for his theory of racial differences, for Freud’s theory of the parricidal crisis, or for the Morgan-Engels theory of the origins of the family and private property. Although these scholars told magnificent stories, they could not provide the sort of evidence that would lead us to accept their stories as scientific knowledge. And it is noteworthy, as Josephine Donovan (1985:73-76) points out,
that Morgan and Engels did not consistently place the male of the species in the dominant position, in their stories.

On the question of the early evolution of sex roles, the best studies that I've encountered recently are summarized in Harris and Ross (1987). Harris is strongly committed to “the struggle for a science of culture”; yet, despite this male-oriented proclivity, the Harris-Ross volume provides great insight into the dynamics of excess female mortality (emphasizing female infanticide), the harsh treatment of women generally, the relationship between sex exploitation and class exploitation, and many other such topics. These studies also have the virtue of directly challenging Malthusian theory as it applies historically, and demographic transition theory as it applies to modern populations.

Another illustration of the ways in which science has contributed to feminist theory occurs in Donovan's book. When I came to the page where we learn that “... the witch craze developed in Europe and in New England at the same time as the Newtonian world view was gaining ascendancy” (1985:29), I was tempted to write a paraphrase in which I would point out that witch crazes were disappearing at the same time as the Einsteinian world view was gaining ascendancy. But before I could do so, I came across the following remarks in a later chapter of the same book (Donovan, 1985:180):

The Newtonian or scientific world view ... is rooted in the ... masculine psychology ... However, developments in twentieth-century science, such as Heisenberg's Principle ... and Einstein's ... Theories of Relativity ... have challenged the validity of the Newtonian paradigm. The new vision of the universe that is emerging is no longer of an Other that operates in predictable, mechanical fashion, but of a contextual network in which every discrete entity is defined relative to its environment and subject to the positional relativity of the observer.

This sort of concession undermines the anti-science arguments presented by Bordo, Mackie, Andersen, Weisskopf, and by Donovan in the earlier parts of her book. But even as a concession, this passage (and its larger context) leaves a lot to be desired: It does not explain what is masculine about Newton, why relativity is non-masculine, or whether it would be appropriate for us to credit twentieth-century physics with having given major impetus to the feminist movements of this century.

In a few recent works of feminist analysis, one notices an occasional indulgence in what Longino and Doell might call unsubstantiated storytelling. Andersen (1988:17-19), for instance, laments the fact that much of the recent literature on the status-attainment process in the United States involves surveys of men only, but when she castigates Christopher Jencks et al. (1979) on the basis that “... his national sample includes only men ...,“ we lose sight of the fact that Jencks and his eleven collaborators (including four women) used five national
surveys and six special-purpose samples, and that some of these datasets did include information about women. It turns out, granted, that information on women was so limited that, as Jencks et al. say (1979:4-6), "... we reluctantly decided to restrict all our analyses to males... Nonetheless, this limitation is both serious and regrettable, since sex is one of the most important single factors affecting earnings." This research team, then, had strong misgivings about the neglect of women in status-attainment research, and it is clear that they believed that the surveys available to them missed a lot.

I mention this example primarily because it illustrates the important distinction between theories and "orientations" (Merton, 1957:87-89): If it is true that the status-attainment literature has given little attention to women, this is a criticism of the orientations of researchers in this area, i.e., their ways of selecting problems for analysis, and not of their theoretical practices. Theories and orientations are largely independent of one another. For instance, one could make excellent contributions to conflict theory by developing comparative analyses of international relations, labor-management relations, and race relations, while rarely or never dealing with questions of gender or male-female interaction. And vice versa.

Nor can I agree with Andersen (1988:19) that matters have not improved, that "... in sociological work, gender is seldom considered to be a factor that influences social behavior." In order to substantiate this large thesis she would have to show, among many other things, that approximately 700 gender-related articles and books based on the GSS and published by 1986 (Smith and Fujimoto, 1986: mnemonic index, 50-51), and the many studies published since that time, had little relevance to social behavior. She would have to demonstrate the irrelevance of the U. S. Census, the Current Population Survey, the National Longitudinal Surveys of men, women, and youths, the NSFG, and many other such enterprises in which large quantities of gender-related data have been produced. And then she would have to extend the analysis from survey-based literature to the many other types of literature produced by social scientists. If she were to take these steps, I believe that she would have to abandon her argument, or at least a large part of it.

From time to time, Andersen seems to suggest that the essential problem is that we do not pay enough attention to the sex composition (the sex ratio) of social groups or organizations. But this situation arises from the fact that in order to calculate a sex ratio, one must be able to identify significant ways of grouping or aggregating the data. It is hard to do this with the General Social Survey. The GSS involves a sample of independent households, and there are relatively few ways of grouping data so as to make the sex ratio a meaningful measure. It is entirely possible, however, to use the sex variable from the American Council on Education's massive surveys of college faculty members as a basis for classifying institutions, departments, academic fields, etc., on their sex composition. Many scholars—even male-oriented scholars—have shown no reluctance to take
advantage of this sort of opportunity. If it is important, as Mackie suggests in the
citation above, that sex be conceptualized as a “principle of organization,” this
surely must refer to such practices as the use of the sex ratio as opposed to the
simple sex classification of individuals. In the Harris-Ross work cited earlier, the
sex ratio of children is the most important means of assessing the degree to which
infanticide selects against females. If gender as a principle of organization is
intended to have additional meanings, they should be stated.

Although feminist theory addresses itself primarily to methodological
questions, it is interesting that the one style of theorizing that seems to call forth
mandatory malediction is functionalism. When Andersen says (1988:20) that “the
focus on norms, roles, and stability emphasizes the status quo ...” and that
functionalist theory traps us into using these limiting concepts, one must ask,
first, how was Mirra Komarovsky limited by her focus on roles and norms?
Second, in what sense are researchers who focus, say, on sex ratios guilty of
overemphasizing sex roles, given that these roles are not defined for aggregate
data? (If a group or social category can have a sex role, this is a neologism
that must be defined with care.) Regarding functionalism, Susan M. Okin
(1979:95,240-41) makes a comparable error when she suggests that scholars still
see women from a sort of Aristotelian functionalist perspective that insists on a
place for everything and everything in its place, including women. The error
arises from the fact that sociological functionalism is hardly more teleological
than Darwinian functionalism—Darwin and Wallace, in fact, borrowed the
central postulates of evolutionary theory from the great functionalists Malthus
and Smith. Contemporary functionalism may give human intentionality a larger
role than that implied by the Darwinian (or social Darwinian) view, but
contemporary functionalism is prepared to take Shulamith Firestone very
seriously when she suggests (Okin, 1979:295) that technology, for instance, is
capable of liberating women from the “barbaric” character of pregnancy, for
starters.

In brief, it is not at all clear what Stacey and Thorne (1985:309) have in mind
when they argue that “… positivist knowledge serves the interests not only of
dominant social classes ..., but also the interests of men ...” Various demons may
rampage through our works, but they are not positivist demons. In the grandest
traditions of androgynous sociology we positivists must insist upon both the
allegedly masculine practices of abstract, etic, quantitative structuralism, and the
presumably feminine practices of giving attention to “… individuals behind the
statistics ...,” emic exploration, intensive case studies, interpretationism,
qualitative methods, and so forth. We also insist that the best sociology is done
when these diverse perspectives are combined and made to work in a
complementary way.

(3) Titles and tribulations
Throughout this book I illustrate the harmful consequences of abandoning the high standards of scientific inquiry. In recent months I have found that whenever I try to discuss openly my misgivings about feminist attitudes toward science, I encounter further illustrations. Something is very wrong when a highly productive and capable scholar tells me, in a personal letter, that “I had written a response to you but I didn't send it for publication, for two reasons: 1) I didn't think it my place to respond, 2) I thought [your] editor was the real culprit.” There have been several letters and a few phone calls from colleagues who implicitly affirm the prescient words of Eri Atlov: “I do strongly agree with what you say, but I shall be scared to death to say so forthrightly.” The sort of colleague who sends these messages reminds me of the “undisclosed source” whose words we read so often even (or perhaps especially) in the better newspapers. But those who are afraid to stand by their own ideas already have surrendered their constitutional rights, no matter how provocative their public expressions; like Emmanuel Goldstein of 1984, these quintessential voices must remain in hiding. And Thomas Jefferson would doubtless remind us that if these anonymous advocates of provocative notions cannot be challenged because they will not reveal themselves, then the marketplace of ideas is already suffering restraint of trade.

As I became more heavily involved in raising the questions of the preceding section, it occurred to me that an appropriate title for this book would be Androgynous Sociology: The Power and the Elegance. After all, the basic thesis of the book is that what we must do is to combine the best of the alleged masculine thoughtways with the best of the alleged feminine (or feminist) thoughtways. But reactions to this idea, from men, were almost always along the following lines: “I would be afraid to self-select a volume with that title for fear of what people might think.” And from women, although I usually hear that there is something simplistic about the androgyny theme, these deficiencies are never specified.

And then there is a madman who wrote to a colleague of mine. He asked how “Herr Professor Faia” was doing, said that Stacey and Thorne are merely revealing the obvious when they write that positivist knowledge serves primarily the interests of men, affirmed that science does not attract women because it is “quantitative and logically rigorous,” argued that presumably feminine (or feminist) thoughtways have no place in science, excoriated a “geek-branch feminist” for something, involving Mormonism, that I did not understand, and then wound it all up by telling us that he “got out” of sociology because it is “too quantitative and pays too little attention to individuals ...” An unforgivably careless reader, who feels a false affinity with an argument that repudiates his own nefarious analyses, virtually at every turn. Herr Professor Faia assumes that, by now, this gentleman-scholar has met the matriculation requirements of Eastern State, a Virginia institution of some repute.

Bologh (1990), Cancian (1990), and several other colleagues insist that I might modify my misgivings about feminist theory (Faia, 1990) if I were to read
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A book by Harding (1986). I have done so (not for the first time), and I therefore reconsider as follows:

Harding's indictment of science, its history and its contemporary forms, contains a lengthy bill of charges. She provides an early summation (1986:9): first, the epistemologies, metaphysics, ethics, and politics of mainstream science are androcentric and mutually supportive in their androcentrism; second, modern science serves “regressive social tendencies”; third, scientific applications, technologies, ways of creating meaning, and modes of defining problems and designing experiments are sexist, racist, classist, and culturally coercive. “... I do not wish to be understood,” says Harding (1986:10), “as recommending that we throw out the baby with the bathwater;” yet, as we get better acquainted with it, the baby appears to be badly in need of an exorcist.

Bologh, Cancian et al. are right: After a careful reading of Harding I have a far better grasp of feminist claims regarding the pervasive character of gender. For instance (Harding, 1986:17,57):

... as a symbol system, gender difference is the most ancient, most universal, and most powerful origin of many morally valued conceptualizations of everything else in the world around us. Cultures assign a gender to such nonhuman entities as hurricanes and mountains, ships and nations. ... Gender is a fundamental category within which meaning and value are assigned to everything in the world, a way of organizing human social relations ...

And this is only the first installment, the first dimension, of a detailed multidimensional definition that clarifies itself as one continues working through the book.

A second feature of gender traditions, in Harding's analysis (1986:20,123), is that they have led us to view the world with inescapable distortions, such as those arising from a tendency to see reality in binary form, to see things as dichotomies corresponding to the bisexual character of the human species. Scattered throughout the book is an argument on the desirability of breaking up the two-valued logic of machine vs. human, animal vs. human, mind vs. body, reason vs. emotion, objectivity vs. subjectivity, abstract vs. concrete, conceptualizer vs. executor. Harding has a powerful aptitude for the dialectical interlacing of these apparent oppositions and contradictions, as in her claim (1986:71) that there are few differences between research and engineering, or her claim that affirmative action programs are simultaneously reformist and revolutionary (1986:247). She tells us that a major objective of feminist theory is the development of “feminist standpoint epistemologies” (1986:24-9,142-62).
We soon realize that these epistemologies, again highly complex and multidimensional, place a strong emphasis on the integration of opposites. They express hostility toward positivist empiricism (1986:33), and they make a strong commitment to postmodernism with its rejection of the notion of “one true story,” an intellectual vice that is alleged to be the essence of science (1986:24-9,194).

One hopes that the feminist standpoint epistemologies, working (at times) within the hoary confines of the American university, will eventually challenge the mindlessness of the EGAD! system—Examinations, Grades, Accumulation (of credits, “contacts,” etc.), Degrees.

In the realm of gender symbolism, Harding especially deplores the metaphorical association of woman with nature (1986:114-16) or with the earth; the latter association leads her to the conviction that the Copernican revolution, displacing the female earth with the male sun, was a world-historical degradation of womanhood (1986:85-92). Here, we begin to comprehend her expansive thesis about the dangers of metaphor (1986:113,116,237):

... feminist historians have focussed on ... rape and torture metaphors in the writings of ... Bacon and others (e.g., Machiavelli) enthusiastic about the new scientific method. ... understanding nature as a woman indifferent to or even welcoming rape was equally fundamental to the interpretations of these new conceptions of nature and inquiry. ... why is it not as illuminating and honest to refer to Newton's laws as 'Newton's rape manual’ as it is to call them 'Newton's mechanics'? ... As nature came to seem more like a woman whom it is appropriate to rape and torture ..., did rape and torture come to seem a more natural relation of men to women?

On this argument, more anon.

Harding argues that science selects the wrong problems, defining excessive population growth, for instance (1986:77), as merely a technological issue. With most of the anti-science scholars discussed in this chapter (e.g., Alexander, 1988:80), she makes the predictable claim that the physical sciences are “much less complex” than the social sciences (1986:44), and that the latter, because of their immense complexity, cannot aspire to high scientific standards. “I need hardly even mention the silliness ...,” says Harding (1986:46), of assuming that the physical sciences provide a model for social-science explanation. Insofar as she does elaborate on this point, the limitations of the social sciences seem to have something to do with subjects' intentions and learned behaviors; mathematical formulations in the social realm, for instance, are impossible because of these human qualities (1986:52). Finally, within the social sciences there are at least five major sources of androcentrism (1986:85-92,114-16): first, neglect of emotion and irrationality; second, neglect of unofficial, private aspects...
of life; third—contradicting earlier claims about dichotomous thought—an overemphasis on male-female similarities rather than differences, especially with regard to the meanings of personal experience; fourth, a tendency to ignore sex—"... probably the single most significant variable in history" (1986:89)—as a factor in behavior; fifth, over-reliance on certain methods—e.g., quantification or the practice of having men study women—that may fail to elicit certain kinds of information.

Harding's book is a most edifying exercise in the thoughtways of reform and revolution. I continue to have major problems, however, with her general hostility toward the social sciences. She believes that the social sciences fail inevitably because their topics are too complex. But her own argument fails primarily because the tasks of defining and measuring the relative complexity of the social and natural sciences are themselves highly complex, and we cannot carry them out. It is ironic that scholars who often do not trust measurement are so cavalier in making a claim that would require an intricate measurement process. Yet, advocates and exemplars of anti-science often make a tropistic rush toward questions that we cannot answer, toward concepts that we can hardly comprehend. In Harding's book, these errors abound.

At the moment, however, I should like to consider one of Harding's claims regarding the role of gender symbolism—the rape-of-nature theme—as a historical factor. Harding quotes from a speech by Richard Feynman on the occasion of his receiving the Nobel prize, in which he says that falling in love with the ideas of physics is "like falling in love with a woman," a woman for whom one's love is not diminished by the discovery that she is less than perfect, a woman whose aging, unfortunately, seems to have created insurmountable problems for Feynman although the implied cardiovascular limitations were doubtless his own. In any case, Harding regards Feynman's speech as a most nefarious act.

Harding's decision to associate Feynman's philosophy with the rape and torture themes of the several preceding pages reflects a failure to apply her own lessons: Feynman's ardent ardor may involve the imagery of unveiling, unclothing, and penetration (1986:117), but he defines an expression of love that, although flawed, is the direct dialectical opposite of rape and torture. If it is plausible to say that Feynman advocates rape and torture, then it is equally plausible to say that devotees of fellation and cunnilingus advocate cannibalism. But a loving practice of fellation and a loving practice of cunnilingus, again, are the precise dialectical opposite of cannibalism. When these forms of lovemaking are conflated with cannibalism we are in the presence, at best, of sado-masochism; at worst, the madness of General Jack D. Ripper of Doctor Strangelove.
Feynman, in short, is the wrong illustration in a situation where right illustrations may be in short supply. Harding does not provide any convincing support for the rape-torture thesis. It is nothing more than a provocative hypothesis, for which relevant evidence is not yet forthcoming.

Working through the book, I developed further misgivings about many of Harding's arguments. Among other faults, these arguments tend to have one thing in common: They are short on evidence.

*Item:* Harding makes incredible over-generalizations. For instance (1986:21):

... science is used in the service of sexist, racist, homophobic, and classist social projects. Oppressive reproductive policies; white men's management of all women's domestic labor; the stigmatization of ... homosexuals; gender discrimination in workplaces—all these have been justified on the basis of sexist research and maintained through technologies, developed out of this research ...

Again it is the lack of evidence that is unsettling, along with the suspicion that if evidence were gathered it would not counterbalance against the more positive accomplishments of, say, the social sciences. How, for instance, can one fail to give major credit to the social sciences for the suppression of racism in the United States and elsewhere? We've come a long way since the early, racist anthropology of Edward B. Tylor (1898: Chapter III).

*Item:* Harding claims (1986:35-6) that science does not try to understand itself in a “naturalistic,” self-reflexive way. But how can this thesis be sustained in a book that does not even mention Robert K. Merton, or research entities such as the Society for Social Studies of Science? Similarly, how is it possible to criticize contemporary scientists for an alleged over-indulgence in two-valued (Aristotelian) logic while ignoring the massive literature produced by the general semantics movement of the middle decades this century, a movement that surely had a large and inescapable impact on most contemporary scientists?

*Item:* Harding (1986:48-52) questions “pure mathematics,” claiming that there is not sufficient reason to believe that mathematical expressions are value-free. She realizes, however, that the burden of proof is upon her, and her initial argument is that mathematics, alas, has the weakness of seeking internal proofs—“... no conceptual system,” she says, “can provide the justificatory grounds for itself” (1986:49). She does not inform us, however, that there are strong traditions of “external,” pragmatic testing in mathematics—e.g., Monte Carlo simulations of statistical models, or predicting the positions of planets in celestial mechanics. Regarding the first of these illustrations, it is possible to say that gambling (“double sixes,” for example) did for mathematical statistics what warfare did for nuclear physics. In essence, then, Harding asks us to fault mathematics for having both internal and external “proofs.”
Given her remarks about internal proofs, it is surprising that Harding (1986:50) makes the Marxist argument that geometry, for instance, is facilitated by systematic agriculture. Here, I see her point: Once we learn about the properties of lines, rectangles, etc., we may begin to value, say, territoriality, a general commitment to private property. But this argument is problematic because it begs the question. There must have been a powerful cultural drift toward systematic agriculture long before geometry appeared (Cohen, 1977), and therefore we must say to Harding precisely what Malinowski used to say to the Freidians: Your theories about the origins of human culture (e.g., the paricidal crisis as invoked among Freidians) assume that human culture is already highly developed prior to the time when your favorite culture-creating mechanisms come into operation. Agriculture formed our minds long before the invention of modern science.

We encounter further elaborations on the alleged poverty of deductive logic: The classic syllogism—“All men are mortal,” etc.—goes wrong in Harding's view (1986:51) because of equivocation in the word “men.” If one eliminates the equivocation by substituting the words “human being” for men, however, her argument collapses immediately. Harding moves on quickly, making the usual claim (1986:52) that mathematical formulations in the social realm are rendered impossible by irrationality, the presence of intentions, etc. But one who wishes to make this argument must present a convincing critique of the many quantitative studies of irrationality, intention, and the like found throughout the social science literature; Harding does not do so. In essence, she double-binds me: She gives me a fabulous set of metric tools, and then demands that I throw away my English-standard tools. I don't like this concept, and neither does my 'sixties-vintage truck.

**Item:** After becoming deeply involved, early on, in Harding's claims about what the male-dominated sciences have left out, we are given to understand (1986:60) that “... the public justification for women's colleges was that educated women could raise better sons.” This is an insult to the Founding Sisters.

**Item:** Harding raises several questions such as these: First, “was it entirely a coincidence that sexology began to gain status as a science hot on the heels of the nineteenth-century women's movement and women's agitation to enter science?” (1986:80); second, “is it an accident that the novel ... emerges only in the modern world purportedly ruled by scientific rationality?” (1986:232); third—a question already discussed—does the attitude of science toward nature make rape and torture appear to be a more natural relation of men to women? What all such questions have in common is that we have not arrived at a way of answering them, and perhaps we never will. Postmodernism may encourage us to answer them as we wish, but to a social scientist these are not meaningful questions unless there is some way of defining them more clearly and ascertaining whether they might be approached, say, in ways implied by Harding. Incidentally,
professors of medieval Spanish literature will be interested in the presumption that the novel emerged “only in the modern world.”

Item: Harding (1986:114-15) finds fault with Copernicus for having replaced geocentrism (earth woman) with heliocentrism (sun man?). But Copernicus knew well that the heliocentric theory violated cultural traditions that tied together religion, the Ptolemaic view, metaphors of earth-as-mother, and other powerful imagery, and that he would therefore encounter serious danger if he were to break up this lovely logic-tight compartment by telling the inexorable “one true story” of the solar system. Yet, because of the compelling nature of the story itself, he, Galileo, and others persisted in acts of immense courage that tell us nothing about their attitude, or the attitude of science, toward women or womanhood. (Nowadays, of course, there are perhaps 1.3 true stories of the solar system.)

Item: Finally, I know of no evidence that Barbara McClintock's “feeling for the organism” (1986:122), along with many additional trappings of civilization, are not shared by other scientists of comparable excellence and stature. Renato Rosaldo, as we saw, has very strong feelings, but it is not at all clear how these feelings contribute to his anthropological research. Toward the end of her book, in fact, Harding (1986:182-85) makes a major concession, presenting a sort of Bridgman-Koestler-Kuhn argument (Koestler, 1964) about the strength of emotional and irrational elements in science. To me, this negates a large part of the preceding thesis, suggesting that many scientists—most of us, one hopes—share McClintock's admirable human qualities, along with the need for making sure that these qualities do not dominate our thinking.

As we have seen, Harding (1986:50) supports the Marxist argument that plow agriculture facilitates geometry, and vice versa. This I buy; this I am prepared to sell. If you wish to understand the anteriority, constraint, and exteriority of mathematics—and to understand quantitative structuralism in sociology—seek out the appropriate machinery: plows, solar systems, engines, differential gears, airports, computers. Play with this machinery.

Take, for instance, loglinear analysis, a method that nowadays calls forth the vituperation once inspired by the statistic known as chi-square (Collins, 1984; Denzin, 1989:74). I would listen carefully to the various detractors—and to Harding—if they would take the following steps, and then provide a critique of loglinear methods:

(1) Gather together, and study for a summer or so, the best literature on the nature and uses of loglinear analysis. I strongly recommend Gilbert (1981). More recently, advances over Gilbert's work have shown, for instance, how to develop loglinear models that encompass the logic of multiple regression. This is not an instance of “fads and fashions,” as
suggested by Denzin, unless one shows that regression logic is no more important than my mothballed Nehru jacket.

(2) Using SAS, SPSS, or other appropriate programs, learn how to request and interpret loglinear output.

(3) Raise several theoretical questions that appear to be approachable by loglinear analysis, especially questions involving interaction effects. Work with qualitative variables, for it is thereby made clear that the qualitative-quantitative distinction vanishes within the socially constructed loglinear world. Several discussions of loglinear methods have used the Stouffer research on reference group theory. In my own work I have pursued an issue raised by a local politician who claims that she has been victimized many times by virtue of being black, by virtue of being a woman, and especially by virtue of being a black woman. This is an interactive hypothesis for which loglinear methods are well suited.

(4) After selecting a theory, don't obtain real data. Start with contrived data. Create many worlds, and make sure that in some of these worlds your theory seems to obtain, while in others it does not. Make sure that you have a hardware-software combination with a quick turnaround: You may have to do many experiments before you understand how the model reflects your various worlds, or what the model will tell you when a given hypothesis has a greater survival prospect than another. Compare loglinear models with simple crosstabulations, and find out what the latter cannot discern. Replicate, say, Gilbert's analyses, modify his data, and make the models behave predictably in supporting alternative hypotheses. In other words, rotate your crops.

After a season or two, reformulate your critique of quantification in the social sciences, and I'll send you, free of charge, a slightly seedy Nehru jacket.

Near the end of her book (1986:250-51) Harding tells us that science is a black box, and that we must beware the GIGO principle: Garbage In, Garbage Out. Where I live, GIGO now receives a novel and truly significant reinterpretation, one that identifies the real danger: Garbage In, Gospel Out.

(4) Solicitous gatekeepers, #1

This section illustrates what is likely to occur when we become cavalier about the rules of scientific procedure. The remainder of this book provides additional illustrations.
Date: 4 December 1992

To: Colleagues and bulletin-board forums
From: Michael A. Faia, Sociology, William & Mary
Bitnet: MAFAIA @ WMVM1

Re: ASA Footnotes and a new concept of censorship

Several weeks ago I sent the following letter to the editorial staff of Footnotes, a publication of the American Sociological Association. The letter is self-explanatory:

16 September 1992
Editorial Staff
Footnotes
1722 N St NW
Washington, DC 20036

Dear Colleagues:

As you know, I believe that my piece “On the Advice of Critics” should be printed in Footnotes as planned, even if Sandra Harding does not wish to reply.

When I talked to you on 1 July, you said that you would make a final appeal to Sandra, trying to elicit her participation. You seemed to think that my paper, at least, would appear in the September issue. Since it did not, I assume that it is delayed again. Given that we have a little time, I should like to add to my paper the following codicil:

While the preceding paper was under editorial consideration at Footnotes, it occurred to me that Harding and I could have a stimulating exchange regarding her writings on science. I suggested such an exchange in a letter to Footnotes on 6 April, conceding to Harding “the last word” on whatever disagreements might occur. I suggested that Footnotes could contact Harding with this proposal or, if preferable, that I could do so. On 15 April the managing editor of Footnotes wrote to me and Harding, saying that she would welcome a “point-counterpoint piece” from us. She set up a few “ground rules,” one of which provided that I would send my paper to Harding by 5/20/92, and that both pieces would be submitted to Footnotes by 6/15/92. The editor said that she did not wish to “go back and forth and back and forth,” which struck me as a good idea.
Therefore, on 1 May 1992 I sent the final draft of my paper both to Footnotes and to Harding. I pointed out again that “... I concede to Sandra the last word in Footnotes ...,” trying to make clear that I had no desire to “... go back and forth and back and forth ...”

Footnotes scheduled the paper(s) for the August 1992 issue.

On 23 June I received a letter from Footnotes saying that Harding “... does NOT want to write something for FOOTNOTES and does not want your piece printed as a dialogue with her.”

Clearly, we cannot allow a situation in which Harding exercises censorship over her critics.

Cordially, etc.

Addendum, 1 December 1992:

For more than two months I received no response to my letter or to my several subsequent phone calls. At last, on November 30, I received a clear decision: “On the Advice of Critics” will not be published by Footnotes, for reasons that have to do with “...the paper itself and the purposes of Footnotes ...”

Mainly, the purposes. “... We try to shy away from involved intellectual debates ...,“ say the Footnotes editors, because of space considerations. Such debates “... generate follow ups (which is a sign of success) ...” The editors (twice) “agreed to try” my paper, but over the last fourteen months, during which the paper has been under consideration at ASA, it must have degenerated, alas, into one side of an intellectual debate. Or, perhaps the taboo against intellectual debate did not exist fourteen months ago; if it did, it should have been articulated at that time and my paper should never have been scheduled for publication. Nor should I have received the ASA's letter of 18 March 1992, written weeks before I suggested a debate with Harding: “Because of the (embarrassing) proofreading last time,” I was told, I should edit my paper down “... to the 1000-1200 words we can accept in an article.” This was a non-sequitur, but it did not seem to rule out my paper on intellectual grounds.

To me, it is reassuring to know that although the editorial staff of Footnotes “... was under the impression ... that [Harding] had indicated interest in responding to you,” it is now “... clear that [Harding's] desire
to stay out for a point-counterpoint does not constitute censorship. You are free to publish any comments you wish ...”

Elsewhere.

Elsewhere: For most of my career I have understood that what was done in the United States to D. H. Lawrence and Lady Chatterley's Lover constituted censorship. I now realize that I was wrong: Those who wished to read Lawrence's chef-d'œuvre were always free to have it published elsewhere, perhaps in Bolivia.

(5) Itching and scratching: a Lazarsfeldian digression through an SPSS hologram

In my experience, for every “quantiphrene” there is at least one advocate of a more or less exclusive use of qualitative methods. We can usually count on the latter to argue that the biggest problem with the former is a tendency to let the methodological tail wag the theoretical corpus—I hesitate to invoke the usual image. The truth is that one cannot see most theoretical problems clearly unless one has the appropriate quantitative models in mind, and one cannot get quantitative models in mind unless one understands the basic logic of theory. There is a beautiful example in Porter's history of statistics (1986:316). Porter says that analysis of variance, a statistical method developed primarily by Sir Ronald Fisher, could be regarded as “... a theory of heredity.” In short, we're dealing with reciprocal causation between theory and method. It is for this reason that books like Analyzing Social Settings: A Guide to Qualitative Observation and Analysis, by John and Lyn Lofland, quickly run up against a dead end (1984:100):

It is sometimes useful and important to count how often a social unit ... occurs and to develop summarizing statistics in such forms as percentages and means. ... Even though they can be useful, such counting procedures are beyond the scope of this manual. There are many guides available ...; thus, to devote attention to them here would be redundant and would only blur our focus on qualitative analysis.

If counts are sometimes “useful and important,” one wonders whether it is wise to ignore them on the arbitrary claim that devoting attention to them would be “redundant”—an extraordinary choice of terminology given the quantiphobic tendencies of this little manual. When the Loflands claim that quantitative procedures “... blur our focus on qualitative analysis” the reader is sorely tempted to point out that the authors could have avoided, through a simple statistical demonstration, the mistaken claim (1984:101) that a low correlation between two variables rules out any possibility of causation between them. A good statistician
could readily demonstrate that the same conditions that give rise to “spurious relationships” may also give rise to spurious non-relationships.

In my courses on SPSS I use a simple program that shows how spurious non-relationships can be discerned through the use of partial tables, in a manner reminiscent of Lazarsfeld (1958). Notice that, as was true of many of Lazarsfeld's demonstrations, the variables used by this program are simple dichotomies. That is, they are the simplest sorts of qualitative variables, so that devoting attention to them should not be redundant.

I shall reproduce the program below, because I believe that anybody who runs it will realize that (1) the Loflands are wrong, in the place cited above; (2) the easiest and clearest way to demonstrate either of the two types of spuriousness is to put together an appropriate statistical demonstration, as is done by this program; (3) the easiest and clearest way to resolve subtle theoretical issues—e.g., the place of reciprocal causation in functionalist inquiries (Faia, 1986)—is to create statistical models that replicate what the theories say. Finally, I believe that this program should be thought of as a tiny slice of film from a larger hologram that represents the social sciences generally, and that this tiny slice, as is true of holograms, contains essentially the entire picture.

```
DATA LIST FIXED FILE=INLINE RECORDS=1
  /1
  FREQ1 1-2
  FREQ2 4-5
  X 7-8
  Y 10-11
  Z 13

VARIABLE LABELS
  X 'PROBLEM'/
  Y 'ADAPT'/
  Z 'LAG'

VALUE LABELS
  X Y -1 'LOW' 1 'HIGH'/
  Z 1 'X TO Y' 2 'Y TO X'

BEGIN DATA
  90 75  1  1 1
  10 15  1  2
  30 15  1 -1 1
  30 75  1 -1 2
  30 15 -1  1 1
  30 75 -1  1 2
  10 75 -1 -1 1
  90 15 -1 -1 2
END DATA

COMPUTE FREQ1=FREQ1+NORMAL(7)
WEIGHT BY FREQ1

CROSSTABS TABLES=Y BY X/Y BY X BY Z
```
This program produces six tables, the last three of which are shown in the appendix. The first table, using FREQ1, demonstrates a spurious relationship. This relationship disappears, in the classic Lazarsfeldian manner, when Z is held constant: There is no relationship in the two partial tables. The fourth table shows a spurious non-relationship: A relationship does appear, in the final two tables, when Z is held constant. The latter kind of spuriousness, contrary to popular opinion, is just as dangerous as the former. For instance, in a functionalist interpretation of a spurious non-relationship, one might argue that the non-relationship occurs because the tendency for X (a problem) to cause Y (an adaptation) is cancelled out by the tendency for the adaptation to reduce the severity of the problem. Think of this as an itching-scratching process: If itching causes scratching, and if scratching reduces itching (this can happen), then it is entirely possible that one will observe a spurious non-relationship between these two variables.

The itching-scratching example is a metaphor for a classic form of functionalist theory. If each of the N=334 social entities in the tables of the appendix were behaving according to the sine/cosine process given in Faia (1986: Chapter 6), and if those N entities were placed in the spurious non-relationship format in which no relationships appear, we would lose sight of the prospect that all values of X approaching 1 may be pushing Y values toward 1 (first partial table, lagged X to Y, wherein itching is now presumably activating a process of scratching), while all values of Y approaching 1 may soon begin pushing X values downward (second partial table, lagged Y to X, wherein scratching is now presumably reducing the amount of itching).

Ironically, the Loflands (1984:113) provide an excellent example of precisely this sort of process. In a brief discussion of functionalism, they develop the example of “deviant roles among policemen,” pointing out that “... the performance of certain unpleasant tasks is necessary for the maintenance of the police department.” As is often the case in functionalist explanations, “maintenance” is not clearly defined. However, it would be appropriate in my SPSS example to define the Y variable as “unpleasant tasks” (i.e., scratching) and the X variable as a problem (i.e., itching) involving maintenance of something within each of N=334 police departments: Arrest records, for instance, adequate to meet legal demands and requiring many person-hours of onerous labor. Within
the framework of purely qualitative methods I do not see how the Loflands could have done anything more with this problem than what they did: They more or less defined the key variables. But social science theories involve a lot more than the mere definition of key variables.10

I maintain that, in demonstrating functionalist theory, no amount of verbal vigor would be as effective as the Lazarsfeldian tradition of playing with partial tables. What is required in this sort of didactic exercise is an appropriate combination of verbal and quantitative insight. When authors and publishers produce books and articles on (purely) qualitative sociology, when undergraduate courses are organized around these proliferating texts, when library references such as Sociological Abstracts classify and code many social science publications as exemplifying (purely) qualitative sociology, we are back to the old high-school bifurcation, the old tracking practices that separated those who wished to take qualitative analysis for their chemistry requirement from those who seemed qualified for the more rigorous and realistic approach. A widely used textbook suggests that quantitative and qualitative approaches to chemistry, ideally, should be “entwined” (Harris, 1987:xiii). Ditto for sociology.

(6) Transcending the transcendentalists

Seidman's [1991] criticisms are nothing compared to the active disregard by theorists in many other fields—literature, feminist studies, politics, anthropology, history, and more—who once looked first to sociology and now hardly notice us at all.

—Charles Lemert (1991:164)

This dreadful indifference has been breached in a recent article by Gross (1989), who demonstrates what he considers a proper use of sociological findings: The 1850 United States census can be used to help students understand the lifestyles of the transcendentalist writers of New England. Gross says (1989:504) that one of his major goals as a scholar and teacher is to help reconcile the “two-cultures” syndrome that separates students who excel in the natural sciences from those who excel in the humane disciplines. This is surely a worthy objective, but Gross (1989:512) quickly gets into trouble when he argues that

By placing the census back into history, my class was led to an important insight into quantitative analysis: numbers are never enough; they must always be restored to the context from which they emerged. Following that intellectual path, we went on to seek in Emerson's biography an understanding of how he and Lidian had fashioned their household over the years ...
The trouble arises because Gross does not make it clear that these same sentences can easily be stood on their head; in other words, he is describing an intellectual process that is capable of running in reverse.\textsuperscript{11} If Gross's remarks above make sense, then it also makes sense to say that

by placing biography back into demographic history, my class was led to an important insight into qualitative analysis: biographies—highly detailed case histories—are never enough; they must always be restored to the larger social context from which they emerged. Following that intellectual path, we went on to seek in the census tabulations an understanding of how Emerson and Lidian and other such families had fashioned their households over the years ...

Gross shows us, in brief, that census tables are subject to interpretation. But why not show us also how interpretations are subject to census tables? Reading about the Emersons and their domestic life, a clever sociologist would quickly arrive at the hypothesis suggested by Gross' Table 1 (1989:509): In the days of the transcendentalists a young man was expected to become head of a household, perhaps a very large household, but was rarely able to do so prior to reaching the age of about thirty or thirty-five. Similarly, anybody who reads the feminist literature of the early years of the twentieth century and discerns the close relationship between feminist issues and those of the temperance movement, would probably be delighted to discover a tattered census volume (Bureau of the Census, 1908) that fell into my hands a few years ago. This book contains a mammoth table, continuing for hundreds of pages, that permits the testing of a classic feminist-temperance interpretation: That whenever large numbers of divorces involve male dominance, it is also likely that large numbers of divorces will involve “drunkenness.” The method of analysis would be correlational, while the inspiration might well have been biographical.

On the Emerson question: If you had one unit of time and energy for study, and devoted .5 to the 1850 census and .5 to Emerson's family, you would probably learn the equivalent of (.5)(.5)=.25, a reasonable proportion of what you need to know. If you devoted .1 to the one topic and .9 to the other, you would learn (.1)(.9)=.09, a far smaller proportion of what you need to know. (Waldo would love this formulation.)

In conclusion, I believe that sociology should continue to emphasize macrostructures, numbers, systematic measurement, quantitative models, empirical data, and carefully selected samples. But perhaps my view on these matters is perverted, and I must confess that in my “methods” classes and elsewhere I love to defend the following truth: For beauty, for clarity, for elegance, for immense power combined with subtle intricacy, there never was a sonnet written by Petrarch or Shakespeare or Elizabeth Barrett Browning, there never was a poem of any genre nor will there ever be a poem of any language,
What's Wrong with the Social Sciences?

That will surpass the IRS 1040 Long Form. This glorious set of documents is the ultimate social science instrument: It is highly complex, with elaborate interrelationships among its many parts, and yet it is parsimonious; it is incredibly informative, yet without overtaxing the mental capabilities of intelligent respondents; it is verbally clean and sharp, and it is mathematically clean and sharp; in its careful construction, it embodies the insights and inspirations of a long series of scientific inquiries. It is a work of the social sciences, and it is a work of art.

And one final thought: Given the sophisticated research that it makes possible, the IRS 1040 Long Form expresses our highest estimation of the capabilities of the “object” of study and our sense that this particular respondent has a strong commitment to the social contract—if a social contract may be said to exist.

I recently had occasion to express openly my convictions and feelings about the IRS 1040 Long Form. The opportunity came during an academic seminar, a weekend retreat, that involved several colleagues charged with the task of developing an interdisciplinary graduate program in American Studies that will span American history, literature, and popular culture, along with several social science fields. As it turns out, all participants in this program have a lot to learn, especially from one another. To me, for instance, the history of “vernacular architecture” in the United States is a highly esoteric realm, one in which I will probably never feel at home, and one in which I may never have the temerity to undertake a research project. But I was nonetheless mildly shocked to discover that several of my humanities colleagues, when they first heard about the sort of interview that occurs in the NSFG, would react in a way that reminded me of Philip Roth's (1971) account of Richard Nixon's reaction upon learning about the sexual practices of homosexuals. My colleagues could not believe that social scientists would have the bad taste to ask highly detailed questions about the marital experiences, sexual behavior, and fertility histories of a cross-section of American young adult women. It was my feeling that this sort of culture shock must be overcome quickly in a cross-disciplinary program, if it is to succeed.

I should point out that the NSFG came up during a discussion of my introductory seminar on American Studies. The discussion followed a cocktail party, and my interpretations of the discussion may have been clouded by vernacular inebriation. However, it would be no more difficult for me, while sober, to explain sociology to my postmodernist colleagues than it would have been for Rosaldo (1989:3-4) to explain exchange theory to Ilongot headhunters. If only he had been willing to take the time.

(7) What do you presuppose, and when did you presuppose it? The Sisyphus of the social sciences
Jeffrey Alexander's *Theoretical Logic in Sociology* is a multi-volume opus in which, among other tasks, the author elaborates his reasons for believing that a major trouble with the social sciences is that they try too hard to emulate the natural sciences; the thesis is developed further in a recent work (Alexander, 1988). Alexander (1982:7-8) is afraid that if we follow the advice of Hans Zetterberg and attempt to set up systems of knowledge replete with axioms, deductions, and theorems, the results are likely to be embarrassing if not disastrous. He believes that the empirical and positivist emphases of contemporary sociology have destroyed any deep interest in theoretical matters of the most abstract nature; he cites (1982:11-12) Robert K. Merton's advocacy of theories of the middle range as a case in point. Although Alexander rejects “human studies” as an alternative to positivist social science (1982:15-18), he believes that we must move toward an “alternative conception of science” in which we recognize, with Alexandre Koyre, that “experimentation is a teleological process in which the goal is determined by theory” (1982:24). As an illustration of Koyre's thesis Alexander cites the case of Einstein and relativity, claiming that the famous Michelson-Morley experiments, contrary to conventional wisdom, were not a major source of inspiration for Einstein (1982:31). And even if social scientists were to clean up their act and begin practicing science a little more honestly (or with a little less self-delusion), they would not necessarily achieve any insight into the larger philosophical issues that interest Alexander.

In Alexander's view, only those scholars “who have thoroughly and explicitly broken from the positivist persuasion will even attempt an exercise in general theoretical logic” (1982:36)—this overstatement recalls Collins' claims (1984:330) about intolerant statisticians. And once having made the crucial break, Alexander's rehabilitated former positivists soon realize that questions of “action and order,” which pertain primarily to the place of human agency and free will as causes of social phenomena, “... represent the true presuppositions of sociological debate; they establish a general framework that cannot be subsumed under other kinds of theoretical dispute and, at the same time, they manifest properties that decisively affect sociological thought at every level of the theoretical continuum” (1982:65). Having deepened our understanding of action and order, we shall come to a clearer appreciation of the “nonrational elements in science,” realizing that “... science itself gives the lie to conceptions of action which presuppose instrumental rationality” (1982:75).

None of these arguments is new, and none is at all persuasive. In the contemporary social sciences one finds elaborate deductive models—for instance, stable population theory, learning theory, computer simulations of “world systems” or national economic systems or other complex elements of societies or human psyches—that make the Zetterberg approach appear primitive, anachronistic, and to be a mere historical relic. One can easily argue that Robert Merton has been the most important philosopher of science ever produced by
American sociology, and that his contributions to “middle-range” theory have not detracted in any way from this accomplishment. When Alexander affirms, as he did at a 1986 symposium, that no significant difference exists between “disaster research” in the social sciences and Shakespearean studies as pursued by English departments, he reveals a surprisingly strong willingness to assimilate the otherwise disparaged modus operandi of human studies. When he cites Koyre's claims about the teleological character of scientific experimentation, he does not bring to our attention the many instances in which social scientists have abandoned major hypotheses on the basis of disconfirmatory evidence—Barber (1987) provides several such instances—nor does he bother to tell us that we could probably rid ourselves of additional untenable notions if we were to emulate more often the natural scientist's routine practice of carrying out replications.

As I've mentioned, our recent repudiation of racism is a case in point, especially when one considers that what social scientists were saying about race, a century ago, would nowadays pass muster with a Kluxer.

On the matter of “general theoretical logic,” Alexander does not produce a trace of evidence for his own presupposition that scholars of the “positivist persuasion” show little if any interest in general theoretical issues. My experience, on the contrary, has been that the best social science philosophers tend to be statisticians with a strong empirical and positivist bent and with a scientist's skepticism; Sir Ronald Fisher is my ideal type. Alexander never tries to persuade us of his claim that questions of action versus order are the central presuppositional issue, nor that one's manner of resolving this issue will “decisively affect sociological thought.” Again, I have equally compelling evidence that one's beliefs about human agency have virtually nothing to do with one's performance as a social scientist, except in the sense that ridiculous, self-delusional beliefs may sometimes cause us to ignore reality while maintaining excellent intellectual interchanges with those similarly deluded. Finally, the rationality-versus-nonrationality issue, as I tried to show in my earlier discussion of Economic Man, is a red herring: The social sciences focus on both types of behavior and ideally show no favorites. Beyond that, if instrumental rationality emphasizes the means-ends nexus, and if (as is perfectly clear) the means-ends nexus is merely a special instance of the cause-effect nexus, then I must point out that I have learned far more about instrumental rationality from statisticians such as Sir Ronald Fisher, who clarify causal analysis, than from philosophers such as Jeffrey Alexander, who do not.

And one minor point: If Alexander really believes that “… science itself gives the lie …” to instrumental rationality, then either (1) his claim about a social-science bias toward rationality goes up in a poof or (2) he has to argue that social scientists do not pay attention to their own findings. I lean slightly toward the first option.
Alexander's approach to the social sciences leads us dangerously astray, and I believe that it does so for a very simple reason: It almost never grapples with real, day-to-day, nitty-gritty problems of social science research and analysis. I don't have any objection to the idea that a small coterie of scholars might practice Alexandrian sociology, but it would be most unfortunate if they enticed the rest of us to follow their lead.

Take the case of sex preselection, the process in which parents try to control the sex of their offspring (Bennett, 1983). An ordinary social scientist struggling to understand the nature, determinants, and consequences of sex preselection is far more likely to learn something about presuppositions of a philosophical genre than is Jeff Alexander to learn anything about preselectivity of an infant's gender. We cannot understand the invention of sex preselection methods unless we first ask whether inventions occur more or less by accident or because “necessity is the mother of invention.” We cannot understand the willingness of parents to use sex preselection unless we first understand the marketability of the methods, their efficacy, and their diffusion through society. We cannot understand the ways in which potential parents negotiate their preferences for boys or girls without first considering interpersonal power and other aspects of interaction of men and women in marriage (Charles, 1993). And these several questions of human agency are a mere exercise in sound and fury if sex-preselection methods, evaluated objectively, do not work, if they are too costly or cumbersome, or if they have damaging side effects. Finally, we cannot understand the demographic and social effects of sex preselection unless we first develop an appreciation of social structure, recognizing on the basis of etic exercises in social simulation that a society with a large excess of marriageable males (or females), yet valuing monogamy and the one-child family, has a large problem.

The problem, to be sure, has to do with “free will,” involving in this instance whether or not we marry the person we prefer at a time we prefer. Whenever the marriage market is tight we experience, under an appropriate operational definition, less free will—it is a matter of degree. Alexander (1982:101), by contrast, tells us that Talcott Parsons once addressed the free-will conundrum in the following either-or language:

Positivistic ... thought is caught in the “utilitarian dilemma.” That is, either the active agency of the actor in the choice of ends is an independent factor in action, and the end element must be random; or the objectionable implication of the randomness of ends is denied, but their independence disappears and they are assimilated to the conditions of the situation, that is to elements analyzable in terms of nonsubjective categories ....

Or, we resolve the dilemma by taking what I shall call the statistician's approach: (1) We know, a priori, that the ends involved in mate selection are neither
“independent” of nor entirely determined by “the conditions of the situation”—they contain a measure of both; (2) therefore, we write a multivariate equation that enables us to explain mate preferences as influenced by “nonsubjective categories” such as the sex ratio and (why not?) various subjective categories, and also enables us to isolate those elements of mate preference that we cannot explain, elements that appear to be random; (3) we then analyze the mate-selection market, perhaps using multivariate statistics again to ascertain whether mate preferences can be fulfilled to an appreciable degree; (4) in a situation where there is a strong preference for, say, endogamous marriage, but where we find that ego's characteristics (or preferences) are a poor predictor of mate's characteristics, we ask ego, in a detailed way, whether he or she has a sense of lacking freedom of choice in mate selection. Whenever we receive a large number of affirmative answers, and especially when these answers seem to have a relationship to largely non-subjective conditions such as the sex ratio, we conclude that free will has been diminished—although we might wish to call the process by another name. This, to me, is the only definition of free will that has any utility in the social sciences, and the Parsons-Alexander formulation simply obfuscates. Colleagues who do not like either approach are free to reject them, of course, in a willful act of polymorphous perversity. In any case, an excellent discussion of the statistician's way of approaching questions of free will is to be found in Berger (1963: Chapters 4 and 5).

But perhaps I only say these things because I do not know, nor have I ever known, what Parsons meant by “randomness of ends.” Again, a matter for hermeneutics. On the other hand, I do understand what physicist Stephen Hawking means when he says that many questions lie outside the realm of science, and that pursuing such questions is a little like asking what takes place a mile north of the north pole. Refusing to heed Sica's (1988:220) “... need, nay, demand to know unknowables ...,” the wise scholar heads south, where he has a prospect of learning something.

Alexander shares Sica's aspiration: He wishes to know unknowables. In many ways he has made himself the Sisyphus of the social sciences: He sets up the goal of becoming a sociologist because this is a way of understanding human action and social order; then, he declares that the goal is unattainable because man is the ultimate mystery. Although it is to Alexander's credit that he shows deference to Heisenberg (Alexander, 1988:80), he generally emulates the various anti-positivists, discussed earlier, who insist upon the complexity of the human and the comparative simplicity of nature:

Insofar as the objects of a science are located in the physical world outside of the human mind, its empirical referents can, in principle, more easily be verified through interpersonal communication. For social science, the objects of investigation are either mental states or conditions in which mental states are embedded. For this reason, the possibility for
confusing mental states of the scientific observer with the mental states of those observed is endemic. This is the social science version of the Heisenberg Uncertainty Principle.

Analyze this statement in detail: It does not make sense. Let the “conditions in which mental states are embedded” be an airliner, or a simulator of an airliner. (It is relevant here that aviation simulations generally put social science simulations to shame.) Suppose that we are studying two-person interaction involving pilots and first officers (co-pilots) under emergency conditions, or under conditions in which a mistake or a distraction could easily create an emergency. We “observe” mental states in the usual ways: We infer them from recorded man-machine interactions, from recorded dyad interactions, from direct observation, from post-experimental de-briefings, and so forth (O'Hare and Roscoe, 1990). When I think about this research setting, I cannot imagine why my mental state, as observer, is at risk of being confused with that of the pilot or first officer. If high risk were demonstrated, we would take the sorts of methodological steps routinely used by psychology: Design double-blind experiments, test for halo effects, and so forth. And remember: These kinds of problems, and these kinds of proposed solutions, are not confined to the social sciences.

In short: A carefully designed experiment creates circumstances under which subjects are likely to fail a “Turing test”; that is, they will not know whether the experiment is conducted by man, woman, or machine. Turing tests, of importance in connection with artificial intelligence, will be discussed in some detail in the next chapter.

The silliness keeps rolling over us. Alexander continues (1988:81): “...while precise empirical measurements of two variable correlations can sometimes be established, it is rarely possible for such a correlation to prove or disprove a proposition about this interrelationship that is stated in more general terms.” Of course it is, and this is precisely why we have learned to use some variant of the Lazarsfeld strategy for generalizing findings. In his ensuing discussion of Blau's demographic structuralism, for instance, Alexander points out that rates of “outgroup relation” are said to be influenced by the relative size of various population segments. But the concept of outgroup relations, he says, “... does not have a clear-cut referent” because Blau relies too heavily on exogamy as an indicator of such relations. Again: This is nonsense. Assuming that Blau has not yet falsified his own hypothesis—and even if he has—it is incumbent on the rest of us to design studies that will use indicators of outgroup relations other than exogamy, such as forming business partnerships, attending the same church, etc. That is, we generalize the hypothesis. We then apply it to different populations, different kinds of outgroups, and so forth, thereby generalizing it further. This entire process of generalizing findings is discussed in a later chapter of this book, in relation to social science taxonomies.
Enough! I always felt that Sisyphus' major problem was that he would suffer a crisis of confidence, and allow himself to be run over by very small stones.

(8) The meaning of politics and the politics of meaning

I was once told by a theorist who denied the possibility of correct interpretation that I had not interpreted his writings correctly. —E. D. Hirsch, Jr.

The theorist, of course, probably had misinterpreted Hirsch; this merely compounds the problem. Novick (1988:278) alludes to the same problem when he points out that relativism cannot become an absolute truth, lest it refute itself.

I have no objection to the idea that “meaning” has a large role in the social sciences. Indeed, it would be impossible to develop any science—especially the social sciences—without devoting appropriate attention to semantics, semiotics, interpretation. My quarrel is with inappropriate attention, and with the notion that an emphasis on meaning requires hostility toward science. In many instances human subcranial phenomena have essentially nothing to do with the subject under investigation—and I willingly concede all the necessary Kantian caveats and disclaimers. Michotte's (1963) famous research into how we perceive causation made it clear that causal interpretations are primarily the ways in which our minds impose order on a complicated, recalcitrant world. Any good statistics professor makes the same point time and again, but in making this point one surely does not have to abandon science; in fact, one must continue to have the scientist's proper humility in the presence of a world in which the human species has a small, uncertain, and confused role.

Again, it is the ego-tripping of the anti-science factions that I find especially onerous. When scholars argue, as they do so often nowadays, about the relative importance of “interpretive” and “structural” phenomena, it is not unusual for the argument to take a political turn. Somebody is likely to proclaim that structuralists are propounding “iron laws,” denying free will, insisting strongly and wrongly that the limitations of these laws will never be overcome. Somebody else is likely to counter with the argument that we control our own destiny by recognizing that iron laws do not exist, and that the fundamental realities have to do with our ideas about a world that remains a “blooming, buzzing confusion” until such time as we define or re-define it. An astute structuralist is then likely to reply that interpretations that enable us to ignore objective social and natural realities, whether or not these realities involve iron laws, are nothing more than an opiate of the masses and that the masses will never be free until they stop deluding themselves. The “opiate of the masses” thesis, interestingly, often carries the implication that power elites—the exploitative class—have a clear
perception of whatever laws may exist, iron or otherwise, and have learned how to make use of these objective realities. The exploiters, in other words, understand politics. Eventually their emics may become the etics of the rest of us. When Anthony Giddens accuses structuralists of regarding people and their perceptions as “bloody silly” he fails to realize that this is surely not our attitude toward the exploitative classes (cf. James, 1984:119).

The above scenario, by accident, eventually associates structuralism with a sort of “liberalism” and interpretive sociology with a sort of “conservatism.” But one could readily design an alternate scenario in which either structuralists or interpretationists—you pick ’em—would be tainted by an association with the extreme left, the extreme right, or the extreme center.

What science seeks to do is to make all of us members of the power elite. It proposes to do so by forcing us to abandon meaningless questions such as the relative importance of interpretive and structural phenomena, the centrality (or lack thereof) of human agency, rationality, irrationality, or nonrationality; as far as I’m concerned, Bourdieu’s “constructivist structuralism” (Turner, 1991:508-18) provides a fine resolution of this question. Once having abandoned this futile inquiry, we will necessarily devote less time to the equally futile search for some sort of correlation between one's politics and one's stand on the meaning-versus-structure conundrum. When Parson Malthus tells us, as he did in the several rapidly evolving editions of his famous essay on population, that we had better not run afoul of the laws of population growth but that, paradoxically, we willfully overcome these laws by exercising “moral restraint,” we realize that we are in the presence of a scientist whose ideas evolved on the basis of new evidence (Petersen, 1979), who therefore had a reasonable prospect of overcoming his various biases, and whose work cannot be elucidated at all by those who insist upon teasing out of it some sort of stand on meaning-versus-structure, or some sort of political ideology.

The labels traditionally attached to Malthus, political or otherwise, are no more justifiable than those attached to contemporary scholars who make the sad mistake of identifying themselves with particular structuralist or interpretive movements; for this mistake one may deserve a friendly excoriation, but one does not deserve to be labelled as a carrier of easily diagnosed political values. To paraphrase an earlier remark of my own: If every political label can be affixed to Malthus, then no political label can be affixed to Malthus. As Collins implies (1987:182), the relationship between one's politics and one's theories is best characterized by the null hypothesis: Often, there is no demonstrable relationship. As an illustration, Collins points out that “the mixture of positivism and Establishment politics that [existed] in the 1950s and 1960s was particular to that historical epoch. In the 1700s, the situation was exactly the reverse: it was the proscience faction—Voltaire, Condorcet, the Jacobins, Bentham and the utilitarians—who were the revolutionaries and reformers ...” If regress we must, let us go back to the Enlightenment!
As we try, perhaps from a Malthusian perspective, to explain fertility trends in contemporary third-world nations, moral restraint doubtlessly has a role. But if we continue shouting at one another about the a priori importance of moral restraint, or about whether good guys or bad guys emphasize it, and if we continue to do this as a substitute for studying moral restraint scientifically, we shall never understand its role. One of my mentors used to teach his students that society is like a baseball game. He spent hours drawing out the various analogies, and his presentation was most edifying. He never tried to decide whether the distance between the bases was more important, or less important, than Casey Stengel's latest philosophical breakthrough. Beyond that, we learned a lot about his attitude (and Stengel's) toward science, and little about his politics or about what happens a mile north of the north pole.
NOTES (Chapter 1)

(1) I adopt, in combined form, all of Halfpenny's (1982:11) various definitions of positivism: “Sometimes, to be positivist means no more than to be scientific ... Sometimes, positivist sociology is synonymous with statistical analysis ... Sometimes, to practice positivist sociology is to seek to establish causal explanations, or to search for fundamental laws ..., or to insist upon objective empirical information systematically organised to generate or test hypotheses.” Halfpenny (1982:32) implies that these dimensions of positivism have not always gone together. The real power of the positivist tradition, in my view, derives from its having combined these dimensions.

(2) MacLaine, however, is not the only scholar thus afflicted. White (1989:27) tells us that “there is no necessity, logical or natural, governing the decision to emplot a given sequence of [historical] events as a tragedy rather than as a comedy or romance.”

(3) Discussing the popularization of Jacques Derrida in the United States, Lamont (1987:614) suggests that “... like Foucault or Habermas, Derrida offered American humanists a criticism of science that was much needed to promote their own intellectual products.” If these humanists compete for resources against social scientists, then it is probably most unwise for us to make remarks such as the following, from a sociologist at Yale University (Stanfield, 1987, xi):

As we near the end of this century, it is becoming apparent that there are inherent dangers in embracing rationalistic approaches to the human world uncritically whether they are found in the social sciences, journalism, corporate management, or in policy studies.

We have been reminded in tragic ways of what happens when we reduce human beings down to path analysis, probability models, and cost/benefit analyses: the escalation of the Vietnam War; the Union Carbide catastrophe in India; in Institute, West Virginia; the destruction of the space shuttle Challenger; and the expansion of an American underclass—disproportionately black and latino—no one seems to know what to do with. In all of these cases, “policy blunders” have been traced back to human mistakes, human coverups, human biases, and human limitations.

As a social scientist, Stanfield is shooting himself in the foot while tying his hands behind his back. But the most interesting aspect of these remarks is the way in which the last sentence contradicts everything that precedes it. Most of the mistakes, coverups, biases, and human limitations that I’ve come across did not
result from rationality, path models, probability theory, and the like. They occurred primarily because human minds and social organizations were not behaving rationally. But given the ambiguity of Stanfield's statement, it is perhaps inappropriate for me to use the words “result from”: This author is not proposing a causal hypothesis; rather, he is proposing a way of feeling.

(4) In a more easterly direction, sociology is said to be flourishing. See Shelley (1989). The practices and perspectives of the social sciences are now widely diffused; fortunately, it is therefore possible to love sociology without loving sociologists.

(5) To be sure, it is possible to create a pseudo-complexity by playing games such as the following (Lukes, 1992:425-26):

[The authors] ... treat their composite Lockean individualism as a narrowing and distorting ideology. In this vein, they have much of interest to say, in condemnation ... of cost-benefit analysis as applied, among other things, to the value of life, ... as when, for instance, an Environmental Protection Agency economist, asked “What about the theory that human life is priceless?” answered, “We have no data to support that.”

Our better seniors, having had a methods course or two, provide a much more compelling answer than does this inept EPA economist: The idea that human life is priceless, they would say, means nothing until we carry out the difficult tasks of defining this idea in sociological terms.

(6) I recently had an opportunity to read about and discuss “black sociology” in some detail (Staples, 1976). Black sociology, like feminist standpoint sociology, has much to recommend it, but it is seriously impaired by the way in which it tries to displace mainstream sociology with specialized interpretations that allegedly can be comprehended only by blacks, or by black scholars. Black sociology, if widely adopted as a unique interpretation inscrutable from a general social science standpoint—i.e., “ambiguous” in Levine's (1985) sense—would be a disaster both for sociology and for blacks. On the other hand, if black sociology is primarily an attempt to help the discipline overcome biases and prejudices, then it makes the same contribution to science that I attribute to Rosaldo earlier.

A major objective of black sociology is to attack racism, and a major objective of feminist sociology is to attack sexism. It would not be at all surprising if the style of sociology discussed in the next chapter, that of Michel Foucault, were enlisted for the campaigns against homophobia. One cannot help but notice that these unique orientations and interpretations, despite their strong commitment to social change, have trouble cooperating with one another.
Harding (1986, 1991) has made little progress in showing why black women should support feminism, and Sawicki's (1991) study of Foucault's contribution to feminism is singularly disappointing: Mainly, we learn that although the “new reproductive technologies,” for instance, may have encouraged American physicians to consolidate control of reproductive behaviors of women (1991:76), it is entirely possible for women to resist this control (1991:80-94). One hopes that those who do resist will not be influenced by the untenable claim (1991:76) that during “…the first decade of this century, physicians waged a successful campaign against midwives…” and that “…maternal and infant mortality rates actually increased after midwives were eliminated.”

Incidentally, I should like to point out, with Jesse Jackson, that if everybody is somebody, the main conclusion is that everybody is somebody.

(7) Caveat: “When men want androgyny, they usually intend to appropriate selectively parts of ‘the feminine’ for their projects, while leaving the lot of real women unchanged” (Harding, 1986:145).

(8) I know little about the interpretation of cannibalism from the standpoint of cannibals. From the standpoint of the victims, however, I would surmise that there is only one true story.

(9) Regarding novels, the rape-of-nature thesis, androgyny, speaking freely, and Harding’s acknowledged problems (Chapter 7) in trying to relate feminism to the needs and epistemologies of the “third world,” I note that one of our colleagues, a novelist as well as a sociologist, has written the following description (Greeley, 1990:134) of a situation in which a preacher has just delivered a homily on how to protect “little people,” such as crickets, grasshoppers, men, dragonflies, women, and frogs, from the cold of winter:

Jamie told it with marvelous skill, recounting it as something he had heard from an Indian storyteller and then putting himself into the storyteller's role (a “Keeper of the Talking Sticks”) so that he actually became a Chippewa/Cree. ...

“So the old man said to the little children, 'Look up at the mountaintops. They are the breasts of Earth, our Mother. She has already taken off her clothes and bared her breasts to the cold so she can wrap us up in her garments and keep us warm during the winter.'”

No applause ..., just stunned silence.
You don't talk about baring breasts in church—not even if Jesus describes himself as a nursing mother in the seventh chapter of Saint John's Gospel.

It is entirely possible that we feminists will have to learn to embrace the ancient metaphor (or archetype) of earth as mother.

(10) Unfortunately, even a few statistics textbooks do not realize the importance of spurious non-relationships. Runyon and Haber (1988:174), for instance, say that “when low correlations are found, we are strongly tempted to conclude that there is little or no relationship between the two variables under study. However, ... the failure to find evidence of a relationship may be due to one of two possibilities: (1) the variables are in fact unrelated or (2) the variables are related in a non-linear fashion.” A third possibility, of course, is that the lack of an observed relationship is spurious.

(11) Throughout this book, I evaluate arguments by re-stating them in reverse. It is a trick I learned from physicists who experiment with the idea of running time in reverse.

(12) For a contemporary illustration of an axiomatic formulation that relies heavily on the Zetterberg approach, see Turner (1986:441-53).

(13) See, for instance, Barber (1987:166), which explains how Alan F. Westin's research forced him to change his mind about the impact of computers on the right to privacy. Another excellent example is provided by Fogel (1989:391), who at one time subscribed to a functionalist incompatibility hypothesis that he later was compelled to abandon:

Between 1968 and 1972 my view of the moral problem of slavery changed considerably. The discovery that slave plantations were more efficient than free farms challenged my beliefs ... The notion of efficient plantations created a dilemma because it was difficult to see how individuals so deeply oppressed ... could nevertheless produce more output per worker than free farmers. It ... seemed to imply a far greater level of labor discipline and a far greater degree of ... acquiescence to the objectives of the planters than I was prepared to entertain.

(14) Twice now—here and in footnote 3—I have adumbrated my feelings about Levine's (1985:15-19,218) hypothesis that one of the functions of ambiguity is “... the bonding of a community through diffuse symbols ...” I have serious doubts about this hypothesis. Levine (1985:14) also expresses agreement with Richard Dewey's claim that sociologists have a “... pervasive indifference ... toward the
absence of semantic consensus in the terms they use ...” But this claim is no more convincing than Sica's (1988) insistence that we tend to ignore non-rational or irrational aspects of behavior. The two positions, in fact, are contradictory: If we had Levine's hypothesized indifference toward ambiguity, then by virtue of that very attitude we would be making a strong commitment to irrationality.

(15) Rorty, in fact, confirms my own Kantianism when he describes Derrida, with admiration, as a kudzu overgrowing the Kantian edifice (Rorty, 1991:235). We are now engaged in a great battle: cutting back the kudzu. The original edifice looked a hell of a lot better.
REFERENCES (Chapter 1)


WHAT'S WRONG WITH THE SOCIAL SCIENCES?


WHAT'S WRONG WITH THE SOCIAL SCIENCES?


### An Adaptation Process Obscured by a Spurious Non-Relationship

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An Adaptation Process Obscured by a Spurious Non-Relationship (cont'd)

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Controlling for...
Z LAG Value = 1 X TO Y

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Cramer's V           .59661
Contingency Coefficient .51235
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Y ADAPT by X PROBLEM
Controlling for...
Z LAG Value = 2 Y TO X

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The major purpose of this chapter is to have a little fun. Foucault and Foucaultians have challenged the social science disciplines in fascinating ways—some good, some bad, some ugly. This tripartite taxonomy will not satisfy everybody; I suspect, however, that Foucault himself would have liked it and that, in the end, there is little difference between Foucault and Comte.

(1) “Man the machine—man the impersonal engine”
(Twain, 1917:5)

During the last few years I’ve taught a course on the social and psychological impact of computers. Several of the students enrolled in this course last spring were computer science majors, and they were surprisingly skeptical about artificial intelligence, about “thinking machines”; a few had read the Dreyfus brothers' critique of machine intelligence (Dreyfus and Dreyfus, 1986). At the time I had not yet discovered that one of the brothers, Hubert, is among those scholars who have introduced Michel Foucault and his philosophy to English-speaking audiences. I would be most interested in pursuing the hypothesis that scholars friendly toward the Parisian brand of structuralism—or post-structuralism, for ideas move fast in the City of Light—tend to be unfriendly toward machines who may soon have the temerity to affirm, with Descartes, cogito, ergo sum. Still, it is a little disappointing that Hubert Dreyfus's book on Foucault does not contain any reference to machine intelligence, because one who understands the basics of machine intelligence has a good prospect of understanding what is wrong (and right) with Foucault. But this, of course, was not Dreyfus' mission.

As my students and I began arguing more vehemently, I made the slightly platitudeous point that just as airplanes finally were not modelled on birds, so the thinking machine does not necessarily have to be modelled on human brains with all their foibles, let alone the human mind (Wolfe, 1991). The students immediately shot back the slightly less predictable claim that birds do not find it necessary to communicate with airplanes, while computers must communicate with humans—at least, so say we. My response, again somewhat predictable, was
to introduce the famous Turing test: If, in a controlled experiment, a human being cannot determine whether he has another human being or a thinking machine on the other end of the line, then it has been established that the machine, presumably, has both brain and mind. Nowadays many chess programs readily pass this test even when there is a possibility that the randomly selected thinker on the other end is a human grand master. I suspect that people (and computers) who make a living by buying and selling stock index futures cannot be certain nowadays whether the competition consists primarily of women, men, or machines. Actually, the computers may have a better prospect than their masters of making an educated guess as to the nature of the recalcitrant being on the other end of a transaction.

At this moment, however, even though things were looking pretty good for my argument, the students were preparing to devastation me by pointing out that my brainless and mindless machine would never realize that the following two grammatical structures, although they have everything in common, are diametrically opposite in meaning:

“The chicken is ready to eat.”

“The tiger is ready to eat.”

Before they had an opportunity to toss off another classic conundrum—I expected “the pen is in the box” versus “the box is in the pen”—I delivered what I considered to be the only appropriate response: “Granted,” I said, “it is not easy to wreck a nice beach.”

Of course they fell for it, hook, line, sinker, body and soul, brain and mind, insisting that the one thing that sets them apart from the lower animals and from the lower forms of humanity and from the inanimate realm is that they have this unique, infinite, God-given capacity to wreck a nice beach, that there never has been nor will there ever be an ugly, disgusting, filthy, greasy, clanking machine that will wreck a nice beach as expeditiously as they have, way back to the days when they were very small children.

The next step in my argument—this is a very mechanical process for a human being, is it not?—was to point out that while it is true that airplanes, when they became successful, were not modelled on birds, it now turns out that birds actually have evolved several features that work to advantage for airplanes. I don’t recall where I read it, but recent research on bird locomotion shows that, at least for some species, feathers located toward the wingtips have a way of twisting as the bird accelerates, so that the feathers function as propellers. This discovery leads me to believe that if human beings must communicate with computers, we might try to emulate some of their more benign characteristics. For starters: the immense capacity for clear communication now built into “user-friendly” computer systems.
I then insisted, as always, that my computer science curmudgeons read an excellent classic by Bolles (1972) dealing with one of the clearest and most computer-like forms of human communication, i.e., baby talk. They soon discover, among other things, that

The Swahili word for mother is “mama.” This is the first word spoken by the majority of babies born into families speaking English, Swahili, French, German, and ... other languages. Words for other close relatives can also sound alike even if they are in widely separate languages. I once talked with two Africans who happened to mention that in their tribal language the word for father was “dadi,” pronounced the same way American children say “daddy” when they start speaking.

Having noticed that telegraph operators are among those who like to say “da-di” a lot, I prepared the following communication—the clincher to my overall argument—and sent it forth not only to my CSc students but as a public announcement over the electronic mail networks that bind my university to the remote outposts of the Western world:

```
  WILL

di-da-da di
  WE

da-da-di da-da-da
  GO

da-di-di-di di-di da-di di-da-di-di
da-di-da-da
  BINARY

di-da-da di-di-di-di di-di di-da-di-di
di
  WHILE

di-di da-di di di-di-di
  MACHINES

di-di-da-di di-da-di-di di-di-da

da-di-di-di
  FLUB
```
Within minutes I had many, many responses either from brains, minds, or machines—sadly, I flunked my own Turing test—and these respondents invariably said yes!, yes!, yes! One CSc major actually quoted James Joyce, “...and his heart was going like mad/ And yes I said yes I will Yes,” having realized that Joyce in this instance was describing a fundamental human situation in which binary baby talk prevails. Because many respondents actually said nothing more than “y”—translated from “da-di-da-da,” a point my newest baby makes every few minutes—I realized that binary baby talk can be very quick and efficient; it's currently the rage around those parts of campus that I visit, or at least hear about. One exception: a slightly bigoted respondent who said that he would never adulterate his own language, he would never lower himself by learning to speak in the style of computers. I told him that he would not have felt that way a century and a half ago when we first achieved a way of speaking in binary mode, by telegraph, to remotely located human beings. Why can we not make the same concession for computers? I tried to remember the lovely lines from the movie 2001 in which somebody makes the point that we should not discriminate against computers simply because they are made of sand; we, after all, are made of an even baser dust. I also suggested that (1) computers are extremely fast and (2) computers are extremely “dumb,” and that for the latter we fault them; if, however, computers were brilliant but plodding, we would still fault them: We would probably call them, in the terminology of some of my students, “greasy grinds.” This is a fixed prejudice: And in the eyes of the victim, as always, one sees oneself—this, at least, is the ironical interpretation of the present chapter.

This brings us to the fringes of a very large question: Why is it so easy to translate from English to Morse code and back to English without making mistakes? Steiner (1975:28) tells us that “when we read or hear any language-statement from the past, be it Leviticus or last year's best-seller, we translate,” and that nothing is tougher than making a good translation. To me, the answer is that Morse code follows conventional rules in all its details, while most “language-statements” are loaded with subtlety and ambiguity.

Having provided a Morse-code illustration, I feel obliged to provide an illustration of the more complex sort of translation that Steiner has in mind. My example is Mark Twain's (1875:15-34) famous story about the jumping frogs of Calaveras County, California. Twain was intrigued by the French translation of his original story, and set about translating the French back into English in order to determine whether “iconoclasm can further go.” One small segment of the original contains the denouement of the story:
Smiley he stood scratching his head and looking down at Dan'l a long time, and at last he says, “I do wonder what in the nation that frog throw’d off for—I wonder if there ain't something the matter with him—he 'pears to look mighty baggy, somehow.” And he ketch’d Dan'l by the nap of the neck, and hefted him, and says, “Why blame my cats if he don't weigh five pound!” and turned him upside down and he belched out a double handful of shot. And then he see how it was, and he was the maddest man—he set the frog down and took out after that feller [gambler], but he never ketch’d him.

When Twain brings these lines back from the French, they read as follows:

Smiley himself scratched longtimes the head, the eyes fixed upon Daniel, until that which at last he said:

“I me demand how the devil it makes itself that this beast has refused. Is it that she had something? One would believe that she is stuffed.”

He grasped Daniel by the skin of the neck, him lifted and said:

“The wolf me bite if he no weigh not five pounds.”

He him reversed and the unhappy belched two handfuls of shot ... When Smiley recognized how it was, he was like mad. He deposited his frog by the earth and ran after that individual, but he not him caught never.

In a sense, Twain fails to make his point: The second version of this incident is indeed funny when compared to the first, but it communicates the original story surprisingly well. Given that the original story is written in a difficult vernacular, given that a French reflexive verb is bound to sound strange when translated literally into English, and given that no serious translator would make such a mistake or write such a mistake into a program, this translation is not at all bad by standards only slightly below Steiner's. But this is nonetheless a tough translation problem. Not as tough as going from Omar the Tentmaker to Edward Fitzgerald and then back again to Arabic (old or modern), which is said to be nearly impossible, but much tougher than the English-Morse-English example developed above. The difference arises because there are fewer conventional rules of translation in the Twain example than in the Morse example; in the extreme Fitzgerald instance, we would have to proceed sans wine, sans song, sans singer, and—sans rules?

Take an intermediate case: A piece of journalism originally written (I believe) in English, then translated into Spanish, and finally translated by me back into English so that I might ascertain whether iconoclasm has any limits,
whether the original author and I seem to be saying the same things even after applying many subtle rules of translation. In the interest of reflexivity I'll translate a paragraph or two from an interview (McDowell, 1986:38-39) with the famous translator Gregory Rabassa (*One Hundred Years of Solitude*), so as to clinch my argument both in terms of what Rabassa says and in terms of what I imply in the process of translating his argument, which of course does not necessarily say what I think it says. We'll have to check the original, and find out.

Remarks such as the preceding [by Rabassa] are disconcerting to many translators, who consider it essential that a given book be read with a critical sense prior to the point when one begins a translation. On the contrary, Rabassa often thumbs through a book sufficiently so that he can see whether it will maintain his interest, but in general he leaves the critical reading for the moment of translation. ...

So: We don't need the *Gestalt* before we set to work. This will be a relief to the average supercomputer. We continue:

Some of Rabassa's ideas, along with his style of work, contravene what translators usually consider to be good practice: for example, his belief that “a book that is well written has only one possible translation.” Even those translators who praise Rabassa argue that there may be many good translations of a given book, just as there may be many bad translations. Everybody agrees that it is possible for a translation to be faithful in all details while at the same time failing to capture the tone of a work.

“Tone,” I'm afraid, gets messy chiefly because there is so little agreement on what it means. My guess is that it has a lot to do with “unity of diction” (Hall, 1973:123-42; Main and Seng, 1961:13-14), that it is therefore a very large and confusing issue, and that if we ignore it we are at risk of creating travesties such as “The Gettysburg Address in Eisenhowerese” (Hall, 1973:128-30). But, fear not: It is inconceivable that anybody will ever program a computer to think, write, and speak in the style of Dwight D. Eisenhower or, better or worse yet, the style of Casey Stengel, with whom Ike was not even ballpark for obscurantism. Rabassa (McDowell, 1986:39) seems to think that tone has a lot to do with having a “good ear,” and this comes as welcome news to intelligent machines who write music. It is also encouraging that one of the world's mightiest translators does not believe that one man's translation is as good as another's—even though it is obvious to me that all the “bad” emic translations are conceived in liberty and created equal.

It is significant that Garcia Marquez himself, interviewed a few years ago in the book review magazine of the New York *Times* (Simons, 1988), claims that his many translators often send him lists of “... things they have doubts about,” and
that these lists tend to be the same no matter what the language. The doubts, then, are not an inherent feature of the translation process; rather, they are elements of the original text. If Garcia gives everybody the same reply, all the translations should be reasonable replications of a difficult literary genre.

In any case, here is the original English version of the Rabassa excerpt:

Statements like that drive some translators to despair, since many of them believe it vital to read a book with a critical eye before ever starting the translation. Rabassa, by contrast, often will flip thorough a book just enough to see if it will sustain his interest, but usually he saves the critical reading for the actual translation.

... Like his method of working, certain of Rabassa's beliefs fly in the face of conventional translation wisdom—for example, his contention that "if a book is well written, there is only one possible translation." Even translators who have nothing but praise for Rabassa argue that it is possible to have several good translations of the same book, as well as several bad ones. What he and they agree on is that it is possible for a translation to be accurate in every detail, yet still be tone-deaf.

My own translation, two steps removed from the original and filtered through a knowledge of Spanish inferior to that of the better educated (programmed) machines, does not seem to have missed anything of importance. We learn, through a "content" of secondary significance, that a translator of immense skill does not need to perceive a work as a whole before he proceeds, and that he believes that a complex translation may be constrained by rules that permit only one result. If I ever try to write an AI expert system for Spanish-to-English translation of sociological literature, I will surely consult for days and weeks with Rabassa. The realm of "one result, one true story," of course, is the delightful realm of replication. And it is therefore, in spirit, the realm of science—and of the edifying technique known as mad-libbing, to be discussed below.

This entire exercise has illustrated nothing more than what social psychologists have understood for many years as the rumor process (Allport and Postman, 1958; Bartlett, 1958), but with this important proviso: Translations distort, but some kinds of translations distort far more than others. The advocates and exemplars of Foucault's brand of structuralism, I believe, urge social scientists to devote most of their effort to the more esoteric problems of translation such as that represented by the Twain and FitzGerald examples; this is what the emic perspective is all about. But the central task of a social scientist,
on the contrary, is to develop a language that translates more in the manner of the English-Morse-English example or, realistically, the Rabassa example, so that when we speak of something called “Pygmalion effect” (Rosenthal and Jacobson, 1968) or something called a “specific deterrent effect of arrest” (Sherman and Berk, 1984; Sherman et al., 1992), there is little if any confusion as to what these phrases mean and there is little if any confusion as to how social scientists might try to replicate the alleged effects. When we have replications that come close to the English-Morse-English example, we are achieving science. When we have a corpus of knowledge that a good AI program could translate, say, from English to Russian and back again without significant distortion, we are achieving science.

Foucault's interpreters can hardly help us with this task, because they insist that we concentrate on Twain-Smiley and his unique, emic perceptions. As one probes more deeply into the literature by and about Foucault, one realizes that the best way to understand his work is to read books that explain the interpretation of poetry. Main and Seng (1961:19-20), for instance, define poetic license as

... the right of a poet to deviate from standard practices in order to achieve a certain effect, ... to ignore rules that prose writers customarily follow. Poetic license takes many forms. A poet may, for instance, invent new words or jam together old ones; he may leave their referents implied rather than stated. He may use a noun to do the work of an adjective or verb, an adjective to do the work of a noun. ... Poets also take liberties with syntax.

Poetic license has its functions, but it should not tempt us toward the popular notion that poetry never demands precision: Main and Seng (1961:7-10) speak of “the indispensable dictionary” as merely one aspect of this precision. It appears, however, that a special sort of license has been granted to Monsieur Foucault, a sort of license that never can be given a large role in scientific analysis and discourse. I have trouble developing enthusiasm for an author about whom admirers say this (Lemert and Gillan, 1982:ix): “He has been accused of willful obscurity. But this complaint runs counter to the fact that, even when one cannot be sure what he is saying, it is evident that he is saying it well.”

This reverential attitude toward rhetoric goes to the essence of my complaint against Foucault and his supporters. Well-spoken obscurantism is an oxymoron. In scientific discourse, if you cannot be sure what somebody is saying, you cannot be sure that he is saying it at all well, and either you or the author or both must learn the appropriate language. One who abandons this principle is abandoning the idea of science—social or otherwise. For those of us who accept the feasibility of a science of society, to be enticed by Foucault's (and/or his interpreters') brand of obscurantism is to be enticed by a quick trip to the edge of a flat earth.
Here is an instance: I am not sure what Foucault is saying, and I am convinced that he is not saying it at all well (Foucault, 1978:92-3):

It seems to me that social evolution must be understood in the first instance as the multiplicity of force relations immanent in the sphere in which they operate and which constitute their own organization; as the process which, through ceaseless struggles and confrontations, transforms, strengthens, or reverses them; as the support which these force relations find in one another, thus forming a chain or a system, or on the contrary, the dysjunctions and contradictions which isolate them from one another; and lastly, as the strategies in which they take effect, whose general design or institutional crystallization is embodied in the state apparatus, in the formulation of the law, in the various social hegemonies. ... Social evolution is everywhere; not because it embraces everything, but because it comes from everywhere. ... One needs to be nominalistic, no doubt: social evolution is not an institution, and not a structure; neither is it a certain strength we are endowed with; it is the name that one attributes to a complex strategical situation in a particular society.

To my mind these endless abstractions, at best, are the grindstones of the garrulous; at worst, they are the word salads of the mentally deranged. And it is not simply that I have lifted this excerpt out of context: Nothing that precedes or follows it will clarify what it is trying to accomplish. It is dead-level abstraction of the sort that general semanticists used to deplore, and not only do I claim that science does not work this way: I claim that the human mind does not work this way. If Papert (Bass, 1990) is correct in saying that one of the best ways to get children to learn algebra is to allow them to fiddle differential gears, then the problem with Foucault is that a good greasy gearbox is almost never in evidence. In this excerpt, Foucault clarifies the concept of social evolution in the same way that Professor A. Parker Nevin once clarified the concept of “justice” in a famous nonsense speech that was almost always treated, by the frumiously gullible, as if it contained important information (Starmen, 1955).

Apropos of children and their games, the game called “mad libs” provides a sort of Turing test for the presence of Foucaultian rhetorical spin. In this game, new words are substituted randomly for those found in the original text—nouns for nouns, verbs for verbs, etc.—and the results are often hilarious. In the above excerpt, I played a trick: I substituted the phrase “social evolution” for every instance in which Foucault, in the original, used the word “power,” and the mad lib sounds at least as plausible as the original. When one does not discern that the game is being played, one has failed a variant of the Turing test and one is almost certainly in the presence of Foucaultian rhetoric. In another place, where Foucault (1984) discusses the relationship between writing and death, I achieved far more
insight (and pleasure) by mad libbing “life” in place of “death” and by making a few additional alterations as appropriate. Similar exercises, involving psychoanalytic writings, appear in Faia (1977).

At this moment, I feel compelled to make a confession: Recently I found myself mad-libbing my own work. Using the SPSS statistical software for loglinear analysis, I had replicated Swafford's (1980) estimates, based on a national survey, of ways in which respondents' attitudes about equal employment opportunity were influenced by education, by region (south versus non-south), by the passage of time from 1946 to 1963, and by interaction among these variables, e.g., the possibility that the effects of region may have been different in 1963 than in 1946, etc. I then carried out another analysis, based on the National Survey of Family Growth (NSFG), of ways in which the experience of unwanted pregnancies—and, presumably, the resulting demand for abortion—were influenced by education, by use of contraception in general, by use of condoms for “safe sex” purposes, and by interaction among these variables, e.g., the possibility that the effects of “safe sex” practices on abortion demand may have been different among poorly educated respondents than among others, etc.

Not surprisingly, the mad-libs essentially wrote themselves. It is therefore crucial to my argument that I state my reasons for believing that my self-reflexive mad-libs are superior to my Foucaultian mad-libs, as a set of scientific findings. Here are the major reasons:

(1) My exercise begins with a precise replication of work done by other scientists. Any competent statistician could replicate Swafford's example; in fact, Swafford was replicating examples already published by other scholars.

(2) Both Swafford and I had in mind a causal model involving four dichotomous, “qualitative” variables, in which one of the variables is dependent on the other three; in the NSFG example, my theoretical question has to do with the unanticipated consequences of technology. Statisticians, social scientists, and natural scientists have struggled for many decades to arrive at a consensus about ways of handling this sort of problem. In so doing, incidentally, they have made it clear that the qualitative-quantitative distinction is an oversimplification.

(3) I was able to mad-lib myself solely because of the fact that the data of the NSFG happened to behave appropriately. Usually, this does not happen, and there are compelling reasons for changing one's causal models and modifying one's conclusions.

(4) Whatever my conclusions, they are totally transparent: They can be replicated by anybody who uses computers, SPSS (or similar software),
and the 1988 NSFG. In the absence of complete replicability, i.e., complete transparency with no nonsense about the delights of obscurity, I would be called upon to produce clearer explanations.

(5) It is fairly clear where my findings end, and my “interpretations” begin. As for the latter, I would say that it is of the utmost importance to those interested in supporting safe-sex campaigns that these campaigns may suppress demand for abortion. Relative to my scientific findings, however, this is an obiter dictum.

Finally, a dialectical insight: The irony of Foucault(ians) is that, while obscurantism cannot properly be translated into English (or any other language) by human intelligence or machine intelligence, it would be easy for a computer to write mad libs based on the more esoteric Foucaultian texts. Such a computer could easily masquerade as an exemplar of artificial intelligence; for one thing, it could probably meet the Turing criterion, even if those on the other end of the line were Foucaultians. If the machine tried to fake a loglinear analysis of the NSFG, a good statistician would catch it. I have 5,000 francs supporting this contention.

(2) Good, bad, or ugly?

I willingly concede that the style is unbearable (one of my flaws is not being naturally clear).

—Michel Foucault

In another part of their book Lemert and Gillan (1982:9) quote Foucault on the topic of Foucault's quotes: “... I quote Marx without saying I am, without quotation marks, and because people are unable to recognize Marx's texts I am considered to be someone who doesn't quote Marx.” If the ordinary rules of intellectual inquiry were not already suspended, this confession would open up a strong possibility of (1) a pervasive carelessness about documentation or (2) plagiarism. Yet Lemert and Gillan tell us not to worry, that while it is “... futile to talk of his sources” (1982:10), “Foucault could not more closely identify himself with Marx. Nor could he more clearly and perversely distance himself from contemporary French Marxism. This is typical of Foucault's method. Neither Marxist nor anti-Marxist, yet both” (1982:9). Now, I generally travel pretty far along this pike: I appreciate Korzybski's (1933) and Ogden and Richards' (1930) ancient warnings about relying too heavily on the Aristotelian notion that an entity must be “either A or not-A”—Foucault, in more ways than one, brings back the basic imagery of general semantics. Further, I think I fathom the subtle ways in which Saddam Hussein sees victory in defeat. But, eventually,
the dialectical pike runs into a dead end: There is no excuse for failing to explain the sense in which one is both Marxist and anti-Marxist, and there is no excuse for incorporating into one's own work the scholarship of others without acknowledgement.

It is outrageous that at least one publisher encouraged Foucault (1970:viii) to continue his bad habits regarding documentation, by promising that the editorial staff would try to do the essential work for him—such are the powers of the marketplace. And in case editorial staffs cannot deliver, Lemert and Gillan (1982:32) suggest that the “impatient reader” is expected to take on the responsibility herself. Foucault and friends may believe that social norms both exist and do not exist, but I for one do not believe that Foucault should have been exempted from the fluffy norm that asks us to acknowledge our intellectual debts; furthermore, science is an elaborate, time-consuming communication process, and most of us do not have time for Easter-egg hunts.

This inexplicable suspension of the rules causes endless difficulty (Eribon, 1991:141-42). When Lemert and Gillan (1982:110) tell us that the theoretical kin of Foucault include Poulantzas, Miliband, Giddens, Parkin, Gouldner, Offe, Habermas, Althusser, O’Connor, Wallerstein, Skocpol, et al., but that Foucault “...gives little evidence of having read these others,” we must ask: What, in such an instance, would constitute evidence? What would constitute theoretical kinship? And when these same authors (1982:121) say that Foucault “...is aware, as is practically everyone else, of the futility of positivism,” we must ask again: What constitutes the evidence? Who is “everyone else,” and has anybody actually read a reasonable sample of their work, or at least interviewed them? Picked up their vibes? Turned up their eggs?

I wish Foucault were still alive, so that he could play Mark Twain in ordre inverse. I would be especially keen on seeing how the following excerpt from Foucault, quoted by Dreyfus and Rabinow (1982:31), would fare if subjected to a three- or four-step rumor process involving word-of-mouth among social philosophers, and then translated back into French by the original author:

In showing that man is determined, [the analytic of finitude] is concerned with showing that the foundation of those determinations is man's very being in its radical limitations; it must also show that the contents of experience are already their own conditions, that thought, from the very beginning, haunts the unthought that eludes them, and that it is always striving to recover; it shows how that origin of which man is never the contemporary is at the same time withdrawn and given as an imminence: in short, it is always concerned with showing how the Other, the Distant, is also the Near and the Same.

I'd love to ask my anti-positivist colleagues—i.e., “practically everyone”—to participate in a multiple-translation process, but they always seem to know
exactly what I'm up to despite their difficulty in reading one another. If the experiment were actually conducted, Foucault, I believe, would soon exclaim, "je te l'avais bien dit!"

To be sure, Foucault had serious misgivings about the human sciences, and his many disciples feel the same way. Most of his arguments, however, turn mainly on tired issues already addressed: Is it appropriate for the human sciences to emulate the natural sciences? Is it possible for man to study himself reflexively without being caught up in those infinite convolutions created by the fact—or the belief—that behind every representation there is nothing more than another representation? Where do we find the divine spark in the human sciences? I've proposed to answer these questions, respectively, by saying yes, yes, and I don't know. These answers should suffice, and I hate to dwell further; however, Foucault and his interpreters leave me no choice.

Cousins and Hussain (1984:63) claim, with Foucault (1970:357), that "obviously the social sciences are and always have been riven with methodological controversy. This is largely because of their parasitic relation to other forms of knowledge from which there has been a perpetual import-led boom of conceptual activity." But this remark is not valid, because we happen to be dealing with an ex-im business: When Darwin and Wallace took ideas from the Reverend Malthus and from Adam Smith, was this not the greatest act of "parasitism" in the history of the sciences? When natural scientists use the logic of the life table and the stable population model, are they not stealing from Edmund Halley, John Graunt, and Alfred Lotka—all of whom were at least part-time social scientists? When the great Lotka moved from biology to demography, was he parasitic on himself? Can we borrow a biological term for reflexive parasitism, or should we get ready to lend another neologism to the biologist? Science is the process of learning, "no holds barred," in a way that permits public verification; if these criteria are met, it does not matter a whit who borrows from whom, and intellectual property remains communal. (It is, however, a fine courtesy to reveal one's sources, and perhaps Cousins and Hussain are merely projecting a latent sense of guilt for the moral turpitude of the master.)

Cousins and Hussain (1984:29) tell us that Foucault's concept of meaning is that there are representations of "things," but that these things are themselves merely representations of other things, and so forth ad infinitum. I don't have any problem with this notion: It merely reminds us that we are wise to arrive at operational definitions of "things." In my research, for instance, "unintentional pregnancy" is merely a representation of other representations that occur during an NSFG interview, representations in which a given woman tries to remember and reconstruct her sexual, marital, and fertility history. This does not vitiate the demonstrable fact that unintentional pregnancies have a large impact on the 1.5 million abortions that occur in this nation each year, and that these abortions are a fundamental and formidable Durkheimian chose.
When we begin to appreciate that representations represent other representations onward to infinity, we soon enter the realm of mysticism. When I try to enter this realm in my capacity as a social scientist, I discover immediately how James Thurber must have felt when he was informed by a self-improvement manual that his major problem in life was his own large ego: He was forever using the first-person singular—it was always I, me, mine, to me, for me, myself, etc. Thurber tried for weeks to break out of this ugly syndrome, and finally declared in obvious despair: “Tried it. Am tied in knots. No can do.” When I read Foucault on sexuality, I love the mysticism (1970:25-26) about how we are “discontinuous beings,” about the sacred character of eroticism, about the meaning of the sperm cell and the ovum. But whenever I read these particular passages I quickly end up at Thurber's dead end, and I cannot help but think of the old story about how the French social welfare system is the world's most advanced, looking after the citizenry not merely from cradle to grave, not merely from womb to tomb, not merely from sperm to worm, but—with tender loving care and all deliberate speed—from erection to resurrection. And that's enough mystery for me; beyond that, no can do.

(3) Mitigating circumstances

Despite his alleged anti-positivism, Foucault consistently spins arguments in a way that aids and abets those who remain strongly committed to the human sciences. He wrote excellent sociological treatises on prisons, mental institutions, and the history of sexuality; in these and other works, he made important contributions to major theoretical traditions having to do with “labelling” as a self-fulfilling prophecy, deterrence as a means of social control (Cousins and Hussain, 1984:182), the nature of social norms (Dreyfus and Rabinow, 1982:81), and structure-functional analysis (Foucault, 1970:168-74; Cousins and Hussain, 1984:196). He had deep insights into the nature, development, and uses of scientific taxonomies (Foucault, 1970). Rather than rejecting an important role for mathematics in the human sciences, he clarified that role in a way that would be warmly endorsed by contemporary positivism (Cousins and Hussain, 1982:58-59; Foucault, 1970:349). He warned us, as we often have warned ourselves, against the temptation to explain major historical events with reference to phenomena such as “Luther's anal retentiveness” (Cousins and Hussain, 1982:64). He introduced us (1970: Chapter 1) to Borges' metaphysical conceits in a way that encourages us to use what C. Wright Mills called the sociological imagination. He traced the evolution from anatomy to physiology (1970:226-32,266), the evolution from normative economics to economics as the study of institutional dynamics, and the development of modern linguistics in a way that should inspire all human scientists who wish to see their own disciplines achieve the successes of biology, economics, and linguistics.
Those who share Collins' (1984) worries (and conclusions) about the dialectic of statistics versus words will find little encouragement in Foucault—although his anti-science interpreters appear to be capable of finding anything, anywhere, anytime. In their treatise on Foucault's major works, Cousins and Hussain (1982:58-59) discuss the association of positivism with mathematization, mentioning Foucault's belief that

... while it is the case that there will be mathematical advances in biology, economics, and linguistics and in the social sciences themselves, one should not mistake the surface effects for the more fundamental event, the dissolution of the *mathesis universalis*. The relation of the human sciences to mathematics can be stated modestly: while mathematical procedures are still necessarily employed as a primary means of presenting and justifying findings and relations, the human sciences are far more importantly determined by their relation to ... the empirical sciences of life [biology], labor [economics] and language [linguistics] and the philosophical reflections which are deployed in the analytic of finitude.

I'm a statistician, and this is good enough for me. But Foucault himself (1970:349) says it even better:

[The] form of empirical knowledge which is applicable to man ... has a relation to mathematics: like any other domain of knowledge, these sciences may, in certain conditions, make use of mathematics as a tool; some of their procedures and a certain number of their results can be formalized. It is undoubtedly of the greatest importance to know those tools, to be able to practice those formalizations and to define the levels upon which they can be performed; it is no doubt of interest historically to know how Condorcet was able to apply the calculation of probabilities to politics, how Fechner defined the logarithmic relation between the growth of sensation and that of excitation, how contemporary psychologists make use of information theory ... But ... it is unlikely that the relation of mathematics ... is constitutive of the human sciences in their particular positivity.

Again: Good enough for me, good enough for Weber, and good enough for vulcanology. If these remarks can be taken to mean that we no longer have to read Lazarsfeld, then they can be taken also to mean that we no longer have to read Poulantzas, Miliband, Giddens, Parkin, Gouldner, Offe, Habermas, Althusser, O'Connor, Wallerstein, Skocpol, et al. In short, we institutionalize Comte's cerebral hygiene.
Even when Foucault seems to criticize quantitative social science, he is encouraging. For instance (1970:351):

... it is with mathematics ... that the human sciences maintain the clearest, the most untroubled, and, as it were, the most transparent, relations: indeed, the recourse to mathematics, in one form or another, has always been the simplest way of providing positive knowledge about man with a scientific style, form, and justification.

Again I say, Precisely! Not long ago I supervised a master's thesis (Flippo, 1987), based on the 1982-83 NSFG, in which the major finding was a pair of beta weights: One is .34, the other is .31. These numbers are clear; they are untroubled, transparent; they have a scientific style, form, and justification. The justification is that they remind me and my colleagues about a vast number of things: about the tremendous care that must be taken, throughout the research enterprise from sample design to coding and analyzing data, in order to isolate a pair of beta weights; about the venerable hypothesis and related theories that hold that, for many plausible reasons having to do largely with an enhanced ability to defer gratification, the sexual conduct of high-status minority women in the United States is different from that of high-status majority women—a hypothesis that our beta weights have led us to question. The beta weights are nothing more than the Torah: They stand alone in their simple beauty, in their elegance, and they sustain an infinitude of commentary and insight.

Sometimes, when I attack Foucault, I realize that I am going for the capillaries; the arteries can look pretty good. For instance (1970:353):

The human sciences are not, then, an analysis of what man is by nature; but rather an analysis that extends from what man is in his positivity (living, speaking, labouring being) to what enables this same being to know (or seek to know) what life is, in what the essence of labour and its laws consist, and in what way he is able to speak. The human sciences thus occupy the distance that separates (though not without connecting them) biology, economics, and philology from that which gives them possibility in the very being of man. It would therefore be wrong to see the human sciences as an extension, interiorized within the human species, within its complex organism, within its behaviour and consciousness, of biological mechanisms; and it would be no less wrong to place within the human sciences the science of economics or the science of language (whose irreducibility to the human sciences is expressed in the effort to constitute a pure economics and a pure linguistics).
For those who subscribe to the “cultural materialist” perspective (Harris, 1979), it will surely come as a surprise that biology (e.g., fertility, mortality, migration), economics, and linguistics are not at the heart of the social science endeavor. To Foucault, they are not fundamental; what is fundamental is man's capacity for knowledge. But there are many excellent social scientists who believe that (1) man's perceptions and beliefs are not the central focus of the social sciences and (2) insofar as they are a topic of interest, they may be studied in the same way in which we study organisms of the phylum Platyhelminthes. It may strike Foucault as the “peculiar configuration” of the human sciences that it is possible to create “human sciences of human sciences” (1970:354-55), the psychology of psychology, the sociology of sociology, etc., but the sociology of sociology is no more peculiar than the anthropology of chemistry, the psychology of physics, the chemistry of biology, or the physiology of platyhelminthic worms. Foucault (1970:361) drifts toward the ecological point of view when he concedes that functions can be performed, conflicts can be developed, and signification can “... impose its intelligibility, without passing through the stage of explicit consciousness ... And now, is it not necessary to recognize that the peculiar property of the norm in relation to the function it determines, of the rule in relation to the conflict it regulates, of the system in relation to the signification it makes possible, is precisely that of not being given to consciousness?” If I read this passage correctly—and Foucaultians are invited to read it as they wish—it comes down strongly on the side of cultural materialism, of human ecology. To me, this enhances the scientific value of Foucault's work.

Dreyfus and Rabinow (1982:66) have a similar impact when they try “to make Foucault's structuralist view plausible” by providing the example of the university. (Never mind Foucault's claim [1970:xiv] that “half-witted 'commentators' persist in labelling me a 'structuralist'” and that the correct label is something that “I have been unable to get ... into their tiny minds ...” It is possible, I suppose, that Foucault is both a structuralist and an anti-structuralist.) The functioning of universities, say Dreyfus and Rabinow, depends on economic, political, familial, institutional, architectural, and pedagogical factors, but these factors coalesce into a university only when we have “the idea of the university.” If we ask about the relative importance of the factors and the idea, we are digging into terrain that lies considerably more than a mile north of the north pole—the question is unanswerable. Dreyfus and Rabinow seem to sense this, but they soon revert to mysticism when they say that the concept, i.e. the “idea,” “is itself a 'secondary relation' conditioned by something else.” But this “final unifying factor” cannot be described in objective or mentalistic terms; it is a “way of talking” that is taken seriously in the domains of higher education. The rules of talking “... ultimately 'effect' or 'establish' university life as we know it” (1982:66). Aside from the obscurantism regarding talk that is not amenable to objective or mentalistic understanding, there is nothing new about this formulation; it is mainstream social science.
In large measure Foucault's *The Order of Things* is about taxonomies, how they are constructed, and how they evolve. Apparently, the book was inspired by Borges:

This book first arose out of a passage in Borges, out of the laughter that shattered ... all the familiar landmarks of my thought—*our* thought...—breaking up all the ordered surfaces and all the planes with which we are accustomed to tame the wild profusion of existing things ... This passage quotes a “certain Chinese encyclopaedia” in which it is written that “animals are divided into: (a) belonging to the Emperor, (b) embalmed, (c) tame, (d) sucking pigs, (e) sirens, (f) fabulous, (g) stray dogs, (h) included in the present classification, (i) frenzied, (j) innumerable, (k) drawn with a very fine camelhair brush, (l) *et cetera*, (m) having just broken the water pitcher, (n) that from a long way off look like flies.”

Foucault (1970:xv) is delighted by the “...wonderment of this taxonomy ...” In a few moments, he asks “... what is the ground on which we are able to establish the validity ...” of a classification (1970:xix)?—a question that he does not answer satisfactorily. In fact he makes big mistakes, like the suggestion (1970:147,268) that the transition from anatomy to physiology destroys the prospect of a workable scientific taxonomy. He should have asked a contemporary Chinese biologist how it is that he knows what the word “Platyhelminthes” means. When Foucault tells us (1970:27) that there may be a “sympathy” (as the translator says) between aconite and our eyes he is offering a metaphysical conceit worthy of John Donne, who once wrote a poem in which the mingling of a man's blood with a woman's blood in the tiny body of an insect becomes the tenuous basis for a seduction, a sort of flea-bitten *fait accompli*. But in calling attention to metaphysical conceits Foucault is not telling scientists anything that we do not already know, especially if we have read Arthur Koestler (1964) or have become familiar with the recent software development known as “hypertext.”

(4) Oodles of Boodles

Foucault begins *The Order of Things* (1970) with a detailed description of *Las meninas*, a painting by Velazquez. This is an extraordinary document, the best of the genre; if I ever have an opportunity to view this painting I'll be able to gaze upon it with a new capacity for vision. But when Foucault claims that this painting demonstrates for us an epistemology that encompasses scientific developments of the era of Velazquez, I have problems. To those who disagree with me, I offer this challenge: Hire a dozen French art critics, and have each of them write a 6,000 word essay—the length of Foucault's chapter—on *Las
meninas. Translate these works into English, and compare them. Then, support a dozen different research teams in replications of the domestic violence-police intervention experiment conducted by Sherman and Berk (1984), and compare the results of these replications. Finally, compare the two processes, thereby obtaining what will surely look like representations of many other representations, and you will see the essential differences between non-science and science. Regarding the second process, one would immediately realize that the scientists are talking about the same sort of object; one could readily design another replication. In the Las meninas example, if the name of the artist and the painting were removed one would hardly be able to identify the object of discussion, and there would surely be no conclusion that one could try to replicate. Show me somebody who denies my claim and I'll show you somebody who believes that art criticism is a science.

Foucault was charmed by the idea that, because the knower and the known are the same (1970:366-67), a science of the social is difficult if not impossible. “Man,” he says (1970:318), “in the analytic of finitude, is a strange empirico-transcendental doublet, since he is a being such that knowledge will be attained in him of what renders all knowledge possible.” This does not vitiate the fact that there is no difference between an American and a Chinese scientist working in the realm of helminthology, or an American and a Chinese conducting an experiment to ascertain whether human-computer interaction is facilitated by an efficient keyboard, a “mouse,” a “joystick,” or a light pen. And this is not a trivial problem: The joystick and its ilk clearly have a fundamental impact on human wellbeing; in fact, they may exceed helminthic worms in this regard. There may be a rationale underlying the reflexivity argument, but I've searched in vain. Those who accept it could perhaps convince me if they were to explain how it is that the knowledge created by vulcanology is superior to the knowledge created by the United States Census Bureau, an example I select primarily because it had a strong appeal to Lundberg (1947:21,37).

I like to point out to my CSc students that certain tasks are “two-sip” tasks: Two sips of one of my infamous macromartinis—I happen to be a Boodles man—and the task cannot be performed well if at all. Writing an AI program in Prolog while operating in a multi-tasking computer environment, or landing an airplane while announcing one's position and maneuvers to controllers and to other aircraft, and simultaneously worrying about a funny engine vibration, are very good examples. In the case of Foucault, however, I usually cannot begin to understand him until I have consumed a triple Boodles. This is one of the chief reasons why I enjoy Foucault so much; he helps me to see that while science is sober, non-science and anti-science are informed by a lovely little buzz.

(5) Why Foucault needed Lindroth, and why Lindroth needed Foucault
The major argument of this chapter is captured perfectly in a story told by one of Foucault's biographers (Eribon, 1991:83-84). As a young scholar at the University of Uppsala, Foucault explored the possibility of taking an advanced degree under the supervision of Professor Stirn Lindroth. He asked Lindroth to read an early draft of his work on the history of mental illness and its treatment. Lindroth, a ”... dyed-in-the-wool positivist and not particularly open to grand speculation...,“ was “frightened” by the pages he examined; he wrote to Foucault and expressed his extremely unfavorable impressions. Foucault replied, somewhat defensively it would seem, as follows:

Your letter has been very useful in making me aware of the flaws in my work ... My first mistake ... was in not having warned you sufficiently that this was not a “fragment of a book” but only rough work, a first draft ... I willingly concede that the style is unbearable (one of my flaws is not being naturally clear). ... I was wrong in not defining my project, which is not to write a history of the developments of psychiatric science, but rather a history of the social, moral, and imaginary context in which it developed.

These scholars had very different intentions, and I hope they continued talking to one another. In any case, the problem did not go away (Eribon, 1991:217).
(1) In general, translating humor is at least as difficult as translating Omar Khayyam, and machines may never do much more than help humorists discover good jokes—here, I think, machines will have an important role. Humor, however, not only makes exorbitant demands on machines; it also operates at the limits of human capability. During the December, 1987 summit meeting between Reagan and Gorbachev, Reagan quipped that he had made a disarmament proposal that was “disarmingly simple.” Gorbachev, as I recall, gave a courtesy laugh, but I suspect that it required at least fifteen minutes for a good translator to explain to him the significance of Reagan's remark. And if Reagan could have seen the resulting elaborate translation, he probably would not have agreed with it or even recognized it. If one were to make a detailed study of translation processes at summit conferences, one would come away with a profound understanding of what the field of social psychology is all about.

(2) A critic of an early draft of this chapter claims that my argument about Morse code is wrong. Dead wrong. Morse code works because it involves *transliteration* (letter by letter replacement) not *translation* (replacement of the meaning of one word with the meaning of another word). Translation is more difficult because the meanings of words are seldom isomorphic across languages. Even transliteration becomes difficult when the letters are not equivalent (as is often the case with clicks or gutturals.)

Whenever I hear that I’m dead wrong, I come alive with powerful counter-arguments; this is part of my aviation training. I think that it is a mistake to assume a sharp qualitative difference between transliteration and translation: These processes vary by degree, and even my critic makes a large concession regarding clicks, gutturals, etc.

In the languages of cultures with phonetic alphabets there is a scale of complexity that ranges, typically, from the elements of alphabet characters—such as dots within matrices or Morse dot-dash sequences—through the characters themselves, words, sentences, paragraphs, paragraphs organized into sections, sections organized into chapters, entire volumes, multi-volume works, etc. At each level there is meaning, although the complexity of these meanings increases exponentially at higher levels. When considered across languages, letters of the alphabet cannot simply be transliterated, because they may convey relatively complex meanings, such as an “F” in the grading system, the letter “X” as a way of indicating negation, or the letter “Z” or “Zulu” in reference to clock time at Greenwich, England. Airplanes have crashed because pilots failed to listen to
three-letter Morse identifiers of navigational radios. We are all taught to “copy” letters, words, phrases (e.g., the standard platitudes), and many sentences, although I doubt that anybody has ever been accused of plagiarism without copying at least a paragraph or two: The smaller units are simple, and cannot sustain a claim of unique authorship. It is perhaps not coincidental that the most impressive computer writings that I've ever come across used short paragraphs as the most complex unit.

The point of my Morse example is that it involves a relatively simple and unambiguous way of communicating, that scientists try to accomplish comparable simplicity and clarity at all levels of meaning, and that we probably have (and should have) a bias toward simplicity of meaning structures: Single-sentence hypotheses rather than full paragraphs, articles or monographs rather than books, etc.

(3) Apparently, Derrida had the same set of bad habits. Lamont (1987:591) says that “Derrida, like other French intellectuals, is renowned for writing in a sophisticated and somewhat obscure style ...” Later in the same paper (1987:604-5) she says that

The decline of his popularity among philosophers can be related to Derrida's refusal to respect academic professional norms by choosing not to write a dissertation until 1980. Others, like Althusser and Foucault, had also decided not to pursue their [doctoral degrees]. One of my informants, who also made this choice, observed that this refusal expressed an important feature of French intellectual ethos: the power of the Cartesian cogito is proved by one's ability to win the game without playing by the rules.

If these rude behaviors can inhere in the social facts of an entire national university system, this is all the more reason for refusing to emulate them. The recurring references to sophisticated obscurantism suggest the presence of an institutionalized oxymoron.

(4) About a week after I wrote the preceding paragraphs, I came across a paper by Ross (1990:262-65) that contains an extraordinary coincidence: Ross quotes Foucault's definition of power, discusses it for a page or two, and then presents a mad lib in which he substitutes technology for power. His results are at least as edifying as mine. Recognizing that Ross is generally friendly toward the Foucaultian style, I found that the scales fell from my eyes as I realized further that mad libbing may be a central feature of the Foucaultian intellectual repertoire. A grammar serves as one's airframe: One fills it with passengers and then sees whether it will fly! It usually does, up to a point.
Jon Stratton (1990:227) has also discovered flying. He quotes Foucault on power and sex, and then tells us that “for our purposes ... the implication ... can be best ascertained by substituting truth for sex in the above quotation.” When I integrate Ross and Stratton, simultaneously substituting technology for power and truth for sex, I experience immediate detumescence although I still have a good time.

Mad libs are a delightful intellectual device. Substitutions generally should not be random within parts of speech, as in the children's game. A given noun, let's say, should consistently be substituted for another. A second non-random form of substitution involves reversals: Substitute antonyms. Note how the following translation of remarks by Rosaldo (1989:224) elucidates the original. First, Rosaldo:

Geertz thus can attack “antirelativism” without committing himself to their [the objectivists'] version of “relativism,” or any other variety of the “unconstrained” vision. His critique frees interpretive social scientists from trying to defend positions attributed to them that they never held in the first place. Those of us who want to decenter objectivism have not been advocates of its supposed opposite—subjectivism, relativism, or whatever red-flag word is being waved at the time.

And my mad lib, not motivated by the rage in grief:

Joe Blogs thus can attack “anti-objectivism” without committing himself to their [the relativists'] version of “objectivism,” or any other variety of the “positivist” vision. Blogs' critique frees objectivist social scientists from trying to defend positions attributed to them that they never held in the first place. Those of us who want to decenter interpretationism have not been advocates of its supposed opposite—objectivism, absolutism, or whatever red-flag word is being waved at the time.

(5) Almost any excerpt from Rubenstein (1988) would serve equally well.

(6) The biologist Ernst Mayr (1991:85-6), asking “how great was Darwin's debt to Malthus?” answers that it was very great indeed. Much was borrowed, including the idea that we live in a “... pessimistic world: there are ever-repeated catastrophes, an unending, fierce struggle for existence, yet the world essentially remains the same.” This is ironic: As is usually true of those who cite Malthus, Mayr shows familiarity with the first edition of Malthus' essay on population but not with the truly scholarly editions of 1803 and later. In the more scholarly work, the pessimism disappears. I urge my colleagues in biology to take delivery on another sociological import. And if we can hold down the tariffs, I'd like to import state-of-the-arts computer graphics on human physiology.
(7) This number has dropped in recent years.


(9) Dear FAA: I've only tried the first of these experiments.
REFERENCES (Chapter 2)


Chapter 3
IN PRAISE OF THE NULL HYPOTHESIS:
THE MYTH OF “THE VALUE-FREE MYTH”

Our aim then should be to rid ourselves of our passions ...

—Fernand Braudel (1990:16)

(1) The nature and extent of bias in scientific research

Back in the 'fifties, when I first began serious study of the social sciences, it seemed to be taken for granted by most of my mentors that intellectual biases, although very subtle, seductive, and dangerous, were subject to a modicum of control. One of the best ways of controlling bias, they believed, was to learn to use the scientific method, especially those aspects of it that had to do with critical analysis (and replication) of the work of others, i.e., those aspects that had something to do with consensus-seeking. I found these arguments to be highly persuasive. Among the latest generations of scholars, however, an entirely new attitude has been emerging: It is commonly asserted nowadays that there is nothing special about scientific methods and scientific discourse as a way of controlling bias, and that the “knowledge” of the sciences—and this surely includes the social sciences—is deeply and inescapably distorted by the prejudices and presuppositions of scholars.

Writers who have major misgivings about the claims of scientific method—Agger (1989:373), for instance, who writes magnanimously that “I want to get positivism off the backs of those of us who claim science for ourselves”—are wont to reject causal analysis, path diagrams, graphic representation of data and hypotheses, and other such devices, but this attitude should not deter us from insisting that hypotheses about intellectual biases are typically causal hypotheses: They state that a given intellectual production is influenced by (1) historical factors, and/or (2) one's social class background, sex, race, etc., and/or (3) other elements of one's intellectual development, e.g., religious beliefs, and/or (4) one's personality characteristics, life experiences, neurotic conflicts, etc. Levy (1989:3) makes exactly the same point when he says that “the idea that an observer's values and concepts influence what he or she observes and concludes is nothing but a special case of the general proposition that an empirical explanation exists for anything empirically observable.”
Moreover, it is also possible that a given intellectual production would be looked upon as a causal agent in itself, influencing, say, historical processes, or the further psychic development of individuals. In a moment, for instance, I shall explore the question whether the Comtean positivism of the Mexican political elite, during the decades prior to the revolution of 1910-20, had any appreciable impact on the development of recent Mexican history.

Admittedly this sort of causal hypothesis is hard to state in specific detail, and the stipulated variables are hard to define and measure, but we must nevertheless recognize that any causal hypothesis should give way to the null hypothesis—i.e., a finding of no (causal) relationship—unless those who advance the causal hypothesis have provided convincing evidence. There is a burden of proof, in other words, on those who allege bias. In general, recent detractors of scientific method as a bias-control technique have not provided such support.

In regard to the following series of statements, collected informally over many years of conversing with or reading the works of many colleagues, I do not reject the null hypothesis of no relationship; that is, I do not believe any of the following statements. They illustrate many of the alleged interactions among intellectual productions, historical factors, socio-economic status, sex, race, religious beliefs, personal conflicts and neuroses, etc. I shall explain in considerable detail why I do not believe statements (1) and (2); the remaining assertions will be touched upon briefly, at least by implication. Although I make a few references to pertinent discussions found in the literature, I do not wish to imply that cited authors believe these statements any more than I do. Nor do I wish to imply that statements of this genre are always wrong. I only wish to suggest that they are wrong far more often than we realize.

Here, then, are the statements:

(1) The Mexican version of Comtean positivism, and the political program derived from it, gave large impetus to the 1910-20 revolution in Mexico. See the discussion below.

(2) Functionalist theory tends to create a conservative political bias. See the discussion below.

(3) The invention of statistical methods gave impetus to an egalitarian attitude regarding human differences. See Barzun (1964:30), who claims that “the statistical outlook ... establishes an equality that dwarfs the individual.”

(4) A Parsonian functionalist, e.g., Parsons, is far more likely to submit to the demands of McCarthyism than a liberal Lundbergian empiricist, e.g., Lundberg. See Schrecker (1986:311,333), where it is concluded that although Parsons performed admirably under substantial political
pressure, “Lundberg may have been more confused than most of his colleagues ...” in responding to McCarthyist investigations.

(5) Capital punishment researchers, as liberal-minded social scientists, would have little if any prospect of finding support for the deterrence hypothesis as it applies to the death penalty. See Faia (1982).

(6) Social scientists who emphasize the “social construction of reality” are more likely to have high-SES origins than social scientists who emphasize “cultural materialism.”

(7) In general, the socioeconomic status background of college and university professors is a good predictor of their current political attitudes, beliefs, and behaviors. See Faia (1974).

(8) Male social scientists have little prospect of understanding feminist theory, and white social scientists have little prospect of understanding black sociology. See Faia (1990).


(10) If a scientist expresses serious concern about “greenhouse effect” and its exacerbation through the burning of fossil fuels, it is likely that the scientist has a remunerative relationship with the nuclear power industry.

(11) If a university such as Harvard accepts $60 million from a corporation such as Hoechst, it is unlikely that the university will produce and promulgate scientific findings that threaten the interests of said corporation.

(12) If one prepares an elaborate study of amniocentesis (or chorionic villi sampling) and selective abortion as methods of sex preselection of offspring, this research itself is equivalent to advocating the practice. See Bennett (1983).

(13) Scientists who have strong religious convictions are likely to have their scientific work impaired by those convictions. See Leuba (1916) and Faia (1976).

Harding (1986) presents many variations on (13).
(2) Quashing the indictment: Can a Comtean rule a country?

I take this as my major operating premise: If one wishes to demonstrate that positivism in the social sciences, over the last century, has not been the demons' dance that it is sometimes said to be, then Mexico during the years of the Porfirian dictatorship (1876-1910) presents the challenge of being a worst-case scenario, or at least a poor-prospects scenario. If the positivist philosophy can be exculpated in this instance, it can be exculpated in virtually any instance. If it receives a large share of the blame for the Porfirian dictatorship, then it is a blameworthy philosophy indeed.

(2.1) Positions

I am reluctant to say that the positivists “ran Mexico” during the Porfiriato, but I am convinced that Mexican positivism provides a rare instance in which a coherent non-Marxist social science theory has been placed in the service—for good or bad—of major political power. “Placed in the service” is a phrase that is subject to interpretation: It is very difficult to determine the degree of Comtean expertise achieved by the Diaz positivists, it is equally difficult to discover the degree to which governmental policies were derived from the positivist premises, and it is nearly impossible to determine whether Comteanism, in the Mexican context centering on the turn of the century, made much progress in achieving its various goals. Nor does Leopoldo Zea (1974:20,21) help when he says that “… neither positivism nor any other doctrine causes social well-being or social misery, but rather this social well-being or social misery expresses itself in a doctrine,” and that “to say that one thing is theoretical positivism and another thing is practical positivism implies a lack of intellectual responsibility … It is like hiding the hand that throws the rock. The purpose of every theory is to put that theory into practice. The theory is responsible for the practical effects of the theory.” As we shall see, Zea cannot have it both ways: A close reading shows that he, along with Carleton Beals, prefers the first of these two theses, seeing in Mexican positivism a sort of post-facto rationalization for policies set in motion by non-ideological factors.

Beals (1932:323) describes an elaborate public occasion, just after the turn of the century, in which a high official of the cabinet of Porfirio Diaz, dictator of Mexico, seems to be practicing racism against Mexican Indians and mestizos in order to impress European guests of the Diaz government. Jose Yves Limantour, the perpetuator of this incident and himself an ardent Comtean positivist and evanescent heir to the Mexican presidency, exemplified the racism of the times. This was the racism of positivistic organicism, Ratzenhofer, Gumplovitz, the social darwinists, various eugenics movements, the American Protective Association, Samuel George Morton, Edward B. Tylor, Lewis Henry Morgan, and contemporary editions of the Encyclopedia Britannica. The intellectual
The portmanteau of Mexican positivism contained the racist paraphernalia found elsewhere in the academic settings of the western world. Beyond that, Zea (1974:xx) believes that, in part, the hidden agenda of the Mexican positivists was “...an attempt, as Justo Sierra picturesquely describes it, to Saxonize Mexico, that is, to convert the Mexicans into 'Yankees of the South,’” and one can readily surmise that this attitude, this strong xenocentrism, was related to the prevalent racism.

Mexican positivists read their Comte with care and diligence. Aside from its social darwinist commitments and its racist undertones, Mexican positivism seemed to have a characteristically Comtean notion of ineluctable historical stages—theological, metaphysical, and scientific. Mexican positivism was a variety of positivistic organicism (Martindale, 1960:65), and therefore it tended to look upon expanding international commerce as an historical inevitability and a very good thing indeed. The belief that theology and metaphysics must give way to science may have added to anti-clerical traditions already firmly implanted in Mexico. And when the Mexican positivists considered the level of domestic tranquillity then established in their nation, they must have agreed readily with Benito Juarez, the greatest of Mexican national heroes1, when he claimed to see in positivism a way of containing anarchy (Zea, 1974:40-41). It was Juarez who in 1867 asked Gabino Barreda to collaborate with a national commission in the writing of a new plan of education (Zea, 1974:39). Barreda had been in direct contact with Comte in France, and was deeply immersed in Comtean philosophy. Octavio Paz surely does not miss the irony in the fact that a nation that had just succeeded in expelling French colonialism would find itself flirting with—indeed, carrying on a courtship with—French social philosophy.

The Asociación metodófila, established in 1877 and largely inspired by Barreda's educational programs, “...was to show how a group of men in different fields of specialization could understand each other and unite by means of certain basic principles, and by means of a method of interpretation that could be applied uniformly to diverse problems.” The program, in other words, contemplates the level of erudition, the synthesizing and generalizing of other scientific disciplines, that has always been the central tenet of Comtean sociology. The Asociación was given to the practice of consensus politics—not so much because it was politic to pursue this practice but because it was good science. Philosophical issues such as the role of human freedom—the free-will controversy—were handled with a deftness that characterizes modern sociology textbooks. That which is taken to be free—say, a body in “free fall”—is behaving in accordance with discernable natural laws, moving in this instance with a strict inertia interrupted only by a predictable acceleration of thirty-two feet per second per second. Whether such a body is truly free, again, can be ascertained just as surely as we ascertain what is transpiring a mile north of the north pole, and just as surely as we ascertain whether Anatole France was speaking of human freedom when he said that both the rich and the poor are free to live under bridges.
IN PRAISE OF THE NULL HYPOTHESIS

So much for metaphysics.

(2.2) Correctives

And so much for Agger's (1989:21) “unalterable world” as a summing up of the positivist Weltanschauung. Mexican positivism demonstrates the clear untenability of the charge (Agger, 1989:21) that “... sociology suppresses its narrative imagination precisely to have its reflection of invariant social patterns acted out uncritically.” Those members of the Diaz government who called themselves positivists were not a uniformly admirable group, but they had a far greater flexibility in the application of their social philosophy than is implied by the label “unalterable world.” Racist philosophy as a respectable academic calling dried up within the Mexican elite just as surely as it has dried up elsewhere. Even though Mexican positivism may have insisted on the anachronistic character of theology, religion, and specifically Catholicism, it eventually developed a surprising tolerance toward religion, an attitude that Beals (1932:340-41), no admirer of the positivists, regarded as highly conciliatory. Perhaps most importantly, there was substantial positivist opposition to the higher immoralities of the Diaz dictatorship (Cockcroft, 1968:59-60; 165), and several positivists eventually found themselves serving in the cabinet of the revolutionary government of Francisco Madero (Cockcroft, 1968:205). In brief, although the positivists emphasized consensus-seeking, they were by no means committed to an unalterable consensus. There were inexorable confrontations of thesis and antithesis: The Mexican political elite, in Beezley's (1987:13) view, “... subscribed to a loose sense of progress, based on Comtean positivism with individual touches of Catholicism or anticlericalism, of Indianism or anti-Indianism, and of greater or lesser doses of the Liberal belief in the efficacy of property.”

(2.3) Motives: A series of acts contrary ...

As I implied earlier, Zea had trouble deciding whether Mexican positivism was a collection of “ideas that matter,” or merely a set of post-facto rationalizations—more cynically, a manipulative ideology of rule. This issue cannot finally be resolved, but those who agree with Agger's contention that the positivist philosophy is a mischievous monkey that must be gotten off our backs would do well to consider the many mitigating circumstances that apply in the Porfirian instance—an instance in which the positivist philosophy is clearly at high risk of high culpability. After cavalierly dismissing the científico version of positivism as “drivel,” Beals (1932:324) takes great pain to show that Mexican positivism was little more than a post facto rationalization for what amounts to systematic social banditry on the part of Diaz' major political henchmen. His chapter entitled “profits of positivism” (Beals, 1932: Chapter 43) makes the case
in detail, and the chapter is not vitiated by the fact that it is titled wrongly—positivism itself produced no profits, but merely served as an element of the legerdemain of very clever social bandits. Zea makes the same case, although far more convincingly. It was not so much positivism per se, he says, that was attacked by its many detractors (Zea, 1974:15-16), but rather the expression of positivism as transformed by the científicos:

Positivism was a doctrine that saved a group of mediocre men the trouble of thinking. It was a doctrine that protected their interests. ...

When one attacked positivism, it was not so much the doctrine that he attacked, but rather the political group behind it. Porfírimo and the political group called the Científicos used positivism as their ideological basis.

The positivist Jose Torres, cited heavily and with strong approval by Zea (1974:17-18,221),

... openly stated that positivism was not responsible for the social misery brought about by Porfírimo. ... Revolutions and social misery, said Torres, are something that we have lived with since our independence. ... To demonstrate his thesis, ... Torres examined the positivist politics of Auguste Comte. He pointed out that these politics had nothing to do with the development of Porfírian politics.

Just as Talcott Parsons appears to have had an attachment to political stability that had nothing to do with his theoretical predilections (Alexander, 1983:186), so the Mexican positivists may have pursued political goals and personal interests that had little if anything to do with their own unique brand of positivism, and much to do with “... one basic idea, to steal, much, often and scientifically” (Beals, 1932:334). Regarding matters of politico-economic policy, Beals (1932:343-44) points to an intellectual tabula rasa: “... Diaz had no economic plan; Goddess Fortune directed it entirely.”

In closing the abridged English translation of his inquiry into Mexican positivism, Zea (1968:221) concludes that “Jose Torres was right when he asserted that positivism was not to blame for the evils of Mexico. The positivist ideal was one thing and the reality called Porfírimo was another. ... Positivism provided the weapon by which to justify a series of acts contrary to the positivist ideal.” In the Mexican edition of 1968, in Zea's Spanish, this final sentence is italicized (Zea, 1968:229).
In the long and tortuous history of functionalist theory, consensus usually has been no more evident than in the case of Mexican positivism. At any given historical moment functionalists of a given genre are struggling valiantly against those of a discernably different point of view, or perhaps against a vociferous school of non-functionalists or even anti-functionalists; anti-functionalism has a history at least as elaborate as that of anti-positivism. Eventually, one sees the wisdom of Ritzer's claim (1980:201-208) that paradigm disputes probably do more harm than good. On the other hand, this may be an auspicious time to rewrite these histories, recognizing (and perhaps insisting) that entire paradigms, in a holistic sense, are rarely what is at stake (Glymour, 1980: esp. Chapter 5). Positivistic organicism, i.e., scientifically-oriented functionalism, has shown an extraordinary capacity for correcting itself piecemeal, as in the case of its repudiation of racism.

In Harris' excellent chapters on the subject (1968), it becomes clear that positivistic functionalism, with its constant internecine warfare, damaged itself in several ways: First, its early overindulgence in the organic analogy, social darwinism, racism, sexism, and unilinear evolutionary theory (parallelism); second, its subsequent over-reliance—a sort of compensation for earlier excesses—on synchronic (cross-sectional) analysis, its occasional repudiation of the idea of structural survivals, its occasional indulgence in hopelessly abstract Hegelian idealism, its apparent conservatism, and its alleged willingness to tolerate questionable research practices. These various elements do not necessarily fit together into a coherent package, and we encounter here the same sort of hodge-podge of ideas that we encountered in the case of Mexican positivism. The Mexican positivists showed a similar high talent for picking and choosing among the great historical fallacies.

While I consider most functionalists to be guilty on at least a few of Harris' several counts, I believe that all sorts of extenuating circumstances now require clemency and perhaps even amnesty. In any case, regardless of how severely we castigate functionalists, we must recognize that the litany of charges against them does not necessarily tell us anything about the strengths and weaknesses of the central premises of functional analysis. We must avoid the synecdochic fallacy: A small part of a theory does not represent the whole, and may prove to be misleading and highly expendable. The occurrence of an occasional illness reveals little if anything about the general condition of the organism; what is important is the capacity for making a recovery. Again, as Glymour (1980) suggests, it is possible to reject part (or several parts) of a large theory without rejecting its totality.

(3.1) From Spencer to Weber
Martindale (1960:65) gives Herbert Spencer credit for having provided the philosophical groundwork for positivistic organicism, and a detailed examination of Spencer's organicism makes it clear that, once we have stripped away a few quaint, inessential, and perhaps outrageous notions no longer taken seriously, it subsumes or surpasses all the central premises of modern functionalism, including “neo-functionalism” with its revival of social evolutionism. Martindale (1960:67) summarizes Spencer's organic analogy as follows:

1. Society undergoes growth; (2) in the course of its growth, its parts become unlike (that is, there is a structural differentiation); (3) the functions of society are reciprocal, mutually independent, and interrelated; (4) like an ordinary organism, the society may be viewed as a nation of units; and (5) the whole may be destroyed without at once destroying the life of the parts.

If the sociology of the twentieth century had been content to pursue the Spencerian premises to their logical conclusions as a way of understanding social dynamics, rather than pointing to the inevitable limitations of the organic analogy and to the many faults, foibles, and egregious claims of Spencer and his followers, contemporary sociology probably would be in a much stronger position than it is, and would not be worried about an alleged tendency to fall into the doldrums. A perusal of Spencer's sociology—never mind his many contributions to philosophy, biology, and psychology—shows that it is, as Lenin once said of Clausewitz' writings on war and diplomacy, a splendid collection of fine points.

In his discussion of “social metamorphoses,” for instance, it becomes clear that Spencer's positivistic organicism has a strong sense of the sometimes discontinuous character of social evolution—that is, a sense of social change that has nothing whatsoever to do with “unalterable worlds”:

Verification of the general view ... is gained by observing the alterations of social structures which follow alterations of social activities; and here again we find analogies between social organisms and individual organisms. In both there is metamorphosis consequent on change from a wandering life to a settled life; in both there is metamorphosis consequent on change from a life exercising mainly the inner or sustaining system, to a life exercising the outer or expanding system; and in both there is a reverse metamorphosis (Spencer, 1969:136).

Major ecological events, then, such as those subsumed under the “hydraulic hypothesis” (Harris, 1979:104-5), continually create massive social dislocations. And warfare, to Spencer, is another powerful engine of social evolution:
... the struggle for existence has been an indispensable means to evolution. Not simply do we see that in the competition among individuals of the same kind, survival of the fittest has from the beginning furthered production of a higher type; but we see that to the unceasing warfare between species is mainly due both growth and organization. ... Mark now, however, that while this merciless discipline of Nature ... has been essential to the progress of sentient life, its persistence through all time with all creatures must not be inferred. ... The myriads of years of warfare which have developed the powers of all lower types of creatures, have bequeathed to the highest type of creature the powers now used by him for countless objects besides those of killing and avoiding being killed. His limbs, teeth, and nails are but little employed in fight; and his mind is not ordinarily occupied in devising ways of destroying other creatures, or guarding himself from injury by them. ... Observe that the inter-social struggle for existence which has been indispensable in evolving societies, will not necessarily play in the future a part like that which it has played in the past (Spencer, 1969:176-77,178).

Here we have an extraordinary irony: The man who coined the phrase “survival of the fittest” (Harris, 1968:128) brings forth a theory that implies a social metamorphosis in which past warmaking either loses its functionality or evolves in such a way as to take on new non-warmaking functions. Spencer, then, clearly envisions the occasional disassociation of structure and function, and makes imaginative use of that disassociation as a mechanism of social transformation—“unalterable worlds,” again, have no place. And if Spencer's functionalism does not have any ultimate commitment to social conflict, perhaps more recent versions of functionalism do not, or need not, have any ultimate commitment to social integration, cooperation, and consensus. Our politics—as in the case of the Mexican positivists—may be largely a matter of personal predilection, not theoretical necessity.

It is arguable, then, that the worst nineteenth-century snafus of positivistic organicism—starting, say, with the cranial-capacity indexes of Samuel G. Morton—resulted from the fact that biology was very much in the air, and nobody was exempt. Similarly, the stigmata attaching themselves to contemporary North American versions of positivist functional analysis may have been created by the prevailing political climate of recent decades. The following comment was written more than thirty years ago:

... the very dates of the steep rise of interest in functionalism among [then recent] sociological theorists also suggest that it may have some ideological import. It arose after 1940 and with particular speed after World War II. Moreover, its ranks have been increasingly swelled by
deserters from social behaviorism—an evidently liberal position. The rise of sociological functionalism thus coincides with the return of the Republican Party to power, the return to religion, the rise of McCarthyism, and other typical manifestations of a postwar conservative reaction. Whether these are just accidental correlations or not, it is certainly true that ideological factors are now far less important for the structure of sociological theory than was once the case (Martindale, 1960:520).

There is a political aura, but the aura does not emanate from the object that it surrounds. I doubt Martindale's claim that ideology—the current political climate—is now less important than heretofore, primarily because I see so little evidence of its past significance as an inescapable determinant or consequence of one's theoretical strategies. I have yet to discover evidence that the political conservatism of a scholar such as Spencer was a necessary consequence of his positivistic organicism, I have grave doubts that the arrival into the functionalist camp of “liberal” social behaviorists had a large political impact on either field (cf. Faia, 1974), and I do not see any reason to believe that Martindale is calling our attention to anything more than “accidental correlations,” as he himself surmises. Accidental correlations, by definition, occur when the null hypothesis prevails, and it is the thesis of this chapter that, in most instances where “ideological bias” is the leading theoretical hypothesis, the null hypothesis will continue to prevail.

On the matter of Spencer's celebrated distrust of government, his consequent laissez-faire predilections, and the relationship of these to his sociological theories, Barnes (1948:129) has provided the definitive alternative hypothesis. Spencer, he says, had an “anti-authority complex” exacerbated by various experiences of childhood, especially his “domination by male relatives, and his confirmed neurotic tendencies.” We must also remember that Spencer came from a “...dissenting family and was reared in that atmosphere.” It seems, says Barnes, “that his attitude toward government must have had a deep-rooted emotional foundation, since it diverged materially from some of the vital premises of his general philosophy.” This style of psycho-history explains Spencer's politics far more plausibly than does his advocacy of evolutionary positivistic organicism. And nota bene: Barnes is explaining Spencer's politics with reference to his personal neurotic conflicts; Spencer's positivistic organicism is not implicated either as cause or effect.

If anybody ever returns a plausible diagnosis of neuroticism in the case of Talcott Parsons, the thesis that political values are sharply constrained by theories, or vice versa, will receive yet another devastating setback. In the meantime, Sica's (1988) book on Max Weber is replete with hypotheses of a psycho-historical nature, and the assumed psychodynamics, again, usually have nothing demonstrable to do with Weber's “general philosophy.”
Even when the psychodynamic explanation of a general philosophy has a surface plausibility, the evidence presented generally leaves a lot to be desired, and again one has no recourse but the null hypothesis: Nothing has been demonstrated, and the theories in question have to be tested and evaluated on the assumption that they have been derived through legitimate scholarly efforts, not through idiosyncratic psychic disturbances. Sica's (1988:221) description of Weber's alleged neurotic interactions with his parents, then, appears to be little more than an exercise in phrenology:

If ... writers are at their best when considering topics closest to their hearts, surely the power, precision, and credibility of these passages allow the reader a rare glimpse into Weber's inmost metaphysical concerns. His mother's uncritical religiosity and his own zealous skepticism, slammed together by a son's confused love for an unbending mother and his simultaneous admiration of his father's unreflective pragmatism, caused no end of personal problems. Some of these are expressed [in Weber's writings] in highly sublimated but nonetheless obvious form.

Needless to say, these expressions were not obvious to this reader.

(3.2) Pareto

I have always believed that many of the laissez-faire philosophers were merely warning us against grabbing the flywheel of the social engine, when the appropriate tactic is to seek out the ignition switch or some way of leaning out the fuel mixture. I therefore believe that Harris (1968:534-35) goes too far when he draws a strong analogy

... between the entire synchronic functionalist movement and the classical laissez-faire doctrines of economics. ... This analogy ... is an undeniable consequence of both the Malinowskian and Radcliffe-Brownian versions of [radical] functionalism that every sort of institution, from witchcraft to war, receives its due as a functional contribution to the welfare and maintenance of the social system. ...

Perhaps this claim can be made against Malinowski and (more debatably) against Radcliffe-Brown, but it can hardly be sustained against Spencer or Pareto, neither of whom ever insisted upon universal functionality and whose laissez-faire proclivities cannot possibly have been derived from the anti-evolutionism of later British anthropologists. Even Malinowski and Radcliffe-Brown did not play down the evolutionary themes because of some latent commitment to know-nothingism or laissez-faire politics, but rather because they wished to
disassociate themselves from the ethnocentric evolutionary theories of some of their predecessors, and to escape the recrudescence of social darwinism, racism, and sexism that had stained the escutcheons of Spencerian theory. In other words, they overreacted. Harris himself (1968:54) points to one of the positive consequences of the self-regulating laissez-faire model when he says that, as exemplified in the works of Jeremy Bentham, James Mill, David Ricardo, and John McCulloch, it contributed to “... the perpetuation of the scientific approach”—in modern times, in the midst of a major assault against positivism, we realize that this function is of the utmost importance.

As for Pareto, perhaps we should conclude by questioning the ugliest of the political accusations against him, viz., that he somehow aided and abetted the Italian fascist movement. In Lopreato's assessment (1980:xix-xx), Pareto realized by 1922

...that fascism was a fleeting phenomenon, greatly inferior to the socialist faith, and defined fascism as a complex of factors destined to “remain secondary and subordinate to the great factors of social evolution” ...

... However, his political theory speaks the loudest. Pareto was a political skeptic. His theory of revolution conveys a powerful message: all regimes become decadent and nasty as they mature. He was also a humanist who fought ceaselessly for democracy of the Swiss variety, for freedom of religion and the press, against racism and anti-semitism, for freedom of any sort of organization. If that is the stuff that fascists are made of, then Pareto was indeed one of them.

(3.3) Malinowski and Radcliffe-Brown

Malinowski's major contribution to positivistic organicism was to raise poignant questions about the unilinear predilections of earlier functionalists, about their use of the comparative method as a way of comprehending the distant history of human societies, and about the use of survivals as keys to the past. Malinowski’s critics have accused him of adopting a horribly constraining synchronic approach to cultural anthropology, but Malinowski himself (1944:16,175-76; 1982:60) makes it clear that his position is far from simplistic:

Modern anthropology started with the evolutionary point of view. In this it was largely inspired by the great successes of the Darwinian interpretations of biological development, and by the desire of cross-fertilizing prehistoric findings and ethnographic data. Evolutionism is at present rather unfashionable. Nevertheless, its main assumptions are not only valid, but also they are indispensable to the
field-worker as well as to the student of theory. The concept of origins may have to be interpreted in a more prosaic and scientific manner, but our interest in tracing back any and every manifestation of human life ... remains ... legitimate ... I believe that ultimately we will accept the view that “origins” is nothing else but the essential nature of an institution like marriage or the nation, the family or the state, the religious congregation or the organization of witchcraft ...

Functionalism, I would like to state emphatically, is neither hostile to the study of distribution, nor to the reconstruction of the past in terms of evolution, history or diffusion.

Malinowski equivocates over the meaning of “origins,” but I do not discern here an attitude of total hostility toward history, and there is certainly no commitment to “unalterable worlds”; perhaps Malinowski was guilty only of taking seriously Pareto's contention (1980:77) that “in general the unknown has to be explained by the known, and the past is therefore better explained by the present than the present is by the past”; teach history, in other words, with the calendar running in reverse.

As for the Malinowskian emphasis on social equilibrium, there is for Radcliffe-Brown (1952:43) nothing magical or inevitable about it:

One ... necessary condition of continued existence [of social systems] is that of a certain degree of functional consistency amongst the constituent parts of the social system. Functional consistency is not the same thing as logical consistency; the latter is one special form of the former. Functional inconsistency exists whenever two aspects of the social system produce a conflict which can only be resolved by some change in the system itself. ... No social system ever attains to a perfect consistency, and it is for this reason that every system is constantly undergoing change.

Perhaps Gregg and Williams (1948) have a point in accusing Malinowski of being charmed and seduced by the notion of “functioning harmonious wholes,” but they cannot make a similar charge against Radcliffe-Brown (Faia, 1986:159-60), and this leads us to surmise that the charge against Malinowski merely reflects a personal quirk.

(3.4) Parsons

For something like thirty years, starting around 1940, Parsons' work provided and reflected a dominant social science paradigm. In Alexander's view (1983:7), the strength of Parsonian thought derives mainly from its multidimensionality,
its systemic features; that is, from its tendency to focus on the interactions of biology, personality, society, and culture. The “cybernetic continuum,” operating through various “exchange media,” makes possible a degree of integration of highly disparate social phenomena, so that society is seen as comprising the ultimate form of Spencer's “coherent heterogeneity” and clearly falls under the rubrics of positivistic organicism. Parsons' predilection for systemic perspectives made him a strong advocate of paradigm integration in the social sciences.

For my present purposes it is important to show the untenability of the conventional claim that Parsonian theory involves an inescapable, albeit tacit, commitment to social stability. Those who make this claim must find a way to explain the following anomalies: First, Parsons has a clear imagery of structural strain and its ability to produce adaptive efforts or to disrupt stability. He occasionally interprets social systems from the standpoint of what I call the “GM analogy” (Parsons, 1937:31, 230), and he never regards reciprocating engines as inherently smooth-running—if abused, they tend to blow up. Second, his insistence that utilitarian theory is built upon the mistaken notion of “randomness of ends,” and that this venerable theory therefore fails to explain ends adequately (1937:59-60, 231), forces us to examine reciprocal interaction between ends and other aspects of the social system. Non-random processes, by definition, are subject to explanation. If explanation implies causation, and if causation implies manipulation of causal agents, and if manipulation of causal agents implies change, then non-randomness of ends implies change. Third, when Parsons (1937:346) points to Durkheim's thesis on the “antiority” of social facts, implying that many social facts therefore do not have utilitarian value for given actors, he inadvertently introduces the important concept of structural survivals and the possibility of a disassociation of structure and function. This disassociation is an essential element of the functional analysis of social change, and Malinowski's work, as we have seen, was surely not aided by his denial of it. Fourth, despite his commitment to requisite analysis, Parsons (1951:167-77) has a clear recognition of the possibility of functional alternatives; selection among alternatives implies change, and it also implies the creation of new survivals.

The modal accusation against Parsons, weak though it may be, is that he had political predilections toward social stability. Many readers who have made a careful analysis of The Social System while remaining a little cavalier in their treatment of other Parsonian works, have had understandable problems with the statement that “... it is essential to the conception of the interaction process ... and of the theorem of the institutional integration of motivation ... that the stabilization of the processes of mutual orientation within complementary roles is a fundamental 'tendency' of interaction” (1951:481). This sounds like an a priori postulate of social stability. Parsons does not help the situation—in fact, he aggravates it—when he tells us that the stability postulate is a theoretical assumption and not an empirical generalization (1951:481), as if there were a sharp discontinuity between the theoretical and empirical levels of analysis.
IN PRAISE OF THE NULL HYPOTHESIS

Parsons further argues that social systems, having properties of equifinality, are often able to tolerate “... fluctuations in the factors of the environment” while maintaining “certain constancies of pattern, whether this constancy be static or moving” (1951:482). Never mind the fact that Parsons’ “moving” constancy is not precisely an instance of stability, or that for every instance of equifinality one can easily identify a departure from equifinality, as Radcliffe-Brown suggests above. If hunting-gathering bands are running out of food, they had better forget about constancies of pattern.

Equifinality implies disturbances of equilibrium in the same sense that the landing of an aircraft implies an earlier takeoff. And my flight instructors have always insisted that the total number of my landings (L) equal the total number of my takeoffs (T). If T - L ever equals 1 or more, I will have experienced profound change.

Perhaps the best instance of an unreasonable adherence to stability and inertia in Parsons' work is his theory of sex roles and their allocation within nuclear families. These writings do seem to exemplify what feminists have called a “functional freeze” (Friedan, 1963; Parsons, 1951:193, 241, 503-505; Parsons et al., 1955: Chapter II). My own work on functionalist theory, however, has persuaded me that the alleged functionalist crystallization of sex roles was the result of an inappropriate application of requisite analysis, and that requisite analysis should long since have been jettisoned as an element of functional analysis.

Consider, for instance, a recent work by Levy (1989). This book seeks to demonstrate the universality of the nuclear or quasi-nuclear family; it seeks to demonstrate the centrality of mothers and mother-substitutes as agents of infant and child socialization in virtually every realm of cultural activity; and it purports to explain these phenomena, these social structures, by means of requisite analysis. A “structural requisite,” as defined in the glossary of this volume, is “a pattern or observable uniformity of action or operation necessary for the continued existence of the unit with which it is associated ...” (Levy, 1989:228). Very early on (Levy, 1989:xv,9) the author invokes the concept of structural requisites, stating that this concept will be fundamental to his method of theoretical inquiry. And yet, one reads the entire volume without finding a single instance in which a social structure has been shown to be necessary for the continued existence of any unit with which it is associated. It is not shown, for example, that the centrality of motherhood is necessary for the continued existence of the nuclear or quasi-nuclear family, for the continued existence of adequate socialization of infant and child, or for the continued existence of the larger social system and its many components. Nor do we encounter any attempt to explain which requisites are met, if any, by the nuclear or quasi-nuclear family.

On the other hand, given that Levy himself (1989:39) agrees that current family structures will not necessarily continue indefinitely, what happens to his brand of requisite analysis? And why does he argue, in spite of what he has just
said, that the current situation will persist? I contend that there is a presupposition in requisite analysis that any social structure, considered abstractly, that is a universal or quasi-universal must be an inescapable requisite for something. But for what? And how do we demonstrate this relationship? Given the fact that functional analysis always involves circular causation, there are a few additional questions to be raised here: How could the current (universal?) family structure be changed? And why are the factors capable of producing such changes never activated, so that the current situation persists? If specific causal hypotheses about requisites were being stated and tested, these questions could be addressed. The fact that they cannot be addressed within Levy's perspective implies strongly that the hypotheses have not been developed. I made precisely the same point several years ago (Faia, 1986:169).

After presenting a large part of his argument, Levy (1989:123) arrives at an inescapable impasse: He confesses that neither he nor anybody else, confronted by the ubiquity of the nuclear family and the centrality of motherhood, can explain these structures adequately. I would argue, therefore, that this is a descriptive study, and not a traditional functionalist study of requisites. When Levy realizes that social scientists are not prepared to answer the questions raised above, he seems to fall back upon a vague hope for some sort of reductionist bail-out by sociobiology, stating at one point (Levy, 1989:26-8) that

> even though none of the elements called “social” here is an elegant biological reduction, the social is not treated as nonbiological but, rather, as a specific subcategory of the biological that is not yet explicable in terms of ... human heredity and the nonhuman environment. Everything not elegantly reduced to biology ... is considered social, but it is not considered impervious to such reduction.

And within a few pages (Levy, 1989:28) we encounter the example of mother cats inducing peristalsis in their kittens by massaging their bellies; and a second cat story, this time involving male and female lions and the nature of their hunting-gathering activities, occurs in a later context (Levy, 1989:86-87). In these commentaries we see what amounts to a prayer for sociobiology to the rescue, despite the fact that sociobiology has had few empirically substantiated successes in explaining the interaction of human biology and social behavior. Phenylketonuria (PKU) and Tay-Sachs disease, involving readily identifiable genes and the prospect of cultural interventions that head off their damaging effects, are among a short list of prime examples (“The Telltale Gene,” July 1990:483-84). I remain skeptical about Desmond Morris’ (1967: Chapter 2) famous speculation that face-to-face sexual intercourse, with its massive social implications, was an evolutionary breakthrough that occurred when the frontal nudity of females began to replicate—perhaps simulate is a better word—their rearward nudity. The biosociology of face-to-face intercourse must be at least as
edifying as the sociobiology of this important invention (cf. Jencks, 1992:92-119).

Levy's best discussion of causal dynamics, in fact, occurs in chapter 10 where he argues that various dimensions of human relationships, within the realm of kinship or elsewhere, are highly correlated with each other, e.g., “arational” relationships tend to be “particularistic,” “intimate” relationships tend to be “responsible,” etc. Such correlations are summarized efficiently in Levy's (1989:174-76) special notation. There is also in this part of Levy's presentation a diachronic dimension: A rational relationship, for instance, typically evolves toward being arational, individualistic relationships evolve toward being responsible, etc. Presumably, combinations of “relationship aspects” (as he calls them) that are not highly correlated with each other are selected against by various social factors. Further, those relationship aspects that tend to occur at the end of social-evolutionary processes seem to receive favorable selection. But Levy's brand of requisite analysis, again, remains silent as to what specific social factors might be implicated in these social-selection processes.

Social relationships that are rational and universalistic, but also functionally diffuse, intimate, responsible, and non-hierarchical appear somewhere on Levy's list of logical possibilities, but relationships with this combination of qualities, in Levy's view, would be highly improbable. Logically, his Table 1 (Levy, 1989:136) permits $2^6$ or 64 distinctive types of social relationships, although Levy considers only two of these types to have a high probability of occurrence. And of these two types, the first has a tendency to evolve into the second.

Most of the other 62 forms, then, appear to be more or less untenable, for reasons never specified beyond noting that they are forms that, in Levy's estimation, tend to be rare. But regarding the argument that absence (or rarity) implies untenability, we must note Dawkins' (1986:9) remark that in the realm of natural history there are many more ways of being dead than of being alive, and that we know far less about ways of being dead than we know about ways of being alive—which admittedly is not much. The problem, then, is that many ways of being dead probably would work well if a resurrection could be arranged in an appropriate context, or ecological niche. Why, then, do such resurrections tend not to occur? In biology, the answer is that nobody knows how to resurrect extinct species; they are infinitely more complex than what is implied, say, by Levy's list of 64 forms of social relationships. Yet, it would appear that in the social realm we have a relatively good prospect of resurrecting extinct structures or creating new structures from the lists of logical possibilities supplied by the likes of Levy. And the best news is that there is nothing about Levy's brand of requisite analysis that rules out the various forms of social engineering implied by his unique set of pattern variables, or by the similar taxonomies produced by other scholars.

Levy's experiments with pattern variables, of course, are a Parsonian penchant, and his analyses of specific instances of social change do not generally
insist on a tendency toward stability. *The Social System* presents an array of conditions and processes capable of producing change from within a society, including not only selection against untenable combinations of the pattern variables (1951:152)—the matter just discussed in some detail in connection with Levy's work—but also several forms of structural strain (1951:179-200), deviant behavior (1951:206), the extreme malleability of the human infant (1951:336, 508), a general strain toward consistency in values (1951:383), new religious ideas, genetic change (1951:493), and the changeability of functions performed by given structures (again, a prospect of survivals), as in the example of horses (1951:511).

Perhaps Parsons' best statements about *external* sources of change are found in the extraordinary series of essays on the historical evolution of Germany and Japan, and on the prospects of social engineering in these societies after 1945 (Parsons, 1954).

In short, the various labels affixed to Parsons are nearly as variegated and inexplicable as those applied to Durkheim (Levine, 1985:55-6):

cognitive relativist
determined positivist
champion of science
moralizing metaphysician
committed nominalist
philosophical realist
methodologically brilliant
methodologically weak
conservative
liberal
socialist
friend of modern individualism
foe of modern individualism
ardent secularist
mystical religionist
his thought highly consistent
his thought highly inconsistent

I don't know what these labels tell us about the eye of the beholder, but they tell us next to nothing about Durkheim or about Durkheim's contributions to sociological theory. Many of these labels have the binary, dialectical, contradictory qualities of folk wisdom, comparable to informing us that "birds of a feather flock together" while "opposites attract," that "absence makes the heart grow fonder" and "out of sight, out of mind," or that "a bird in hand is better than two in a bush," but "if at first you don't succeed, try, try again."
(4) Overturned on appeal: Research on political power

Procedurally, the case looks strong (Walton, 1970:443):

The procedure employed here is unique in that it uses the results of other studies as data for testing a number of hypotheses concerning substantive and methodological correlates of community power structure. ... while such “tests” are indirect when dealing with propositions about power in communities, they are direct and entirely appropriate when dealing with propositions about previous research.

This inquiry, in other words, does not tell us much about power structures in American communities, but it may give us insight into the ways in which methods and “substantive” factors relate to what we think we know about community power. That is, it may give us insight into our biases.

And there are, indeed, some fascinating findings. Most importantly, it appears that the “reputational” method usually identifies pyramidal power structures, while those methods that focus on actual decisionmaking processes are more likely to discover factionalized and coalitional power structures (Walton, 1970:452). Regarding the “substantive” factors it was found, for instance, that communities with high population growth tend to have more “concentrated” power structures (Walton, 1970:452).

In evaluating Walton's work as a study of investigator bias, we should first realize that the “substantive” findings do not necessarily tell us anything about bias: It is entirely possible, for instance, that the statement about population growth and power concentration may be an accurate, unbiased description of the realities of various types of American communities. The case for bias must be built on findings of the first kind: When the selection of one's method seems to have a strong influence on one's findings, we begin to suspect that those who have a liking for certain findings will consciously or unconsciously select their methods accordingly. Clearly, whenever a relationship exists between methods and results, the grand traditions of scientific skepticism must come into full force.

In the present instance, however, inexorable skepticism leads to the realization that the reputational method tells us primarily how (presumably) knowledgeable members of a community perceive local power structures. This method produces findings that have more to do with the sociology of knowledge than with the sociology of social classes. Interestingly, the judges used in reputational studies typically perceive elitism; this is in contrast to what is usually an early lesson in our social stratification courses, to the effect that American public opinion tends to deny the existence of an elaborate system of social classes, and certainly to deny elitist dominance of the political process. Apparently the reputational studies have often found judges of high sophistication: Perhaps many of these judges have taken our courses.
In any case Walton's study, which at first appears to be an inquiry into the nature and causes of bias, may actually be—thanks to a dialectical one-eighty—an inquiry into the processes of controlling biases.

(5) **A new hypothesis**

Without comment (Aldous, 1991:661):

Interestingly enough, the perspective on how the family is faring in these books appears to bear some relation to methodology. The books by Rossi and Rossi and by Whyte, based on survey research with large samples, present more continuities with families as we have generally liked to picture them. In the more qualitative researches (Hochschild, Stacey, and Weiss) and historical analysis (Dizard and Gadlin) the disjuncture between past and present family arrangements comes through most sharply.

(6) **Conclusion: Science as bias control**

For nearly thirty-five years I have been misinforming students (and more than a few colleagues) about Malthus: Until recently I subscribed to the belief—a commonplace among demographers—that the original version (1798) of Malthus' theory of population, that of 1798, made sense for the centuries preceding 1800 or thereabouts, but became invalid for subsequent times. Over the last few years, however, I've found it necessary to change my mind. I now believe, with Harris and Ross (1987), that Malthus' famous postulate about the “passion between the sexes” as a source of constantly high fertility throughout human history was probably incorrect: The human race has had a considerable aptitude for controlling fertility by means that include feticide, infanticide, pedicide, and harsh treatment of women—the latter having a powerful anti-natalist impact.

On the matter of Malthus' early bias against contraception and other means of fertility control, I now realize that it must have been easy for him eventually to control this bias and to support “moral restraint” as a means of reducing fertility, for modern populations. It was easy for him because, first, the data suggesting fertility reductions among European populations during his lifetime could not be ignored, and, second, there was nothing in his version of functionalism that made such a change difficult. Malthus' functionalism did not distort his socio-demographic analysis any more than Parsons' functionalism created his alleged commitment to social stability as an operating postulate. The major reason why Malthus' modified theory of population—the 1803 version emphasizing moral restraint—is far more interesting and valuable than his first formulation is that it introduces new variables, new instabilities, and the prospect of feedback and control, and it thereby transforms itself into a much more
sophisticated functionalist theory than it had been originally. Still, it did not capture the earlier historical patterns, as Harris and Ross make clear.

As I suggested earlier, it is often claimed that biases tend to occur because scholars are subject to the limitations of historicism: They wear the intellectual blinders characteristic of their times. When Marxists criticize Malthusians, however, they usually allege that Malthusians have deluded themselves into believing that they have a socio-demographic theory that is valid for all times and for all populations. This claim makes Marxists very uncomfortable, because they have a strong commitment to the notion that every historical epoch produces its own typical distortions. It is ironic, of course, that Marxists themselves have the same intellectual aggressiveness, or arrogance, that they see as the bane of Malthusianism, when they suggest that dialectical materialism is the master process of all human history.

As a Marxian functionalist with a strong admiration for Malthus, I experience infinite agonies and ecstacies from all three traditions, but what I find particularly compelling and comforting about Malthusian, Marxist, and Mertonian functionalism (Merton, 1957) is that each of these lines of analysis has the courage to claim sociological laws of wide applicability (Levy, 1989), laws that have a built-in capacity for protecting us not only against the biases of historicism but also against the sorts of biases to which I alluded earlier. What these three traditions have in common is a commitment to the scientific method, and in my estimation the scientific method itself has proven to be among the most powerful and universal of sociological laws. This set of laws clearly has the power to protect Harvard from Hoechst.
(1) Juarez was the constitutional president of Mexico from 1857 until his death in 1872, and played a major role in the expulsion of the French forces supporting Maximilian. Maximilian was emperor of Mexico from 1864 to 1867. See Fehrenbach (1973:432-38).

(2) An earlier, more detailed version of section 3 appears in Faia (1986:144-65).

(3) Spencer's claim (1867:485-6) that modern women have a “... deficiency of reproductive power ...” due to an “... overtaxing of their brains ...” is a sexist claim in the sense that it assumes that reproductive roles are biologically linked to traditional sex roles that allegedly rule out heavy mental demands on women. And yet, Spencer's analysis consistently regards sex roles as highly changeable. It should be remembered that his speculations about declining reproductive power were an answer to Malthus: Spencer believed that modern women were indeed entering mentally demanding occupations in large numbers and that this continuing trend would relieve them and modern societies of the Malthusian curse. In other works by Spencer there are similar extenuations. It is shockingly sexist to say, for instance, that mental differences between men and women arise from an “... arrest of individual evolution...” in women, a physiological retardation that seems to make women especially fit for reproduction (Spencer, 1910:340). Yet, the same discussion anticipates Michel Foucault by claiming that women, adapting to male dominance, have developed excellent manipulative skills and an impressive ability to disguise their feelings, apparently with minimal risk of overtaxing mental faculties. In another work, Spencer argues that “... it would ... be impossible to reduce the theory of unequal rights to practice. We should yet have to find a rule by which to allot these different shares of privilege” (Spencer, 1851 [1969]:108). This thesis would seem to imply equal rights regardless of sex.
REFERENCES (Chapter 3)


This chapter is a detailed critique of three published articles. What these articles have in common is the fact that they express serious misgivings about quantitative structuralism and its positivist underpinnings. In my view these papers also share a large problem: Most of their claims are untenable.

(1) Three papers in search of an argument

Let me begin by saying that I do not share Andrew Abbott's (1988:169) pessimism: I do not believe that there is a “growing chasm” between sociological theory and sociological research despite the fact that a few scholars, such as Sica (1988:20,40,78-9, 94-5,118, 136-41,259), grasp at every opportunity to show that the contemporary program of social theory is badly hampered by such alleged inanities as empirical research and statistical analysis. The traditional rift, if anything, has become smaller in recent decades. It has become smaller, I believe, because more and more social scientists have come to the realization that those who are weak in methodology cannot fully comprehend sociological theory, that those who achieve little understanding of theory have equally limited prospects in the methodological realm, and that the most precious intellectual abilities have to do with “translating models” (Moreno and Glassner, 1982) that may seem to involve incompatible ways of thinking, but which in reality do not. Abbott claims support in the writings of Peter Abell (1987), but Abell's method of “comparative narratives,” beginning with the type of idiographic story analysis that Abbott finds so alluring, ends by combining story analysis with traditional “variable-centred” modes of social research. Abell believes that theory and research should be complementary. He also believes that we should take advantage of “triangulation,” the use of multiple methodologies—a theme to which I shall return.

The relationship between theory and method, then, involves precisely the sort of reciprocal causation that, ironically, will often become a matter of controversy in the pages that follow. If we could somehow make this nexus serve us more effectively, we could rise above the theory-methods rift by creating a magnificent structure, a structure that would be as wondrous, imposing, and unforgettable as the huge arch bridge that spans the New River Gorge of West Virginia. As we begin building the foundations, we must ask the likes of Stephen Turner, Andrew
Abbott, and Randall Collins to cease and desist from the practice of placing dynamite charges deep within the escarpments of granite.

(1.1) Turner (1987): The causal conundrum

Turner's article, I believe, is wrong in its basic thesis despite the fact that it makes some very clever points. The central thesis is that theory is “underdetermined” by data, and that statistically-oriented social scientists and our many mentors among professional statisticians do not realize this. I disagree with the argument for several reasons: First, any science progresses insofar as it is able to reduce (not eliminate, as Turner implies) the underdetermined character of theories, and the social sciences have made considerable progress in this regard. Second, the most capable statisticians—in particular Blalock, who is used as a whipping boy by both Turner and Abbott (1988)—are deeply aware of the problem of underdetermination and constantly warn that statistical procedures cannot substitute for other elements of the theoretical enterprise. Third—and most importantly—Turner opens his article by referring to an alleged lack of “progress” in sociological theory. Yet after he arrives at what he considers to be an adequate diagnosis of our problems—the fundamental tension between social theory and statistical methodology—we are left without any suggestion as to what sort of theory would be appropriate for the contemporary social sciences, what sort of theory would be capable of meaningful progress, or what sort of theory would escape the various pitfalls of underdetermination as Turner understands them. Turner may share Jeffrey Alexander's belief that there is little difference between the Department of Sociology and the Department of English; indeed, he may even believe that the various fields of the natural sciences have so much in common, and that the various social science/humanities fields have so many features that distinguish them from the natural sciences, that the universities should reorganize themselves immediately into two large departments: The Department of Natural Sciences and the Department of Natural Languages—the latter phrase is an artificial intelligence usage.

I am pleased that Turner mentions Blalock as one of the major malefactors of the 'sixties and 'seventies, because Blalock's popular undergraduate statistics textbook (1979) provides a perfect illustration of my second claim above. As the 1979 edition of this text was being prepared, McGraw-Hill asked me (with no assurance of or request for anonymity) to evaluate the manuscript; there were about a dozen places where I had a discernable impact on the final text. In several of these instances I merely asked Blalock to illustrate the underdetermination thesis more compellingly. The best example is found on page 470, where it is stated that in a simple three-variable causation problem, there are 64 (4 to the 3rd,
not 3 to the 4th, as the typo says) different causal diagrams that one could propose after making simplifying assumptions about linearity of relationships. Allowing nonlinearity, the possibilities become indefinitely large. But note that Blalock presents only ten of these models on the following page, and then discusses how one might eliminate various possibilities from this already short list.

The remaining 54 models are not simply ignored, as Turner often implies. An attempt is made to eliminate many of them through statistical tests, and many are eliminated through triangulation, i.e., through information obtained outside the framework of a given study. Although Turner does not seem to subscribe to Collins' mistaken conviction (1984:336) that one cannot use statistics to test the appropriateness of statistical procedures, he makes many passing references to these tests without telling us, for instance, that the rules governing the transition from ordinary least squares (OLS) to generalized least squares (GLS) have been among the more impressive accomplishments of mathematical statistics. I presume that it is these rules—the Gauss-Markov principles (Hanushek and Jackson, 1977: Chapter 5)—that Turner has in mind when he says (1987:175) that “with different and equally plausible assumptions, one will derive different estimates.” Or when he alludes (1987:175) to “... the kinds of assumptions needed to make causal inferences ...” Or when he refers (1987:177) to “the problem of the status of assumptions about correlated error terms ...” Or when he says (1987:180) that “... mathematical results, such as multiple correlations, can be fixed only by making additional kinds of assumptions” for which “the values are not arbitrary, but they are determinate only under assumptions for which there are ... valid alternatives.” Or, finally, when he warns us (1987:182) that there is no simple meaning to the ceteris paribus clause, “... for there are many plausible assumptions about 'extraneous variables' which might be made ...”

Turner creates a world of chaos, a blooming, buzzing confusion, a place of darkness without form and void, a place where only the null hypothesis has a right to exist. But in fact the Gauss-Markov postulates and other comparable postulates enable us to eliminate large families of plausible models describing, say, the causal dynamics among a given set of variables. Turner himself shows awareness of the power of this selectivity: In his discussion of the problem of linearity and departures therefrom (1987:176), he concedes that “... classes of alternative hypotheses which could not be readily excluded now can, and it is consequently more difficult to devise plausible counterinterpretations to established findings.”

When one realizes that ways of categorically rejecting families of hypotheses have been invented and improved upon many times in the history of statistics, one also realizes that Turner, in the remark above, is on the verge of abandoning his central argument. When he tells us (1987:176) that “... these [selective] devices increase the cognitive weight of the hypotheses by reducing the weight of other alternatives,” he presages an argument that will be developed below, by Collins: The argument that the null hypothesis is a weighty and meaningful
theoretical statement. Turner soon regresses, however, describing situations in which, allegedly, there is not “... any sort of decisive victory for one model or another” (1987:178); this, despite the fact that many such victories are reported routinely in the research literature. Although the use of statistical controls is a standard means of eliminating large numbers of alternative hypotheses, controlled scores are disparaged by Turner as “fictional” (1987:181). But controlled scores are no more fictional than non-controlled scores. If I say that “my prestige score is 60, and this score is 10 points higher than the average prestige score for those who have the same level of education I have,” why is the 10 less real than the 60? As a matter of fact, since the 10 can be interpreted in relation to an expected (“controlled”) value for those at a given level of education, it strikes me that the 10 is less fictional than the 60. When we see that ideas in science are “fictionalized” (and de-fictionalized) to various degrees, we perceive the error of Agger’s (1989:21) either-or decision to “... abandon the distinction between science and fiction ...” Fictionalization is a variable.

In each of these instances Turner speaks of alternative methods of estimation as if these methods were selected arbitrarily. They are not. One should apply the appropriate rules—e.g., the Gauss-Markov postulates—before one makes a choice. Some choices are better than others—this is known through mathematics, through simulations, and through various kinds of practical experience. On the other hand, it is also known that statistical methods have a surprisingly high degree of robustness, so that even when researchers select the wrong model—say, OLS when they should use GLS corrections—they tend to end up with a model that has a good prospect of revealing something about social realities. Taylor (1987) provides a fine example: Her argument teaches us something about status attainment in socialist nations despite the fact that her dependent variable violates Gauss-Markov. One day, somebody will test her most interesting hypotheses using a dependent variable that does not commit the indiscretion of violating Gauss-Markov.

Aside from the Gauss-Markov postulates there are many other properties, inherent in statistical procedures, that protect us from a fatal underdetermination of theories. For instance, although Turner mentions (1987:178) that measurement error alone may make it impossible for us to avoid a “... large element of 'unexplained' variation in most empirical social science models ....” he neglects to inform us that (1) biased errors of measurement that merely add constants to a series of scores will have no impact on the correlational structure among variables; (2) random errors throughout the measurement process will tend to reduce the level of all correlations among several variables, thereby increasing the likelihood that the surviving model will be our most conservative, low-risk “theory”—the null hypothesis; (3) indices are often constructed in such a way that systematic biases cannot distort them at all, as, for instance, when we define the crude birth rate as total births divided by total population: An underenumeration
These controls and cross checks have always been the state of the arts in statistics, far more than in verbal formulations. Along with the fail-safe devices just enumerated, it should be noted that the null hypothesis, which receives considerable deference from Collins (see below) as a theoretical formulation, is in the final analysis a way of protecting ourselves from producing large numbers of underdetermined theories—underdetermined in the sense of arising solely through type I error. A powerful convention insists that we maintain a low risk of type I error, and suggestions have been made that a similar convention be established for the complementary error, called type II error, in which we fail to discern patterns, structures, or regularities in the social realm that have sufficient importance that they should not be overlooked (Faia, 1966; Cohen, 1982; Moore and Gledhill, 1988).

To his credit, Blalock proceeds in his statistics textbook and elsewhere (Blalock, 1989) as if we were trying to eliminate models that must be falsifiable, not as if we were trying to prove some model beyond a reasonable doubt; his attitude is strictly Popperian. This is a distinction of fundamental importance, and Turner tends to obscure it. I think it is plainly incorrect to assert, as Turner does, that statisticians of Blalock's ilk are out to "prove causation." I could provide any number of illustrations of ways in which good statisticians maintain the appropriate scientific skepticism, seeking to eliminate hypotheses rather than trying to corroborate some preconceived point of view. I doubt that Turner can name a serious sociologist who believes that "... at the end of inquiry ... data would determine a unique theoretical result" (1987:173).

In my experience, few social scientists have taken such a naive view of the nature of theory construction, and it therefore strikes me as a classic loaded question for Turner to ask (1987:173) "why have the ... successes of statistical methods in 'bringing evidence to bear on theoretically important ideas' ... been so modest?" He seems to assume that the frustrating search for a unique theoretical result “... is not ordinarily a practical problem in the natural sciences” (1987:173), but just a few sentences later he refers to Pearson's claim that physical science theories—for instance, the kinetic theory of gases, so important in the development of statistics—are little more than "idealizations," a label that surely implies the same level of underdetermination that is supposed to characterize the social sciences. I hope that the second of these claims is the closer to the truth, because it seems to me that if natural scientists have learned to live with underdetermination, then social scientists should be able to do so.

At several places Turner (1987:173-174,178) refers to scholars who feel that we ought to try to get our coefficients of determination—“explained
variance”—close to 100 per cent, but I do not believe that there are many social scientists who entertain this aspiration or who are terribly frustrated when we fail to attain it. “Saturated models” used in loglinear analysis “explain” all variance, but Halli and Rao (1992:120,128) regard such models as “exploratory” rather than “explanatory,” and suggest “... that any log-linear analysis begin with an examination of the saturated model,” in order to obtain “... a good idea of which interactions are good candidates for deletion in a more parsimonious treatment of the data.” I find myself more likely to be irritated by research workers who seem to be satisfied if their theoretical model explains, say, 15 per cent of the variance in some social phenomenon. On the other hand, a student and I just completed an MA project on fertility in which we account for about 25 per cent of the variance in our dependent variable, and I believe that this work has considerable theoretical import partly because it makes use of elegant multivariate methods. This study provided a classic instance of tension between statistical significance and “substantive” significance, and we opted for the latter. The playing down of statistical significance was the major philosophical outcome of the strident “significance test controversy” of the 'fifties and 'sixties (Morrison and Henkel, 1970), and I believe that this outcome undercuts Turner's basic argument and perhaps Collins' argument also: It teaches us that statistical outcomes should not be taken as absolute, and ordinarily have not been so taken.¹

Incidentally, we happen to have many models in which we do “explain” essentially all the variance (Faia, 1989). Simulation programs, nowadays widely available, provide an excellent example. Elaborate simulation packages such as the Florida Interactive Modeler (FIRM) make possible a clearer understanding of the implications of our hypotheses about, say, the factors that influence various social attitudes. A more compelling example would be Bongaarts and Potter's empirically grounded studies (1983) showing that fertility trends and differentials can be decomposed into their components, with these components being selected from the standard Davis-Blake (1956) “intermediate” determinants of fertility. Bongaarts and Potter are creating very good theory in this instance, and their efforts are informed by an excellent, widely used taxonomy (Davis-Blake) as well as considerable statistical acumen. There are many edifying examples of the use of deterministic models in the social science disciplines, including the determination of size, distribution, and composition of populations by fertility, mortality, migration, and vertical mobility, and the determination of the sex ratio at birth by parity level, the sex composition of children already born, and various methods for implementing boy/girl preferences on the part of parents. In addition, it has been recognized for many years that when we aggregate data on social behavior into rates, ratios, proportions, etc., we maximize our prospects of explaining variance.

Turner (1987:174) worries unduly about the fact that when we add new variables to a regression equation we often fail to explain any substantial amount of additional variance. To me, this alleged defect has its advantages, especially
if the original equation seems to be doing a reasonably good job: What Turner calls “redundancy” is one of the few ways in which nature encourages us to be satisfied with parsimony. Beyond that, it may well be that the best discussions of redundant variables have come from sophisticated methodologists addressing the fact that measures of social mobility, status discrepancy, cohort effects, etc., typically contain no information beyond that contained in their components. Vast amounts of verbal speculation have been devastated by the simple algebraic demonstration of this redundancy, or by the fact that our computers immediately send out error messages when we try to tell them, for instance, that prejudice may be influenced by parental socioeconomic status (SES), filial SES, and by social mobility defined as the difference between filial and parental SES. None of the three causal variables, taken alone, contains any information not contained in the other two.

iii

Turner (1987:177,180-82) cites an earlier paper by himself and Wilcox (1974) that deals with the idea of axiomatic theory as developed by Costner and Leik (1964), Zetterberg (1965), and others. In essence, the argument of these theories is that if A is related to B, and B to C, we can deduce a relationship between A and C. This is proposed as a straightforward use of the transitivity assumption or the pure conditional argument, but it must be taken with the proviso that any qualifications that appear in the premises of a syllogism must be retained in the conclusion. In other words, if the premises involve probable relationships or weak relationships, then the conclusion must retain these qualifications. In a study involving data from Textor (1967), to be summarized in a moment, I found that this argument, the transitivity argument, has considerable utility. But Turner's basic claim (1987:180) is that there is no logical way of testing the propositions of axiomatic theories by means of correlation and regression analysis.

The rationale is found in Turner and Wilcox (1974), a fascinating paper that provides an opportunity for me to demonstrate the essence of my argument against Turner. In presenting their “A Calculus,” as opposed to more complex B and C calculi, they enumerate (in their own notation) several syllogisms (1974:574) that represent Zetterberg-type arguments. One of these arguments, for instance, may be translated as follows:

If x is positively related to y, and y is positively related to w, then x is positively related to w.

They then point out that, because the qualifications mentioned above are not made explicit, this argument does not necessarily lead to a true conclusion. If, however, we allow the syllogism to take the form of the modus tollens, a form in
which the _negation_ of a conclusion leads us to deny at least one of the premises thought to generate it, then we create a situation in which premises are subject to being eliminated, to being falsified. The test involves the valid argument known as denying the consequent rather than the invalid argument of asserting the consequent. If, in the Turner-Wilcox example, we find through observation that \( x \) is _not_ positively correlated with \( w \), we reject the premise that \( x \) is related to \( y \) and the premise that \( y \) is related to \( w \). The fact that we do not retain any qualifications—that is, we do not acknowledge that it is logically possible for these premises to be true despite the fact that the conclusion is false—is non-problematical: This is a highly conservative decision rule, comparable to our having a strong bias toward the null hypothesis.

A work by Textor (1967) provides many opportunities to construct syllogisms of the form described above. Based on the Human Relations Area Files and written primarily by computer, this large tome defines nearly 500 variables ("finished characteristics" or FC's), classifies 400 societies according to each of the variables for which ethnographic data have been provided, and then presents a computer printout that shows how the variables are interrelated. The format is simple: Each variable is a dichotomy, and all tables are bivariate two-by-two crosstabulations. Below each table is a chi-square value or a Fisher exact test, a phi coefficient provided as a measure of strength of relationship, and the level of significance. A simple, computer-written paragraph describes the relationship. In order to appear in the printout, tables had to attain significance at the .10 level or better.

Under my sampling protocol, I ended up with 155 syllogisms that had the Zetterberg-Turner-Wilcox form. For example:

\[
\begin{align*}
44 - 36 & \quad (\text{phi} = -.315) \\
36 - 51 & \quad (\text{phi} = -.115) \\
44 + 51 & \quad (\text{phi} = .537)
\end{align*}
\]

The code numbers 44, 36, and 51 are Textor’s identifiers for a set of fixed characteristics, the signs between the FC's indicate whether a relationship is positive or negative, and the line segment should be read as "therefore.” Using the slightly artificial language of Textor's computer-written propositions, in which verbs such as “tend” or “tilt” reflect the level of statistical significance, the syllogism would be described as follows:

First premise: Cultures where settlements are fixed tend more to be those where the natural environment is other than “very harsh,” or sub-tropical bush, or temperate grassland.
Second premise: Cultures where the natural environment is other than “very harsh,” or sub-tropical bush, or temperate grassland tilt less toward being those where subsistence is primarily by food production—i.e., agriculture or husbandry—rather than by gathering.

Therefore, cultures where settlements are fixed tend to be those where subsistence is primarily by food production—i.e., agriculture or husbandry—rather than by gathering.

Already, then, we have a nicely structured theory linking three ecological features; our next task would be to search for the “intervening variables” that encouraged early agriculturalists to remain sedentary (Lenski, Lenski, and Nolan, 1991:158-60). The syllogism representing the theory contains a conclusion with a phi coefficient that is both high and in the anticipated direction, and we therefore conclude that the premises have not been falsified by the many field studies that contributed to the Textor dataset.

Among the 155 syllogisms constructed from Textor's data, 117 or 75 per cent produced truthful conclusions. The 38 negative instances occurred either because conclusions were false or because the number of societies classified on both of the variables contained in conclusions fell below 100, which I used as an arbitrary minimal N. What, then, is the probability of producing a truthful conclusion when at least one premise is false? Fortunately Textor (1967:54-60) answers this question by providing the cleverly conceived “whiskers” test. The whiskers FC's are ten random dichotomous variables that assign whiskers of various colors to the men of varying numbers of societies. As random variates, these FC's should be related to other FC's only by chance, by the occurrence of type I error. Because there are ten whiskers FC’s, and because each of these variables could potentially be related to about 250 other FC's used as predicates, there are potentially about 2500 tables to be printed out for the whiskers variables. However, only 126 significant relationships actually appeared, about 5 per cent of the potential number. Therefore, the likelihood of deriving a truthful deduction from demonstrably false premises is probably less than .05. These findings seem to vindicate the axiomatic format as a way of generating theories, and to demonstrate that the falsifiability or “bootstrap” criterion (Glymour, 1980:291-321) is an appropriate strategy for testing this type of theory.

The falsifiability criterion is widely applicable, and it is not limited to the type of generalization developed by Turner, Wilcox, and Zetterberg. I shall argue in a later chapter that in developing and testing theories it would be advantageous to make use of taxonomies with hierarchic properties (Faia, 1991): These hierarchies provide additional opportunities for falsification, as we shall see.
I cannot agree with Turner's recurring suggestion that the more highly developed sciences, due to the nature of their subject matter, are free of the ambiguities of statistical interpretation with which social scientists must struggle. For instance, geology and its subdivisions such as seismology and vulcanology, because of their deep dependence on field observations, can hardly avoid a serious risk of underdetermination of theories. When animal ecologists use ANOVA and related multivariate designs to test the notion that rapid, “explosive” evolutionary changes may be brought about by positive feedback—i.e., reciprocal causation—among (1) sexual ornamentation, (2) mate selection favoring the genes that make for ornamentation, and (3) mate selection favoring the genes for preferring certain kinds of ornamentation (Dawkins, 1986: Chapter 8), we encounter the full range of causal dilemmas that, following Turner, one might consider unique to the social sciences. In this instance, then, social scientists are not at a disadvantage. And on a related matter, animal ecologists have little prospect of obtaining random samples that approach the quality of those designed by, say, the U.S. Census Bureau for the current population surveys (CPS). Nor do animals and plants seem to encourage the emic option, always available to social scientists.

In a personal letter, Turner refers to the difficulty we have in convincing “students or anyone else outside sociology that sociology has anything to say.” I find this claim bothersome because, although I believe that we have an etic discipline that should not devote excessive attention to the gathering of emic data, I nevertheless realize that we must impress a broader audience than just ourselves. Physicists couldn't care less whether the public at large reads Physical Review Letters, but they have all sorts of Carl Sagans, Freeman Dysons, and Steven Hawkingins in the field busily translating findings for popular consumption, in ways that are most impressive. I believe that we in the social sciences must do this sort of translation, or we may not survive. It is encouraging that Footnotes, the newsletter of the American Sociological Association, now shows a little deference to sociologists who make contributions to the mass media, but this effort will hardly suffice. We should do infinitely more.

(1.2) Abbott (1988): The transcendence of triangulation

Abbott has a way of pulling punches. His concessions are so large and so frequent that one cannot help but doubt his central arguments. He says, for instance (1988:170), that it is commonly assumed by statistical workers that causal relationships do not vary by social context, but he is quick to point out that “there are, of course, ways of relaxing some of these assumptions.” Indeed there are! For many years the literature on contextual analysis has been large enough to support introductory textbooks (Boyd and Iversen, 1979; Iversen, 1991). I
suspect that researchers who understand contextual analysis in the Boyd-Iversen sense are better prepared than most of us to do the sorts of causal analysis suggested by Abbott. Again, while the basic intention of Abbott's article is to persuade the reader that the general linear model (GLM), along with the corresponding delusional system that he calls “general linear reality” (GLR), have achieved few if any successes, he says early on (1988:171) that he intends no “derogation” of the “very great successes” of the GLM. In truth, Abbott's article is highly derogatory in that it says next to nothing about ways in which the more subtle uses of the GLM—those suggested by Boyd and Iversen, for instance—might help us to close (or avoid) the “growing chasm” that constitutes the opening allegation. To Abbott, the GLM has few if any redeeming virtues. It is most extraordinary that he believes that his aim in making these arguments “... is not controversial” (1988:183).

I cannot, for instance, accept the suggestion (1988:175) that the GLM somehow precludes our hypothesizing that a given causal agent may have multiple effects. I do not believe (1988:173) that those who apply the GLM are so committed to “monotonic causal flow” that they automatically acquiesce in the notion that “a given cause is equally relevant at all times ...”; I've never encountered a sociologist this naive. I cannot agree (1988:173) that the GLM assumes that “cause can never flow from small to large, from the arbitrary [?] to the general, from the minor event to the major development.” I do not recall (1988:179) having read anything by or about Max Weber that suggests that worries about multicollinearity among variables will preclude our appreciating Weber's elective affinities or his ideal types, or will make it difficult for us to address the issue of “elitists against pluralists.” Finally, I cannot accept the argument (1988:172) that those who study a profession such as accounting by means of the GLM do not realize that, over the last several decades, many such occupations have been “reshaped by technology,” by competition, or by other factors.

On the question of multiple effects, all one must do to undermine Abbott's argument is to locate a few classic path diagrams—the “weapon on the wall” of the GLM—and show that these diagrams have at least a few causal factors that have more than a single arrow flowing from them to some effect. Take, for instance, an influential paper by Duncan (1966) that introduces the basic logic of path analysis. Duncan's Figure 1, a path diagram, proposes that social background may influence ambition, class values, and intelligence; that ambition, in turn, may influence class values and intelligence; and that the socio-economic rating of schools may influence ambition, class values, and intelligence. Figure 2(b) and Figure 4 present the same sorts of causal hypotheses, replete with multiple outcomes. Similarly, Alwin and Hauser (1975:38) present a causal diagram in
which a man's education is thought to influence both his occupational prestige and his income—a multiple-effects hypothesis that has been tested scores of times in the status-attainment literature. The major purpose of the Alwin-Hauser article, in fact, is to explain how to separate direct effects, indirect effects, spuriousness, and other distortions in complex causal processes in which there are many variables with multiple effects or "sequence effects" (Abbott, 1988:177). In brief, then, those who construct analyses based on the GLM (and, presumably, its associated GLR) do contemplate the possibility that a given causal agent may have a multiplicity of subtle effects. Users of the GLM/GLR occasionally may miss these subtleties, but if they spend as much time worrying about making proper "specifications" (Boyd and Iversen, 1979: Chapter 2) as they do about multicollinearity, they may have a better eye for subtleties than those who do not have the benefit of the GLM/GLR worldview. The presumption that this worldview creates a general lack of insight must give way, at least for the moment, to the null hypothesis.

Perhaps, however, Abbott is addressing another type of multiple-outcomes situation. I recently came across fascinating findings (James, 1987a, 1987b) regarding social class variations in the sex ratio of children at birth. At present these variations are small, but at some future time they may become much larger due to the introduction of cheap, safe, and effective methods of influencing the sex of embryos prior to conception. If there is a strong preference for male offspring within, say, the lower strata of a given society (Williamson, 1976:54-65), one would expect that the introduction of sex-preselection technology would tend to raise the sex ratio at birth (SRB) within these strata, thereby creating an inverse relationship between SES and the SRB. On the other hand, if there is a tendency for technological innovations to be adopted earliest within the higher socioeconomic strata, and assuming that the higher strata have at least a moderate preference for boys, we would anticipate that for some time this same technology would tend to raise the SRB within the higher strata. In one instance, then, the SES → SRB causal arrow would seem to carry a negative sign; in the other instance, a positive sign. How do we handle this kind of situation?

For one thing, it is clear again that those who have a commitment to the use of the GLM are not at all handicapped in approaching this type of problem. In the case at hand, the typical procedure would be to find an index of sex preference and an index of willingness to adopt innovations, declare these indexes—in classic Lazarsfeldian style—to be intervening variables between SES and the SRB, and then decompose the SES-to-SRB relationship into these two elements. I do not see how this kind of problem could be handled any more deftly by those who insulate themselves against the GLM, by those theorists who for inexplicable reasons “may reject” (Abbott, 1988:176) an emphasis on both antecedent and intervening variables. Abbott would probably argue, as we shall see, that it is important to tell detailed idiographic “stories” about how particular couples would decide the sex of a child, how they would decide about whether
to use fertility-related innovations, and how they would decide several other such questions. I agree strongly. But once again I fail to see how a commitment to the GLM/GLR worldview precludes an interest in these kinds of stories or undermines our ability to tell them well. At one point (1988:177), in fact, Abbott credits GLM practitioners with telling a good causal story based on a proposed path diagram; clearly, these stories are a wonderful source of new specifications. Ward's (1986) book on the American railroads presents a fascinating collection of such stories, which we examine in a later chapter.

There is a generic multiple-outcomes story that occurs in several academic fields, including sociology, economics, and psychology. This story assumes that various kinds of needs, problems, or disequilibria may be so compelling that there are many different means of satisfying them, and that the denial of one set of means will usually increase the likelihood that an alternative set of means will appear. The title of this story is “substitutability,” and its themes have been captured in many renditions by GLM practitioners and by others. In abnormal psychology, for instance, it is often assumed that one set of psychiatric symptoms, or a given adjustment technique (direct or indirect aggression, identification, projection, compensation, etc.), may be substituted for another. In economics, one might entertain the hypothesis that, in a developing nation such as Mexico, meat will be substituted for other foods when the opportunity arises, or that poultry will be substituted for beef among well educated consumers (Contreras and Cifuentes, 1987: Table 5). In sociology, one might suggest that the denial of access to one type of weapon will create a strong demand for other types of weapons (Wright et al., 1983). In all these instances we are in the presence of multiple outcomes, with “ends” presumed to give rise to “means” not through teleology but through causation. And because it is assumed that these multiple means, whether substituted for one another or not, may have an impact on the need, problem, or disequilibrium that generated them in the original instance, we are also in the presence of functionalist hypotheses. The GLM, of course, would be applicable to all these examples.

Abbott argues that the GLM makes it difficult for social scientists to evaluate a given causal relationship within several contexts, or to entertain the possibility that causation may flow from “small” events to “large” events. But one cannot reconcile these claims with Boyd and Iversen's demonstration (1979) that the GLM helps us to explain variations in individual behavior from one structural context to another, or their demonstration that we may also reverse this process by “explaining aggregate relationships in terms of individual behavior” (Boyd and Iversen, 1979: Chapter 2, part III). Once again, I do not see anything about the GLM that reduces the likelihood of our perceiving these opportunities, and in fact it appears to me that careful study of the Boyd-Iversen texts (and related
literature) would enable us to exploit the GLM effectively to facilitate the perception of important contextual effects or important causal flows from the microsocial level to the macrosocial level. Statistical models, in other words, help us to have these important theoretical perceptions. 

Boyd and Iversen, then, make it clear that causation may flow from small causes to large effects. One easily finds scores of illustrations, and one could readily invoke the GLM to study most of them. When tens of millions of individuals release small amounts of chlorofluorocarbons (CFC's) into the atmosphere, these are small events that may have large effects on global ecology (Schneider and Londer, 1984:197). When President Ford made a “misstatement” about Poland during one of his debates with Jimmy Carter, this was a small cause that had a large impact. The meetings between Carter, Begin, and Sadat at Camp David were a small event that had large consequences (Carter, 1982:319-403), and the same may be said for the meetings between Truman and MacArthur during the Korean war (Spanier, 1965). Michel Foucault had little enthusiasm for the idea that Martin Luther's “an al retentiveness” explains the Reformation (Cousins and Hussain, 1984:64), but Isaac Asimov's brand of psychohistory (Kanfer, 1988:82) has been taken seriously enough by scholars that there is at least one academic journal devoted to the Great Man and the Not-So-Great Man theory of leadership and social change; surely, the psychohistorical orientation involves small causes and large effects. Social scientists interested in what Abbott calls “elitists against pluralists” generally regard elite networks as a servomechanism in which small amounts of energy are used to control large amounts of energy—this, to me, is a good definition of elitist power. Large, continent-wide systems of production, such as Anheuser-Busch breweries, are controlled by just such servomechanistic elites. Finally, although “catastrophe” (or “chaos”) theorists tend to focus on large events, even they may concede that “thou canst not stir a flower without troubling of a star” (Woodcock and Davis, 1978:67).

In essence, Abbott accuses GLM devotees of having a bias against reductionism, i.e., against the strategy of explaining large phenomena with reference to small phenomena. On the contrary, it may well be that our established predilections have made us rely too heavily on reductionism, and that therefore any tendency for the GLM to sustain the opposite bias may serve as a necessary corrective. Again, however, I see nothing inherent in the GLM that would preclude our testing reductionist models adequately, should we choose to do so. Small insects and small rodents, for instance, do not look like major historical actors capable of troubling stars, but Zinsser (1935) argues that these tiny vectors indeed may have a stupendous impact, influencing the course of important events such as wars. Or, consider AIDS. Studies of the micro-interaction involved in perpetuating the AIDS epidemic (a macrosocial event) clearly involve small causes combined with large effects. I suspect that the GLM is the modal method in studies currently underway in this area.
The essential problem with Abbott, however, has little to do with causation: The problem is that he has a philosophical commitment to idiography, to what he calls storytelling. He develops a pair of stories about Kenya and Nigeria (1988:181), and quickly brings us the distressing news that it is virtually impossible to generalize about the developmental dynamics of these two nations. All we can do in attempting to understand social development across Africa is to tell “... thirty particular stories” in detail. I certainly do agree with Abbott that if we can do nothing more than tell these thirty stories, if we cannot generalize about Kenya and Nigeria and other nations with comparable characteristics, then indeed we do not have much of a prospect of creating a science about social development or about any other aspect of social systems. However, I do not agree with Abbott's premise. I don't think he agrees with it either, in the sense that he could probably not tell Kenya/Nigeria stories for any length of time without proposing some sort of generalization.

I don't believe that one can tell detailed sociological stories without generalizing, any more than one can think randomly: My students are forever confident that they can readily conjure up a set of random numbers from their heads, and they invariably lose (i.e., they fail to win) a good three-dollar cigar when I show them that their proposed numbers depart significantly from randomness. (Once on a fluke I lost the best, and I had to suffer the indignity of trying to cut their cigar into 34 segments of random size, using a straight-edge.) Abbott would have a chance to get his storytelling program under way by one means only: He would have to devote himself to poetry. He would have to thrust himself into the type of idiographic exploration defined by Jacques Barzun in his critique of modern science (1964:198): Law and poetry, says Barzun, are highly exact because they are highly idiographic, laying out a particular case in the finest detail. Science, in contrast, is very inexact because it is extremely abstract much of the time. The major trouble with science, says Barzun (1964:202), is that the scientist often cannot see the trees for the forest. A scientist with this affliction, he continues, is likely to try something like scientizing Tennyson (1964:129). Or simonizing her grandma.

Incidentally, although I do not propose to get into a detailed argument with Barzun, my instincts tell me that poets and legal scholars, despite their passion for idiographies, do not work for long without making generalizations. Nor did Michel Foucault, despite his insisting to his students that “general psychology, like anything general, does not exist” (Eribon, 1991:139.)

Mass murderers are fascinating, and Newton's Mass Murder (1988) is filled with the details of their hideous lives; but to a sociologist the best part of the book is the place where Newton generalizes about research into the kinds of social conditions that create these lives (1988:3-21). Again, college novels tell fascinating stories (Kissiah, 1969); but the most interesting features of these
stories have to do with their ability to say what is right and what is wrong with higher education in the United States. The apparent suicide of Marilyn Monroe makes a gripping story; but far more interesting than Monroe's life and career is the prospect that mass media reports of her suicide may have inspired imitative suicides—think of Tarde's and Le Bon's principles of crowd contagion—across the nation, especially in places where media coverage was highly detailed (Phillips, 1979). LeMasters' (1975) intensive account of the social life of a working-class tavern is a fascinating case study; yet, virtually every chapter of this work begins with a set of generalizations drawn from the research literature, and the work is enriched by the way it deepens our understanding of these generalizations. In each of these instances the stories are a means of arriving at better generalizations. The stories are essential, they may be excellent poetry or excellent legal briefs or both, but they alone cannot constitute a social science. At best, they lend themselves to Abell's method of comparative narratives. And in this case, best can mean very, very good.

In large measure Abbott criticizes the GLM unfairly, by invoking the law of plenitude. As the philosophical basis of scientific skepticism, this law states that the subtlety and complexity of nature are infinite, and that therefore nature tends to flamboozle scientists most of the time. But the law of plenitude applies to every scientific method in the same way that it applies to the GLM. Therefore, showing that the GLM is subject to the law of plenitude is not a critique of the GLM; rather, it is a simple reaffirmation of the principle of plenitude. When Abbott (1988:170) tells us that we cannot trust our momentary, untested intuitions about “causal time,” we must immediately concede the point and seek alternative methods that would allow us to approach issues of causal lags and periodicities more effectively. When we use several approaches to the same question we take advantage of Percy Bridgman's admonition about science as a process of “doing one's damnedest with one's mind, no holds barred.” Thus, Phillips (1979) not only uses empirical data and the GLM to test the notion that imitative suicides are induced over a matter of days, or hours; he also invokes the theories of Gabriel Tarde in order to strengthen the argument that, if one does not have daily rates for these kinds of suicides, one probably cannot conduct the study at all.

Gottman (1981), in an excellent treatise on time-series analysis, tells us that unless we experiment with different assumptions about the timing of cause-effect relationships, we risk arriving at conclusions that are precisely the opposite of the truth. His best illustration has to do with the way wagon wheels in motion pictures often appear to be rotating in reverse: The “methodology”—in this instance a camera with a particular shutter speed—relates to the rotation of the wagon wheel in a way that grossly misleads us. But all we have to do in order to
overcome this distortion is to run a series of experiments, using variable shutter speeds. When we combine intuition, theories, multiple observations, data reduction, and instrument experiments, we are making use of triangulation. We are also conducting an analysis—albeit a statistical analysis—to which Agger (1989:90) contributes precisely nothing with the remark that the trouble with suicide research is that it accepts the Durkheimian presupposition “... of a world that makes people want to kill themselves.” Phillips shows us that, on some happy days, hardly anybody wishes to kill himself.

Triangulation, in this sense, is essential for a seismologist trying to locate the epicenter of an earthquake, and it is a matter of routine for a good biologist. Dawkins' treatise on Darwinism (1986:99-100) provides illustrations as compelling as those of Phillips and Gottman. He asks, for instance, why it is that some types of cicadas have a lengthy 13-year cycle, while other types have an even longer 17-year cycle. His tentative hypothesis: These two numbers are primes, not divisible by any number other than unity and themselves; therefore, predators are not likely to have cycles that correspond to cicada rhythms, and cicada periodicity may serve to protect this organism from potential predators. Using a similar theory-based triangulation, a social scientist might propose that Presidential cycles of four years, combined with Congressional cycles of two years, create a situation in which Congressmen are slightly more likely to be beaten than cicadas are to be eaten (Tufte, 1974).

It turns out, however, that not all social scientists share my admiration for Dawkins' clever use of numbers, prime or otherwise.

(1.3) Collins (1984): *The contours of quantiphobia*

As far as most students are concerned, mathematics was invented to complicate, rather than facilitate, the study of science. ... But in fact, the primary function of mathematics is not computation; it is discovery. Mathematics is the only language by which statements about nature can be made in symbolic form and then combined according to logical rules to lead to new knowledge of the universe. ... Avoiding mathematics, which seems to be the principal attraction offered by some [social?] science courses these days, may be a way of drawing students; but by ignoring the theoretical structure of science, this strategy conveys a false impression of the overall scientific enterprise.

—Morris Shamos (1990:92)

Collins (1985:41) makes critical comments about Adolphe Quetelet, a scholar of renown and of central importance in the history of social statistics (Porter, 1986). He claims that because Quetelet's statistical laws involve no more than “a few simple probabilities” and a reasonable prospect of predicting “rates
of population change or crime," interest in Quetelet soon "fell away as the statistics failed to live up to the claims ..." (1985:41). This alleged failure has never been demonstrated, and the claim is surely vitiated by the fact that statistics, a relatively new academic discipline, has had immense successes over the last century or so. Furthermore I suspect that this claim, because it contains an obvious contradiction, never will be demonstrated: Nothing is more delightful for a scientist than the discovery of laws that can be stated in “simple” (i.e., parsimonious) ways, or the discovery that an event can be predicted (and perhaps explained) through the application of such laws. A good scientist pursues parsimony as resolutely as she respects plenitude.

An equally untenable stand—one that goes far beyond his relatively harmless remarks about Quetelet—is taken by Collins (1984) in a paper revealingly entitled “Statistics Versus Words.” Although I do not agree with most of the arguments of this paper, it makes several excellent points: First, that the null hypothesis—the idea that many social processes occur by chance, or seem to occur by chance because we do not understand them, or both—should be treated as a theory per se. The null hypothesis as theory would imply, for instance, that Wall Street is the world's foremost gambling establishment in the sense that the factors that determine outcomes seem to elude everybody except (I gather) Ivan Boesky, Michael Milken, and their ilk. Susan Strange's *Casino Capitalism* (1986) is an excellent introduction to this topic, and in reading this book one has a clear sense that we have learned little about global financial systems, beyond the extent of our ignorance. Second, Collins argues that there are ways of testing (or formulating) theories that do not involve the gathering of statistics; the periodic table of the elements, in Collins' example, was arrived at through "induction." Third, he points out that it is difficult to study small numbers of large organizations as compared to large numbers of small organizations, with the result that historical sociology, often dealing with small numbers of whole societies, is at risk of being idiographic.

Collins' ideas about the null hypothesis deserve far more attention than they have received thus far. First of all, it is clear that if the null hypothesis is actually an important theoretical formulation, then Collins' larger arguments about quantitative methods are undercut by the fact that it is difficult to make decisions about null hypotheses without understanding statistics. But Collins' argument is strengthened when one realizes that the null hypothesis, as theory, could more accurately be called the “debunking” hypothesis. As understood by Berger (1963:38-43), debunking might involve the demonstration that if the protestant ethic gives rise to the spirit of capitalism, this result is not necessarily intended (cf. Thompson, Ellis, and Wildavsky, 1988: Weber section, p. 19). In general, members of society do not anticipate future events accurately, so that when they
try to explain their behavior with reference to some plausible cause-effect scenario, they are usually just winging it. They do not really understand as much as they think they do about cause-effect relationships, and it is the task of the null hypothesis—that is, the debunking process—to make this limitation clear. When the null hypothesis is a prominent theoretical construct, it becomes difficult for us to believe our own propaganda; Berger suggests (1963:41,42) that sociology flourishes when accepted interpretations of society become “shaky.” When Sica (1988) urges us not to lose sight of the irrational aspects of human behavior, I believe that he is arguing essentially for the debunking function.

Wildavsky (1988), for example, argues that the process of searching for safety needs to be debunked. He believes that we try too hard to conduct our lives on a basis of “trial without error,” and that this attitude so diminishes our risk-taking aptitudes that it actually becomes maladaptive, keeping us from discovering better (and safer) ways of doing things. Wildavsky believes that the usual justification for trial without error is the unthinking conviction that we have such complete a priori knowledge of what is safe and what isn't, what works and what doesn't, that we need not attempt serious experimentation. He continues (1988:57):

When I said that “without trial there can be no new errors,” I should have emphasized the word “new.” For, in regard to such matters as curing a disease we do not know how to cure, ... we are already making mistakes. Everything we do not yet know how to do could ... be called error; so, to be denied trials means that we forgo the opportunity of reducing existing defects. ... methods to limit or adjust to danger can be developed only through experimentation.

But most of the time, he fears, our reliance on conventional wisdom is so strong and uncritical that we do not correctly anticipate either costs or benefits. Therefore, the appropriate thing to do is to admit that (1) the null hypothesis prevails in most instances and (2) we discover opportunities for rejecting the null hypothesis, for learning new ways of doing things, only through serious forms of investigation involving trial and error. And it should be noted that many of the experiments he cites, such as those involving the Salk and Sabine vaccines against polio (1988:55), make use of the general linear model and other statistical paraphernalia in epidemiological and human-ecological studies, and that the attitude of “statistics versus words” would be a serious impediment to research that treats the null hypothesis as a major theoretical formulation.

In contrast to the incisive discussion of the null hypothesis, Collins gets into deep trouble in other parts of his article. He claims that “... statisticians are the
worst offenders in imposing a narrowly positivist orthodoxy on the field” (1984:330), but deigns not to offer a trace of evidence. The example of William H. Sewell (Collins, 1986:1341) does not convince us any more than does the unsubstantiated claim by Agger (1989:29) that the science's have an “... imperial urge to control the entire cognitive map, to banish the nonquantitative ...” And although Lazarsfeld is often numbered among the worst offenders, he seems to be vindicated by his admission that “I always believed in the interdependence of quantitative and qualitative work ...” (1972b:xvii); he admits, however, that he tended to drift toward the quantitative. In any case, it is hard to think of Lazarsfeld as a major malefactor, imposing a rancorous orthodoxy on students and colleagues alike.

Historical sociology, says Collins, “… can proceed best without inroads from the statistical side” (1984:331)—a finding that will surely come as a shock to the average cliometrician, if one may speak of the average. When he writes that statistical procedures create a bias toward “... our theory, the one that is being tested” as opposed to “chance” (1984:331), Collins is simply wrong: In fact statistical procedures, based on a “minimax” strategy that tries to hold down the magnitude of our biggest mistakes, create a strong bias in favor of the null hypothesis, in favor of chance as an explanation. When the null hypothesis itself is disparaged as “empty” (1984:335), the preceding three pages of text—the best part of the article, as I've already tried to show—are contradicted as we lose sight of the need for debunking, that is, for protecting ourselves against our dangerous tendency toward Kantian overinterpretation of social realities, for protecting ourselves against the opportunity costs that so worried Wildavsky.

In asserting that “there is no way to test a statistical model statistically; one simply demonstrates that the pattern of data is consonant with it” (1984:336), Collins leads one to suspect that (1) he does not clearly understand the reciprocal and symmetrical relationship between statistical models and data, the ways in which, to return to an earlier example, a model such as ANOVA may relate to theories such as natural selection; and (2) he hasn't given serious attention to Monte Carlo simulations, which literally do test statistical models statistically and show us, happily, that these models have considerable “robustness” (Hanushek and Jackson, 1977:77).

Robustness is a term that critics of quantitative sociology might well ponder: It means that even when the real world—that is, the plenitude of nature—forces us to violate the basic mathematical assumptions of statistical models, the models still give us reasonably accurate approximations of what is going on.

Collins claims wrongly that social mobility studies have ignored mobility that is “forced” by changes in the occupational structure (1984:346,348)—note that Abbott (1988:179-80) discusses this distinction in some detail. He then tells us 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” (1984:357), and brings us to the consummating clincher of the argument
(1984:350-53): that his perusal of assorted mathematics journals shows that mathematicians, after all, speak and write English or French or German or Swahili or some other natural language, even while doing mathematics. This finding is supposed to tell us something about the prospects of quantification in the social sciences—but, alas, the argument is a non-sequitur: Surely one may concede that natural languages will continue to have a large role in social science analysis and in mathematical analysis, without also conceding that the role of mathematics must therefore be cut back in the social sciences.

In any case, criticizing statistics by citing the “worst offenders” is a little like criticizing written English—the “words” approach—by citing the most horrendous undergraduate bluebooks.

Finally, when Collins says that “the work of Erving Goffman, for example, ... would not have been improved by carrying it out under a rigid program of statistical measurement and hypothesis testing” (1984:340) and that “... someone following such methods would not have been able to do it at all,” one must reply that the work of Christopher Jencks or James S. Coleman or William H. Sewell or Fogel and Engerman, for example, would not have been improved by carrying it out under a rigid program of quantiphobia, and that a quantiphobe would not have been able to do the work at all. Again, Collins' argument is a non-sequitur, proving nothing. Actually, I believe that a good statistician has a better prospect of becoming a good Goffmanian than a quantiphobe has of becoming a good cliometrician. But the anticipated failure by either party would make our fundamental problem clear.

The fundamental problem is clarified further by Mackie (1988:4-5,10-13). If, as she says, the quantitative, “agentic,” “controlled reality” style of sociology prevails among male sociologists while qualitative, “communal” approaches are more likely among female sociologists, and if it is deemed essential that the discipline move away from its allegedly male-dominated theories, methods, and social structures, we are at risk of producing a self-fulfilling prophecy: Those who wish to repudiate traditions of male dominance at a structural level by creating, for instance, a more just distribution of career opportunities and rewards in the academic world, and who also repudiate particular theories and methods thought to be associated with maleness and male dominance, may ultimately strengthen whatever small association currently exists between theories, methods, and the sex of scholars. As a consequence, increasing numbers of both male and female scholars will make the sound of one hand clapping.

In sum: What we must all strive for is an androgynous methodology.

(2) Solicitous gatekeepers, #2

Without comment:

...
Editor,  
*Sociological Theory* 

Dear ...:

Your recent rejection of my article entitled “Three papers in search ...” is the most poorly argued editorial decision I have ever encountered in my twenty-five years as an author—and there has been a lot of competition for this title. The most glaring weaknesses of your review are the following:

(1) ...

(2) One of the referees points out to you that “concerning the commentary (‘Three papers ...’), your indication (in your memo to its reviewers) [was] that your practice has been to publish commentaries without review ...” When you decided to single out my paper as one that was uniquely subject to rejection, you should have been especially careful in the selection of referees. The referee just quoted sets the tone for the entire enterprise when he/she continues that “... readers of *Sociological Theory* will not be competent to judge merits of [this] critique or response of Turner or Abbott.” Incredibly, both this referee and, especially, the second referee seem to believe that the stimulating articles by Turner, Abbott, and Collins should not have been published in the first place. I've always been a strong advocate of practices such as retroactive birth control, but it is logistically impossible to cause articles already published to evaporate. They remain out there, and they should be treated as fair game by any journal that wishes to be considered competent. (I won't pursue the first referee's misgivings about the competence of ST readers.)

Indeed, I fall back in disbelief when the second referee asserts that he (or perhaps she) is “... in a bit of a quandry” [sic] largely because “I'm not sure why one should bother. This probably reflects a puzzlement about why the source pieces made it into print to begin with.” What this amounts to is that my paper is being rejected largely because it is a commentary on three articles that your journal is now apparently disclaiming *sub rosa*. In the recent “Nova” (PBS) program, “Do scientists cheat?” an argument is made for the ethical norm that whenever the editor of a scientific journal believes that he has published an incompetent, misleading, or fraudulent piece of work—or three such pieces—it is incumbent on the editor to provide the essential rebuttals
and correctives. To fail to do so, per se, borders on scientific fraud. I believe that, as an ASA-sponsored journal, ST has this responsibility.

(3) In part, you reject my paper because “... it leans too technically statistical for this journal.” In its fifty-one manuscript pages, my article greatly exceeds the Collins criterion by containing not a single number, with the exception of page references and the frightening triad 44, 36, and 51, numbers used by Robert Textor solely as labels for certain cross-cultural variables. Unless there is by now a ... corollary to the Collins doctrine—viz., that it is also inappropriate to use English in discussing statistics—I find it difficult to understand how one criticizes articles entitled “Transcending general linear reality,” “Statistics versus words,” and “Underdetermination and the promise of statistical sociology,” without discussing statistics, at least in English. How does one debate the General Linear Model without giving some attention to the General Linear Model?

Incidentally, your second referee suggests that I should develop a new paper on “a statistical analysis of the demographics of semiotics,” and I propose to do so in the language of Borland International Turbo Prolog, which is relatively non-quantitative and non-English.

(4) At various places your referees label me or my article as “pretentious,” “unfocussed,” “gratuitous,” and “egocentric.” If we have learned anything about the process of social labelling, it is that labels are often applied with little justification, are often a substitute for thought, and that they work their damage in ways that are ipso facto unjust. In the present instance, the referees attempt no justification at all. I suspect that a mighty grease-pen slash across an entire manuscript page is nothing more than a way of slaying the egocentric dragon who would have the temerity to write this sort of letter and stand behind it. (Although I concede that the “egocentric” label was not used by The Slasher.)

I find it most encouraging when you say that “I ... hope you can recycle this material in some way. I think it would be good if it could be published in some form.” If and when it is published, it will have an excellent self-illustrating feature: Your editorial style, at least in the present instance, shows perfectly the consequences of abandoning the norms of scientific inquiry and conduct. It will therefore be incorporated into the revised version of my article ...

Regards,
WHAT'S WRONG WITH THE SOCIAL SCIENCES?

...
NOTES (Chapter 4)

(1) Recently Turner and Turner (1990:114-18,131-32) have claimed that the “... enormous and obscure ...” literature of the 'fifties overlooked the fact that null hypotheses were “... usually known to be literally false or highly improbable.” These authors are aware of the possibility of using confidence intervals to assess the strength of statistical relationships; yet they insist that the trouble with the practice of testing hypotheses is that “... it confused, or encouraged confusion over, the relationship between 'substantive' and 'statistical' significance.”

Most of the Turner/Turner polemic is highly questionable: What, for instance, makes this body of literature “obscure”? If it was usually known a priori that null hypotheses were false, has this situation corrected itself given the fact that nowadays so many null hypotheses are not rejected? If so, don't statisticians deserve some credit for correcting bad habits? If we tend to ignore the strength of relationships in instances where null hypotheses have been rejected, why does virtually every contemporary statistics textbook devote a large amount of attention to measures of strength of relationship? Again, I would argue that the major conclusion of the 'fifties debate was that statistical significance is only one means of attaining substantive significance, and I believe that anybody who re-examines this literature will agree that these authors are discussing it in an oversimplified way.

(2) Notice, for instance, that contextual analysis is closely related to analysis of variance (Boyd and Iversen, 1979:7-12), and that analysis of variance, as Porter says (1986:316), constitutes “a theory of heredity.” Actually Porter understates the case: Analysis of variance is better understood as a theory of genetics and natural selection, as suggested by Dawkins' claim (1986:41) that “[genetic] mutation is random; natural selection is the very opposite of random.” ANOVA was made for precisely this sort of reality, this sort of GLR, and it provides help of inestimable value in our efforts to perceive such realities whenever and wherever they occur.

(3) In folklore, there is another rendition of the same idea: “For want of a nail ...,“ etc. This story entails two questions: (a) Can a nail cause the loss of a kingdom? and (b) What became of the nail that lost a particular kingdom? The first is eminently answerable; the second—without a lot of luck—is not. Here we have another instance in which it is not impossible to explain large effects through small causes. How would one use the “want of a nail” explanation? In the usual way: One would have an appropriate theory, and one would test it by means of standard social science methods. (For starters, one would set up a health survey of the horses maintained by what may turn out to be a careless cavalry. It would also be important to investigate one of the most important of all military inventions, the stirrup—see Volti, 1988:172-73.) How would this inquiry relate
to the claim of “chaos” theorists, among meteorologists, that an asymmetrical sea
gull in Singapore eventually can cause a hurricane in South Florida? I’m not sure,
but I can guarantee that we shall never apprehend any sea gulls implicated in

Incidentally, Spencer's (1910:295) term for situations in which small causes
produce large effects was “fructifying causation,” a term that could readily have
been used by Ogburn in his discussions of inventions and their way of producing
“many results from a single cause” (Ogburn, 1946:55).

(4) The aircraft accident reports prepared by the National Transportation Safety
Board, and by similar boards of inquiry in other nations, are highly detailed case
studies, idiographic analyses par excellence. Yet most of these studies would not
be undertaken except for the fact that the results are eminently generalizable,
typically serving to warn pilots and others against the sort of error that led to an
accident in the case at hand. The recent crash of a Scenic Air Tours flight at
Maui, Hawaii, for instance, was due to continued flight into bad weather. The
NTSB (1993:v) generalizes that

contributing to the accident was the failure of Scenic Air Tours to
conduct substantive pilot preemployment background screening, and the
failure of the Federal Aviation Administration to require commercial
operators to conduct substantive pilot preemployment screening.

The safety issues raised in this report include:

1. Visual flight in instrument meteorological conditions.
2. Pilot qualifications and preemployment background checks.
3. The overall safety of the air tour industry.

As a result of this investigation, the Safety Board issued safety
recommendations ...
REFERENCES (Chapter 4)


Cohen, Patricia. 1982. “To be or not to be: Control and balancing of Type I and Type II errors.” Evaluation and Program Planning 5:247-53.


Moore, Dan H. II and Barton L. Gledhill. 1988. “How large should my study be so that I can detect an altered sex ratio?” Fertility and Sterility 50:21-25.


Only when there are a great many ... “quiet cases” ... will the effectiveness of social science become more clearly visible and then come to be as much taken for granted as is the effectiveness and usefulness of natural science.


(1) Introduction

This chapter begins with a debate, turns to a discussion of applied social science, arrives at a distinction among “reverse” regression models, “forward” regression models, and natural experiments, and then attempts to bring it all together within the framework of Henshel's (1976) argument regarding the prospects of sociological prognostication. The chapter implies that one of the advantages of a federated nation-state, such as the United States, is the presence of many jurisdictions able to carry out social experiments. This implication leads into the final chapter, which argues that the spatial areas that make up nation-states usually form a hierarchy, and that we should routinely make sure that all of our concepts have hierarchical organization, i.e., that they fit into taxonomies. Another implication, carried forward from the preceding chapter, is that while we are already very good in the realms of induction and syntactics, we should seek ways of making ourselves much better.

Social scientists should be conducting systematic experiments in large numbers, and we should introduce the results of these experiments into our best social simulations, post-facto designs, and projections of future events. I believe—and this belief is highly testable—that we have few experiments of this sort in progress, and that we therefore lose opportunities for the prestige-enhancing activities advocated by Henshel (1976). Social experimentation is costly, and it is risky. But as Wildavsky (1988) argues, when searching for safety precludes trial and error (or “learning by doing”) solely on the basis that errors hurt, it also tends to preclude safety. And as Barber implies, Earth should be a hotbed of social science experimentation.
A few years ago I had the honor of an argument with Marion J. Levy. Levy (1988) had written a critique of an earlier work of mine (Faia, 1986), and I did my best—as I'll show in a moment—to make an adequate reply to his more compelling points. If I had been afforded space for extended remarks, I probably would have written something along the lines of the present chapter.

Levy (1988:244) has the opening allegation: “Nowhere in his book,” he says, “can I find Faia's definition of the term functionalism or a forthright declaration that for his work that concept is an undefined predicate.” At first blush, I feared that Levy was correct. But then I reexamined the pages in which I discuss Kingsley Davis (Faia, 1986:4-5), and I soon realized that Davis' provisional definition, as I present it, should have sufficed—for functionalism, dynamic functionalism, and Marxian functionalism. Each of these modes involves an attempt to relate parts of society to the whole and an attempt to explain social structures with reference to their consequences.

Admittedly a little vague, this definition was nonetheless a fine basis on which to start a book. By the time I got deeply into this volume (Faia, 1986:128ff.) I was prepared to offer a highly detailed definition of functionalism: It is a form of analysis that scores a 4 on my “hypothetical Guttm an scale of sociological propositions” in that it involves multivariate causal analysis with time-series data and makes use of feedback and circular causation, with feedback and circular causation having an impact on the survival of an organization (or some part of it) or on the survival of an interest. Under this definition, it will soon become clear that this chapter is about functionalism and its prospects. I agree with Levy's remark that scientific concepts must have clear denotation; and I contend that nothing carries a more precise denotation than a Guttman scale. It was the major purpose of my earlier book to take the prevailing definitions of functional analysis and transform them from a gooey muddle into a Guttman scale.

One can readily imagine a study of organizational survivorship, or the survival of some interest, that would be ordered (i.e., the study would have time as a variable), multivariate, and causal; but such a study would not immediately be functionalistic. It could become so, however, if the investigator were to follow my admonition—one which Levy quotes but does not seem to grasp—that “powerful forms” of functional analysis would occur if survival were treated as an adaptation process involving feedback. In other words, when the survival of an organizational type (or interest) is threatened, what sorts of changes take place that may (or may not) lead to enhanced survival prospects? Note that I do not use tempting phrases about changes that might occur in order to enhance survivorship, leaving open a question that Levy wrongly considers closed. On Levy's question whether Lola gets what she wants, I reserve judgment: Let
Mephistopheles bring her front and center and I'll be delighted to ascertain, empirically, whether she achieves her objectives.

This brings us to Levy's second major contention (1988:244): “Faia heavily emphasizes what he refers to as 'circular causation'—a term that in itself should have set off warning lights and buzzers in his mind.” Believe me, it did and it does. In large part my book dealt with the costs and benefits of trying to assess two-way causal interactions through time. There are many pitfalls, but there is also a possibility of placing functional analysis—our best established and, with Marxism, our most durable theoretical tradition—on a firm methodological foundation that would force us to eliminate the traditional mumbo jumbo about prerequisites, teleology, stability, and consensus while testing falsifiable hypotheses about system interactions through time.

Incidentally, I'm a little mystified by the fact that in the same paragraph where Levy warns us about circular causation he expresses his worries about possible neglect or abuse of the “point of view of the actor.” At the moment I see no connection between circular causation issues and the question of the actor's perspective. The latter is just another set of variables that we introduce into an analysis whenever it appears to be relevant. It is entirely possible to deal with circular causation in studies that give little attention to the point of view of the actor. And therefore I am not at risk, despite Levy's apprehensions, of accusing black holes of deliberately gobbling up little stars—for whatever nefarious and polymorphously perverse purposes these Sultans of Suction may have. On the other hand, natural scientists and engineers cannot live without circular causation (Faia, 1986:Chapter 5); recall Dawkins' discussion, cited earlier, of the complex causal interactions involved in the evolution of sexual ornamentation. I am therefore mystified further by the claim (Levy, 1988:244) that circular causation “... is a vice in all empirical sciences.” We should realize, for instance, that when Clausewitz tells us that warfare is a continuation of diplomacy by alternative means, he implies that diplomacy is a continuation of warfare, that war and diplomacy may therefore operate as functional alternatives to one another, and that we would do well to examine the causal interaction between them.

Despite Levy's misgivings and hesitations, the functionalist model encourages us to focus on time as a variable more than do alternative social theories; circular-causation hypotheses encourage us to use methods that capture time as a variable. Until Levy demonstrates the irrelevance of my best examples—the Malthusian theory of population (which evolved into a highly sophisticated adaptation theory between 1798 and 1803), the Club of Rome world model, Zerubavel's work on the social definition and social organization of time, the sex preselection of children, sex discrimination in academic salaries, the delict-sanction model as applied to capital punishment, the interaction of military expenditures and economic stagnation at the state or nation-state level, the Clausewitzian hypothesis—I will continue to believe that each of these research traditions has achieved strength insofar as scholars have been willing to examine
the reciprocal interaction of variables through time. I will continue to believe further that, even though the scholars in question may not realize it, every time they examine interactions through time in a way that emphasizes organizational survival and/or the degree to which interests (i.e., manifest functions) are served, they are practicing functional analysis. If functional analysis is nothing more than intellectual self-abuse, most of us will soon be blind.

In a brief but provocative discussion of social prediction, Henshel (1976:26-32) describes “engineered systems” of the sort that regulate everything from refrigerator temperatures to airline flight paths, from carburetor fuel-air mixtures to automatic elevator doors, from household water pressure to freeway traffic flow. These systems impinge on our daily lives in ways that are so smooth and unobtrusive that we hardly ever take notice of them—until something goes wrong. Henshel explores the possibility that the social sciences might achieve comparable successes in social engineering, and might thereby enable themselves to take advantage of a benign circle in which a few modest successes—Barber’s “quiet cases”—would create opportunities for us to try again, with larger stakes. I don't know whether the expanded opportunity structure envisioned by Henshel will ever come to pass, but I do know that from time to time social scientists are called upon to engage in social engineering, and that whenever we accept these challenges we must be attuned to the need for “triangulation” (Babbie, 1989:99). That is, we must learn to use a variety of conceptual and theoretical perspectives and a variety of methodological strategies and tactics, lest the sorts of opportunities held forth by Henshel be lost.

Henshel's inquiry, in essence, develops a functional theory; it is self-exemplifying. By definition, this theory involves feedback. It is a theory about the interaction of social scientists with their constituencies, their publics: If we can manage to produce useful knowledge, says Henshel, we shall eventually be afforded many new opportunities to produce such knowledge. By useful knowledge, I refer to theories that enable us to explain and (especially) to predict social phenomena that threaten organizational survival and/or various interests. We immediately become uneasy, because we know that our historical record in predicting future social phenomena has been considerably less than perfect. However, one implication of Henshel's theory is that expanded opportunities for social engineering—and especially for designing controlled natural experiments—would enable us to improve predictions dramatically. In other words, we should try to generate a positive feedback process that would enable us to become competent social engineers, to transform ourselves. In general, we explain and predict the behavior of engineered social systems much more successfully than we explain and predict the behavior of systems that we have not designed. What Henshel implies, of course, is that experimental research is
superior to non-experimental research, provided that one has opportunities for conducting experiments both within laboratories and (especially) within natural environments.

(2) Sex discrimination in faculty salaries: Toward projections of the middle range

I shall illustrate several features of social engineering by describing a participant observational study in which I was involved over a period of several years.

Toward the end of 1981, the Affirmative Action Advisory Committee (AAAC) of my university issued an annual report that contained the following paragraphs:

Once an academician has received an appointment at William & Mary, it is the responsibility of the university to see that racial, sexual, and other forms of discrimination have no place in promotion, retention, or tenure decisions. Such discrimination, if it exists, clearly falls within the purview of the university's affirmative action program.

On the matter of faculty salaries, the university has recognized explicitly the strong possibility of sex discrimination: Twice within the last eight or nine years the administration has found it necessary to make upward adjustments in the salaries of significant numbers of female faculty members. The 1980 report of the AAAC recommended that the committee be given access to the master personnel file so that it might undertake periodic statistical analyses of the faculty salary schedule; we hereby reiterate this recommendation.

AAAC access to the master personnel file cannot be interpreted as an invasion of faculty privacy since the AAAC is requesting only those salary data that must be made available under the state freedom-of-information law, along with data already in the public domain—e.g., rank, seniority-within-rank, field, and so forth, as found in the university catalogue. In the typical statistical analysis, faculty salaries will be assessed in relation to such characteristics as rank, seniority, academic field, etc., in order to determine the average net impact of each of these factors on salaries. This process, typically, will generate an equation; if the salary of a given faculty member varies substantially from this equation, then the AAAC or the affirmative
action office, without any presumption of discrimination, will inquire as to why such variation exists. It is entirely possible that satisfactory explanations would be forthcoming. Indeed, we anticipate that in the typical instance disparities between actual and “predicted” salaries will be readily explainable; it is our purpose to find what is probably a small number of instances in which injustices occur.

It should be noted that there is nothing in this request that could not be granted to any Virginia citizen under the state freedom-of-information act. In fact, one member of the AAAC a few years ago received the faculty salary schedule in machine-readable form, and supplemented these data with information on faculty characteristics drawn from the university catalogue; statistical analyses were undertaken along the lines suggested above. The present AAAC recommendation is that this type of analysis, if it proves feasible, be made routinely at least once a year, using both current salary and rate of increase in salary as the “dependent variable.” (The master personnel file contains salary data for the current year and for the preceding four years.) In making these investigations, the AAAC will consult various faculty members knowledgeable about statistical methods and about the decisionmaking process for faculty salaries.

It was clearly the first responsibility of the committee to build support for the idea that these periodic salary assessments should be made; for many faculty members, the notion of having regular evaluations of the salary structure was totally alien, an alleged invasion of privacy and a violation of professional integrity. The committee therefore voted (for this reason and others) to make its report public, and when the university administration resisted this idea the AAAC chairman found it necessary to release the report to the local media. This action caused considerable consternation in many quarters, and the chairman found himself summarily relieved of duties as committee chair during the spring of 1982. It is my belief that the publicity surrounding the AAAC report did not retard implementation of its recommendations in any way—if anything, prospects were enhanced—and I do not believe that social engineers need to tread softly. They have to take pains, however, to avoid major blunders of a methodological or theoretical nature.

As the fall semester, 1987, was getting under way, I was asked by the chair of the faculty women's caucus to prepare a memorandum that would explain to caucus members my perceptions as to how salary assessments, with a focus on gender equity, would be made. This colleague knew that I had continued
gathering data on the local faculty, and she also knew that I had supervised graduate research on the university faculty and the nature of its reward system. Incidentally, I had found that because of my willingness to comply with the ethical standards against identifying individual faculty members by name (and salary)—despite the fact that Virginia law does indeed permit such identification—I had the full cooperation of the administration in all data-gathering activities and was allowed access to the university’s master personnel file as a way of ensuring accuracy and speed of data recovery.

The following is my memo:

To: [name deleted], faculty women's caucus
From: [author]
Re: Faculty salaries

I've completed a preliminary analysis of my 1986 data on the university faculty, and some of the findings may interest you and other members of the women's caucus.

When I obtain a regression equation giving the square root of male salaries as a function of various factors, the factors that attain statistical significance are the following:

1) having a 12-month contract vs. a 9-month contract;
2) whether or not one is a department chairperson;
3) one's rank;
4) one's highest academic degree;
5) affiliation with a professional school;
6) the number of federal grants one has received;
7) one's seniority, in years of service;
8) the number of committees on which one serves.

These factors account for 67 per cent of the variance in the square root of male faculty salaries. Non-significant variables—those having no important net impact on salaries—include receipt of teaching awards, service on elective committees, number of committees that one has chaired, number of books published, and number of articles published.
It should be noted that many of the “merit” factors—such as publications—may be reflected, for instance, in one's professorial rank.

When the male regression equation for salaries is applied to female faculty members, we obtain the average salary (square root) that females would receive if their salaries were determined by the male equation; presumably, such a salary-determination process would be free of sex discrimination. For 1985-86, this average would have been 179.23 compared with the actual 172.49. When these amounts are squared, the difference amounts to $2371, which is nearly 8 per cent of the average female salary ...

This 8 per cent discrepancy, of course, is an average. You will note that DISCREP, for many of the 61 women on the following list, is very high. A woman with a DISCREP score of -20 may have an actual salary with a square root of, say, 150, and a predicted salary (derived from the equation for males) with a square root of 170. Clearly, this is a discrepancy worth investigating.

[I believe] ... that such discrepancies, for male or female faculty members, should be routinely looked into—with an open mind. As you know, I would be willing to have some sort of consultative role in the proposed study of the salary schedule, a study in which (among other things) DISCREP scores for specific individuals would be explained in one way or another. ...

Here are the discrepancy scores for female faculty members. For these women, a variable called FEMALE was coded 1. SQRTSAL is the square root of each woman's current salary, PREDUAL is the square root of a woman's salary as predicted by the male salary equation, and DISCREP is the difference between SQRTSAL and PREDUAL:

<table>
<thead>
<tr>
<th>FILE: FACULTY DATA BASE</th>
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<tbody>
<tr>
<td>FEMALE: 1.00</td>
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<table>
<thead>
<tr>
<th>FEMALE</th>
<th>SQRTSAL</th>
<th>PREDUAL</th>
<th>DISCREP</th>
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<tbody>
<tr>
<td>1.00</td>
<td>184.12</td>
<td>205.70</td>
<td>-21.58</td>
</tr>
<tr>
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<td>171.46</td>
<td>175.73</td>
<td>-4.27</td>
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<td>1.00</td>
<td>184.39</td>
<td>195.79</td>
<td>-11.40</td>
</tr>
<tr>
<td>1.00</td>
<td>204.94</td>
<td>199.89</td>
<td>5.05</td>
</tr>
<tr>
<td>1.00</td>
<td>137.84</td>
<td>159.91</td>
<td>-22.07</td>
</tr>
<tr>
<td>1.00</td>
<td>167.63</td>
<td>180.27</td>
<td>-12.64</td>
</tr>
<tr>
<td>1.00</td>
<td>160.00</td>
<td>166.22</td>
<td>-6.22</td>
</tr>
</tbody>
</table>

...
These data do not compel the belief that discrimination has occurred, but they do constitute what jurists would call probable cause, and they do necessitate further investigation.

Just after the end of the semester, around January 1988, it became clear that some sort of corrective action would have to be taken by the university administration. Specifically, the administration was exploring the possibility of bringing in an outside consulting firm that would evaluate the salary equity situation at William & Mary. The chair of the women's caucus and the dean of the faculty asked my opinion about the study proposed by the consulting team:

To: [chair, women's caucus; dean of faculty]
From: [author]
Re: Faculty salary equity

... The study proposed by [names deleted] appears to be sound in its basic approach. I am particularly gratified to know that, once regression analyses have been completed, the consultants plan to spend some time on campus during which they will ask the appropriate parties for explanations of salary residuals, i.e., the discrepancies between actual salaries and those predicted by the equation for faculty men. This part of the proposal is crucial, and should be worked out in more detail. In cases where adequate explanations cannot be provided, one has a good basis for insisting on a salary adjustment.

The major problem I have with the consultants' proposal, however, is that I do not think the university is getting much of a dataset for its (ballpark) $3000.00. Their “example of one faculty record” contains a dozen or so variables ranging from tenure status to rank to sex. Our own office of institutional research already has this information on the master personnel file; it was the basis for the datasets I brought together throughout the 'seventies and 'eighties. It takes our office of institutional
research about ten minutes to prepare a COBOL program that loads the information onto a special magnetic tape. I suspect that the consultants will end up by simply asking our institutional research office to undertake this task once again.

Although the consultants' standard faculty data record requires about half a card image, I recall that the dataset gathered by [name deleted] and me for a master's thesis (1986) involved about four card images, with lots of information about "merit"—e.g., committee assignments, administrative assignments, grants received, publication of books and articles, a few (unsatisfactory) measures of teaching ability, etc. I believe that the proposed study will be an exercise in futility unless these sorts of data are again made available. It is easy for those who explain salary disparities to make vague claims about merit (or relative lack thereof) as a basis for the explanation; in the absence of data, it is difficult for aggrieved parties to challenge these claims. The beauty of regression analyses is that they contain the weights assigned to given faculty characteristics, as well as the measures used to assess these characteristics; each type of information is important in assessing the legitimacy of a given faculty member's salary.

Incidentally, the equations also force us to justify assumptions such as linearity and additivity: For instance, should each step in professorial rank have about the same net monetary value, or are some steps more important than others? If a professor is an average teacher and an average researcher, should this combination have about the same monetary impact as being an excellent teacher and weak researcher, or vice versa?

In brief, I have three conclusions: First, the university should find a way of creating datasets routinely on the faculty reward process that would be far more thorough than that proposed by the consultants. Second, a systematic procedure should be worked out so that those faculty members with high (negative) salary disparities will be notified about them. Third, these disparities should be thoroughly investigated, explained, and resolved by appropriate administrators, department chairpersons, personnel committees, consultants, and the affected faculty members. This sort of activity should be performed routinely each year. If the task were done routinely, it would usually be much smaller than it is likely to be this year.

By 1 March 1988 matters were proceeding apace: Consultants had been hired, they were in the process of studying the faculty salary structure and formulating their recommendations, and these recommendations were likely to
be implemented. In my discussions with various colleagues it became clear that the administration, for reasons on which I can only speculate, was becoming involved in a rush to judgment, and that detailed case studies of alleged instances of discrimination were not likely to be made.

Apparently, this problem arose out of bureaucratic necessity:

To: [name deleted], chair, faculty women's caucus
From: [author]
Re: Faculty salary equity

I get the impression that some members of the administration would like to move very quickly toward resolving salary inequities involving female faculty members. In so doing, they would rely heavily—perhaps too heavily—on regression models of the faculty salary structure. As you know, I believe that regression equations provide nothing more than probable cause, and that they should serve primarily as a basis for a series of ad hoc investigations in which, typically, a given female faculty member would have a hearing involving herself, her department chair, her personnel committee, and an administrative representative. This procedure would take lots of time, and I suspect that it could not be completed (this year) before June at the earliest.

I believe that those administrators who wish to move quickly on these matters are concerned that, under a scenario involving hearings, any salary inequities would be resolved after this year's contracts have gone out to the faculty, so that adjustment funds would have to come from some source other than the funds that will go into the general faculty contracts. I don't blame administrators for not wishing to paint themselves into a corner, but I do not believe they have any option: We simply cannot handle female faculty grievances adequately in two or three weeks of over-interpreting regression equations.

Furthermore, there was a large ethical question: Should the funds necessary to correct discrimination be taken from male faculty members (or administrators) who (1) may have had no role in perpetrating discrimination, (2) may not have benefitted in any way from the existence of discrimination, and (3) may actually have suffered an impaired collegiality arising from discrimination and the low professional morale that it may create among its victims? As a social engineer I have no qualms against saying that in this instance the system needed an infusion of new funds. As nearly as I can tell, however, this infusion did not take place.

In my opinion, the decision not to make detailed case inquiries was a mistake. As I will try to show below, it would be highly desirable for a given university to arrive at a time when its faculty would be convinced that
discrimination essentially had been eliminated, so that from that moment onward procedures could be established that would ensure that the salary structure would not again be distorted.

Around 23 March 1988, word began circulating that the administration was proposing a simple regression model that would employ rank, seniority, and field—with weights for the latter derived from national surveys. Apparently a small number of women—a dozen or so—were to receive contractual adjustments based on this model. Nobody seemed especially happy about the procedure; to the administration, however, it probably looked fast and smooth.

Around 28 March 1988 a local committee was formed to deal with the gender-equity issue. This committee was chaired by a sociologist (not the author) and included several faculty members and administrators who had insight into the faculty salary process. The major purpose of the committee was to clarify for various faculty members, including women who were alleging discrimination, what sorts of efforts would be made to address gender-equity problems. Although the state of Virginia has a legal requirement that meetings of this kind be open to the public unless the discussion involves specific cases, it is a part of the folkways of the university that highly controversial meetings involving faculty and administrative matters are held in closed sessions, and I felt that my own position as a somewhat distant observer would have been impaired if I had insisted on attending. Consequently, my knowledge about it is indirect. Judging by the testimony of participants, I believe that the meeting clarified the nature of the corrective actions on which the institution was embarked, and there was no significant opposition expressed at the meeting against the administration's proposals.

After a period of relative quiescence that lasted for nearly a year, on 6 February 1989 the Dean of the Faculty of Arts and Sciences announced “gender equity monitoring,” to be conducted on an annual basis. By now we had come full circle: A key recommendation made by an affirmative action committee almost a decade earlier would finally be implemented. I do not believe that most members of the current administration or faculty remember this recommendation, and I do not believe that the affirmative action committee, per se, had much to do with the administrative decision to implement annual salary-equity assessments. I believe that these assessments have been established out of a sort of functional necessity, and that past advocates of them were merely recognizing a process that was more or less inexorable.

In the nearly twenty-five years that I have been a member of the William & Mary faculty, the sex-discrimination issue has exploded three times in a manner so compelling that, in each instance, adjustments have had to be made in the
salaries of several female colleagues. I believe that the number of cases involving salary adjustments has generally been in the neighborhood of a dozen, out of a faculty of about 300. The stability of this number supports the functionalist-necessity argument: After a time lapse of perhaps five to ten years, the magnitude of the discrimination gap—both the amount of income denied and the number of victims identified—arrives at a certain threshold, and the institution must take corrective action. At least until annual equity monitoring was introduced, the process had a characteristic frequency and amplitude. Every five to ten years, a discrepancy of about five to eight per cent in female salaries has been evident and has been sufficient to activate a “critical mass,” to bring about effective protest.

In developing critical mass theories of the rise of protest movements (Sullivan, 1977; Newcomb, Jesness, and Erickson, 1978; Tsai, 1978; Rahav, 1981; Wilson, 1983; Oliver, Marwell, and Teixeira, 1985; Oliver and Marwell, 1988) one would be wise to take into account both the number (or proportion) of potential protesters in an organization, their interaction, and the nature and magnitude of their grievances. When these quantities coalesce in a certain way, there is a qualitative transformation and the institution acts abruptly in an attempt to resolve its internal tensions. Or, as an alternative, it tries to introduce a qualitatively distinctive form of self-surveillance—in this instance, annual gender-equity assessments. As I write these words, I believe that this institution is about to undertake its first annual gender-equity assessment, and I am not optimistic: I have many reasons for doubting that the pattern of the last twenty-five years, involving periodic protests and precipitous fix-ups, will be interrupted.

I have implied throughout this analysis that the most effective way of interrupting the established pattern would be to put together a workable combination of etic modelling, emic investigation, and concurrent efforts to explain to the entire university community the nature of the salary-setting process and its outcomes.

The basic patterns and trends uncovered by regression modelling of the salary structure should be discussed in detail each year, in the various university news media, administrative memoranda, committee reports, etc. Any trendlines in sex disparities would be highly reliable if essentially the same regression model were applied each year—do not, however, lose sight of Abbott’s admonitions, discussed in the preceding chapter, against neglecting the causal context. Each faculty member would be able to see his or her own relative position in the faculty salary hierarchy, and to see any changes that might occur in that position. Inquiries about perceived injustices could be addressed immediately, and misunderstandings could potentially be resolved. The sorts of tasks that we believed were to be undertaken by the university’s consulting team in 1988—but which were not undertaken—should be pursued diligently each year. My prediction is that the salary-setting process would soon be demystified by these sorts of activities, and that “the functions of secrecy” would turn out to be dysfunctions.
It requires a degree of temerity to advocate social engineering at a time when the social sciences, *qua* sciences, are being challenged from many directions. But I continue to share Henshel's confidence that if we select opportunities carefully, we have good prospects of success. In the case at hand I exercised considerable care, even though I cannot claim to have been the chief engineer, or even a low-level foreman. I was aware, for instance, that academe is a place in which there is a relatively strong commitment to the idea that rewards should have a demonstrable relationship to productivity. There are, in short, more or less explicit rules about how rewards such as salaries should be allocated. For salaries, it is possible to explain statistically as much as 80 per cent of the variability that occurs within a given faculty. At the national level, this percentage usually drops to around 55 or 60, and my intuitions tell me that it would be far more difficult to implement salary-equity procedures at the national level than at the level of a given college or university, a given type of institution, the several institutions that comprise the higher educational system of a given state, etc. In the realm of higher education, in general, the more localized the situation, the clearer the social norms.

For the case at hand, another selection criterion was the fact that essential variables—salaries, salary discrepancies related to sex, the sense of equity resulting from a given reward structure—were readily measurable. If instead of manipulating these sorts of variables we had been dealing, say, with the development, implementation, and assessment of ways of reducing alleged sexual harassment involving faculty members and students, the tasks of social engineering would have been formidable indeed. There is little consensus about whether such an effort should be undertaken at all, it is unclear what we mean by sexual harassment, we have little knowledge about means of social control that would influence the likelihood of sexual harassment, and it would be difficult to determine whether any set of means were having the intended impact. I do not discourage social engineering in this realm; I merely warn that the prospects of success, in Henshel's sense, are far more limited.

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**Codicil 1993:** My pessimism about salary-equity monitoring appears to be justified. From time to time I have spoken to the appropriate administrators and official faculty monitors. Clearly they have the data, they have done the analysis, and they have arrived at some conclusions. It is equally clear, however, that they will not release their results: They do not wish to stir things up.

As Henshel's work suggests, aspiring social engineers must first acquire sufficient prestige that we might begin the tortured process of educating our colleagues. In Comtean terms, we must become the high priests of our local administrations.
(3) Allocating faculty salaries with Minitab BASIC, and allowing feedback from future states

The preceding paragraphs apply a hypothesis that will be developed further in a later chapter, with reference to the production of beer and electricity. The hypothesis suggests that, in general, there is a tradeoff between frequency and amplitude. In Kuhn's words (1974:31), the size of an amplitude varies inversely with the speed and sensitivity of negative feedback. We might therefore expect that in the present instance frequent and sensitive “equity monitoring” of a reward process would reduce the magnitude of actual and perceived injustices.

In this section I design the second part of a two-tiered salary-determination process. In the initial phase, faculty members generate merit scores for one another; one must be assured that these scores are arrived at without prejudice and discrimination. The second phase involves the administrative contribution to the setting of faculty salaries. In my view, administrators should have hardly any role in evaluating individual faculty members, i.e., in generating merit scores. They should evaluate divisions, departments, programs, and they should participate in decisions about financial policies—such as the amount of money to allocate for “merit” rather than cost of living, or the overall difference that ought to exist between the highest and lowest salaries. My discussion assumes that these norms have been established, that administrators do not examine the merit scores of individual faculty members except when there are fairness issues such as discrimination, and that the best administrative procedures, again, are those in which etic abstractions interact with emic confrontations.

The appendix to this chapter presents a detailed design, written in Minitab BASIC, for a faculty reward process that would operate with unusual transparency, would allow various salary-distribution policies to be implemented, and would permit simulation experiments. Notice that these experiments begin with past conditions (e.g., last year’s salary schedule for a faculty), enter various contemporary data (e.g., current merit scores), and project a future state (e.g., next year’s salary schedule). In a moment, I shall refer to this sort of experimental model as a PPF (past-present-future) simulation.

The program is entirely deterministic: Once a given set of faculty merit scores has been inserted into it, and once an explicit set of assumptions has been made about how the program will process these scores, there is only one outcome for next year’s salary schedule. Anybody who wishes to challenge this outcome will be able to identify precisely those factors that account for it. I argued in the preceding section that when the etics of the salary-determination process are made totally transparent, the emics of the situation will tend to become far less confrontational. In this section (and the associated appendix) I try to show how to develop a transparent etic model—itself a rather dialectical if not oxymoronic proposition, since we generally define etic models as being intelligible only to a small number of specialists. However, one consequence of good social
engineering is that a wider public will learn to be less intimidated by, say, regression equations.

Under this program, for instance, administrators (and, one hopes, faculty) must make several interrelated policy decisions. First, they must establish (or ascertain) the size of next year's salary budget. Second, they must decide what proportion of new funds (if there are any new funds) will go to cost-of-living increases. Third, they must decide what proportion of new funds will go to “pure” merit percentage increases—i.e., a percentage increase over current salary—as opposed to a flat-dollar merit component, which provides the same monetary increase for all faculty members who have the same merit score, regardless of their current salaries. Finally, administrators must decide which departments, if any, are worthy of special over-reward (or under-reward), and which are not.

All these decisions are political, and they should all be fought out as such, openly. If there is a policy that involves “levelling” of faculty salaries, for instance, this policy will tend to increase the cost-of-living component and the flat-dollar merit component. Such a policy might affect some individual faculty members adversely, but these adverse effects can be explained entirely with reference to the policy and not with reference to alleged injustices involving personal victimization. The same can be said about the policy of rewarding and punishing specific departments, as units.

A multivariate model that makes projections into the future, and that operates deterministically or nearly so, typically involves a PPF simulation: Instead of asking, as we do in regression analysis, “What processes could have produced these results?” PPF simulations ask “What results would be produced if we were to set up such-and-such a process?” The salary-determination program asks the latter question; a program package such as the Florida Interactive Modeler, to be introduced in a moment, enables us to ask many such questions.

Again, it should be noted that the best social simulations are essentially “ideal-type” experiments. Most of us recognize why it is that strict experimental methods are superior to our typical post-facto designs. We also know that the central problem of experimentation is that, usually, we cannot carry out experiments in “natural” situations where there is a lot at stake. Henshel asks us to contemplate what we would do if we were to achieve drastically expanded opportunities to design experiments in situations where weighty variables would be manipulated. He also challenges us to have the courage to project the results of such experimentation into the future by means of social simulations.

(4) From a FIRM grip on reality to the AIDS epidemic: The future of social simulations
The Florida InteRactive Modeler (FIRM) is a highly generalized simulation program that has a lot in common with other well known programs such as the Club of Rome world model, SLAM, Simscript, the Wharton model of the U.S. economy, and the computer simulations that accompany textbooks such as Bainbridge (1987) or Feigelman (1993). A major advantage of FIRM—one that makes this program directly comparable with SLAM or Simscript—is its generalized character that enables it to model just about any socio-psychological or socio-economic process in which there is a dependent variable that is influenced by combinations of causal agents, their interactions, constants, and random disturbances. The diskettes that accompany FIRM contain an illustration of socio-economic processes—a price-prediction model for a grocery basket filled with a standard list of “junk food” items—and several illustrations of socio-psychological processes such as a model of the famous “sleeper effect” in studies of public opinion.

FIRM simulations involve what I call forward regression models, as opposed to the usual reverse models. In creating reverse models, we gather vast amounts of information about variables that have already been processed by nature as causes and effects, and we then try to ascertain and reconstruct what nature has been doing. In forward models we make intelligent decisions about what nature could be doing or could have done under various circumstances, and then we activate the model, allow its causal factors to vary, and see what happens. Critics of this approach often make a GIGO argument, claiming that simulation models have a lot in common with Tom Lehrer's famous sewers: You get out of them exactly what you put into them. But experienced users of elaborate simulation models know that this claim is untenable. In a SLAM traffic-control simulation involving only a small number of intersections and bottlenecks, and perhaps a frantic rush hour or two, one cannot possibly know a priori how to set the timing of traffic lights so as to minimize waiting time for drivers, and one's intuitions on such matters often turn out to be misleading. Similarly, in the complicated renditions of attitude formation and change that are available in the FIRM package, one encounters many surprises.

It occurred to me recently that forward regression models have a lot in common with “counterfactual history.” In her excellent book on social explanation Susan James (1984:60-62,134-36) discusses the ways in which arguments contrary to historical fact have been used to test ideas such as the “great man” theory of leadership. Fogel (1964), in a study to be discussed in the next chapter, asks what the cost-benefit consequences would have been if the United States had made the same commitment to waterways and highways that it made to the railroads—a strong commitment to the wrong transportation system could be a lot more costly than merely timing traffic lights wrongly. Counterfactual historical analysis often involves (at some level of development) the kinds of measures, weights, interactions, constants, and unknowns that comprise FIRM (and comparable) simulations.
One of the best features of FIRM as a learning tool is that it forces the model designer—and presumably the model user—to become familiar with the literature pertaining to a given process; one surely cannot write a realistic model without this familiarity. Furthermore, the FIRM approach gives us an incentive to keep up with the literature—whenever a new finding appears it is possible to incorporate it into one's models and see how large an impact it is likely to have. Finally, it would be a good training exercise to use (reverse) regression analysis in order to tease out the properties that some programmer has written into a (forward) simulation model.

Taking time as a criterion, there are at least three types of social simulations: First, those that pertain entirely to past events, such as the “counterfactual history” simulation presented in the next chapter. Second, those that try to capture events that would take place entirely in the future, such as the sex selection of children (prior to conception) or mandatory AIDS testing for couples applying for marriage licenses. Finally, there are simulations that center on the present, have origins in the past, and must therefore pass muster with the contemporary and historical record, such as it is. Simulations of this type are PPF simulations. Using such a simulation, it should be possible to make projections into the future, to predict (with qualifications) events that we might otherwise miss. If one were to experiment with a large array of PPF simulations one would soon realize, with Comte, why sociology is at the apex of all academic disciplines and is therefore the most demanding of all disciplines.

The current AIDS epidemic, for instance, has been modelled as a PPF simulation by Whicker and Sigelman (1991: Chapter 6). Their program, written in BASIC, permits experiments with variables such as the transmission probabilities for various combinations of AIDS carriers and their partners (homosexuals, bisexuals, heterosexuals), the use or non-use of condoms, frequency of intercourse per month, number of new sex partners per month, and so forth. Values for these variables are allowed to vary through a certain range as the program executes, but the model will become more realistic as empirically-based estimates of actual values become more reliable. The same may be said about estimated numbers of individuals infected with AIDS at the time when the simulation begins, say 1988. For this variable also, Whicker and Sigelman experiment with several different settings.

In my own experiments with the Whicker-Sigelman program, I began by corroborating their results; I also found that when their model iterates through more than twelve months, incredibly large numbers of new AIDS infections are produced, which suggests that the model may have its greatest utility over a short term. In my runs with the model as presented (Whicker and Sigelman, 1991:106-7), the number of new infections among heterosexual females in the
United States varied, in a twelve-month simulation, between 841 and 1,184,880; needless to say, these projections are not highly precise. The low estimate is based on the assumption that (1) condoms are consistently used, (2) frequency of intercourse is low, (3) there is one new relationship per month, and (4) the starting population of infected persons consists of gay males only. The high estimate resulted from a reversal of (1) through (3), while (4) remained unchanged.

The extremes of the model, of course, are unrealistic: We cannot assume, for instance, either universal condom use or consistent non-use. Again, the simulation will have utility only insofar as we build into it realistic assessments of such practices as condom use, the nature and frequency of various sexual practices, etc. If we were to make serious efforts along these lines, we would almost surely do better than Masters, Johnson, and Kolodny (1988:15), who estimated that the United States would experience 300,000 AIDS deaths by the end of 1991. Total mortality does not appear to be nearly this high; by the end of 1991, the number dead is around 120,000, well below even the 179,000 estimated a few years ago by the U.S. Public Health Service (Masters, Johnson, and Kolodny, 1988:15).

I do not wish to imply that the above estimates were “wrong” in a simplistic sense; they were both intended—as in the case of the Club of Rome world model, another PPF simulation—to act as self-destroying prophecies. Clearly Masters, Johnson, and Kolodny are social actors with a point of view: They intended their projection to serve as a warning, and they now may be delighted by the fact that this projection, whatever its basis, turned out to be wrong. The lesson for them, and for us, is that if their warnings actually served to modify behavior, then these behavior modifications need to be incorporated into our more advanced theories and simulations.

We must also incorporate the possibility that, as we learned repeatedly from the Club of Rome world model, it is hard to distinguish between a self-destroyed prophecy and a self-delayed prophecy. By the end of 1991 we did not need the celebrated case of Magic Johnson to suggest that the nation had relaxed its guard in the struggle against AIDS. One only has to spend a few moments perusing the pages of the New York Times to discover substantial evidence that, by early 1991, it was widely believed that the AIDS threat had diminished considerably (Kolata, 1991). Self-delaying (or destroying) prophecies involve exactly the form of feedback discussed throughout this chapter, and it is entirely possible that adaptive efforts in the realm of AIDS prevention have already gone through several cycles of unknown frequency and amplitude.

(5) Conclusions
In the wake of the 1990 census, many states again are finding it difficult to draw a new set of congressional and state-legislative districts. In the typical instance, simulation programs design a long series of redistricting proposals that try to satisfy simultaneously the demands made by political parties, by individual politicians intent on escaping (or taking advantage of) gerrymandering, by minority segments of the electorate who wish to be represented by their own kind, and by various constitutional principles as interpreted by the Department of Justice and the courts. Given the rapidity with which high-speed computers create new designs, it is not surprising that many simulations are evaluated and that most of them quickly become self-destroying prophecies. Eventually, however, “one true story” must emerge from these inexorable exercises in social engineering.2

The process of creating new legislative districts has a lot in common with the process of allocating faculty salaries: It must be done from time to time, at fixed intervals; there is a high level of normative regulation of the process—a high degree of consensus as to how it should be carried out—and therefore many potential solutions may be eliminated a priori; nonetheless, there remains an indefinitely large number of plausible simulations that must be examined in detail; finally, the data necessary to make such simulations are readily available and have a high quality. In Henshel's view, if social scientists seek the power to change society for the better, if we seek prestige and ways of creating new knowledge, then these are precisely the circumstances under which we need to move quickly.

If Henshel's analysis is correct, the lives of social scientists are likely to become a series of ethical choices and ethical crises, as we decide whether it would be right to design a particular feature into an organizational process. This transformation is inescapable: The centrality of ethical issues in the lives of natural scientists is clearly shown by every issue of the Bulletin of the Atomic Scientists, by every meeting of the “committee on human subjects research” of one's university, and by every instance in which a medical doctor must make an appearance before an abortion committee. It may seem ironic that social scientists, currently, are far less likely than their natural science colleagues to face agonizing ethical choices: I assume, however, a very high correlation among the prestige of an academic discipline, its competency, the extent to which it has opportunities to conduct controlled natural experiments, and the frequency with which its practitioners must face ethical dilemmas.

In brief, we need a code of ethics for social engineers. This code must be based on adaptation theories of high credibility, so that what appear to be successful ventures will not turn out to be a series of disasters. It is arguable that, up to now, our best opportunity for social engineering was provided by the
American military establishment (Bowers, 1967); this venture did not have a happy outcome.
NOTES (Chapter 5)

(1) For an excellent discussion of the uses of the terms “etics” and “emics,” especially among cultural anthropologists and linguists, see Harris (1979:32-45). In emic analysis, the “native informant” is the “… ultimate judge of the adequacy of the observer's descriptions and analyses.” In etic procedures, the observer is the ultimate judge (Harris, 1979:32). Regression analysis is essentially an etic operation; depth interviews that probe perceptions, explanations, and judgments of various aspects of, say, salary distributions would be an emic operation.

(2) For a discussion of Virginia's situation, see Baker (1991) and Harris (1991).
REFERENCES (Chapter 5)


APPENDIX (Chapter 5)

This program is intended as an efficient, philosophically defensible way of allocating faculty salary increases, where these increases have three components: a cost-of-living increase received by everybody, the usual percentage-based merit increase, and an unusual merit increase that provides the same amount of money for all faculty members who have a given merit score, regardless of their initial salaries. The last component will be called a flat-dollar merit increase.

The program is very simple. Administrators of the salary budget must enter the following items:

- Raw merit scores awarded by each department (or other appropriate unit) to its members (lines 0040-0510);
- The current salary schedule for each department or academic unit (lines 1420-1680);
- Next year's total salary budget (lines 2060-2070);
- The proportion of the budget add-on intended for percentage-based merit increases (lines 2140-2160);
- The proportion of the budget add-on allocated for cost-of-living increases (lines 2190-2230);
- The “over-reward” (or “under-reward”) adjustments that may be made for particular departments or other academic units (lines 2750-2990).

The last three items involve “political” decisions that are usually the prerogative of local administrators and faculty, and under this program these decisions must be made clearly, explicitly, and understandably. Administrators are able to experiment with any number of combinations of values for the last three “inputs” listed above. The program, in other words, is a simulation; as such, it works as a forward regression process in the sense defined earlier. It is interesting to note that the usual reverse regression analysis would not be able to solve this simulation program correctly—to figure out, in other words, how it was put together—unless the regression model were written by somebody who understood the unusual way of rewarding “merit”: One way involves a percentage increase in salary, while the other involves a flat-dollar amount for anybody with a given merit score.

The listing below is output from a Minitab program that simulates the process of setting next-year salaries for all members of five departments. Any number of departments can be written into the program, and a given department
can have any number of members. Departments are allowed to use any merit evaluation scheme they wish, with the understanding that they must provide a single numerical score for each member, and that all these scores must be positive numbers. Departments should be made to understand, further, that merit scores will be treated as a “ratio” scale, so that a score, say, of 6, ceteris paribus, will have twice the impact on one's salary increase as a score of 3.

This program uses the “proportions-of-pies” method: All merit scores are converted to proportions of all points awarded by a given department, and these proportions are then multiplied against three “pies”: the cost-of-living increase pie (PIE(1)), the percentage-increase pie (PIE(2)), and the flat-dollar pie (PIE(3)).

The Minitab statistical package has been used to simulate scores for merit (lines 0110-0510), and these scores are printed out. Note that proportional merit scores all sum to 1.0 within a given department, so that any pie made available to a given department will be allocated completely according to these proportional merit scores. In actual implementations of the program, of course, the administrator must enter the faculty merit scores into the program. I assume that administrators regard the faculty merit scores as sacrosanct, and that they (the scores) cannot be changed except in cases involving provable injustices such as discrimination based on age, sex, race, etc.

Minitab next simulates a salary schedule for the current year, with departmental means and standard deviations varying considerably (lines 1420-1680). In actual implementations of the program, again, administrators must enter the current salary schedule.

In lines 1720-1860, Minitab calculates the correlations of merit and current salaries, by department. If these correlations tend to be above 0.0, it is likely that the percentage-based merit increase part of the budget will rise above the amount targeted (e.g., .5). If correlations tend to be below 0.0, the opposite effect is likely. My own simulations indicate that this proportion remains stable, despite the fact that the percentage-based merit increases are allocated without taking into account the distribution of base salaries.

The program calculates the total size of the current budget (lines 1900-1910), the size of each department (lines 1940-1990), and the size of the total faculty (lines 2020-2030). All these values should be used as checks.

After the administrator has entered the size of next year's total salary budget (lines 2060-2070), the program obtains the amount of the budget increase (lines 2100-2110). The proportion of this amount that is intended for pure merit increases should be entered at line 2160, with the understanding that if percentage increases, when applied to current salaries, do not precisely equal the proportion of the budget add-on that is intended for this purpose, the discrepancy is allowed to stand. Percentage-based merit increases are taken as inviolable, so that if (due to base salaries) they take a larger part of the budget add-on than intended, they are not reduced. The excess is taken out of the third pie, the flat-dollar pie. (In this tactic, the loss of a degree of freedom, usually imposed on percentage
increases, is imposed on the size of the third pie. This pie must therefore be a residual of the first two pies, assuming that the overall budget increase is a fixed amount.) The rationale for this feature is that those faculty members who have high merit should not, for purposes of the percentage-based merit increase, be penalized solely because they have relatively high base salaries. (It should also be noted that the second pie could be smaller than "intended," if high merit scores tend to go with low base salaries.)

The cost-of-living increase pie (PIE(1)) is fixed at a given value, and it cannot be changed (line 2220).

The various pies are calculated in lines 2190-3240. Note, again, that the third pie is a residual of the first two. The method used to calculate the second pie is to obtain the average salary increase that will be based on pure merit, and then to multiply this amount by the size of each department, so that we have a "percentage point pie" for each department that is allocated on the basis of merit scores. The second component of one's salary increase, then, is the merit score times the percentage point pie times the current salary. The second component of all salary increases is listed separately.

The residual flat-dollar pie, PIE(3), is allocated as follows: Proportional merit scores are multiplied by the proportional size of a given department, and this product is multiplied by PIE(3). This third component of all salaries is listed separately.

Salaries for the coming year are then calculated (lines 3410-3660) by adding, within each department, the current salary of a given faculty member, the cost-of-living increase, the percentage-based merit increase, and the flat-dollar increase. These salaries, followed by overall percentage increases in salaries, are printed out separately. Percentage-based merit increases and flat-dollar increases are also printed out separately. The salary-modification process for any individual can be traced by following that individual from one data matrix to the next. For instance, the upper-left entry for each matrix refers to the first individual listed for department 1, etc.

In any instance where the administrator wishes to over-reward a given department, this should be done by increasing the department's share of the residual flat-dollar pie (PIE(3)), primarily because the other two pies are considered sacrosanct. (It is helpful that the third pie is a relatively new invention.) At the point where the third pie is allocated among departments (lines 2930-2990), a given department may be over-rewarded by arbitrarily "adding" to its size. A 7-person department, for instance, deemed to have the productivity of an 8-person department, could be rewarded by merely raising its size constant, called k82, by one unit. (Fractional values may also be used.) This method has the effect of rewarding that department by giving it a larger part of the third pie, while allowing all other departments to absorb equally this instance of over-reward. The method has the virtue of making the administrator's action in rewarding or punishing given departments completely clear and understandable.
Further, it can be used in a way that emphasizes the “carrot” rather than the “stick.” That is, over-reward may be the only technique explicitly used, so that the most under-rewarded departments would be those that rarely receive an over-reward and are thereby constrained to help absorb a series of over-rewards going elsewhere.

The output listing for this program requires more than 500 lines; only a few excerpts will be shown here. I will provide the complete listing for anybody who wishes to see it. Notice that I have selected a year that reflects prosperous times.

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MTB > NAME C13 'MERIT3'
MTB > NAME C14 'MERIT4'
MTB > NAME C15 'MERIT5'
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MTB > note
MTB > note : Check sums of merit scores, all equalling 1.0.
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MTB > SUM 'MERIT2'
SUM     =       1.0000
### WHAT'S WRONG WITH THE SOCIAL SCIENCES?

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MTB > note

MTB > note

MTB > note: Enter current salaries within each department.

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: List salaries for next year.

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K91 4399996

: Check that k91 equals k31, that is, total of all salaries equals the total budget.
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</table>

: Calculate total percentage increases for each dept.
Chapter 6
CULTURAL MATERIALISM IN THE SYSTEMIC MODE

The first part of this chapter suggests that functional analysis, also known as general systems theory (Turner, 1991:93-130), remains one of the more vigorous theoretical traditions of the contemporary social sciences. “Cultural” functionalism, as understood by theorists such as Thompson, Ellis, and Wildavsky (1990), has many features in common with recent versions of functionalist theory, and it is possible to combine these approaches in a way that resolves many of the alleged weaknesses of the functionalist paradigm. The second and third sections develop the thesis that the systemic emphasis of demography and human ecology, which involves interaction among the POET variables (population, organization, environment, technology), provides a natural starting point for an integrated functionalist/systemic mode of analysis. A partly counterfactual study of the history of the American railroads provides an extended illustration of the utility of the neofunctionalist point of view.

(1) An excursion into “cultural” functionalism

Objections to teleology may be partially based on the assumption that “purpose” must necessarily involve some conscious intent, but one can very well speak of a “teleology of function” without implying such intent. The function of the distinction between /t/ and /d/, for example, is to keep morphemes distinct, but this does not mean that speakers are necessarily consciously aware of it.


Toward the end of 1988 I received a manuscript by Thompson, Ellis, and Wildavsky—hereinafter referred to as TEW—on the history of functional analysis and its prospects for the future.¹ I found this work to be most provocative for several reasons: First, what these authors say regarding the strengths and weaknesses of functional analysis closely parallels my own arguments in Dynamic Functionalism (Faia, 1986)—referred to below as DF. I believe that TEW and I have arrived independently at similar conclusions about how to resolve difficult questions of teleology, requisite analysis, social change, etc., within a contemporary functionalist framework. Second, we have arrived at a common understanding of functional analysis despite the fact that we have sharp
disagreements about the proper central orientations of the social science disciplines. In Wildavsky’s work, especially that done in collaboration with Mary Douglas, I discern a large emphasis on social constructionism, a highly ideational and mentalistic approach to topics such as the ways in which environmental hazards threaten human health and tranquillity (Wildavsky, 1988). My own preferences tend toward the “cultural materialist” perspectives of scholars such as Marvin Harris (1979). As in the case of Harris, I strive to reconcile my work with the Marxian traditions, and this effort is apparent throughout DF.

In the rest of Section (1) I shall make a series of comments on various parts of TEW’s work on functionalism. I believe that most of these comments will elaborate and elucidate the claims made above.

(1.1) An emergent consensus

There are at least four major points on which TEW and I agree:

First, TEW say (1990:105) that in recent years it has become fashionable to dismiss functionalism as static, conservative, tautological, irrelevant, and so forth, with little or no justification. Some critics, they maintain, have tried to account for the dominance of the functionalist perspective by pointing to its alleged utility as an ideology of rule, to its role in maintaining the “colonial commitments” of various Western nations, or to its role in maintaining the “social legitimacy” of the middle class (1990:105). These critics, in other words, have offered a functionalist explanation for functionalism itself—an early instance of reflexive sociology that was surely not committed to maintaining the intellectual status quo. I mention a similar irony in DF (1986:21), where I review Kingsley Davis’ Presidential address before the American Sociological Association: In the process of criticizing functionalism, Davis (1959:768) offers a functionalist theory to explain the rise of functionalism as a highly successful academic pursuit. TEW find the irony delightful, I suspect, because they believe, as I do, that most critics of functional analysis end by conducting precisely the sort of analysis that they claim to eschew. In effect they are closet functionalists, and they themselves may not comprehend the truth about what they have become.

Second, TEW (1990:107) and I share the belief that the old lists of functional “requisites” were trite, and surely did not further the interests of those who advocated functionalist theory. In the opinion of TEW,

The result of this search [for requisites] was a banal list of (largely definitional) functions that every system must perform. It is not very illuminating to say, as Parsons does, that a “functional imperative” of every society is that it be integrated, i.e., have a means for regulating internal conflicts. If, moreover, one attempts to turn this largely definitional statement into a testable hypothesis—for example, in order
to cohere, relations between individuals must be well-defined—then it becomes demonstrably false.

I argue in *DF* (Chapter 1) that traditional requisite analysis should be abandoned altogether, regardless whether we analyze whole societies, ecosystems, organizations, cultural traits, personalities, or whatever. In working toward this conclusion I found the banality argument to be highly compelling, but I was far more firmly convinced by Fogel (1964), who provides an excellent illustration of how one might go about testing the hypothesis—more accurately, the conventional wisdom—that in nineteenth-century America the railroads were “indispensable.” As it turns out, Fogel does not really indulge in requisite analysis. His work has immense value, however, for functionalist/systemic sociology, and it will be discussed in detail in a later section of this chapter. The general utility of Fogel's style of counterfactual history, especially for social scientists interested in testing functionalist hypotheses by means of computer simulations, is discussed in Faia (1989a).

Third, TEW’s section on Comte argues that a major difficulty of his work was his insistence that whole societies be taken as the dependent variable, for most social science analyses. But it is precisely those forms of functional analysis that focus solely on whole societies, contend TEW (1990:114), that produce banalities (the manuscript version says “tautologies”) such as the lists of requisites mentioned earlier. It must be pointed out in his favor, however, that Comte advocated a nomothetic approach to social analysis, as contrasted with Montesquieu's drift toward idiographies (TEW, 1990:113), and that Comte also advocated careful attention to taxonomies despite his interest in global social entities. It appears, then, that Comte maximizes our opportunities to pursue either the general systems form of functionalism or TEW's brand of culture-functionalism: Both structures and cultural traits will be found at various locations in a good taxonomy, and it is encouraging that TEW recognize (1990:103) that sociology gets into lots of trouble when it fails to clarify its typologies, i.e., its taxonomies.

Fourth, TEW include both Montesquieu and Weber as major exemplars of the functionalist traditions. For the early years I tend to cite Smith and Malthus rather than Montesquieu, but I never had the courage to claim Weber explicitly—despite the fact that my intuitions have always told me to do so. I don’t know Montesquieu well, but I once found in *The Spirit of the Laws* what I regard as an excellent functional theory about why parliaments tend to be heavily populated by lawyers. Weber's significance in the history of functionalism seems to tie in with my earlier remarks about ironies: There are major Weberian scholars abroad at this moment who pride themselves on having moved beyond the outmoded traditions of functionalist theory, and who yet do not understand, as TEW do, that *Verstandnis* may be pursued effectively within a functionalist framework. Sica (1988:210), for example, discusses Weber's insights into the
persistence of narrow managerial control of firms, but does not recognize these insights as being essentially a functionalist theory.

(1.2) Sub-cranial worlds and other pitfalls

I turn now to the major issue on which TEW and I do not agree—their overemphasis on Mind, and on socio-psychological phenomena.

As I suggest above in reference to Weber, I am bothered by TEW's strong focus on mental events, e.g., their ubiquitous arguments about “consensus” and about its alleged effects in keeping society together. These arguments start with the discussion of Comte and continue in the Parsons section. In the latter the authors assert that the United States coheres well despite the fact that there are major forms of disensus. But one could argue, for instance, that even the most vehement pro-life or pro-choice advocates in the United States have far more in common than is implied by the fact that they may be assaulting each other at the entrances of abortion clinics. They have a common culture. They may cooperate through various forms of exchange such as impersonal market transactions, by forcing legislatures and courts to seek a popular consensus about means of fertility regulation, by encouraging researchers to seek more effective means of contraception, by signalling their own intention—conscious or otherwise—to use a confrontational style that will avoid violence, etc. TEW do not address this sort of question adequately; their analysis implies that the issues are so complex as to be beyond solution.

After reading the TEW section on Marx (1990:147-60) I am convinced once again that an ethically-oriented cultural materialism, shorn of Hegelian notions about inexorable dialectical forces and allowing an important (though not predominant) role for Verstandnis, provides a grist of high quality for the functionalist mills. It is encouraging that Harris usually describes himself as a non-dialectical materialist, and it is also encouraging to realize that TEW (1990:154) overgeneralize when they say that Marx and Marxism posit a universal “world-historical” triumph of capitalism: It is entirely possible, for instance, that capitalism will not triumph, despite a strong attempt, as a feature of Mexican agriculture (Dumont and Mottin, 1982:17-78). And my volume on functionalism contains many examples that raise doubts about the TEW assertion (1990:148)—really a postulate—that “... assigning primacy to technological-material forces is misleading. ... The human controls the nonhuman, not the reverse. Technologies come and go, but the viable ways of life are always with us.” One of my favorite contrary examples occurred to me recently: There are interactions now taking place among the earth's atmosphere, biosphere, and hydrosphere that humans clearly do not control despite the fact that they involve technology, and these interactions, perhaps creating “greenhouse effect,” could ultimately thrust us into grave difficulties. Interactions
of a global magnitude can be understood properly from a functionalist point of view, and they may involve both equilibria and disequilibria.

I would not have mentioned this example, however, if I had not discovered that Douglas and Wildavsky (1982:63) cite with approval the work of James Lovelock, an atmospheric scientist. Lovelock may think that the goddess Gaia will ultimately protect us, but his theories and those of other scholars such as Stephen H. Schneider and Randi Londer (1984) provide many examples in which the human psyche clearly is not in the driver's seat, in which the Parsonian cybernetic hierarchy is inoperative, and in which there are objective risks that may result in massive eco-catastrophes whether or not we distort these risks through social construction. I fear that Wildavsky, Douglas, Parsons, and Lovelock all have been inspired indirectly by Bishop Berkeley, and that at the apex of their cybernetic hierarchies we find the Deity; we find, as TEW say in the manuscript version of their work (1988, Parsons section, p. 2), that in the Parsonian scheme as in that of Parson Malthus our standards seem to “... descend from on high ...” Incidentally, Schneider and Londer (1984:235) say that Lovelock's work has a “... philosophical—perhaps theological—significance ...,” and one can only hope that the Gaia hypothesis will evolve into a formulation of greater scientific utility. A fine example of this sort of evolution is provided by James (1987a; 1987b), who points out that “... wartime rises in sex ratio ... have hitherto defied explanation: indeed it has been acknowledged that divine intercession is as good an explanation as any ...” (1987a:734), but who then suggests (1987b:876) that high sex ratios during wartime may be associated with “the coital excesses of returning servicemen ...” The latter hypothesis strikes me as being far more down to earth.

There is a similar issue, raised in TEW's Marx section. Under socialist theory, contrary to TEW's view (1990:150), it is not anticipated that we will witness “... the replacement of latent functions with manifest functions.” Cubans, for instance, must study sugar production functionalistically; if they do not do so, the resulting ignorance will invariably get them into trouble. I anticipate that socialist revolutions (and “free-market” revolutions), wherever they occur, eventually will create many job opportunities for functionalist/systemic marxist sociologists; there will surely be enough false consciousness to go around, and substantial numbers of latent functions will be discovered. In B. F. Skinner's Walden Two by contrast, very few functions are ever made manifest: Only Skinner's alter-ego, sitting on a hillside above the idealized community, understands how things work—and he (the character, if not Skinner) keeps all insights to himself. We Marxian functionalists intend to be a part of the community, and we hope to be teachers. Given their sophistication, TEW will surely wish to join us.

It is clear, then, that we are dealing with differences of degree. Those who are exploited by the ruling class have only a partial understanding of class-based ideologies of rule: They do not understand how these ideologies result in that very exploitation of which they are the victims. This misunderstanding is called
false consciousness, and around page 7 of *DF* I provide many examples of functions manifest primarily to elitists. Marxists have no qualms against saying that “wants” may not be the same as “needs,” and that we may have a sense of fulfilling wants while at the same time not realizing the extent of misery due to the frustration of basic human needs. These frustrations are not unlike risks: They can threaten us even when we do not recognize their existence. Presser (1974) and Zelnik (1979), for instance, make it clear that many teenagers are highly at risk of unwanted pregnancies without knowing it and without knowing the degree of misery that may be theirs as a consequence of these pregnancies.

In TEW’s discussion of Merton (1990:199-200), the question of manifest versus latent functions is confused even further:

The main difficulty posed by the concept of latent functions lies in demonstrating a feedback loop between the consequences of an act and the act’s causes. When the consequences are intended, the feedback loop is unproblematic: action X furthers system Y, and actor Z takes action X in order to further system Y. But how can unanticipated consequences sustain a social system? If an action’s consequences for the system are unintended and unrecognized, what impels people to continue to act in this way? The analyst who wishes to demonstrate a latent function seems to have to posit a “group mind,” i.e., to endow institutions with human qualities of intention and purpose.

Why should feedback loops provide a special problem? Feedback loops involve reciprocal causation, and reciprocal causation has been reasonably well understood by statisticians since the days of the geneticist Sewall Wright. We should all learn the statistical theory a lot better, but the theory is definitely in place. Beyond that, *DF* contains any number of instances in which circular causation operates independently of actors' intentions and perceptions, in which functions remain essentially latent. TEW’s argument would make no sense to the linguist who provides the epigram at the head of this chapter. These authors themselves—irony again—provide an excellent illustration: Distorted comparisons of the United States and, say, the Soviet Union may serve various political interests (i-functions) and may also have something to do with maintaining political stability and the survivorship of various social arrangements (s-functions). And scholars such as TEW, producing and perpetuating these distorted comparisons, may receive positive feedback, positive reinforcement, from several sources including the political interests served. Stryker (1990:694-95) provides an excellent example, pointing out that

... NLRB [National Labor Relations Board] and SSB [Social Security Board] economists played different roles in early implementation of New Deal legislation. While NLRB economists pushed the NLRA toward its
potential for undermining market logic and even U.S. capitalism itself, SSB economists took the lead in ensuring that social-security legislation would strongly reinforce both American capitalism's markets and its traditional market ideology. In 1940, in a rider to a supplemental appropriations bill, Congress abolished the NLRB's economic unit.

Thus, we have a feedback loop that is not generally recognized, and it is therefore an instance of a latent function.

It is therefore inappropriate to claim, as do TEW (1990:169), that The Protestant Ethic is non-functionalist because it is concerned primarily with unintended consequences. Stryker's study deals with unintended consequences, and it is explicitly functionalistic. Functionalist explanations do not presuppose intentions, and they do not presuppose that anybody (beyond a few social scientists) knows about a given function. Intentions and understandings may not exist at all, they may exist in the unconscious minds of some members of society in a form that psychologists would call unconscious affectivity or unconscious cognition, or they may be largely manifest. Both manifest and latent functions, of course, are ideal types: As for the former, no knowledge is perfect or shared universally; as for the latter, the moment we discover a latent function and begin to talk and write about it, it is no longer latent by definition. But for long periods of time the etic knowledge of the social sciences may remain known only to a few—those who are lucky enough to be social scientists.

I allude to Freud and the unconscious because a large part of this discussion reminds me of that ancient staple of graduate seminars, the Freud versus Malinowski debate. My recollection is that whenever Malinowski demanded that competition between fathers and sons be explained in sociological terms, Ernest Jones (representing Freudian theory) would insist that the oedipus complex, instinctively based, is an adequate explanation. When Malinowski then demanded evidence for the existence of this instinct, Jones invoked a sliding infinity of regressions, maintaining that the oedipal instinct may be buried so deeply in the unconscious that it should be regarded as an unobservable. Malinowski, of course, called it a metaphysical construct. We fledgling social scientists, while realizing the manifest and latent functions of siding with Malinowski, may actually have found his explanation to be the more plausible. In any case the debate made it clear that social structures, although they must be demonstrable in some way, may not be widely recognized and need not be motivated by conscious or unconscious human volition. As Harris and Ross show (1987), we learn next to nothing about female infanticide as a potential fertility regulating device when we ask people about the motives and understandings that underlie this practice.

In their Stinchcombe-Elster section (1990:200-209), TEW surprise their readers by abandoning the notion that all functions must have elements of intention and/or understanding. Elster seems to support their position, but
Stinchcombe completely undermines it with the concept of social selection modelled on natural selection. Natural selection works entirely without intention or design, and social selection is alleged to have the same characteristic. TEW have trouble with this idea, and their position becomes highly ambiguous: They discuss (1990:202-03) a hypothesis about the “survival” of members of society who either do or do not engage in what Veblen called conspicuous consumption, and it is implied that people in this situation do not always realize that survival—whatever the term means in this context—is at stake. In DF, as I’ve said, there are many examples of functionalist explanations that do not involve affect or cognition. And in a discussion to follow, I argue that Fogel's (1964) study of the railroad and its alternatives, in nineteenth-century America, arrives at a functionalist explanation that cites intentions and understandings primarily to show that they had little to do with the realities of the situation.

Although the TEW history does not include Pareto, whom I find to be of immense value as a source of functionalist ideas and hypotheses, it does present a discussion of Spencer. As I suggest in an appendix to DF, Spencer remains excellent reading for those who wish to get a handle on early modern functionalist theory. Again, since I consider the search for “feedback loops” to be a crucial aspect of functionalism, I consider Spencer's formulations to be a crucial part of the history of this form of theory. As an example of Spencerian functionalism, TEW (1990:118) cite the hypothesis that societies with high levels of sexual promiscuity have tended to produce fewer and weaker offspring, and have tended as a result to disappear. Whether or not this hypothesis is correct, we encounter here the idea of dysfunctions that remain latent, unrecognized, unmotivated, and we also encounter the idea of social selection as a form of feedback. It should be remembered that Spencer regarded natural selection (“survival of the fittest”) and social selection as more or less identical processes, and he therefore implies that we need not worry unduly over questions of teleology and/or motivation. Social selection can be latent in the same way that natural selection is almost invariably latent.

(2) The POET paradigm: A transcendence of hierarchy

Although Namboodiri's (1988) defense of classic ecological demography is highly compelling, I believe that he misses several opportunities to demonstrate even more strongly the power and utility of this neglected perspective. My own position on this question has been worked out in some detail in a separate paper (Faia, 1989b), and holds briefly that the strength of ecological demography lies in its having developed conventional definitions—what Levine (1985) would call univocal definitions—of many of its key concepts; in its ability to exploit deterministic models that explain essentially all the variability of certain social phenomena, as in the explanation of labor force composition (and its changes) with reference to fertility, mortality, migration, and social mobility; in its
sophisticated level of theoretical development, involving Malthusian and neo-Malthusian perspectives, Marxism, optimal population theories, demographic transition theory, the stable population model, various socio-psychological theories of vital phenomena, decision theory, functionalist/systemic analysis, and so forth.

The POET acronym refers to the interaction of population, social organization, environment, and technology. In cause-effect terms, interaction may run both ways: Climate, for instance, may affect social organization, and social organization may have an impact on climate (see, for instance, Schneider and Londer, 1984). And these interactions, of course, are not limited to two elements. Phenomena such as greenhouse effect involve complex interactions among several population variables, several organizational variables, and many environmental and technological factors. If we assume that ecological variables may be selected from any combination of the four POET categories, we discover that there are 25 areas in which we might develop ideas about multivariate interactions. Human population, for instance, might affect O, E, or T; or human population might affect an interaction of O and E variables, or O and T variables, or E and T variables; or it might affect an interaction involving variables drawn from all three of the other sectors. When all such interaction prospects are added up, they total 25.4

The two-way character of social and ecological interaction is a matter that cannot be ignored.5 In general, it implies that no variable has a predominant role as a cause, and none acts consistently as an effect. In fact, the reference above to the many multivariate interactions among the POET variables implies that, in many cases, we are not explaining things from causes to single effects: We might, for instance, try to determine whether an alleged causal agent (or several causal agents) has an effect on the interaction of two other variables. If we are interested in the interaction of the human population with computers, for instance, the idea of two-way causation reminds us that we must not assume a priori, with TEW, that “... the human controls the non-human ...” From an ecological standpoint it is true that families identified by the Bureau of the Census (1988) as having personal computers in their homes may have acquired these machines because “they wanted to do so,” but members of these families will probably be changed by computers in ways that they cannot anticipate. If we ask, for instance, how the nature and types of computer use in a community's schools and offices influence the relationship between sex and computer use among the children of these families, we are raising a question that renders the TEW claim unintelligible.

(3) Reciprocal interaction: Organization and technology

Fogel's study (1964) of the American railroads is a prime illustration of the preceding arguments: It shows us (1) how functionalist/systemic analyses are conducted when they avoid the vagaries of requisite analysis; (2) what the POET
paradigm is able to accomplish when it is applied to a large ecosystem; (3) how simulation models help us to understand social development, i.e., the ways in which ecological systems select among structural alternatives.

(3.1) Wagons, waterways, and William F. Ogburn

Tables 1 and 2, based on data available in Fogel (1964, Tables 3.7, 3.12, 3.13, and 3.14), provide a summary of the study's main arguments. Fogel's intention was to measure the economic impact of the railroads as compared with hypothetical (i.e., counterfactual) canals and wagons, which could have been constructed instead of railroads. Counterfactual history, incidentally, makes heavy use of phrases such as "could have been" and "would have been," and these expressions capture clearly the essence of this approach to historical analysis. As Fogel (1964:10) says, "evaluation of the axiom of indispensability ... requires not only an examination of what the railroad did but also an examination of what substitutes for the railroad could have done." TEW would be pleased by the idea of substitutability, which necessarily raises questions about the existence of prerequisites: If tea makes us just as happy as coffee, then coffee is not a prerequisite.

Table 1. Replication of Fogel's Estimate of the Loss in National Product due to Substitution of Canals for Railroads (thousands of dollars)

<table>
<thead>
<tr>
<th>State or Division</th>
<th>Value of farmland beyond region, before canals</th>
<th>Value of farmland beyond region, after canals</th>
<th>C1</th>
<th>C2</th>
<th>C3</th>
<th>C4</th>
<th>C5</th>
<th>C6</th>
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WHAT'S WRONG WITH THE SOCIAL SCIENCES?

Kentucky    178670     1893  1.06      1893   1.06       119    119.00
Tennessee   123633     7936  6.42      7936   6.42       475    475.00
Alabama      32795     1223  3.73      1223   3.73       102    102.00
Mississipp   51166      342  0.67       342   0.67        33     33.00
Louisiana    42901          0  0.00         0   0.00         0      0.00
Texas       265841   145248 54.64     73854  27.78     13261   6742.80
Arkansas     43327     2224  5.13      2224   5.13       201    201.00
West-Div    800952   218216 27.24    218216  27.24     19919  19919.00
U.S. Total 8120572  1942537 23.92    571447   7.04    153372  47453.83

Tabl-3.13    x 000
Prelim      248100
Chg in L   -105918
Chg a-bar    16100
Chg ind-c    16600
Total       174882   Pct GNP  1.46

Source: Fogel (1964, Tables 3.7, 3.12, and 3.13)

The part of Fogel's analysis that I wish to discuss deals with the intra-regional distribution of agricultural products, i.e., how farmers got their livestock and crops to the primary markets in cities such as Chicago or St. Louis. Interregional distribution systems—from, let us say, a primary market in Chicago to a secondary market in Boston—and non-agricultural products are treated elsewhere in Fogel's book. Each of my tables provides data for the several states and divisions indicated in the leftward column.

Table 2. Replication of Fogel's Estimate of the Effects of New Canal Construction and Reduced Wagon Rates on Loss in National Product (thousands of dollars)

<table>
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<tbody>
<tr>
<td>Reduced C4, as % of national product</td>
<td>Reduced loss in national product due to improved wagon rates, .5*C4</td>
<td>Reduced loss in national product before canals and improved road tran., (CR4/C2)*C6</td>
<td></td>
</tr>
<tr>
<td>CR4</td>
<td>CR5</td>
<td>C6</td>
<td>CR7</td>
</tr>
<tr>
<td>2819</td>
<td>0.26</td>
<td>331</td>
<td>165.50</td>
</tr>
<tr>
<td>58933</td>
<td>10.57</td>
<td>8452</td>
<td>4226.00</td>
</tr>
<tr>
<td>10103</td>
<td>1.52</td>
<td>1332</td>
<td>666.00</td>
</tr>
<tr>
<td>9075</td>
<td>1.88</td>
<td>2587</td>
<td>605.34</td>
</tr>
</tbody>
</table>
I shall define and discuss the columns of Tables 1 and 2 for the state of Indiana (fourth data row) because it appears that new canals and better wagon transport would have had a relatively large impact on this state. For Table 1:

(1) Column 1 (C1) tells us that the estimated value of all farmlands in Indiana for the year 1890 was $481,684,000.

(2) Column 2 shows that the value of Indiana farms that did not have access to adequate wagon/canal transportation was $38,783,000.

(3) Column 3 gives Column 2 as a percentage of Column 1: Just over 8 per cent of Indiana farms (by value, not number) did not have access to wagons/canals.

(4) Column 4 shows us that, if various proposed waterways had been constructed in Indiana, the value of farmlands that still would not have...
had access to these waterways would have dropped from $38,783,000 to $18,150,000.

(5) Column 5 shows that, with new canals, only 3.77 per cent of Indiana farms (by value) would still not have affordable access to this means of transportation.

(6) Column 6 estimates that, without railroads and prior to the construction of new canals, the farms represented in column 2 would have been economically unfeasible, reducing Indiana's contribution to the yearly national product by $2,587,000.

(7) Column 7 estimates that, without railroads and after the construction of new canals, fewer of the Indiana farms represented in column 2 would have been economically unfeasible. This would have reduced the national product by only $1,210,690 rather than the earlier $2,587,000. Notice that Fogel's (1964:99n) definition of this column is incorrect: It neglects the division by C2.

For Table 2:

(1) Column R4 (reduced version of the earlier column 4) indicates that if expanded waterways had been combined with more efficient wagon transportation, the value of Indiana farms that would still not have ready access to markets would drop to $9,075,000, compared with the earlier $18,150,000. The latter estimate was based on the assumption that expanded waterways would not have brought about more efficient road transportation. As we shall see, Fogel does not accept this assumption; nor would Ogburn; nor does functionalist/systemic theory (Volti, 1988:5).

(2) Column R5 gives column R4 as a percentage of column 1, Table 1. It shows that, with new canals and more efficient road transportation, only 1.88 per cent of Indiana farms (by value) would have remained unfeasible, compared with the earlier estimate of 3.77 per cent, which ignored the possibility of improved wagon transportation.

(3) Column 6 is copied from Table 1, for reference. Again, it shows loss in national product, absent railroads and expanded waterways.

(4) Column R7 tells us that if Indiana had had the benefit of new canals and better wagon transportation, only $605,000 worth of agricultural production would have been lost for lack of access to markets, as
compared with $1,210,690 under the assumption of new canals without improved wagon transportation, and $2,587,000 under the assumption that neither better road transportation nor better waterways would have been made available.

The same data are provided for all other states and regions listed in the tables. At the foot of Table 1 is Fogel's Table 3.13, which calculates the proportion of national product that would have been lost if canals had replaced railroads. At the foot of Table 2 we find calculations for Fogel's Table 3.14, which shows the proportion of national product that would have been lost if canals and better road transportation had replaced railroads.

Let me anticipate my explanation of these calculations by saying that Fogel regards a .98 per cent reduction of national product—the bottom line of Table 2—as trivial, since national product was growing by something like 3-5 per cent per year. His general conclusion, then, is that the United States would have had to absorb a lag in economic growth of only a few months' duration if the railroads had been replaced (or displaced) by wagons and canals (Fogel, 1964:47n). Let me also anticipate another point: In Tables 1 and 2 most of the columns contain raw data, the only exceptions being the columns with percentages and columns C7, CR4, and CR7. This means that, using a spreadsheet, we might experiment with changes in columns C1, C2, C4, and C6. And since CR4 involves a formula with a multiplier (.5) that represents Fogel's rough estimate of the effects of cheaper wagon transportation, this formula also would allow simulation experiments. In a later section, I'll present an experiment involving CR4 of Table 2.

Fogel's (1964:100,110) calculations for Tables 3.13 and 3.14 assume that substituting the existing waterways, as of 1890, for railroads would have cost the nation (according to his “beta” estimate) about 221 million dollars, plus another 27 million dollars due to additional cargo losses, slowness, and the seasonality of water transportation. (In my table, the total comes out to 248.1 million dollars.) Some of these losses, however, could have been avoided by expanding the canal system. In Fogel's Table 3.13, losses are thereby reduced by 106.1 million dollars; due to rounding error, my figure is 105.9 million dollars. After we add small adjustments for changes in the size of the feasible farming region and for additional indirect costs, the substitution of waterways for railroads costs about 175 million dollars per year, or about 1.46 per cent of the national product of around 12 billion dollars. Similar calculations for Table 3.14, assuming new canals and cheaper road transportation, place the loss in national product at 118 million dollars, around 1 per cent of the national product.

But there is a big question here: How cheap could highway transportation have become? William F. Ogburn (1964) showed that when technological innovations diffuse through a society, they are not used in exactly the same way by all adopters. For instance, once the internal combustion engine became readily available and highly efficient, an infinitude of unanticipated uses for it were
discovered. Similarly, an innovation such as expanded inland and intercoastal waterways would have had ramifying effects on other parts of the North American ecosystem. Fogel (1964) alludes to this possibility in several parts of his book: On page 15: “The axiom of indispensability proceeds on the implicit and unverified assumption that the success of the railroads did not choke off the search for other solutions to the problem of overland transportation.” On page 20, in purely Ogburnian phrases: “Forcing the pattern of shipments in the non-rail situation to conform to the pattern that actually existed is equivalent to the imposition of a restraint on society's freedom to adjust to an alternative technological situation.” Page 91: “The discussion thus far has been based on the assumption that in the absence of the railroad all other aspects of technology would have remained fixed in the 1890 pattern. This is a severe assumption.” And the following twenty pages or so discuss the prospect of building better waterways and roads, a more rapid development of the internal combustion engine, etc.

Often designed to be counterfactual, simulations allow us to explore these hypothetical prospects. In one series of experiments I modified the data for four areas listed in Table 1 where there seemed to be little chance of improved waterways (column 5). Each of these states or regions (South Atlantic Division, North Dakota, Texas, and the Western Division) had large areas of farmland remaining without access to waterways even after a substantial (hypothetical) improvement of the waterways. I found that under highly optimistic assumptions about the possibility of new canals in these four areas, Fogel's estimate of a 1.46 per cent loss in national product would have been reduced to about 1.19 per cent—not an impressive degree of elasticity. In another experiment, summarized in Table 3, I show what would have happened to the national product if the multiplier used in column CR4 were changed from .5 to .1 as a result of a larger reduction in the cost of wagon transportation than that contemplated by Fogel—again, this is a highly Ogburnian experiment. Such a large reduction would have allowed farmers to get their products to the nearest waterways at a relatively low cost. Examining Table 3, we see that this change would have reduced losses in national income from .98 per cent to .82 per cent. This is a surprisingly small change, and it suggests that extraordinary improvements in roads and in the efficiency of internal combustion engines would have had a small impact on intraregional agriculture as a component of national income. To be realistic, however, cost-benefit calculations for intraregional agriculture would have to be combined with similar calculations for other segments of the economy dependent on efficient short-distance hauling.
Table 3. Replication of Fogel's Estimate of the Effects of New Canal Construction and Reduced Wagon Rates on Loss in National Product (thousands of dollars), With Further Reductions of Wagon Rates

<table>
<thead>
<tr>
<th>Reduced C4, as % due to improved wagon rates, .1*C4</th>
<th>CR4</th>
<th>CR5</th>
<th>C6</th>
<th>CR7</th>
</tr>
</thead>
<tbody>
<tr>
<td>Reduced C4, as % due to improved wagon rates, .1*C4</td>
<td>CR4</td>
<td>Loss in national product, before canals canals and improved road tran., (CR4/C2)*C6</td>
<td></td>
<td></td>
</tr>
<tr>
<td>564</td>
<td>0.05</td>
<td>331</td>
<td>33.10</td>
<td></td>
</tr>
<tr>
<td>11787</td>
<td>2.11</td>
<td>8452</td>
<td>845.20</td>
<td></td>
</tr>
<tr>
<td>2021</td>
<td>0.30</td>
<td>1332</td>
<td>133.20</td>
<td></td>
</tr>
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<td>1815</td>
<td>0.38</td>
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<td>0.00</td>
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<td>0.43</td>
<td></td>
</tr>
<tr>
<td>1664</td>
<td>0.63</td>
<td>3610</td>
<td>112.29</td>
<td></td>
</tr>
<tr>
<td>1202</td>
<td>0.47</td>
<td>6499</td>
<td>95.70</td>
<td></td>
</tr>
<tr>
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<td>0.00</td>
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<tr>
<td>126</td>
<td>0.03</td>
<td>5539</td>
<td>10.13</td>
<td></td>
</tr>
<tr>
<td>1948</td>
<td>3.80</td>
<td>1808</td>
<td>180.80</td>
<td></td>
</tr>
<tr>
<td>327</td>
<td>0.41</td>
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<tr>
<td>2137</td>
<td>0.69</td>
<td>15328</td>
<td>175.05</td>
<td></td>
</tr>
<tr>
<td>2980</td>
<td>0.68</td>
<td>23143</td>
<td>249.70</td>
<td></td>
</tr>
<tr>
<td>189</td>
<td>0.11</td>
<td>119</td>
<td>11.90</td>
<td></td>
</tr>
<tr>
<td>794</td>
<td>0.64</td>
<td>475</td>
<td>47.50</td>
<td></td>
</tr>
<tr>
<td>122</td>
<td>0.37</td>
<td>102</td>
<td>10.20</td>
<td></td>
</tr>
<tr>
<td>34</td>
<td>0.07</td>
<td>33</td>
<td>3.30</td>
<td></td>
</tr>
<tr>
<td>0</td>
<td>0.00</td>
<td>0</td>
<td>0.00</td>
<td></td>
</tr>
<tr>
<td>7385</td>
<td>2.78</td>
<td>13261</td>
<td>674.28</td>
<td></td>
</tr>
<tr>
<td>222</td>
<td>0.51</td>
<td>201</td>
<td>20.10</td>
<td></td>
</tr>
<tr>
<td>21822</td>
<td>2.72</td>
<td>19919</td>
<td>1991.90</td>
<td></td>
</tr>
<tr>
<td>57145</td>
<td>0.70</td>
<td>153372</td>
<td>4745</td>
<td></td>
</tr>
</tbody>
</table>

Source: Spreadsheet experiment based on Table 2
(3.2) A functionalist overview of an ecosystem

Fogel's work provides strong support for TEW's (1990:114) suggestion, in reference to Comte, that functionalists are not likely to carry out their most effective analyses when they focus on whole societies (cf. Tilly, 1984:11-12) and the conditions under which these societies survive. It is arguable, of course, that the United States could not have achieved the degree of social integration necessary for nationhood without an adequate interstate and intrastate transportation system, but this insight has little value. The real question is: What sort of system, in what part of the country, why, when, and under whose ownership and control? As Fogel works at answering this question, he seems to be saying that while the railroads served the national interest well, there were no highly compelling reasons for selecting them rather than an alternative transportation system emphasizing some combination of waterways and roadways. Not only was the nation as an ecological entity not at stake as a result of this selection, but the economic consequences of the selection, while not at all clear, do not seem to have been critical: One can hardly argue that railroads were selected because of powerful economic advantages to the nation as a whole. In reference to specific interest groups, as we shall see, the story may be very different.

As we try to explain this selection—rather, the many selections involved in this historical process—it would be appropriate to examine public opinion, what I have called elsewhere the POETry of the mind. The railroads generated a large amount of public interest, and according to one scholar (Ward, 1986) those who supported railroads seemed to do so for the following reasons (notice that the first of these factors makes it clear that requisite analysis was not alien to the thought processes of the American public):

(1) The railroads would fulfill the hope of unifying the nation, and would overcome the fear of disunity and strife (Ward, 1986: Chapter 1). Various “railway writers” essentially made use of the Spencerian organic analogy in arguing this theme (Ward, 1986:17).

(2) The locomotive, far from being a filthy, noisy, monstrous machine, was in reality a benign Iron Horse that reflected many of the more positive features of American national character (Ward, 1986: Chapter 2).

(3) The railroads would add substantially to the military security of the nation (Ward, 1986: Chapter 3), to its ability to defend itself against invasion or insurrection.
CULTURAL MATERIALISM IN THE SYSTEMIC MODE

(4) The railroads would elevate the cultural and moral character of the nation (Ward, 1986: Chapter 4). One variant of this argument essentially proposed the Ogburnian hypothesis that the diffusion of railroad technology would stimulate all sorts of new inventions and discoveries (Ward, 1986:59-60). Another variant held that railroads would promote public health through the diffusion of knowledge, by bringing food to cities and fertilizer to farms (Ward, 1986:65), and by being constructed on high ground. Regarding the latter claim, Ward (1986:65) says that the belief that railroads would typically (or necessarily) be built on high ground was a misconception.

(5) The railroads would provide the lower-class strata with good service, while simultaneously providing a modicum of luxury for the affluent (Ward, 1986: Chapter 5).

(6) The railroads would have a large impact in raising land values (Ward, 1986: Chapter 6). Railroad promoters, however, “... understood that land values did not always rise equally along a road. ... often people opposed their projects on the grounds that they would aid some areas and not others, or that a particular city would benefit to a greater degree than the surrounding countryside” (Ward, 1986:85).

(7) Railroads would “... unlock the treasures of the West” (Ward, 1986:94).

If any of these assertions were supported by scientific evidence as strong as the public sentiments which, in Ward's view, often underlay them, they would constitute manifest functions. Therefore we must analyze the POETry of the mind both for its cause-effect role and because it is essential if we wish to identify manifest functions, i.e., to pursue an important dimension of the sociology of knowledge. In general, Ward's book discusses attitudes that tended to provide political support for the railroads; in his chapters 8 and 11, however, he introduces the intriguing possibility that the railroads may have had the unintended consequence of increasing the cultural emphasis on punctuality—the strict scheduling of activities by “railroad time” (Ward, 1986:107)—and of introducing the “corporate state image” in American society. In the similarly edifying chapters 9, 10, and 12, Ward qualifies generalization (1) above: Eventually the railroads entered a phase in which they competed furiously among themselves and against other modes of transportation; in these struggles, railroad company leaders often used war metaphors in discussing their relationships with competitors (Ward, 1986:155-69).

It is clear, then, that the major utility of public opinion in functionalist/systemic inquiries is that it provides hypotheses about interest
functions (i-functions) and/or survivorship functions (s-functions) for large social entities—perhaps an entire nation—or for smaller subsystems such as cities and towns. It is also clear that such hypotheses will often be untenable both because they are internally contradictory and because they may fail to survive systematic empirical testing along the lines suggested by Fogel. It is entirely possible, however, that in the early years of the nineteenth century powerful interests in the United States were solicitous about, say, military security, that these interests consciously and effectively promoted railroads as a way of allaying these anxieties, and that hypotheses about the tactical and strategic value of the railroads (during the Civil War, for instance) may have been essentially correct. If each of these generalizations turned out to be valid, we would be able to claim support for a series of functionalist hypotheses regarding i-functions, and these i-functions could be regarded as manifest i-functions among the appropriate segments of public opinion and among the appropriate interest groups.

I suggested earlier that historical studies of the railroads, counterfactual or otherwise, would show us, first, how functionalist/systemic analyses are conducted when they avoid the pitfalls of traditional requisite analysis; second, that they would show us what the POET paradigm is capable of accomplishing when it is applied to a large ecosystem such as the United States in the 1890’s; third, that they would enable us, through simulation experiments, to understand more clearly the historical development of the American railroads. My introductory remarks must now be qualified: It would be inappropriate to conclude that, because of the weaknesses of requisite analysis, all forms of survival analysis should be abandoned. There must have been thousands of instances in which farms failed to survive for lack of access to adequate transportation, in which industrial firms perished for the same reason; and scores of instances in which railroad companies fell into bankruptcy because of intense competition, in which firms providing other modes of transportation could not meet competition from their peers and from the railroads, and so forth. If one were to identify the circumstances under which these forms of organizational mortality took place, one would be conducting something akin to requisite analysis. But in this instance survival prospects would be measured in probability terms, not in terms of necessary and sufficient conditions. And, more importantly, this would be a form of analysis in which large numbers of small organizations would be the major focus, rather than small numbers of large organizations, e.g., entire societies (Marple, 1982).
NOTES (Chapter 6)

(1) The book has now been published; however, I wish to make a few comparative references to the manuscript version. This is not unfair, since the manuscript version appears to have been sent to a substantial number of scholars.

(2) I always felt uncomfortable with the older designation, structure-functionalism, and I do not believe I've ever used it extensively. The social sciences deal with many entities that may not be regarded as social structures, and there is no reason why these entities—perhaps cultural or personality traits, or perhaps linguistic forms—should not be explained. Furthermore, recall the earlier discussion of the irony involved in explaining functionalism functionally even while expressing one's dislike for it.

(3) An earlier version of section (2) is found in Faia (1989b).

(4) For n elements, the formula is as follows:

\[ C = \frac{3^n - 2^{n+1} + 1}{2} \]

where C represents the number of interaction possibilities.

(5) Aday (1990:53-60, 69-75) presents a theory of deviant behavior that holds that there is a “dialectical” (or reciprocal-causation) relationship between deviance and social control, such that declines in social integration bring about increasing violation rates, increased violation rates bring about increased intensity of social control, and increased intensity of social control restores a modicum of social integration. Social “attachment” and the learning of ways of carrying out violations may act as intervening variables in these reciprocal-causation processes. Aday mentions (1990:235) that his formulation uses the general functionalist causal model as set forth by Arthur Stinchcombe some years ago, but his diagrams of the theory employ a confusing graphic that obscures the relationship to Stinchcombe's work and also obscures the important fact that the algebraic product of his circular-causation (or dialectical) processes is always negative. In essence, then, we are dealing with a negative-feedback functionalist formulation. As is often the case, available data on topics such as violence (Aday, 1990: Chapter 8) do not seem to provide a very convincing test of the theory, especially its dialectical or reciprocal-causation properties.
REFERENCES (Chapter 6)


A former student of Mills and mine once published the following remark:

One of my favorite fantasies is a dialogue between Mills and Lazarsfeld in which the former reads to the latter the first sentence of *The Sociological Imagination*: “Nowadays men often feel that their private lives are a series of traps.” Lazarsfeld immediately replies: “How many men, which men, how long have they felt this way ...?”

I probably would have been marginally interested in such a count. But my real query would have gone in a different direction: What does Mills mean by “alienated”? ...

—Paul F. Lazarsfeld (1972:xvi-xvii)

... there has been little or no favorable change in our level of interest in problems of conceptualization and measurement.

—Hubert M. Blalock (1989:449)

Lord, give me the capaciousness and wit to tolerate and enjoy ambiguity when it is appropriate, the clarity of mind and firmness of will to be unambiguous when it's not, and the wisdom to know what time it is.

—Donald N. Levine (1985:220)

(1) Introduction

The major purpose of this chapter is to evaluate the semantics of the social sciences, their conceptual organization, by ascertaining whether the Lazarsfeld generalization-specification model, also known as the “elaboration model,” has been routinely used by social science researchers. If the Lazarsfeld model were widely exploited, then highly abstract assertions—e.g., “factors of type X tend to have a causal impact on variables of type Y”—would be tested for their
applicability to a broad range of specific instances of both X and Y. For example, the literature discusses a number of situations in which deterrence allegedly increases conformity; generalizations have been derived from these specifications, and the generalizations seem to produce new specifications.

Using the Sociological Abstracts thesaurus of sociological concepts, in combination with online and CD-ROM searches of the social science literature, this chapter assesses the extent to which researchers use systematic, hierarchic taxonomies and exploit them through application of generalization-specification strategies. Despite S4's listing of “broader” and “narrower” terms in relation to a given thesaurus entry, we shall see that there is little evidence of the use of highly structured taxonomies in the social science literature.

In a work of my own (Faia, 1986:84-88) I gave a large amount of attention to ways in which social science research makes use of taxonomies, or systems of classification. I was especially curious as to whether scholars use taxonomic hierarchies in a way that exemplifies the generalization-specification processes advocated by Lazarsfeld (1972a, 1972b) and others (e.g., Selvin, 1972; Lazarsfeld, Pasanella, and Rosenberg, 1972:119-25; McKelvey, 1982). I provided several illustrations:

The relationship between economic deprivation and the lynching rate, for instance, is merely one among scores of instances subsumed under the frustration-aggression hypothesis. The relationship between criminal court [or prison] overload and plea bargaining is a specific instance of more general queueing processes, and there are many specific instances of deviance/conformity theories. Other homeostatic or problem variables—among them the achievement of competitive advantage, maintenance of motivation, maintenance of bureaucratic legitimacy, information overload, maintenance of social boundaries—are highly abstract, and under such abstractions one hopes to place myriad instances.

The social science disciplines would benefit immensely, I suggested, if more attention were given to the creation, application, and development of systematic taxonomies comparable to those found in biology, chemistry, mineralogy, and other disciplines.1 To illustrate: Disparate phenomena such as male initiation rites, traffic citations, lynching, magic, revolution, and warfare have all been proposed as specific instances of a general relationship between frustration and aggression. Human fertility, to cite another instance, is a composite of the several factors that comprise the famous Davis-Blake “proximate determinants” (Bongaarts and Potter, 1983). As determinants of fertility, these variables have an important characteristic of a good taxonomy: They are exhaustive and mutually exclusive. Finally, pursuing the biological analogy further, McKelvey (1982:6) cites an example of taxonomic abstraction suggested by Konrad Lorenz:
Lorenz used the example of bodies passing speedily through a resisting substance, bodies such as a swift, a jet fighter, a shark, a dolphin, or a torpedo. Knowing its function and what a shark looks like, we are led to suppose, at a fairly abstract level, that a streamlined sharklike shape would be something to look for in the other organisms or objects.

One does not have to concentrate for long on analogies between birds and torpedoes to have a sense of indulging in a metaphysical conceit worthy of John Donne (Main and Seng, 1961:94-100). The idea that “hypertext”—a systematic means of producing what appear to be metaphysical conceits—may be an essential tool for searching through massive “read only” computer memories (Byers, 1987) again invokes the memory of John Donne and metaphysical poetry. McKelvey, too, makes an early mention of the utility of taxonomies in automated searching (1982:17). With the advent of database searching by computers, it is perhaps worth remembering that history's first computer programmer was Ada Byron, daughter of the famous poet; systematic taxonomies may be the place where science and poetry join forces, the place where Jacques Barzun's “mysteries” of art and science begin to find resolution.

Science, of course, cannot be satisfied with the deliberate ambiguity of poetry, its heavy use of metaphor, simile, allegory, irony, and its emphasis on connotative rather than denotative aspects of language. On the other hand, there is a lot to be said for the social scientist who realizes that when we speak of courtship practices that may precede marriage, and when we ask about the impact of these practices on later marital adjustment, we are really asking abstract questions about the “differential association” that precedes a “rite of passage” and establishes a new “primary group” with its various degrees of “adaptability.” Furthermore, the same sorts of processes occur in realms of social behavior that seem, at first glance, very different from courtship and marriage—a little like birds and torpedoes. For instance, we might study the differential association of two nations prior to the rite of passage of establishing an alliance, we might ask about the structural features of this new alliance, and we might ask whether earlier differential association has any appreciable impact on the durability and workability of the new alliance. In brief, we must have ways of discovering that hypotheses about marital stability may be applicable to the ANZUS Pact, and vice versa. Was the ANZUS Pact a mere marriage of convenience, or did it arise from a long-term commitment? Similarly, what is the nature of the balance of power that currently characterizes my neighbors' marriage? Barzun (1984:208-16) finds this sort of metaphor to be offensive—he calls it bad poetry, and even castigates Ada Byron for encouraging it—but metaphor is clearly essential to scientific progress.

In a textbook that trained a generation of social scientists, Goode and Hatt (1952:58) provide excellent examples of the uses of taxonomic hierarchies, from which I have selected one:
Principle: Rather extensive, but relatively unsystematized, data show that members of the upper occupational-class strata experience less unhappiness and worry and are subject to more formal controls than members of the lower strata.

Deduction: Our hypothesis would then predict that this comparison also applies to the marital relationships of members of these strata and would predict that such differential pressures could be observed through divorce rates. There should be an inverse correlation between class position and divorce rates.

Berelson and Steiner (1964) compiled a lengthy collection of broad generalizations similar to the “principle” cited above by Goode and Hatt, and it is an edifying exercise to deduce from these generalizations a range of specific hypotheses; a comparable compilation of generalizations based on disaster research was brought together recently by Drabek (1986; 1989). Similarly, one is able to read virtually any contemporary journal article presenting specific findings and to derive abstract generalizations from these specifications; the process, in other words, works both ways (Cohen, 1959:124), and therefore Dawkins (1986:13) is surely mistaken in the claim that “hierarchical reductionism,” in which one explains phenomena with reference to the “next lowest” level of a hierarchy, is an adequate form of explanation. In functionalist/systemic analysis, a highly abstract assertion that “Y is functional with respect to X” presumably would be applicable to a wide range of structures of type Y and organizations or individuals of type X. For instance, we now have in the literature highly abstract generalizations about the ability of various forms of deterrence to bring about conformity (Gibbs, 1975). These generalizations are based on appropriate specifications that range from the prospect of deterring robbers from murdering their victims to the prospect of deterring motorists from driving non-HOV's (high-occupancy vehicles) during rush hour; they can be used as part of an explanation.

For another instance, we turn to a paper by Bredemeier (1955:174):

We can ... be more rigorous methodologically by reformulating the incest hypothesis as follows: Certain status relationships are comprised (by cultural definition) of rights and obligations and sentiments which are psychologically incompatible with certain sentiments that might be associated with sexual relations. For example, the employer-secretary, professor-student, father-daughter, priest-parishioner relationships are conventionally defined in our society so as to make sexual relationships (as they are conventionally defined) inconsistent with performance of the defined responsibilities. We understand the incest taboo, then, as one of
a class of taboos, existing *because* the role players *have been trained* into responses incompatible with the sexual response.

To me, the most interesting feature of this statement is that it involves a conceptual hierarchy: Several specific situations are recognized as being subsumed under a broad category—the “class of taboos”—and ideas developed in one situation are tested for their applicability under comparable circumstances elsewhere. In recent years, scholars who work in the information sciences have been actively developing this sort of taxonomy for several fields. There is now a more or less standardized database query language, and it probably provides an excellent instance of Ogburnian diffusion. It was invented largely under the auspices of Richard S. Angell, who was chief of the subject cataloging division of the Library of Congress from 1957 to 1966 and who advocated use of taxonomic hierarchies—the BT, NT, and RT designations—for the L. of C. classification system.² It has diffused rapidly, and the hierarchic features of this approach are now incorporated into many of the thesauri published in recent years as aids to users of online information services (Chan and Pollard, 1988). Finally, as Ogburnian theory would lead us to predict, the special adaptive needs of various disciplines are expressed by these thesauri and by the databases themselves.

My hope, of course, is that we will organize the knowledge of our field in such a way that new hypotheses, such as those suggested by Bredemeier, will appear to be generating themselves spontaneously; with the advent of AI machines, this appearance will become a reality. This is what taxonomies accomplish. Truly innovative ideas may appear at first to be metaphysical conceits, but it should not take long for the sociological imagination to realize that Lorenz makes good sense when he suggests that sharks and airplanes have a lot in common, or to realize that the interaction of the Thames and its banks, elucidated by Michael Faraday, may be simulated by blood flowing through an artery, or to realize that successful international treaties may be preceded by long periods of differential association among nations just as successful marriages may be preceded by long periods of courtship. Apparently these distant analogies had a special appeal for Blalock (1991:405), who used to try to get graduate students to invent them:

Students were asked to review and synthesize relatively abstract bodies of theory appropriate to two or more substantive bodies of literature. For example, one could consider power and social control mechanisms as they applied within families and among ethnic groups. Exchange theory might be applied to industrial settings and to international relations. Socialization processes might be used to analyze patient care and the experiences of minorities at predominantly white universities. The aim
of this exercise was to encourage students to raise the level of abstraction in their analyses ...

When the Educational Resources Information Clearinghouse (ERIC) tells us something about the evolution of the Quiche language, and that Quiche and Yucatec are both Mayan, the next appropriate step is a comparative analysis of the evolution of Quiche and Yucatec. When Treiman (1977) tells us that the stratification systems of countries X and Y have various elements in common, and we know that country Z is of a type similar to X and Y, the next logical step is to apply the Treiman hypotheses to country Z. When Melbin (1987) invents a fabulous metaphysical conceit that compares contemporary nightlife in America with nineteenth-century life on the western frontier, we realize that a proliferation of new hypotheses awaits us as we turn to discussions of the Turner thesis.³

Detailed discussions of the value of systematic taxonomies are found in Blau (1980) and McKelvey (1982).

(2) Contemporary thesauri with taxonomic overtones

There is an intensive interaction among concepts, methods, theories, and instruments, and I am strongly convinced that we cannot consider issues of theory-method-lexicon without devoting attention to the information sciences and their impact on scientific knowledge and information retrieval. Those who disparage instrumentation should read about Seymour Papert, a computer-oriented Piagetian who believes that the best way to get youngsters to comprehend the basic concepts of algebra is to let them play with differentials. Not differential equations—here, we're talking about greasy gearboxes. Let a child play with differential gears for a while and before long she will be writing sophisticated equations of great consequence for those who wish to avoid a dangerous skid; perhaps more importantly, she will begin to understand the abstract concept of an equation. And regarding Ada Byron, whom I described earlier as the first software author, it is well worth noting that she was inspired to invent the concept of software by Charles Babbage's digital computer.

It is arguable that, today, the most elaborate sociological lexicon is found in the online thesaurus for Sociological Abstracts; we learn to use it by playing with the appropriate machinery. The thesaurus is now available both in print and online, and may be used for bibliographic searches involving either medium. The first edition of the thesaurus contains 6,748 concepts, of which 3,563 (52 per cent) are Main Terms and 3,185 are discontinued terms listed for user convenience only. Thus, nearly half the basic terminology found in the social science literature or in existing dictionaries has been eliminated either because of non-use or because better synonyms have become available.⁴ On the other hand, among the 3,563 Main Terms there are 866 new terms, added in 1986. In the view of Sociological Abstracts, Inc., then, social science terminology evolves
at a rapid rate; if this view is correct, it implies that the task of systematizing and updating classifications will be a very large one indeed. It is noteworthy that the second edition of the *SA* thesaurus, available in mid-1989, had 179 new terms (Sociological Abstracts, Inc., 1989:2); the latest edition has 126 new terms (Sociological Abstracts, Inc., 1992:vi).

Thesaurus entries may have the following components, although most entries do not have all of them:

1. A Main Term (MnT), appearing in boldface letters
2. A Descriptor Code (DC), which may be used in online searches
3. A Scope Note (SN), providing definitions or special user instructions
4. A special designation (“context-dependent”) for highly abstract, multidimensional concepts
5. A History Note (HN), used primarily to separate new from old terms
6. A Used For (UF) designation, indicating synonyms not used as MnT's
7. A Use designation, the reciprocal of the UF designation
8. A list of Broader Terms (BT)
9. A list of Narrower Terms (NT)
10. A list of Related Terms (RT) not classified as BT's or NT's

The editors of the thesaurus make the claim (Sociological Abstracts, Inc., 1986:vii; cf. 1988:2 and 1992:vi) that

Broader Term/Narrower Term relationships create thesaurus *hierarchies*, sequences of class relationships that may extend upward more generally or downward more specifically through several levels. At any given point in the hierarchy, Broader Term/Narrower Term designations refer upward or downward only to the next most general or specific level. However, by tracing these references, a complete hierarchy or “family tree” can be approximated.

The editors then present a segment of a five-level hierarchy in which the term “Groups” subsumes “Organizations (Social),” which subsumes “Collectives,”
which subsumes “Communes,” which subsumes “Kibbutz”; they warn, however, that “searching a Broader Term in the SA online databases will not automatically retrieve abstracts representing the concepts of its Narrower Terms ...” (Sociological Abstracts, Inc., 1992:viii)—a concession that, as we shall see, turns out to be crucial. Although such hierarchies would appear to have utility along lines suggested earlier in this chapter, they are not readily discernible in the thesaurus and cannot be easily (or cheaply) produced either through online searching or through use of the printed SA volumes. Facilitating the discovery of taxonomic hierarchies, even if they must be “approximated,” may be the next logical step for Sociological Abstracts, Inc.

When taxonomic hierarchies are accessible through search media such as Dialog Information Services, there are powerful ways of exploiting them. Table 1 illustrates the use of the “expand” command as a means of identifying taxonomic hierarchies within a database called GEOREF, produced by the American Geological Institute. I have selected GEOREF for this illustration because GEOREF hierarchies, typically dividing up a two-dimensional space (the earth's surface), indicate clearly what occurs in a hierarchy that often has the formal properties of mutual exclusiveness and exhaustiveness. With occasional exceptions the smaller spaces add up to the larger spaces under which they are classified, and there are few spaces left unclassified at any level of the taxonomy.

Table 1. Use of the “Expand” Command in Dialog, with the GEOREF Database

<table>
<thead>
<tr>
<th>Set</th>
<th>Items</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>?EXPAND NORTH AMERICA</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Ref | Items | RT | Index-term |
--- | ----- | ---- | ----------- |
E1 | 2 | NORTH AMER |
E2 | 1 | NORTH AMERIC |
E3 | 28806 84 | *NORTH AMERICA |
E4 | 1 | NORTH AMERICA (PART) |
E5 | 1 | NORTH AMERICA AND |
E6 | 1 | NORTH AMERICA AND ASIA |
E7 | 1 | NORTH AMERICA AND AUSTRALIA COMPARED |
E8 | 1 | NORTH AMERICA AND CANADA |
E9 | 1 | NORTH AMERICA AND CENTRAL AMERICA |
E10 | 1 | NORTH AMERICA AND CHINA |
E11 | 1 | NORTH AMERICA AND CUBA |
E12 | 1 | NORTH AMERICA AND EASTERN ASIA |

Enter P or E for more

?EXPAND E3

Ref | Items | Type | RT | Index-term |
--- | ----- | ---- | ---- | ----------- |

In interpreting Table 1, note that Dialog user commands are prompted by a question mark. In this example I begin by expanding the index listings adjacent to NORTH AMERICA. Finding that there are 84 related terms (RT's) associated with NORTH AMERICA, I expand the appropriate reference, E3, and the RT's are listed. (Notice that I have deleted part of the listing.) The expansion of narrower terms (N) under NORTH AMERICA continues until we arrive at MEXICO, which I then expand by citing its reference, R41. We see that
MEXICO has NORTH AMERICA as a broader term (B), and we also see a listing of narrower terms subsumed under MEXICO. Unfortunately these narrower terms do not all operate at the same level of the spatial hierarchy—CERRO PRIETO, for instance, is a part of BAJA CALIFORNIA. (As we shall see, the ERIC database has a way of resolving this problem.) Finally, we expand BAJA CALIFORNIA. Notice that BAJA CALIFORNIA is indicated as part of MEXICO, that it subsumes six other areas from BAJA CALIFORNIA NORTE to TODOS SANTOS BAY, and that several terms, e.g. AGUA BLANCA FAULT, are listed as related to BAJA CALIFORNIA but are neither subsumed under it nor do they contain it. This, I believe, is the sort of taxonomic hierarchy of which Lazarsfeld and his advocates speak. As we shall see, the SA taxonomy does not work in this way at the present moment, although its potential will become obvious.

Let us consider a second database. The printed version of the ERIC thesaurus has much to recommend it (Houston, 1986). It contains a “two-way hierarchical term display”—still not available in the SA thesaurus—of which the following is an example:

:LANGUAGES
  AMERICAN INDIAN LANGUAGES
    .ATHAPASCAN LANGUAGES
      ..APACHE
      ..NAVAJO
      .AYMARA
    .CAKCHIQUEL
    .CHEROKEE
    .CHOCTAW
    .CREE
    .GUARANI
    .MAYAN LANGUAGES
      ..QUICHE
      ..YUCATEC
    .OJIBWA
    .POMO
    .QUECHUA
    .SALISH
    .TZELTAL
    .TZOTZIL
    .UTO AZTECAN LANGUAGES
      ..HOPI
      ..PAPAGO
The colon tells us that AMERICAN INDIAN LANGUAGES are subsumed under LANGUAGES. The single dot preceding ATHAPASCAN LANGUAGES informs us that this category is immediately subsumed under AMERICAN INDIAN LANGUAGES, along with other languages such as AYMARA and UTO AZTECAN LANGUAGES. The double dots tell us that HOPI and PAPAGO, for instance, are forms of the UTO AZTECAN LANGUAGES; and so forth. The ERIC hierarchical display contains many terms that have RT's located five or six levels above or below. This is most impressive; however, an online analysis comparable to that reported below for SA convinced me that the ERIC taxonomy has many weaknesses, primarily the fact that terms at a given level are neither mutually exclusive nor exhaustive. The same problem occurs with SA, and it appears also to be present with GEOREF.

Within these limitations, an ERIC search produced several intriguing results. For instance, among 18 items that focus primarily on KANNADA, and 6 items that focus primarily on MALAYALAM, there are 3 items coded as discussing both of these languages simultaneously. In examining these three articles, one would look for findings that encompass the Dravidian languages in general. From this standpoint, the articles listed below appear to be well worth pursuing; the same may be said about several additional examples not listed here.

?SS KANNADA/DE AND MALAYALAM/DE
   S36   18 KANNADA/DE
   S37   6 MALAYALAM/DE
   S38   3 KANNADA/DE AND MALAYALAM/DE

?TYPE S38/3/1-3


38/3/3 ED126674 FL007122 Indian Languages Bibliography of Grammars, Dictionaries and Teaching Materials. Pattanayak, D. P.,
(3) The logic of taxonomies

Taxonomies in many of the natural sciences have an ideal form—what Lazarsfeld (1972b:227-28) calls “articulation”—that involves several clearly separated levels. Each level has the properties of exhaustiveness and mutual exclusiveness (McKelvey, 1982:23,146; Lazarsfeld, 1972b, 229-30). In the words of Hempel (1952:51), an ideal taxonomy is “... a classification of the objects in a given domain D (such as numbers, plane figures, chemical compounds, galactic systems, bacteria, human societies, etc.) ... effected by laying down a set of two or more criteria such that every element of D satisfies exactly one of those criteria.” The reference to “every element” arises from a striving for exhaustiveness, and the reference to “exactly one” from a striving for mutual exclusiveness. Hempel points out (1952:50) that techniques such as factor analysis have great value in defining such taxonomies.

A formal taxonomy with several definitive levels and with exhaustiveness and mutual exclusiveness at every level has characteristics in common with formal bureaucratic hierarchies (Faia, 1972), a fact that recalls Durkheim's abstruse speculations about ways in which language categories may be influenced by social categories, and vice versa. (Hypothesis: Perhaps the apparent preponderance of Related Terms in the SA thesaurus, as compared with hierarchic terms, arises from the alleged “levelling” tendencies of sociology, in which one man's idiosyncratic conceptual scheme is as good as another's.) If the editors of the SA thesaurus are correct in the supposition that hierarchies of four or five levels may occur frequently in the social sciences, we would do well to examine the properties of such hierarchies. In a taxonomic hierarchy having four levels, with a single Main Term (MnT) at its apex and with each MnT subdivided into exactly two Narrower Terms (NT's), we would find a total of fifteen separate concepts. Among these concepts, six would be below the apex and above the “species” level—that is, six concepts would have both Broader Terms (BT's) above and NT's below and presumably would have special utility in “middle-range” theories (Merton, 1957:9-10,280). As we shall see, however, the proportion of concepts in the SA thesaurus that have both BT's and NT's appears to be far less than 6/15 or .4, which leads one to suspect that ideal four-level hierarchies of the sort defined above do not prevail in this thesaurus—or in the social science disciplines generally.

Consider the simple hierarchy found in the ERIC thesaurus, in which DENTISTS and PHYSICIANS along with many other types of professionals are
classified as NT’s under PROFESSIONAL PERSONNEL (Houston, 1986:187). The CD-ROM version of ERIC identified five published items with descriptors for PROFESSIONAL PERSONNEL and DENTISTS and PHYSICIANS. Two of the five items (Institute of Medicine, 1985; Ake and Johnson, 1981) provide generalizations having to do with the supply of and demand for dentists and physicians. By reading these works one learns about similarities and differences among dentists, physicians, and several other types of professionals. One inevitably wonders, however, why the search identifies no more than five studies, and whether studies were excluded simply because ERIC coders neglected to cite all three categories as descriptors. Perhaps one could search Dentists and Physicians without citing the Professional Personnel category, but this strategy might identify many studies that would be irrelevant to the original objectives of the search. Clearly we are largely dependent on coders to identify studies properly, and this is an expectation that is probably unrealistic given the fact that so many of our taxonomic hierarchies do not meet elementary criteria such as exhaustiveness and mutual exclusiveness.

I believe that in the best of all possible online worlds, authors of social science books and articles would routinely be asked to provide their own keyword coding. My anxieties about failing to identify excellent comparative studies of dentists and physicians would be allayed if I were assured that each time an author had an article “in press,” he or she would be queried for the appropriate coding categories by a user-friendly interview program (e.g., the Sawtooth Ci2 system). For the author of a comparative study of the professions, the dialogue might go more or less as follows:

(Q1) Under which of the following descriptor codes would you place this article?

(A) [Author indicates “Professional Personnel” and perhaps other keywords, from thesaurus lists of keywords.]

(Q2) The following keywords are subsumed under Professional Personnel. Does your article give special attention to any one of these keyword categories? [Repeated for other keywords cited in preceding response.]

(A) [Author indicates Dentists and Physicians from thesaurus-based list, but not other professions.]

(Q3) Does your article develop generalizations applicable to both dentists and physicians simultaneously, or are these two categories discussed more or less separately?
One readily thinks of additional questions about the scope of any generalizations, etc., but the point is obvious: Nobody is better qualified to code an author's work into a database than the author herself, and computer networks are now sufficiently accessible that it would not be unreasonable to request that authors provide this sort of information. I suspect that one of the major reasons for the failure of Earle Eubank's (1932) early efforts to develop social science taxonomies was the absence of appropriate data-processing machinery.5

The taxonomic hierarchies deemed valid at a given moment, and presumably used in a self-coding process by authors, are subject to falsifiability, a property shared with other scientific formulations. Suppose that in a simple taxonomic hierarchy categories \( l \) and \( m \) are subsumed under category \( C \), just as, in the example above, Dentists and Physicians are subsumed under Professional Personnel. We could take advantage of the *modus tollens* by constructing a syllogism that would generate a prediction based on this hierarchy, as follows:

*First premise:* If (clause a) categories \( l \) and \( m \) are subsumed under category \( C \), and if (clause b) variable \( x \) is related to variable \( y \) within category \( l \), 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 \( l \) and \( m \) are not subsumed under category \( C \)—clause a is false—and/or variable \( x \) is not related to variable \( y \) within category \( l \)—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 \( l \) to category \( m \), and if this variation recurs for many other relationships, we conclude that the taxonomic distance between \( l \) and \( m \) should be increased: The internal dynamics of entities of type \( l \) 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. (The second author query, above, would then be modified to incorporate this change.)
This approach to taxonomic logic, I believe, is precisely what is advocated by Walker and Cohen (1985) when they speak of “scope statements.” Scope statements are falsifiable.

Returning to the *ERIC* example: If it turned out that most generalizations about dentists were not applicable to physicians and vice versa, we would be forced to consider dividing professional personnel into two categories, one of which would subsume dentist-like professionals while the other would subsume physician-like professionals. It should be noted that this sort of evolving taxonomy would be a boon not only to researchers but also to readers in the early stages of familiarizing themselves with the literature of a given topic. If one were interested in reading a corpus of literature dealing with dentistry and dentistry-like occupations, one would know where to turn. If one wished to broaden the scope of an inquiry, the next logical step might be to identify the literature on physician-like occupations and begin the appropriate comparative analysis of two large occupational categories with their partially known patterns of similarity and difference. Comparative analysis, in consequence, would not be so closely tied to the relatively easy Durkheim-dissected strategy of placing geographic areas side by side.6

A colleague who read an earlier draft of this chapter has pointed out that “... successfully institutionalized taxonomies not only reflect scientific dominance, ... they also have to rest on some kind of unifying rationale, i.e., a theory in some sense.” This unifying rationale “... is the octopus, whose tentacles hold together the parts, including the classification schemes, of the theory.” While I have little expertise on the “scientific dominance” aspect of this claim, I would merely reply that even though social scientists have difficulty in finding agreement on conceptual schemes, we must reach a threshold of agreement in order to communicate with one another and in order to produce the sorts of dictionaries and thesauri that have appeared over the years. Furthermore, I cannot see any reason why social scientists should argue with one another about taxonomic categories any more than botanists and zoologists do: A good taxonomy accommodates any conceptual scheme that passes muster among the practitioners of the discipline, and social psychologists, let us say, should not wish to erase the demographic aspects of a taxonomy (or vice versa) any more than helminthologists should wish to stamp out the arthropodous realm.

On the theory question, I maintain that one cannot develop an adequate taxonomy, an evolving taxonomy, without making use of existing theories: Taxonomic hierarchies imply that a discipline has arrived at a degree of consensus about what to measure (or classify) and how to measure (or classify) it. Everything said above about falsifiability implies a continuous program of research in which various propositions, cast at various levels of abstraction, are tested through empirical observation and through the manipulation of models that are typically multivariate and causal. In such a program, furthermore, there would surely be a place for exploratory studies that use intensive case-study methods.
and that might lead one to propose an entirely new taxonomic realm. These activities produce theory, they are sustained by theory, they cannot exist without theory, and theory cannot exist without these activities.

(4) Sampling, search, and test strategies for *Sociological Abstracts: Hierarchies without perks*

In the first phase of this study a sample of MnT's was selected from the *SA* thesaurus (1986 edition) by taking the first entry at the upper left corner of each even-numbered page; the sample size was N = 129. For each of the 129 MnT's I coded the Descriptor Code, presence or absence of a Scope Note, information from the History Note as to whether a given concept was newly added (1986) or old, and the number of Used For synonyms, BT's, NT's, and RT's. Preliminary analyses suggest that (1) there is no significant relationship between oldness/newness of terms and the presence of a (usually) definitional SN; (2) the distribution of numbers of UF, BT, NT, and RT concepts is usually non-normal, with high positive skewness; (3) when presence/absence of NT's is given as a loglinear function of presence/absence of UF, BT, or RT concepts, no significant relationships appear, suggesting that the presence of NT's does not necessarily indicate the presence of multilevel conceptual hierarchies; (4) the proportion of MnT's having both BT's and NT's is 17/129 or .13. In general these findings seem to suggest that the development of social science taxonomies is not a highly structured, systematic process and that the proportion of middle-range MnT's is well below what one would expect in a highly developed taxonomy. McKelvey's hope that systematic classification will help us to generate middle-range theories “... between the extremes of total uniqueness and total similarity” (1982:169) apparently has not been fulfilled. Other interpretations, however, may be equally plausible.

Results from my initial *SA* sample are no more impressive than those from *ERIC*; in neither instance do we find clear evidence of the Lazarsfeld tactic, or even “approximated” conceptual hierarchies. It is a keen disappointment, for instance, that no studies are listed that deal simultaneously with ARCTIC REGIONS and ARID ZONES, with NAMING PRACTICES and PROJECTIVE TECHNIQUES, with the NORTH PACIFIC OCEAN and the SOUTH PACIFIC OCEAN, with ANGOLA and BOTSWANA, despite the fact that each of these pairs is listed under a common Main Term. Perhaps the appropriate comparative analyses appear in textbooks, but if research scholars were consistently applying the Lazarsfeld logic such combinations would be evident also in the basic research literature.

Early in 1988 the “expand” command was implemented for the online version of *Sociological Abstracts* (Sociological Abstracts, Inc. 1988:1-2), making possible a far more rapid and efficient search of keywords with alleged hierarchical properties. A second sample of MnT's was selected from
odd-numbered pages of the thesaurus. Only MnT's printed in boldface were selected, since other keywords listed in the thesaurus have been deactivated. On each page, the first MnT with two or more NT's was included in the sample regardless of the year in which the MnT was added—some of the MnT's have been active only since 1986, and will therefore refer to relatively small numbers of publications. The final list of MnT's (N = 104) is exemplified by terms in Table 3 preceded by asterisks.  

In preparing to investigate pairs of NT's subsumed under each of the 104 MnT's, I found it necessary to skip many MnT's that were classified as "context-dependent"; apparently, the compilers of the SA thesaurus believe that these MnT's are defined so broadly that working definitions must be based on the context in which the term occurs, and it appears to be highly improbable that NT's under these vaguely defined keywords would meet the criteria of exhaustiveness and mutual exclusiveness. Table 2, containing an excerpt from my interaction with Dialog, illustrates how each of the 104 MnT's was searched by means of the "expand" command. Note that in the output from this command the designation S refers to Scope Notes, U and F make cross references to more appropriate terms, and B, N, and R refer to Broader Terms, Narrower Terms, and Related Terms respectively. The RT column provides a count of the number of references (S's, B's, etc.) associated with a given MnT. In examining this interaction, notice that large geographical entities such as South America are broken down into their components, as in the case of GEOREF; this is an instance in which exhaustiveness and mutual exclusiveness are likely to be present. The listing of types of BLUE COLLAR WORKERS, in contrast, is considerably less reliable: INDUSTRIAL WORKERS may overlap with MANUAL WORKERS, for instance, and one thinks of many categories of blue collar workers, absent from this listing, that are more numerous than LONGSHOREMEN. The listings for CHILDREN and for TECHNOLOGY have similar problems. For WESTERN EUROPE, only partially illustrated, we return (as Durkheim might say) to more solid ground. These examples suggest that if conceptual hierarchies in the social sciences imply systematic taxonomies with exhaustiveness and exclusiveness, we have a long way to go.

Table 2. Use of the "Expand" Command in Dialog, with Sociological Abstracts

<table>
<thead>
<tr>
<th>Ref</th>
<th>Items</th>
<th>Type</th>
<th>RT</th>
<th>Index-term</th>
</tr>
</thead>
<tbody>
<tr>
<td>R1</td>
<td>26</td>
<td></td>
<td>18</td>
<td>*BLUE COLLAR WORKERS ...</td>
</tr>
<tr>
<td>R2</td>
<td>0</td>
<td>S</td>
<td>18</td>
<td>(FORMERLY (1964-1985) DC 058470, BLUE-COLLAR.)</td>
</tr>
<tr>
<td>R3</td>
<td>10741</td>
<td>B</td>
<td>47</td>
<td>WORKERS</td>
</tr>
<tr>
<td>R4</td>
<td>50</td>
<td>N</td>
<td>12</td>
<td>AGRICULTURAL WORKERS</td>
</tr>
<tr>
<td>R5</td>
<td>183</td>
<td>N</td>
<td>9</td>
<td>DOMESTICS</td>
</tr>
<tr>
<td>R6</td>
<td>1917</td>
<td>N</td>
<td>10</td>
<td>FARMERS</td>
</tr>
<tr>
<td>R7</td>
<td>136</td>
<td>N</td>
<td>4</td>
<td>FISHERMEN</td>
</tr>
</tbody>
</table>
WHAT'S WRONG WITH THE SOCIAL SCIENCES?

R8     65 N  5 FOREMEN
R9     57 N  9 INDUSTRIAL WORKERS
R10    9 N  4 LONGSHOREMEN
R11    11 N  7 MANUAL WORKERS
R12    258 N  5 MINERS

? expand (children)

Ref Items Type RT Index-term
R1    12112       36 *CHILDREN (Persons aged 24 months to 12 years.)
R2        0 S       (FORMERLY (1963-1985) PART OF DC 081000, C...)
R3     380 F  2 BOY (1963-1985)
R4     1534 F  2 BOYS (1963-1985)
R5     434 F  2 GIRL (1963-1985)
R6     1494 F  2 GIRLS (1963-1985)
R7      10 B 21 AGE GROUPS
R8      31 N  8 ADOPTED CHILDREN
R9      13 N  8 FOSTER CHILDREN
R10     55 N  3 GRANDCHILDREN
R11    580 N 15 INFANTS
R12     4 N  5 ONLY CHILDREN

? expand (technology)

Ref Items Type RT Index-term
R1    16809       35 *TECHNOLOGY
R2        0 S       (FORMERLY (1963-1985) DC 456860, TECHNOLOG...)
R3     14 F  1 APPLIED SCIENCES
R4      20 F  2 HYDRAULIC (1977-1985)
R5     81 B 12 SCIENCE AND TECHNOLOGY
R6      35 N 18 AGRICULTURAL TECHNOLOGY
R7       3 N  9 APPROPRIATE TECHNOLOGIES
R8     473 N 13 AUTOMATION
R9      62 N 13 BIOTECHNOLOGY
R10     217 N 12 CYBERNETICS
R11     49 N 12 ELECTRONIC TECHNOLOGY
R12    821 N  8 ENGINEERING

Enter P or E for more

? expand (western europe)

Ref Items Type RT Index-term
R1      112       44 *WESTERN EUROPE
R2        0 S       (FORMERLY (1984-1985) DC 160274, EUROPE, W...)
R3     3363 B  9 EUROPE
R4       1 N  2 ANDORRA
R5     493 N  3 AUSTRIA
R6       9 N  3 AZORES
The most promising NT combinations—those pairs referring to the largest sets of items in the literature—were then searched, with partial results shown in Table 3. As one examines this table the following points should be borne in mind: (1) In the typical instance, such as *ABILITY, the two NT's referring to the largest numbers of published items—COMPETENCE and SKILLS in this case—are searched by means of the Boolean “and” connector. In this instance 95 items were identified in which COMPETENCE and SKILLS are referred to in the text of the record; (2) “context-dependent” terms, such as *ACTION, were skipped; (3) for several MnT's, such as *BLUE COLLAR WORKERS, the list of NT's was relatively lengthy (requiring more than one “page” of printout from Dialog), and in these instances our search of the literature was conducted in such a way that both NT's would have to appear in the Descriptor Codes (for which the search suffix is /DE) of published items. For instance, although not a single item in the literature has descriptor codes for both FARMERS/DE and MINERS/DE, there are two items coded for both JUDGMENT/DE and THINKING/DE. Presumably, these are publications in which authors make an explicit attempt to relate thinking and judgment, and to generalize about them.

Table 3. Boolean combinations of NT's

<table>
<thead>
<tr>
<th>Ref</th>
<th>Items</th>
<th>Type</th>
<th>RT</th>
<th>Index-term</th>
</tr>
</thead>
<tbody>
<tr>
<td>R1</td>
<td>3502</td>
<td>17</td>
<td>*ABILITY</td>
<td></td>
</tr>
<tr>
<td>R5</td>
<td>1171</td>
<td>N</td>
<td>5</td>
<td>COMPETENCE</td>
</tr>
<tr>
<td>R6</td>
<td>2496</td>
<td>N</td>
<td>9</td>
<td>SKILLS</td>
</tr>
<tr>
<td>S3</td>
<td>95</td>
<td></td>
<td></td>
<td>“COMPETENCE” AND “SKILLS”</td>
</tr>
<tr>
<td>R1</td>
<td>8288</td>
<td>13</td>
<td>*ACTION (A context-dependent term ...</td>
<td></td>
</tr>
</tbody>
</table>
WHAT'S WRONG WITH THE SOCIAL SCIENCES?

R1  3996  5 *AFRICA ...
R3  11  N  14  NORTH AFRICA
R4  53  N  50  SUB SAHARAN AFRICA
S11  0  "NORTH AFRICA" AND "SUB SAHARAN AFRICA"

R1  8294  29 *AGRICULTURE
R6  8  N  12  ANIMAL HUSBANDRY
R7  17  N  8  PART TIME FARMING
S14  0  "ANIMAL HUSBANDRY" AND "PART TIME FARMING"

R1  39376  33 *ANALYSIS (A context-dependent term ...)
R5  51  N  11  INDUSTRIAL AUTOMATION
R6  14  N  5  OFFICE AUTOMATION
S7  0  "INDUSTRIAL AUTOMATION" AND "OFFICE AUTOMATION"

R1  20028  63 *BEHAVIOR (A context-dependent term ...)
R5  8572  N  20  BIOLOGY
R7  5507  N  22  ECOLOGY
S8  102  "BIOLOGY" AND "ECOLOGY"

R1  27273  28 *CHANGE (A context-dependent term ...)
R10  55  N  3  GRANDCHILDREN
R11  580  N  15  INFANTS
S2  0  GRANDCHILDREN/DE AND INFANTS/DE

R1  1008  33 *COGNITION
R9  1092  N  16  JUDGMENT
R12  1706  N  7  THINKING
S3  2  JUDGMENT/DE AND THINKING/DE
Again results are disappointing in that the number of items in the literature that appear to generalize across a pair of related NT's is very small compared to the number of opportunities for doing so—1203/37344 or 3.2 per cent. Furthermore, the alleged taxonomic hierarchies of Sociological Abstracts, once again, usually do not meet the criteria of exhaustiveness and exclusiveness in a convincing way.

(5) First conclusion

McKelvey (1982:32-33) begins his book on organizational systematics with the observation that taxonomy as a profession does not exist in the social sciences. In biology, by contrast, confusion remained rampant “... until international organizations were founded which began to bring order into the methods by which taxa were recognized and named.” McKelvey implies (1982:23) that the same process occurred in physics, chemistry, mineralogy, linguistics, and library science. “Organizational science,” he claims, “will be in a similar state of confusion until there is a recognized council that can set up basic ground rules and procedures” for various aspects of the taxonomic enterprise. I strongly agree, and I believe that the appropriate council should be established with the following points in mind:

(1) It must be a body representative of the social science disciplines as a whole.

(2) The effort must be international in scope, involving organizations such as the International Sociological Association and the International Institute of Sociology. Both British and American databases, for instance, use a standard industrial code (SIC), and it would be reassuring to know that one could count on the comparability of these codes.

(3) The effort must be pursued in cooperation with producers of social science databases. Sociological Abstracts must be included, but the larger effort should involve organizations that produce the many additional social science databases, as well as organizations such as Dialog Information Services and System Development Corporation, which disseminate information from databases. It is important that future cohorts of social scientists be trained in the logic of database searching, and that every effort be made to develop search procedures and aids based on artificial intelligence.

In a moment of facetiousness, McKelvey (1982:xviii) says that it is a central principle of “creative research” that one must “give up when they think you are
right”—but that he does not intend to hold his breath. I think he is, and I hope he doesn't.

(6) Codicil 1991

During the months preceding the 1991 convention of the American Sociological Association, Walter L. Wallace sought support for a formal proposal that the ASA develop and adopt a systematic lexicon for the discipline. This proposal generated a fascinating debate. As Levine (1991:6) pointed out, Wallace's impulse is not at all new: Hobbes, Malthus, Durkheim, and Weber had similar predilections. In more recent times Eubank's (1932) volume, now nearly forgotten, captures the spirit of Wallace's proposal and also goes a long way toward suggesting how this proposal would be implemented. In his chapters 9 through 11, for instance, Eubank makes it clear that a social-science lexicon, contrary to the claims of several Wallace detractors, would not be static: It would evolve as part of the same processes that lead to the evolution of theories and methods, the same processes that cause social-science thesauri to evolve. We saw that when Eubank speaks of ways of classifying factors involved in “societary causation,” he makes it clear that taxonomies do not refer merely to static qualities but also to the dynamics of interrelationships among social variables, and that the concepts will succeed or fail along with the hypotheses that contain them. McKelvey (1982) argues on the same premises. The syllogism developed earlier in this discussion makes reference to relationships among social processes and to their role in the evolution of taxonomies.

Wallace's proposal is an idea whose time has come. Conceptual clarity is a commodity that we must now demand; in a scientific discipline, obfuscation is a norm violation. Among our brethren in the natural sciences, of course, clarity is demanded just as Lazarsfeld, in the story that opens this chapter, should have demanded it: What do you mean, esteemed colleagues, by room-temperature fusion? Just a few days ago, as I was getting ready to demand that my colleagues remove some of the ambiguity of the term “postmodern social theory,” I came across Denzin's (1991) helpful prolegomenon. It made my day, although my day may not arrive until after the future.

(7) Frequencies and amplitudes: Toward a taxonomy of time series

Oscillation: that is the essential process.

Tylor's theory of the incest taboo provides a fine example. Summarized and discussed in detail by Faia (1986:22-34), the theory argues that the requirement of exogamy,

... enabling a growing tribe to keep itself compact by constant unions between its spreading clans, enables it to overmatch any number of small
intermarrying groups, isolated and helpless. Again and again in the world's history, savage tribes must have had plainly before their minds the simple practical alternative between marrying out and being killed out.

In a recent sociology textbook (Lenski, Lenski, and Nolan, 1991), Tylor's assumptions about the survival of societies have become the major organizing premise. This work suggests, for instance, that during the hunting-gathering phase of human social evolution, it is highly unlikely that women were heavily involved in hunting. Such an activity would have interfered with childbearing, and a high birth rate had to be maintained in order to overcome the high death rate of such populations. “In other words,” say the authors (Lenski, Lenski, and Nolan, 1991:105), “if there ever were societies that used women extensively in hunting, they probably did not survive because of low birth rates.” In such theories it is clear that each variable, if not unduly disturbed, would tend to oscillate in a way that we could probably discern if we observed the appropriate societies. Deviations from incest restrictions, for instance, might grow large for a time due to “variation”; however, these very deviations, through the operation of social selection that would tend to select against them, would eventually be self-correcting. All intervening variables, such as success in internecine war, would describe similar oscillations. The same conclusion would apply to woman-as-hunter.

Lenski, Lenski, and Nolan (1991) believe that most human societies have disappeared because they adopted dysfunctional practices, or found that new conditions had made their old practices dysfunctional. The Innu of Northern Canada provide a contemporary example of a society that may be in the process of disappearing.

Consider a few more examples:

... the rearing of pigs serves to schedule the occupation of garden lands acquired in warfare. As such land tends to have been intensively cultivated, it requires fallowing before it can be productively employed by a victorious community. The length of the fallow period is determined by the amount of time it takes to build up pig herds to a size considered adequate for ritual sacrifices to ancestral spirits, but it is also effectively determined by the toleration by adult women of the increasing work required to care for such pigs. When this reaches its limits, the pig feast takes place, the new gardens begin to be cultivated, and war breaks out anew (Harris and Ross, 1987:66).

Clearly, this pattern seems to imply regular oscillation. The example of the Anheuser-Busch corporation, cited in an earlier chapter, suggests again that
similar patterns may occur in modern societies. In Anheuser-Busch we encounter a congeries of production facilities spread across the entire North American continent, with each unit having its particular labor-force demography and a surrounding market demography, the latter involving both suppliers and distributors. Production facilities are tied together and coordinated by a control system headquartered in St. Louis, with the St. Louis center assigning production and distribution quotas across the entire system. Production schedules are influenced by many environmental factors including the weather, which may have an impact on pricing of various beer ingredients as well as on market demand. The “frictions of space” (e.g., transportation costs), another techno-environmental factor, must always be included in cost/benefit calculations. Finally, technological developments such as the highly automated Brewhouse B at the Williamsburg plant have a large impact on the flexibility of production schedules: It is much easier to manipulate the productivity levels of automated machinery than it is to manipulate productivity levels of the human work force.

The result of these arrangements is that there is a constant fluctuation or oscillation of production schedules at a given plant, and this causes considerable consternation among workers. It is entirely possible that, because of these fluctuations, Busch may make beer in California that ends up being sold in North Carolina. By contrast, the Virginia Power Company never buys electricity that is brewed in California because transmission costs, at the present time, would be exorbitant. Is this situation likely to change as we move into the age of superconductivity? How big can electric grids become? Will they become international? Will they have a basic North-South orientation as well as following population distributions? Would a North-South orientation, by reducing the amplitude of seasonal variations, reduce the amplitude of demand? Does this question have anything to do with Virginia Power's expressed preference for buying power from Canadian utilities rather than from utilities that are located in a westerly direction and that tend to have similar demand patterns? What we are discussing here, of course, is Perrolle's (1987:161-63) “global factory” and the structural features that it is likely to develop as it tries to control the costs associated with demand variations.

Nearly all these examples involve “cycles.” And, contrary to Zeitlin's claim (1973:14), the cyclical processes all have a fascinating “history.”

Frequency and amplitude, of great utility in defining the amount of lag in a causal process, may also be presumptive indicators of the presence of powerful social forces (Faia, 1986:58-61); as Kuhn says (1974:31), “the amplitude of the oscillation will vary inversely with the speed and sensitivity with which the negative feedback operates.” At Anheuser-Busch, the most effective way to damp the amplitude of quotas ordered by St. Louis would be to increase the frequency with which the quotas are changed. But this tactic would have its costs. I suspect, however, that the amplitude component of ecological costs has a larger impact than the frequency component, so that it would be wise for us to invent and
discover more efficient social servomechanisms—efficient in the sense that they would cause us to change direction more frequently. On the other hand, remember the Prudent Pilot's Principle: It is better to be far from one's destination and moving in the right direction than close to one's destination and moving in the wrong direction. Merely changing the direction of important social processes—e.g., levels of beer production—may involve substantial costs.

Statistical methods that organize data and enable us to perceive relationships among variables should be exploited to the fullest extent in the process of creating all taxonomies, including taxonomies of temporal change. For instance, it is inappropriate for social scientists to use metaphors such as diamonds, triangles, pyramids, hourglasses and the like when describing the shape and span of stratification systems, and changes therein (Barber, 1957:91). Statisticians have developed excellent methods for describing the variability or “inequality” of univariate distributions (Allison, 1978), and we should use these methods routinely in classifying social stratification systems and other social entities. A recent paper by Anttila (1989), for instance, shows how data on “long waves” may provide a basis for classifying social structures as they change through time. Similarly, ARIMA (Auto-Regressive Integrated Moving Average) models have been in use for twenty-five years or so, and it is clear that these models involve a sort of built-in taxonomy. In the Minitab version, for instance, ARIMA models have six numerical properties as follows:

\[
\begin{align*}
\text{p} & \text{- order of the AR part} \\
\text{d} & \text{- number of differences} \\
\text{q} & \text{- number of MA part} \\
\text{P} & \text{- order of the seasonal AR part} \\
\text{D} & \text{- order of the seasonal differences} \\
\text{Q} & \text{- order of the seasonal MA part}
\end{align*}
\]

A given variable could be classified, say, as a 1 0 1 0 1 0 time series. These six digits contain information about the trend line of a time series, whether the series has an autocorrelation from one time point to the next that would give it a sort of momentum or predictability, whether the changes from one time point to the next are more important than absolute values, whether the series is subject to high levels of random disturbance, whether there are predictable changes in the series based on seasonality, and so forth. Social variables that embodied a Marxist-Leninist tendency toward accumulation of contradictions, for instance, would leave a recognizable ARIMA signature.

(8) Online searches as a data source: Notes on the future of taxonomy

... archival databases ... have become increasingly common in comparative and historical work. In contrast to “socialized” databases ..., 
many of these are comparatively privatized, and I imagine that much of the knowledge about them lies in relatively inaccessible sources ... This and other issues concerning archival data should be addressed in a visible disciplinary outlet, in view of the rising degree to which sociological knowledge depends on knowledge drawn from these sources.

—Peter V. Marsden (1989:669-70)

Among the hundreds of databases available through Dialog Information Services, there are twenty-three that provide numeric data (Dialog Information Services, 1988:62-67; cf. Dialog Information Services, 1993). In the following list, I have provided annotations for those numerical databases that seem to have special value for social scientists:

1. American Library Directory (R. R. Bowker)

2. American Men & Women of Science (R. R. Bowker)

3. Cendata (U.S. Bureau of the Census): Contains statistical data, press releases, and product information. Demographic data include excerpts from the CPS, the 1980 census, and information on more than 200 countries. Data generally consist of tables.

4. Chem-Intell (Chemical Intelligence Services, London): Contains data on the organization and operations of the chemical industry.

5. D&B - Donnelley Demographics (Donnelley Marketing Services): Contains demographic and market data.

6. D&B - Dun's Financial Records (Dun's Marketing Services): Contains up to three years' financial data for over 700,000 companies. Individual companies may be compared against industry norms.

7. D&B - International Dun's Market Identifiers (Dun's Marketing Services): Contains data on organization and operations for over 500,000 non-U.S. companies.

(9) Disclosure/Spectrum Ownership (Disclosure Information Group): Contains data on stock ownership for approximately 5,000 companies. Based on SEC files.

(10) Econbase: Timeseries & Forecasts (WEFA Group): Contains econometric time series on business and demographics.


(12) Investext (Technical Data International)

(13) Media General Plus (Media General Financial Services, Inc.)

(14) Moody's Corporate News - International (Moody's Investor Services, Inc.)

(15) Moody's Corporate News - U.S. (Moody's Investor Services, Inc.)

(16) Moody's Corporate Profiles (Moody's Investor Services, Inc.): Contains organizational and financial data for about 1,300 U.S. firms highly active on the stock market.

(17) PTS Annual Reports Abstracts (Predicasts): Contains abstracts from annual reports for over 3,000 U.S. and international firms.

(18) PTS F&S Indexes (Predicasts): A large database containing company profiles and financial data, and information on mergers, acquisitions, new products, technology, and sociopolitical factors. Also contains bibliographical data.

(19) PTS Forecasts (Predicasts)

(20) PTS PROMT (Predicasts): Contains data on organizational and financial characteristics of companies, including information on international trade and technology.

(21) PTS PROMT Daily (Predicasts)

(22) PTS U.S. Time Series (Predicasts): Historical and projected time series for the United States, including data on population, GNP, prices,
wages, employment, production, consumption, energy, vehicles, agriculture, mining, manufacturing, foreign trade, etc.

(23) Standard and Poor's News (Standard & Poor's Corporation): Provides news, company profiles, and financial information for more than 10,000 U.S. companies.

(8.1) Data reduction: obtaining simple counts

The appendix provides an illustration involving data from ICC British Company Financial Datasheets. When we examine the complete ICC record for Horsham Brewery (Part I), which happens also to be a distributor of computer technology, we realize that we are in the presence of a taxonomy that contains (or implies) concepts such as spatial location, administrative structure, type of industry, sales, exports, pre-tax profits, employee remuneration, fixed assets, net assets, and so forth; it also contains an assortment of ratio variables. Incidentally, it is incumbent on social scientists to try to influence this sort of taxonomy in the same way that we influence the taxonomies of, say, the General Social Survey of the National Opinion Research Center, or the Data Archive for Adolescent Pregnancy and Pregnancy Prevention (DAAPPP).

Variation in rate of profit per employee, perhaps brought about by variation in “organic composition of capital,” is the major focus of this illustration. Wallace (1986:8) suggests that post-industrial societies experience “... periodic 'crises' which impede the profit-making process, and which subsequently bring forth innovative methods for restoring profitability.” He suggests further that, at present, economic crises tend to occur in the traditional industrial sector while more profitable “innovative methods” are found in industries such as electronic data processing. One presumes that, holding constant for the size of firms, innovative methods and products would be associated with a relatively high profit rate; holding constant for type of firm, profit rates would be highest for relatively small firms (Mintz and Cohen, 1971:69-87). The SPSS analysis presented in remaining parts of the appendix provides support for these hypotheses.

The twenty-seven record sets listed by Dialog in Part II of the appendix (partly deleted) were created through use of the SELECT command. Set S1, for instance, contains records for 1,797 British firms that have descriptors indicating that they deal primarily with computers or with personal computers. The use of a question mark at the end of keywords indicates that any suffix may be substituted for the question mark—computers, computerized, etc. The set S4 begins the process of grouping firms by number of employees (size), and by profit per employee. The frequency counts for sets S10 through S27 can be treated as cell frequencies for a 2 by 3 by 3 table involving type, size, and profitability of firms. Set S10, for instance, contains 234 computer firms that are small (99 or fewer employees) and have a low profit rate (2,499 pounds or less per employee).
In Part III of the appendix, I suggest a reasonably efficient way of listing data for analysis by SPSS. Notice that the three variables—type, size, and profitability—were all coded on the basis of their Dialog set numbers, a practice that minimizes errors. The information contained in Dialog output can readily be transformed into a raw data file through manipulation by an effective editor such as EMACS. This process is facilitated considerably when the original interaction with Dialog is saved to a PC disk by PClink, Kermit, or a similar program. Part IV of the appendix contains SPSS tabular output, with appropriate statistical tests.

(8.2) Data reduction: the REPORT command

Several databases, including Donnelley Demographics, have a REPORT capability that permits datasets to be constructed according to user specifications and to be saved in a rectangular format. The dataset found in Table 4 was created by the following series of commands:

```
SELECT LV=CITY AND ST=VA
SELECT S1 AND AG=30000:999999
SORT S2/ALL/AL,D
REPORT S3/CY,AH,AL,LA,HA
```

The first command creates a set containing all cities located in Virginia. The second command selects cities in the first set that have at least 30,000 inhabitants. The third command lists these cities by descending order of median family income. The final command causes the cities to be printed out (or saved to disk) by name, number of households, family income, socioeconomic status, and years of schooling.

Table 4. Dataset Illustrating REPORT Command, Donnelley Demographics

<table>
<thead>
<tr>
<th>CITY</th>
<th>TOTAL HHOLDS</th>
<th>MED HH INC ($)</th>
<th>SOC/ECON STATUS</th>
<th>MED YRS EDUC ADULTS</th>
</tr>
</thead>
<tbody>
<tr>
<td>MCLEAN (CDP)</td>
<td>13,479</td>
<td>64,503</td>
<td>92</td>
<td>16.1</td>
</tr>
<tr>
<td>WEST SPRINGFIELD (CDP)</td>
<td>8,583</td>
<td>56,557</td>
<td>93</td>
<td>16.1</td>
</tr>
<tr>
<td>BURKE (CDP)</td>
<td>16,757</td>
<td>54,827</td>
<td>94</td>
<td>16.1</td>
</tr>
<tr>
<td>DANVILLE CITY</td>
<td>17,522</td>
<td>20,527</td>
<td>38</td>
<td>11.4</td>
</tr>
<tr>
<td>ROANOKE CITY</td>
<td>41,284</td>
<td>20,384</td>
<td>42</td>
<td>12.2</td>
</tr>
<tr>
<td>NORFOLK CITY</td>
<td>97,918</td>
<td>19,486</td>
<td>40</td>
<td>12.3</td>
</tr>
</tbody>
</table>
Notice that, with minimal editorial manipulation, the data could be transformed into a fixed-format file for analysis by SPSS, SAS, etc. Dialog Information Services (1987) has brought it to the attention of market researchers that these made-to-order demographic datasets can be matched with market data such as warranty registrations or sales; the same principle applies to sociological research using combined datasets. In fact, I should mention in closing that Dialog Information Services (1988:80-89; 1993:141-64) has implemented a “Dialindex” procedure that makes it possible to search several databases simultaneously on a given topic. The keyword taxonomy available for multiple database searches is not highly sophisticated, but this sort of procedure may well be the wave of the future.12

(9) Final conclusion

In the epigraph of this chapter, the first sentence of The Sociological Imagination addresses the feeling of being trapped. In the paragraph following this sentence, Mills (1959:3) begins to explain how we gain the understandings necessary to extricate ourselves:

Underlying this sense of being trapped are seemingly impersonal changes in the very structure of continent-wide societies. The facts of contemporary history are also facts about the success and failure of individual men and women. When a society is industrialized, a peasant becomes a worker; a feudal lord is liquidated or becomes a businessman. When classes rise or fall, a man is employed or unemployed; when the rate of investment goes up or down, a man takes new heart or goes broke. When wars happen, an insurance salesman becomes a rocket launcher; a store clerk, a radar man; a wife lives alone; a child grows up without a father. Neither the life of an individual nor the history of a society can be understood without understanding both.

Clearly, this understanding requires a willingness to move freely between the abstract and the concrete, the etic and the emic, the large social structure and the quotidian realm.
NOTES (Chapter 7)

(1) Back in the years when I received my graduate training, we used to devote what was probably excessive seminar time to evaluating Somerville's (1941) facetious claim that “umbrellaology” constitutes a science. My intuitions always told me that something was obviously wrong with Somerville's subtle arguments, although it was hard to articulate my skepticism. I eventually realized that the basic problem with umbrellaology is that it indulges in dead-level abstraction: The principles applicable to umbrellas are never tested for their applicability to other types of commodities.

The same problem occurred in reverse among surgeons who work with human organ transplants. It was known for many years that identical twins could readily donate organs to one another, but the tissue incompatibility problem and the consequent immunological rejection were not adequately approached until detailed tissue typologies, going way beyond the concrete instance of identical twins, were developed.

(2) The 1988 edition of the three-volume Library of Congress catalog is dedicated to Angell. It is ironic that Dawkins (1986:256-60) claims that library taxonomies have a high degree of arbitrariness as compared with Willi Hennig's cladism in biology, because in a sense Angell showed us how to overcome the limitations of what might otherwise have been an arbitrary way of classifying books.

(3) Melbin's chapter 3, entitled “Frontier Comparisons,” carries out exactly the sort of hypothesis-generating process discussed here.

(4) One of the major advantages of systematic taxonomies is that they control the proliferation of inefficient synonyms. In a recent study of my own, for instance, my search of the literature was slightly impaired until I realized that “accumulative advantage,” “labelling,” “self-fulfilling prophecy,” “Pygmalion effect,” and “Matthew effect” are virtually synonymous terms. As nearly as I can tell, the same is true of “interaction ritual chains” and “differential association.” One could readily cite scores of additional examples, and one hopes that the thesauri currently available for many bibliographic reference works, as these works go online, will help to dispel the confusion.

(5) Eubank (1932) had great prescience. In his chapters 9 through 11, he speaks of ways of classifying factors involved in “societary causation.” In other words, he believed that taxonomies do not refer merely to static qualities, but also to the dynamics of interrelationships among social variables. Note that the syllogisms developed later in my discussion make reference to “relationships” and their role in the evolution of taxonomies.
(6) Durkheim claimed that verbal categories reflect social organization (Benoit-Smullyan, 1948:516-18). Given that territoriality is an essential element of many definitions of human society, especially those informed by the work of Spencer (Barnes, 1948:119-27; Martindale, 1960:532; Turner, 1986:410-11), it is not surprising that the most clearly organized social science taxonomies are those based on territorial boundaries. It is these taxonomies that are most likely to meet criteria of exhaustiveness and mutual exclusiveness, at each of a multiplicity of taxonomic levels. This is an encouraging development: Science is based on consensus, and the existence of spatial hierarchies as social facts, along with the incorporation of these social facts into scientific parlance, leads us to the belief that other aspects of the scientific lexicon might also be organized at a comparable level. The area hierarchies of GEOREF, illustrated in Table 1, are another prime example of the utility of spatial hierarchies. I suspect that the most successful implementations of the “comparative method”—a method that generates relatively abstract generalizations—have occurred in fields in which spatial hierarchies have been more or less self-evident. See, for instance, Lipset (1963:343-48) and Lipset and Bendix (1966:97-199) on the uses of the comparative method in cross-national research.

For instance: The current (April, 1993) CD-ROM version of Sociological Abstracts lists sixteen publications coded for ENGLAND, FRANCE, and GERMANY (simultaneously) as descriptor terms, and these publications contain many abstract generalizations. A few typical titles convey their flavor:

- Images of National Character: The French, the English, and the Germans
- Growth and Recession in Western Europe
- Sociological Thought on Organizations: The State of the Art
- Formal Education Agreements as a Screening Device for the Work Market: A Comparative Analysis of France, the Federal Republic of Germany, and Great Britain
- Growth and Recession in Western Europe
- Pronatalist Population Policies in Some Western European Countries
- Continuous Education within the Educational Systems of Six Countries: Comparative Study
- Young People's Integration in Society
The Growth of the Welfare State in Britain, France, Germany, and Italy: A Comparison of Three Paradigms

Income Equality and Economic Development in Great Britain, Germany, and France: 1850 to 1970

The Policies of Britain, France, and West Germany towards the Soviet Union 1955-1975

Finally, it is highly significant that, of the 126 new terms incorporated into the latest SA thesaurus, many were made necessary by the geopolitical instabilities of Germany, the former Soviet Union, and Eastern Europe (Sociological Abstracts, Inc., 1992:vii).

(7) The complete table is available from the author.

(8) Examining Table 3, we see that the number of opportunities for publications to focus simultaneously on COMPETENCE and SKILLS is 1171, on NORTH AFRICA and SUB SAHARAN AFRICA it is 11, and so forth. The total number of such opportunities, for the larger of my two samples, was 37,344.

(9) The cost of this dataset, covering 2,756 British firms, was $51.31, and available software packages with search protocols could have reduced the cost further. The low cost is one of the major reasons why I believe that databases such as ICC will be drawn upon more heavily in future research. (Information deleted from the appendices is available from the author.)

(10) A separate loglinear analysis confirms that type of industry, net of size of firm, has a significant impact on the likelihood that a given firm produces a high profit. For instance, when we compare medium-sized automotive firms against medium-sized computer firms, the odds for high profitability increase from around 54/565 for automotive firms to around 55/153 for computer firms. The logarithm of the odds ratio increases from -2.35 to -1.02. A loglinear model giving the odds ratios as a function of type of industry and size of firm produces expected cell frequencies that do not differ significantly from observations. The interaction of type of industry and size of firm as a factor affecting profitability was non-significant.

(11) Use of the REPORT procedure is relatively expensive; in the example cited, each element of the data matrix cost 25 cents. Although I do not know of any special sampling procedure in Dialog, it is possible to create a “systematic” sample (Blalock, 1979:558-60) by using the TYPE or PRINT command for every kth unit of a large set.
(12) DAAPPP uses a general taxonomy of variables that is applicable across many datasets. The generic variable called CBHNN207, for instance, refers to one's age at the birth of a first child, an aspect of childbearing history (CBH) for survey number NN. In the General Social Survey, variables are named according to standard mnemonics, they are classified according to a general taxonomy (Davis and Smith, 1985: Appendix P), and the literature based on the GSS may be searched by means of the mnemonics used as keywords (Smith and Fujimoto, 1986; Smith and Arnold, 1990). The GSS bibliography is now available as a machine-readable database.
REFERENCES (Chapter 7)


APPENDIX (Chapter 7)

Part I: Full Record, ICC British Company Financial Datasheets

1998307 ** FULL DATASHEET AVAILABLE **

KING & BARNES LTD

REGISTERED COMPANY NUMBER: 00039422

TRADING OFFICE:
The Horsham Brewery,
18 The Bishopric,
Horsham,
W. Sussex RH12 1QP

<table>
<thead>
<tr>
<th>DATE OF ACCOUNTS</th>
<th>30Sep85</th>
<th>30Sep84</th>
<th>30Sep83</th>
<th>30Sep82</th>
</tr>
</thead>
<tbody>
<tr>
<td>NUMBER OF WEEKS</td>
<td>52</td>
<td>52</td>
<td>52</td>
<td>52</td>
</tr>
<tr>
<td>Total Sales</td>
<td>NA</td>
<td>NA</td>
<td>NA</td>
<td>3,292</td>
</tr>
<tr>
<td>U.K. Sales</td>
<td>NA</td>
<td>NA</td>
<td>NA</td>
<td>NA</td>
</tr>
<tr>
<td>Exports</td>
<td>NA</td>
<td>NA</td>
<td>0</td>
<td>NA</td>
</tr>
<tr>
<td>Pre-tax Profits</td>
<td>448</td>
<td>428</td>
<td>314</td>
<td>385</td>
</tr>
<tr>
<td>Interest Payable</td>
<td>78</td>
<td>34</td>
<td>19</td>
<td>14</td>
</tr>
<tr>
<td>Non-Trading Income</td>
<td>1</td>
<td>1</td>
<td>1</td>
<td>0</td>
</tr>
<tr>
<td>Operating Profit</td>
<td>525</td>
<td>461</td>
<td>332</td>
<td>399</td>
</tr>
<tr>
<td>Depreciation</td>
<td>212</td>
<td>147</td>
<td>123</td>
<td>108</td>
</tr>
<tr>
<td>Trading Profit</td>
<td>737</td>
<td>608</td>
<td>455</td>
<td>507</td>
</tr>
<tr>
<td>Taxation</td>
<td>NA</td>
<td>0</td>
<td>NA</td>
<td>NA</td>
</tr>
<tr>
<td>Auditors Fees</td>
<td>8</td>
<td>10</td>
<td>10</td>
<td>7</td>
</tr>
<tr>
<td>Directors Remun</td>
<td>120</td>
<td>124</td>
<td>110</td>
<td>103</td>
</tr>
<tr>
<td>Employees Remun</td>
<td>677</td>
<td>608</td>
<td>532</td>
<td>439</td>
</tr>
<tr>
<td>Employees (actual)</td>
<td>77</td>
<td>73</td>
<td>72</td>
<td>68</td>
</tr>
</tbody>
</table>

Part II: Cell Frequencies for 2 by 3 by 3 Contingency Table

<table>
<thead>
<tr>
<th>Set</th>
<th>Items</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>S1</td>
<td>1797</td>
<td>COMPUTER?/DE OR PERSONAL(W)COMPUTER?/DE</td>
</tr>
<tr>
<td>S2</td>
<td>5230</td>
<td>AUTOMOBILE?/DE OR MOTOR(W)VEHICLE?/DE</td>
</tr>
<tr>
<td>S4</td>
<td>29005</td>
<td>EM=00:99</td>
</tr>
<tr>
<td>S5</td>
<td>11477</td>
<td>EM=100:999</td>
</tr>
<tr>
<td>S6</td>
<td>2030</td>
<td>EM=1000:9999999</td>
</tr>
<tr>
<td>S7</td>
<td>18144</td>
<td>PE=00:2499</td>
</tr>
<tr>
<td>S8</td>
<td>6471</td>
<td>PE=2500:4999</td>
</tr>
<tr>
<td>S9</td>
<td>7374</td>
<td>PE=5000:999999</td>
</tr>
</tbody>
</table>
SOCIAL SCIENCE SEMANTICS

S10 234 S1 AND S4 AND S7
S11 104 S1 AND S4 AND S8
S12 196 S1 AND S4 AND S9
.
.
.

Part III: SPSS Input File

DATA LIST FIXED FILE=INLINE RECORDS=1
/1
FREQ 1-3
INDUSTRY 5
SIZE 7
PROFIT 9
.
.
.

BEGIN DATA
234 1 4 7
104 1 4 8
196 1 4 9
.
.
.

Part IV: SPSS Output File

- - - - - - - - C R O S S T A B U L A T I O N O F - - - - - - - -
HIGHPROF
BY INDUSTRY
CONTROLLING FOR.. SIZE

INDUSTRY
COUNT I
COL PCT ICOMPUTER AUTO ROW
I TOTAL
I 1I 2I

HIGHPROF
---------+---------+---------+
.00 I 338 I 1068 I 1406
I 63.3 I 86.6 I 79.6
---------+---------+---------+
1.00 I 196 I 165 I 361
I 36.7 I 13.4 I 20.4
---------+---------+---------+
COLUMN 534 1233 1767
TOTAL 30.2 69.8 100.0
.
.
.
PHI 0.26563
CONTINGENCY COEFFICIENT          0.25673

.
abstraction
accumulative advantage
action versus order
affirmative action
Affirmative Action Advisory Committee (AAAC)
AIDS epidemic
ambiguity
American federated
American military
amplitude
analysis of variance
anatomy
androgyne
Anheuser-Busch
animal ecologists
anti-positivism
anti-science
apathy
applied social science
archival databases
ARIMA
artificial intelligence
aviation
axiom of indispensability
axiomatic theory
BASIC
benign circles
beta weights
bias
biological analogy
biology
black holes
bootstrap
Braess' paradox
capital punishment
causal analysis
causal relationships
CD-ROM searches
chaos
checkers programs
chemistry
chess programs
circular causation
class discrimination
Clausewitz
cliometrics
Club of Rome world model
cognition
coitus
comparative method
computers
computers, personal
Comteanism
conceptualization
condoms
consensus
consensus seeking, in science
consequences, of social structures
conspicuous consumption
contraception
controlled scores
correlation
cost-benefit model
counterfactual history
courtship practices
critical mass
cultural materialism
cybernetic hierarchy
Darwinian functionalism
Data Archive for Adolescent Pregnancy and Pregnancy Prevention (DAAPPP)
debunking
decision theory
decisionmaking processes
deferred gratification
delict-sanction model
detachment
deterministic models
deterministic versus stochastic models
derrence
deviance, and social control
deviant
dialectical
dichotomous thought
differential association
disaster research
divine intercession
dysfunctions
ecological demography
economic man
economics
ecosystems
Eisenhowerism
elaboration model (generalization-specification model)
electoral districts
elitism
empirics
epistemology
estimation, statistical
ethical choices
etic
evolution of taxonomies
exchange theory
exhaustiveness
explained variance
explanation
falsifiability
feedback loops
femininity
feminist theory
fertility
feticide
Florida InteRactive Modeler (FIRM)
free will
frequency and amplitude
Freud versus Malinowski debate
Freudianism
frustration and aggression
functional analysis
functionalism
Gaia hypothesis
Gauss-Markov postulates
gender-equality
general systems theory
generalization
geology
great man
grief
Guttman scale
harsh treatment of women
headhunting
heredity
heterosexual
hierarchic taxonomies
historians
historicism
history of sexuality
holistic
human ecology
human-computer interaction
humanities
hunting and gathering societies
hydraulic hypothesis
idiographic story analysis
idiography
Ilongots
incest taboo
Index
Indiana, state of
indices
indispensability, of railroads
infanticide
intentions
internal combustion engine
irony
irrationality
IRS 1040 long form
jumping frogs of Calaveras County
keywords
keywords
labelling
Las meninas
learning theory
levelling of faculty salaries
linguistics
lynching
mad libs
magic
male initiation rites
Malthusianism
manifest versus latent functions
marital adjustment
market segmentation
Marxian functionalism
masculine bias
masculinization of thought
mass murderers
mate selection
mathematics
Matthew effect
measurement
measurement error
mental institutions
mentalistic
merit scores
metaphor
Mexican history
Mexican positivism
Mexico
middle-range projections
middle-range theories
military expenditures and economic stagnation
military security
minimax
Minitab BASIC
minority women
modus tollens
molecules
Monte Carlo simulations
moral restraint
Morse code
mortality
multiple outcomes
mutual exclusiveness
narrower
national product
National Survey of Family Growth (NSFG)
natural experiments
natural sciences
Newtonian world view
niche width
noise
novels
nuclear family
null hypothesis
objectivity
obscurantism
oedipus complex
Ogburnian
Ogburnian experiments
online searches
organic analogy
orientations
oscillation
parsimony
path diagrams
path diagrams
pedicide
perception
Phillips school
phylum
physiology
plenitude
Plessy v. Ferguson
POET variables
poetic license
point of view of the actor
political swings
population growth
Porfiriato
positive feedback
positivistic organicism
postmodernism
power struct
PPF simulations
prediction
prerequisites
price variability
primary group
prisons
prophecies
proximate determinants
psychohistorical orientation
Pygmalion effect
pyramidal power structures
qualitative sociology
quantitative methods
queueing
R-SQUARE, in Statistical Analysis System (SAS)
racial desegregation
racism
railroads
random error
rationality
reciprocal causation
reductionism
reflexive sociology
regression analysis
replication
reputational method
requisite analysis
reverse regression models versus forward regression models
reverse versus forward regression models
revolution
rites of passage
robust
rumor process
s-functions
scientific method
search, strategies of
seismology
select command
semantics
servomechanisms
sex discrimination
sex preselection
sex ratio
sex ratio at birth
sexism
sexual harassment
sexual intercourse
sign rule
Simscript
WHAT'S WRONG WITH THE SOCIAL SCIENCES?

simulation
simulation experiments
SLAM
slavery
small causes
social change
social constructionism
social control
social evolution
social metamorphosis
social norms
social prediction
Sociological Abstracts
sociological laws
sociology of knowledge
solipsism
spectral analysis
spurious
stability
stable population theory
stagflation
statistical laws
statistical variability
statistics
status attainment
storytelling
structuralism
structure-functional analysis
substitutability
survival
syllogisms
synonyms
syntactics
systematic agriculture
tautologies
taxonomy
technological innovation, in Ogburnian theory
teleology
temperance movement
territoriality
thesauri
time-series analysis
tone
traffic citations
traffic flow
transitivity
translation
transportation systems
trial without error
triangulation
Turing test
two-cultures thesis
two-valued logic
type I
umbrellaology
unconscious
underdetermination
unintended consequences
unity of diction
value-free social science vernacular architecture
Verstandnis
Virginia Power Company
vulcanology
warfare
waterways
Wharton model of the U.S. economy
women's colleges
zulu
expand