

**Interconnectedness in Nature
and Cooperation in Science:
The Case of Climate and
Climate Modeling**

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Interconnectedness in Nature and Cooperation
in Science: The Case of Climate and Climate
Modeling

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To Steve Schneider, whose interdisciplinary expertise and social responsibility as a climate modeler demanded philosophical recognition; and to Bill Wimsatt, whose philosophical analysis of biological complexity demanded application to the physical sciences.

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Preface to Second Edition

This second edition has taken advantage of LaTeX's ability to typeset mathematics to give the many nested expressions in Chapters IV and V a more attractive and readable form. Since mathematical meanings became more self-evident in many cases, the accompanying discussions were adjusted to refer more directly to the mathematics. Editorial liberties were taken in this process, but no substantial changes were made.

In the five and a half years since the completion of this dissertation, chaos and complexity theory have become legitimate scientific interests and the interdisciplinary aspirations of the *Earth System Science* and *International Geosphere-Biosphere* initiatives have matured into the *U.S. Global Change Research Program* and similar interagency programs around the world. I have been tempted to move beyond my few typographical corrections and the above-mentioned refinement of the book's mathematical discussions to prepare a more substantial revision that reviews some of these recent developments. Since doctoral dissertations usually don't enjoy wide circulations, however, it has seemed more appropriate to address these developments in separate works. I have had the opportunity to develop several new global change courses as a research associate at Texas A & M University and to begin work on a text (*The Science and Philosophy of Global Change*) designed to meet the integrative needs of such courses. I plan to update my reviews of complexity and the global change initiative in this context. In addition, my involvements at the San Diego Supercomputer Center and the Institute for Scientific Computation at Texas A & M University have led me to appreciate more deeply the importance of computer, network, and information technologies in global change research and education and, more generally, the interdisciplinary study of complex systems. I am currently preparing a paper that analyzes some of the impacts *distributed computational science* is likely to have on the future of global change research and planet management.

Despite the rapid pace of related scientific developments in recent years, the central theme of this dissertation seems no less relevant today than it did in 1989: Interlevel and intersubsystem interactions in the earth's climate system and many other complex systems often necessitate interdisciplinary as well as multidisciplinary collaboration; and methodologies are beginning to emerge that may provide a basis for this new level of scientific integration. This basis is neither reductionist nor holistic, but rather selectively cuts across levels, subsystems or other domains of specialized interest as necessary to develop causal accounts of the variety of phenomena that contribute—often probabilistically and conditionally—to the systemic structure, behavior, or evolution of interest. Such interfield explanations can play a critical role in motivating model development and, along with empirical tests of the descriptive and predictive value of models, in model confirmation.

Larry Goldberg, Ph.D.
Institute for the Study of Complex Systems
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Chapter 1

Introduction

This study explores what the philosophy of science and climate modeling can teach one another about the challenges of developing interdisciplinary mathematical models of complex systems. Such a purpose may seem of dubious promise to philosophers who turn to the biological and social sciences to unravel the peculiar problems of explaining the structure, behavior, and evolution of complex systems, and who presume that areas of applied physics such as meteorological and oceanographic modeling are philosophically uninteresting applications of established physical laws to special boundary conditions. Equally, climate modelers may wonder whether philosophers of science, well-known for their divergent interpretations of science, could contribute more than unconfirmed speculation about the modeling sciences, akin, perhaps, to the controversial Gaia hypothesis.

However warranted it may have been several decades ago for philosophers of science and so-called applied physicists to view one another's fields with suspicion or disinterest, it would be little more than professional arrogance to do so today. The study of physical complexity has enjoyed something of a scientific revolution, thanks to observation technologies such as satellite monitoring programs; computers that can solve large sets of equations, and heretofore intractable sets of nonlinear differential equations by numerical approximation; and theoretical progress in our understanding of many-scaled interactions in such fields as turbulence theory, far-from-equilibrium thermodynamics, and chaos theory. The philosophy of science, for its part, has undergone something of a revolution in its more realistic appreciation of high degrees of systemic complexity and in its analysis of the complicated

forms of interdisciplinary cooperation often required to study such complexity.

Climate modelers, in particular, have gone a long way toward healing the methodological schism between holists (energy balance modelers) and reductionists (general circulation modelers) that divided the field in the 1970s, and are today approaching a sufficiently unified methodology to be considered a discipline—a discipline that not only integrates consideration of many types of physical phenomena defined across many scales of motion and temporal variation, but also integrates the contributions of numerous physical, biological, and social areas of science. And likewise, philosophers of science have gone a long way toward healing the antagonisms among positivists, historicists, and system scientists that divided the field several decades ago, and in the past decade have begun to integrate methodological, historical, and systems approaches in analyses of the interdisciplinary study of complex systems. Along with their growing credibility, climate modeling and the philosophy of science share their interest in exploring the nature of the causes of the stable and changing features of complex systems, the status of theories and models as explanations of these features, and the appropriateness of various forms of interdisciplinary cooperation in theory and model development.

What, then, can the philosophy of science and climate modeling teach one another? The climate system is an extremely complex system characterized by the strong interactions between its primary subsystems (the atmosphere, hydrosphere, cryosphere, lithosphere, and biosphere) and by the high degrees of interdependence between different physical processes and between different scales of motion in the atmosphere and in the oceans. Although philosophers and philosophically oriented scientists have identified many similar forms of complexity in biological (e.g., Wimsatt 1972, 1975; Bechtel 1984, 1986a) and social systems (e.g., Berlinski 1976, Corning 1983), physical complexity has been neglected in the philosophy of science literature.¹ It should be of no little interest that the atmosphere and oceans achieve much of their complexity, at least, without the help of the biosphere,

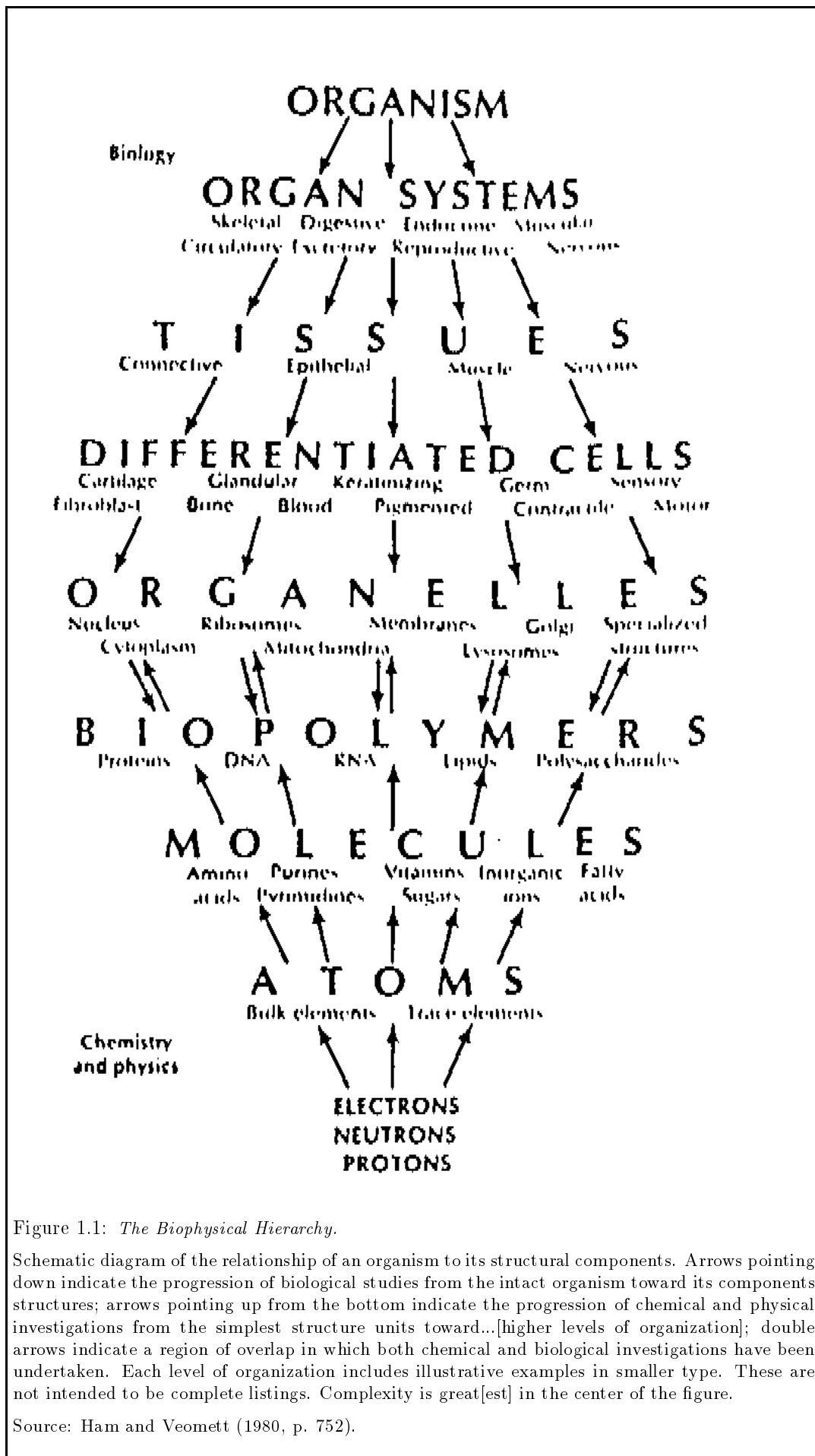
¹In the popular literature on science, however, physical complexity has often been the model for all forms of complexity. Prigogine and his school, for example, have done much to popularize the nondeterministic implications of self-organizing systems which can change steady-states in response to small perturbations. Of Prigogine's work, Prigogine (1980) and Prigogine and Stengers (1984) are perhaps the most accessible to nonmathematicians. Helpful reviews by sympathetic scientists in other fields may be found in Jantsch (1980), Laszlo (1987), and Davies (1988). A philosophical review of the implications of Prigogine's work—the exception, it seems, that makes the rule—may be found in Denbigh (1975). From another point of view, various approaches to chaos theory explore the way small sets of deterministic equations can generate genuinely random behavior, behavior that despite its randomness often approaches or alternates among attractors in phase space. An excellent popular review may be found in Gleick (1987).

and that this complexity fits the standard philosophical model (Wimsatt 1972, 1975) of intersubsystem and interlevel complexity inspired by biological examples. (See 1.1.)

Climate modelers, on the other hand, may find the philosophical classification of systems according to their degree of complexity to have direct implications for standards of when to represent, if possible, intersubsystem or interscale interactions in climate models. Such considerations are relevant, for example, in strategies for the coupling of atmospheric and ocean models and for the indirect representation of the contributions of subgrid-scale phenomena such as clouds and boundary layer turbulence. Philosophers of science have distinguished carefully among simple systems that lend themselves to reductionistic analysis, hierarchically organized systems that warrant the neglect of intersubsystem and interlevel interactions in the development of higher-level models, and interactionally complex systems that require integrated consideration of the contributions of several interacting levels. Depending upon the purposes of particular models, aspects of the climate system may qualify as simple, hierarchical, or interactionally complex.

The explanatory status of climate models should be of philosophical interest in that parameterizations which appear to serve lawlike purposes in model simulations and predictions are not quite like any scientific laws—statistical or otherwise—which have been identified in the philosophy of science literature. Two roles of parameterizations—their development as empirical generalizations or curve fits to empirical data and their confirmation in terms of their contribution to the predictive (or simulative) power of specific models—have been recognized widely (if not integrated adequately) by philosophers. However, a third role of parameterizations—their indirect representation of interactions between explicitly modeled phenomena and processes which are spatially or temporally unresolved or otherwise omitted from models—has not been recognized (except in passing by Bohm (1957) and Salmon (1971)) in the philosophy of science literature. Although parameterizations look like laws at the level of spatial and temporal detail of the model, they are really projections into the state space of the model of the resolvable consequences of interlevel interactions. This role of parameterizations stands in contrast with philosophical accounts of interlevel explanation that presume that laws apply to their own levels—with especially artificial consequences for interpretations of explanation in evolutionary biology and psychology.²

²If computers had been around before positivists froze evolutionary process in their deductive-nomological



The disciplines that study the different climate subsystems (such as meteorology and oceanography) or different scales of motion (such as dynamic meteorology and turbulence theory or cloud physics) ideally should cooperate in the development of climate models in proportion to the interdependence of their respective domains of specialized concern. Most philosophical models of interdisciplinary cooperation consider the exchange of knowledge between methodologically autonomous fields rather than the integration of methodologies in cooperative research.³ Bechtel's (1984, 1986a) work seems to stand alone in exploring how several disciplines may become methodologically integrated in a new discipline in response to their growing realization of the interdependence of their respective domains of concern.⁴ Climate modeling is an unusually rich example of the progressive integration of contributing disciplines or areas of research in the treatment of complexity in that physical, biological, and social disciplines are involved. Philosophers of science who wish to explore the unity of science without reduction will find the example of climate modeling invaluable. Climate modelers, on the other hand, may benefit from a philosophical view of cooperation which links the need for cooperation with the degree of perceived systemic complexity.

There are other reasons for undertaking a philosophical study of climate modeling. One of the most urgent challenges facing humankind is to develop the ethical commitment, the scientific knowledge, and the political and economic means to anticipate, and in some cases to prevent, damages to the biosphere and to society associated with natural and socially caused environmental change. The type of scientific knowledge required, and our preparedness to achieve it in sufficient time to be of help, is a matter of continuing debate among scientists, science and public policy makers, grantors, philosophers of science, business leaders in potentially offending industries, and in general, among a growing number of academics, intellectuals, and concerned citizens of all nations.

Climate models in particular are of great social importance in that their simulations of the natural variability of climate and their calculations of the sensitivity of climate to human activities promise to offer guidance in agricultural, energy, and military policy, and

account of explanation from unconditional covering laws, philosophers might today speak of fluidlikeness rather than lawlikeness.

³In their original studies of interfield theories, for example, Darden and Maull (1977; Maull 1977), consider how a problem which arises in one field or discipline can be solved through the independent research of a second field or discipline, and in a more recent study, Darden (1986) considers how several established fields contribute to the development of interfield theories.

⁴Laudan (1977) and Hooker (1987) emphasize the importance of interdisciplinary research traditions or programs, but do not develop models of how they come about or work.

in general, in the management of our planet. If philosophers of science could provide a perspective for the assessment of climate modeling research programs which adds any insight to the perspectives of climatologists and policy makers, it would seem professionally responsible to attempt to do so.

The current study explores, from the point of view of the philosophy of science, and in the context of a case study of climate modeling, the methodological requirements for adequate modeling of complex systems, and in particular, for adequate modeling of the global environment. If there is a single philosophical theme underlying this thesis, it is that in many areas of physical, biological, and social science the description, explanation, and interdisciplinary modeling of complex systems are all too complex to lend themselves—at least for all modeling purposes—to the hierarchical forms of analysis that have monopolized the philosophy of science until perhaps the last decade; and that climate modeling, which integrates the contributions of many areas of physical, biological, and social science, dramatically illustrates these descriptive, explanatory, and interdisciplinary complexities.

To provide background for scientific readers and a foil for the philosophical view of the science of complexity developed in Chapter III, we review in Chapter II the three primary schools of thought in this century's philosophy of science. We show in Chapter II that the three schools or research traditions in the philosophy of science which have been concerned, respectively, with the problems of systemic complexity, the justification of theories and explanations, and the disciplinary organization of scientific research—system science (or system philosophy), logical positivism, and *Weltanschauung* analysis—have all presupposed (until recently) strictly hierarchical perspectives.

In Chapter III we review and extend recent philosophical models of systemic complexity, interlevel explanation, and interdisciplinary cooperation in order to provide a coherent view of the interdisciplinary study of complex systems that avoids the unjustifiable assumption of nearly isolated levels of organization, explanation, and research. The models of interactional complexity (following Wimsatt (1972, 1975)), interlevel explanation, and (bona fide) interdisciplinary cooperation developed in Chapter III will be illustrated by the case of climate modeling considered in Chapters IV and V.

The analysis of climate modeling found in Chapters IV and V both applies the philosophical perspective of Chapter III, and reflects the author's original motivation for its

development.⁵ This case study will analyze the complexity of the climate system (in Chapter IV) and the explanatory status of climate models and the interdisciplinary cooperation often required for their development (in Chapter V).

The more global implications of this study are considered, along with an assessment of its philosophical significance, in the concluding chapter (Chapter VI).

⁵Any circularity should invite other case studies of areas of science that model complex systems.

Chapter 2

Hierarchies Everywhere

2.1 Introduction

There have been so many different issues and positions in this century's philosophy of science, and they have been interconnected in such complicated ways, that any partition of the field into schools could be misleading if taken too literally. For introductory purposes, however, and for the purposes of the present study, the approximate outlines of three major schools may be discerned. These are system philosophy, logical positivism (or logical empiricism), and Weltanschauung analysis.

It is a matter of no little interest that these three schools—while diametrically opposed to one another in purposes—each came to espouse as a central doctrine a hierarchical interpretation of science. System philosophy explored the significance of various types of hierarchical analysis for justifying the omission of detail and the use of higher-level laws or theories in models of complex systems (e.g., Simon 1962, 1973, 1976). Positivists examined *theoretic levels* (or *reductive levels*—such as physical, chemical, biological, and social theories) as the occasion for a program of the gradual reduction of higher-level to lower-level theories (e.g., Kemeny and Oppenheim 1956; Oppenheim and Putnam 1958). And if concepts of hierarchical organization have served the purposes of holistic system theorists and positivistic reductionists alike, they have also served the purposes of pluralistic

Weltanschauung analysts¹ who wished to avoid preference for science at either higher or lower levels of organization. Darden and Maull, for example, modeled the contributions of several methodologically autonomous and mutually irreducible *fields* of science with primary empirical concerns defined at different *levels* of the same system to the development of *interfield theories* (Darden 1974; Darden and Maull 1977; Maull 1977; Darden 1986).

A sketch of how the three schools came to hold their hierarchical views may help to reveal the irony of their agreement.

2.2 Levels of Organization in System Philosophy

System science arose in response to the necessity of neglecting detailed consideration of constituent processes and of applying laws or theories that are not derived from first principles in models of many complex systems.² Models of such systems are always simplified to some degree or another, and it is the concern of system science to develop a general methodology of system simplification.

System philosophy and system science overlap considerably, but the distinction is perhaps that system philosophy is concerned with the justification—or unjustifiability—of the various types of simplification, while system science is concerned with the methods. The two are intimately interconnected, however, and the following discussion will not emphasize the distinction.

The central problem of system philosophy may be stated in terms of the notion of *levels of description* developed by Pattee (1973a,b). A descriptive hierarchy is any ordering

¹For reasons explained below, I may be considering the class of Weltanschauung philosophers to be a somewhat broader class than it often is considered to be.

²One of the earliest treatises on irreducible levels in biology is the classic by J. C. Smuts (1926) in which the term *holism* apparently was coined. The work of Ludwig von Bertalanffy (1950, 1952) in the early 1950s was pivotal in creating the field of system science. His better-known later works on system theory include von Bertalanffy (1962a, 1968, 1975), and an application of his systems perspective to his own field of theoretical biology may be found in von Bertalanffy (1962b). Helpful anthologies on system theory include those of Buckley (1968) and Emery (1969). A history of the development of system science may be found in Part I of Lilienfeld (1978). A critique of the field which shares many of the concerns of this study may be found in Berlinski (1976). The present discussion will be limited to a review of those aspects of system theory relevant to the notion of hierarchical levels of organization. Helpful volumes on hierarchy theory include the anthologies of Whyte et al. (1969) and Pattee (1973c), and Allen and Starr's (1982) systematic application of hierarchical perspectives to problems of ecology.

of different degrees of physical, spatial, temporal, or other detail relevant to the description of the system in question. This type of hierarchy makes no assumptions about the adequacy of theories, laws, or models defined at less detailed (higher) levels. In terms of such descriptive levels, the central problem of system philosophy is to explore the general types of justification for modeling at less (than maximally) detailed levels of description.

There are two different categories of system simplification which must be distinguished. The first involves an omission of detail which still allows the laws or theories that apply to the more detailed level to fulfill the purposes of the model. An example of this situation is Sir Isaac Newton's original model of the solar system, which omitted interactions among planets (with the exception of perturbations in Saturn's orbit during its conjunction with Jupiter) and perturbations in the sun's position due to the moving center of gravity of the planets.³ The laws which applied to Newton's simplified model of the solar system were the same laws which in principle would have applied to a more complete set of interactions. The justification for this type of simplification is mathematical and, although it has implications for the stability of the solar system, it does not raise, in the opinion of this writer, the most interesting philosophical questions.

A second category of system simplification is more radical and of more central interest in system theory: the laws or theories which may be known to apply at a more detailed level of description do not apply, or are not by themselves sufficient for modeling purposes, at a less detailed level of description. In this case theories, laws, regularities, or other principles that are not known to be derivable from lower-level laws or theories, but are believed to hold uniquely at the higher (less detailed) level, may—whether together with or in place of lower-level theories or laws—contribute to mathematical closure and help to fulfill the predictive, simulative, explanatory, or other purposes of the model. A model that appeals to such higher-level principles in its omission of lower-level detail may be said to be *holistic*.

System philosophy largely has been concerned with implications which the organization of a system and its relationship with its environment may have for the credibility of holistic models (in the second category of system simplification above) which appeal to higher-

³Newton's lament on the necessity of omissions may be found in the Scholium to Theorem 4, Version III, of *De Motu* (Newton 1685). His special concern with the perturbations of Saturn's orbit during its conjunction with Jupiter is reviewed in I. B. Cohen (1980). Gravitational forces are superposable in Newton's theory, so he was able to calculate the effects of Jupiter on the fiducial orbit of Saturn to obtain the perturbation of Saturn's orbit due to Jupiter.

level assumptions. Another area of the philosophy of science, logical positivism or logical empiricism, also has been concerned with the status of higher-level theories. This concern, however, has centered on general requirements for theory justification and general conditions for the reduction of higher-level to lower-level theories, rather than on considerations of the organization and boundary conditions of complex systems. The present discussion will be limited to the concerns of system philosophy. The positivistic approach to theoretic levels will be considered separately in the section below.

The category of systemic organization that has enjoyed the most study by systems theorists—and the one that has provided the most common justification for higher-level modeling—is hierarchical organization.⁴ There have been many criteria for hierarchical orderings of levels, including decompositions—which often are interrelated—according to size, time scales of variability, structural stability, variable interdependence, and causal autonomy. The analysis of a system into levels of organization has found many applications in biology, and is illustrated in the special case of an organism in *Figure 1.1* (Chapter 1).

How can the possibility of analyzing a system into appropriately defined levels of organization justify higher-level modeling? Hierarchies of stability and time scale, for example, help to explain why chemistry can afford to neglect considerations of nuclear physics and quantum chromodynamics. Intranuclear events occur at higher frequencies and energies and shorter wavelengths than electromagnetic phenomena. The higher energy requirements for intranuclear interactions make nuclear structure more stable than the electromagnetic structure of atoms. This allows chemists to presuppose the stability of nonradioactive nuclei, at least, at the energies of chemical interactions. The interactions between nucleons—or on a still lower level, the interactions among quarks—are much more confined in space and time than electromagnetic interactions, so that the former have little opportunity to influence the latter. It is only the temporal and spatial averages of intranuclear events, as manifested in nuclear structure, that are chemically significant; and chemical reactions do not affect nuclear structure. The negative correlation between size and stability in our highly structured universe is illustrated in *Figure 2.1*, and the correlation between size and time scale in our universe of nested boundary conditions is illustrated in

⁴The following discussion is not intended to provide an extensive review of hierarchy theory, but only to highlight the importance of several well-known types of hierarchical organization for issues regarding the adequacy of higher-level (holistic) models. Helpful anthologies on hierarchy theory include those of Whyte et al. (1969) and Pattee (1973c). Critiques of hierarchy theory which share many of the concerns of this study may be found in Wimsatt (1972a, 1975).

Table 2.1.

As counterintuitive as it may seem, the *parts* or constituent patterns of systems are not always more stable than the system itself (or more properly, more stable than the systemic behavior of interest). The general circulation of the atmosphere, for example, is much more stable than the short-lived storms that contribute to its statistics.⁵ Although it is a subject of debate as to what extent it is justified to neglect detailed consideration of daily weather in the prediction of climatic change,⁶ the behavior of many simpler fluid systems is quite insensitive to small-scale variability in its internal motions or molecular distributions. Hierarchical analysis is relevant here, except that it is a hierarchy of time scales without a hierarchy of stability.

The relatively faster time scales generally associated with the interactions of smaller constituents (see *Table 2.1*) in many cases justifies the definition of higher-level variables in terms of the time averages or other statistical properties of constituent variables. In isolated thermodynamic systems (i.e., systems closed to external sources of mass and energy), for example, constituent (molecular) processes occur at such small time scales—and with such small probabilities of extraordinary states—that only their averages are relevant to the higher level at which thermodynamic variables are defined.

Boltzmann showed that configurations of molecular velocity which deviate significantly from the Maxwell-Boltzmann distribution are extremely improbable if each possible molecular configuration is assigned the same probability. In the context of an ergodic hypothesis which equates the phase averages of such logically possible systems with the time averages of the evolution of the given system, Boltzmann (1871) and Maxwell (1879) were able to show that thermodynamic equilibrium is highly probable for an isolated system.⁷ Nonequilibrium states may occur over short time scales, but they do not contribute significantly to the average state of the system over long time scales. Phenomenological thermodynamics is therefore justified in its neglect of lower-level considerations of molecular dynamics in the

⁵The statistically defined behaviors of many systems are more stable than the transient patterns that produce that behavior. Nowhere, perhaps, is this more dramatically the case than in chaotic systems which never exactly repeat their earlier states, but despite their random behavior often reveal highly predictable attractors which can be defined statistically.

⁶The case study of climate modeling in Chapters IV and V will consider this issue.

⁷See the remarkably clear discussion of what Boltzmann and Maxwell did and did not show in the excellent philosophically oriented history of statistical physics and the atomic theory of matter by Brush (1983).

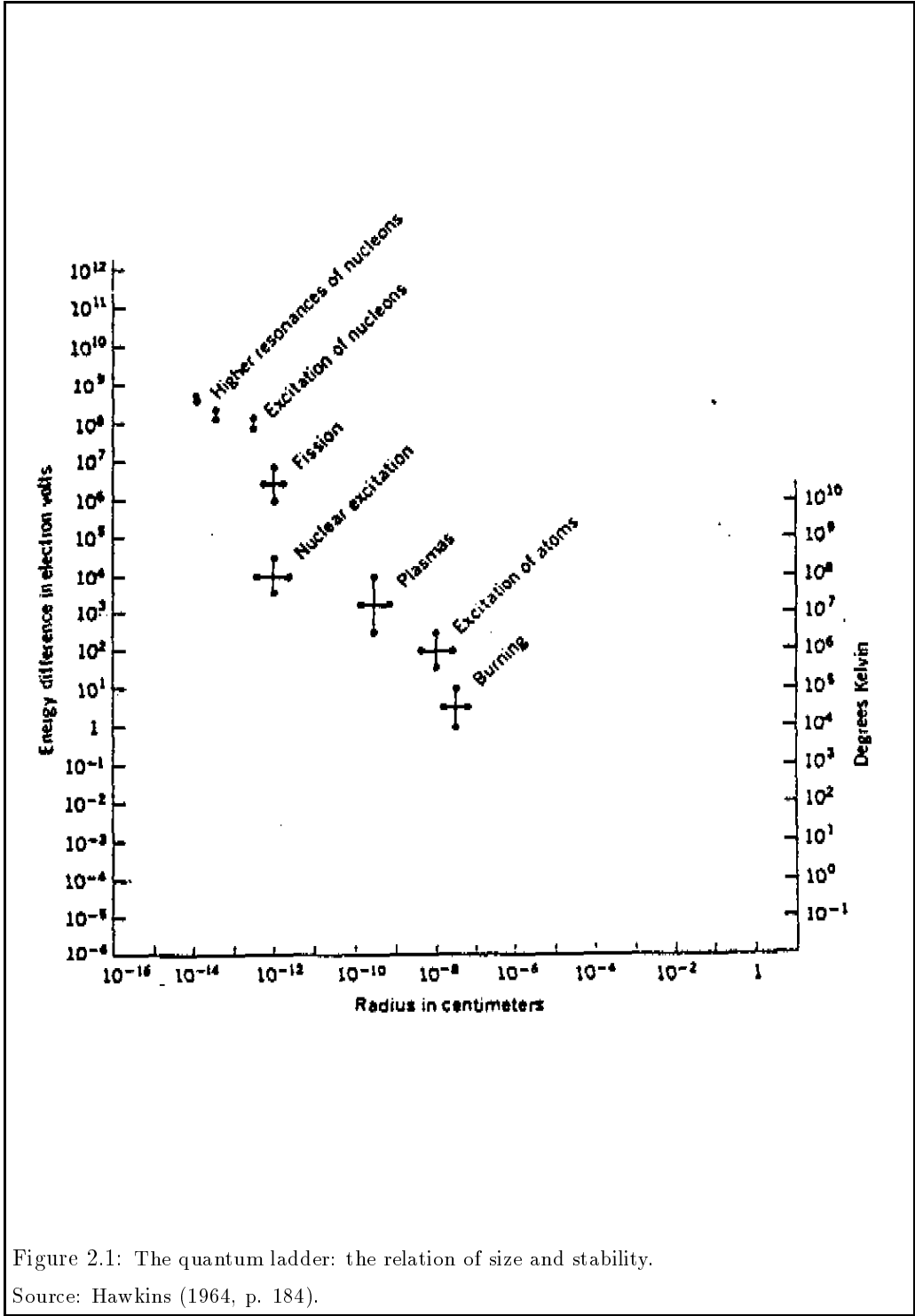


Figure 2.1: The quantum ladder: the relation of size and stability.

Source: Hawkins (1964, p. 184).

Table 2.1: Scales of Structure.

The table shows the principal steps in the structural hierarchy that builds our physical universe. Numbers quoted are taken to the nearest power of 10. The characteristic time is chosen to be the shortest duration over which the system transmits appreciable information or would undergo major structural change. For the first four entries this is the light travel time across the system. In the case of biological and social systems it is the reproduction or growth time. For stars, the average lifetime is given, but for the other gravitationally bound systems the free fall time (roughly the time for the system to implode under its own gravity) is more appropriate. The entry for the atom is the electron orbital time. The final entry refers to the age of the universe.

System	Size (m)	Characteristic Structural Feature	Mass (kg)	Time Scale(s)
Quantized gravity	10^{-35}	Spacetime foam	10^{-8}	10^{-43}
Quarks, leptons	$< 10^{-18}$	Structureless (elementary)	?	$< 10^{-26}$
Nuclear particles	10^{-15}	Union of quarks	10^{-27}	10^{-24}
Nucleus	10^{-14}	Union of nucleons	10^{-25}	10^{-23}
Atom	10^{-10}	Nucleus and electrons	10^{-25}	10^{-16}
Biological molecule	10^{-7}	Union of atoms, ions	10^{-20}	10^3
Living cell	10^{-5}	Complex order	10^{-10}	10^3
Advanced life form	1	Intelligent organization	10^2	10^9
City	10^4	Social order	10^{11}	10^9
Mountain	...			
Asteroid	$10^4 - 10^5$	Irregular	$10^{12} - 10^{13}$	–
Planet	10^7	Gravitationally dominated	10^{24}	10^4
Star	10^9	Nuclear reactions	10^{30}	10^{17}
Planetary system	10^{11}	Star and planets	10^{30}	10^8
Star cluster	10^{18}	Gravitationally bound	10^{35}	10^{15}
Galaxy	10^{21}	Nucleus and spiral arms	10^{41}	10^{16}
Cluster of galaxies	10^{23}	Largest known structure	10^{43}	10^{17}
Universe	10^{26}	Uniformity	10^{53}	10^{18}

Source: Davies (1982, p. 45).

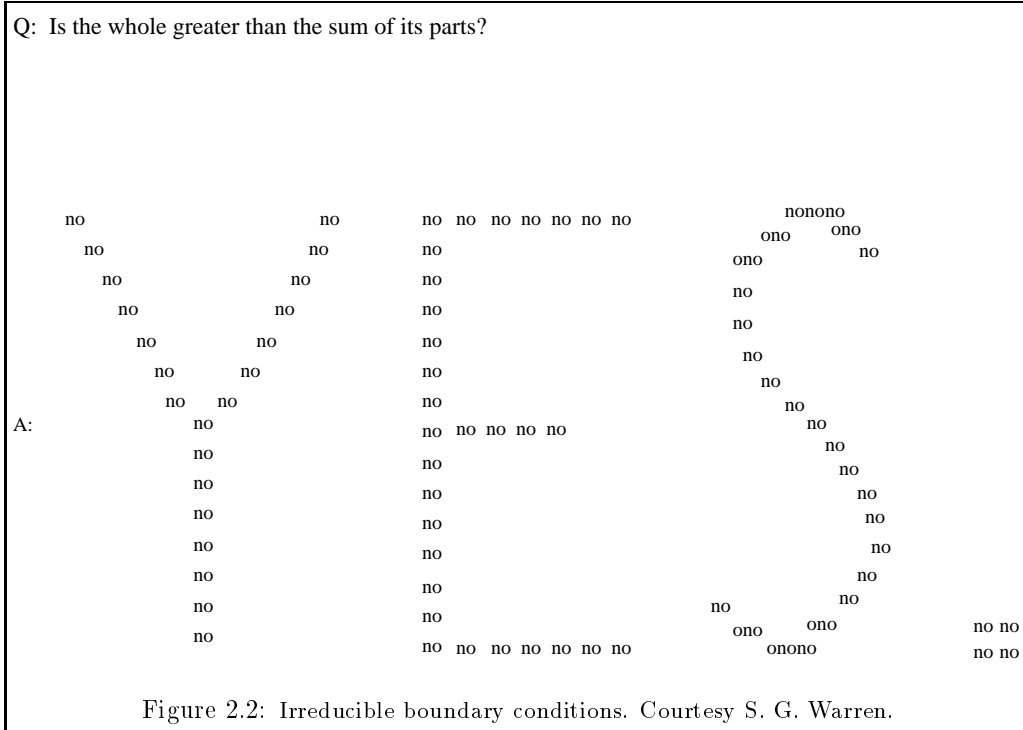
special case of isolated systems.⁸

The relevance of boundary conditions to the justifiability of modeling at less detailed levels of system description has also been conceived in hierarchical terms. Hawkins (1964), for example, has analyzed three levels of evolution: genetic, cultural, and individual. The sequence of codons in DNA is not determined by chemical law, but is a cumulative record of successful evolutionary experiments of the past. The gene pool of a population imposes boundary conditions on social and learning capabilities, and ultimately provides the capabilities for culture.⁹ Culture is similarly a cumulative record of successful human learning, and imposes boundary conditions on the learning potential of individuals. And individuals selectively introject elements of their cultural heritage. Only over time scales long compared with those of human learning do individuals transform their received culture through their cumulative contributions. And one day, I might add, we may, through eugenics and genetic engineering, on time scales long only as compared with those of our accelerating cultural innovation, transform our genetic capabilities.

Hawkins' three-level hierarchy reveals a distinctive characteristic of hierarchies of boundary conditions which inform or constrain: the ordering of time scales. The gene pool of the human species changes more slowly than our societies change, and our societies change more slowly than individuals. Thus, even though humans make small cultural waves, and cultures make small genetic waves, the information flow is generally greatest in the other direction. This situation is depicted humorously in *Figure 2.2*, in which the arrangement of the little component “nos” is entirely determined by a higher-level boundary condition: the shape of “yes.” (So much for rebellion.) Arguments for the irreducible contribution of higher-level boundary conditions to the evolution, development, and behavior of living things have been immortalized in the philosophical literature by such boundary condition designations as *the language of nature* (Hawkins 1964), *life's irreducible structure* (Polanyi

⁸Similar results have been obtained for near-equilibrium systems in which diffusion rates are found proportional to temperature gradients—independently of molecular detail. Note that as long as ergodic assumptions are not derived from first principles (see, for example, Sklar 1973), phenomenological thermodynamics that derives its legitimacy from ergodic assumptions must be considered a holistic alternative to molecular dynamics. In contrast with isolated and near-equilibrium thermodynamic systems, far-from-equilibrium thermodynamic systems—such as fluids systems that are open to sufficient energy to be unstable to convection—are extremely sensitive to small-scale perturbations, and do not lend themselves to simplistic forms of holistic modeling. In the cases of the atmosphere and oceans, which are dramatically far-from-equilibrium, the possibility of alternative solutions to their equations of motion, which meteorologists call *almost intransitivity* (Lorenz 1970), may jeopardize the adequacy of models developed at any level of detail. The challenges of modeling the atmosphere and oceans are considered in Chapters IV and V.

⁹A similar point was made by Polanyi (1968).



1968), and *downward causation* (Campbell 1974).

The causal role of slowly changing boundary conditions was on Darwin's mind when he proposed that the environment selects the genetic variant, but is itself only gradually altered by the resulting speciation.¹⁰ Since Darwin, variously conceived forms of functional explanation in evolutionary biology often have derived their legitimacy from philosophical arguments which appeal to the selective role of environmental boundary conditions in favoring functional genetic variations.¹¹ One step down the selection pressure/genome/culture hierarchy of boundary conditions imposed on organisms, behavioral genetic studies of the contribution of genes and cultural environment to learning have often played upon the statistics of familial genetic boundary conditions in seeking possible genetic contributions

¹⁰Recent evidence of the contributions of the first photosynthesizing bacteria to atmospheric oxygen levels and the impact of contemporary human activity on the climate suggest that both speciation and learning may have strong feedbacks on environmental selection pressure. Perhaps more dramatically, a simple act of parasitism that leads to symbiotic coevolution can have a profound effect on environmental selection pressures in a single generation (Corning 1983).

¹¹Wimsatt (1972b) and Wright (1976), for example, have appealed to the role of selection pressures in favoring variants that are *functional*. Machamer (1977) and Bechtel (1986b), on the other hand, have emphasized the irreducible contribution which the biological organization resulting from variation makes, along with higher-level selection pressure, to survival and reproduction.

to learning abilities and disabilities.¹²

One last type of hierarchical organization will be reviewed here, because its formal definition in terms of an ordering of degrees of causal interdependence appears to provide a framework for understanding all of the types of hierarchy considered above. Simon (1962, 1973, 1976) defines a *nearly decomposable* system as one that is decomposable into subsystems, subsystems, and so forth, in a special way: The components or variables that constitute or define each subsystem are on the average more interdependent than the components or variables of different subsystems.¹³ Nearly decomposable subsystems are defined, in other words, as clusters of components or variables with relatively strong interactions.

In models of system behavior not specifically concerned with the details of *intrasubsystem* processes, the relatively weak connections between the processes of different subsystems that characterize nearly decomposable systems justify the omission of much of the (*lower-level*) detail of interactions between subsystems. Subsystem interactions then are defined in terms of the spatially aggregated, temporally averaged, or otherwise *lumped* response of each subsystem to the similarly lumped outputs of other subsystems. The mathematical functions which map each subsystem's inputs to its outputs constitute a holistic model of the system.¹⁴ A special case of such a *near-decomposition* is clearly the structural hierarchy considered above.

Another special case of a near-decomposition arises if the time scales over which the constituent variables of subsystems vary are much shorter than the time scales of variation

¹²See the introduction to behavioral genetics by McClearn and DeFries (1973).

¹³Simon and Ando (1961) developed a mathematical definition of near-decomposability which relies upon a measure of variable interdependence, and upon which Simon's later discussions are based. Similar mathematical criteria for the appropriateness of neglecting certain variables or interactions of variables in models which predict or simulate certain aspects of complex systems have actually been developed in many areas of science. We will explore in Chapter V below a straight-forward measure of variable interdependence employed by climate modelers in the development of mathematical models of the earth's climate, namely the *sensitivity* of one variable of modeling interest to fractional changes in another variable ($\frac{dy}{dx/x}$). The present discussion does not presuppose a particular interpretation of variable interdependence, save for our assumption that the measure employed facilitates the calculation of average degrees of variable interdependence within and between candidate subsystems for the purpose of justifying omissions of specific intersubsystem or interlevel interactions.

¹⁴In modern system theory, as distinguished from the classical, state-variables mediate between inputs and outputs. Both treat linear systems, and it would seem that Simon's definition of near-decomposability may be designed, in part, to take advantage of linear system analysis. An introduction to some of the issues surrounding linearity assumptions in system theory may be found in Weinberg (1975). A relentless but mathematically supported diatribe against mindless linearization may be found in Berlinski (1976).

of the (aggregate or higher-level) variables which define subsystem behaviors.¹⁵ The high-frequency variability of constituent variables cannot (almost by definition) directly affect the much more slowly changing higher-level variables. Since over subsystem time scales their high-frequency constituent processes presumably have had time to reach some kind of statistically defined equilibrium, interactions between subsystems can be modeled in terms of aggregate (higher-level) subsystem variables. Hierarchies of boundary conditions, as interpreted by this model, are simply hierarchies of time scale.

The formality of Simon's definition of near-decomposability, combined with his persuasive arguments for the prevalence of nearly decomposable systems in nature and even in human problem-solving,¹⁶ have made his model of hierarchical organization one of the most widely discussed in the philosophy of science. One of his arguments for the prevalence of nearly decomposable systems in nature is cast in the memorable form of a metaphor of two watchmakers (Simon 1962). The story is retold here to dramatize how easily we can slip from an acknowledgment of the interesting hierarchical features of many systems to a world view that counsels a *decompose-and-conquer* strategy, as it were, for the modeling of every system in nature.

Hora and Tempus each have watchmaking businesses which are limited in success only by the swiftness with which the watchmakers can assemble watches from approximately 1000 components. Tempus assembled his watches one component at a time. Each time he had to put the watch he was working on down to pick up the phone and take an order, the watch fell apart into its original components. When he returned to work on the watch, he had to begin constructing it from scratch.

Hora, on the other hand, assembled his watch in three steps: (1) He first assembled about 100 subassemblies of about 10 parts each; (2) he next combined these into 10 larger subassemblies (containing about 100 of the elemental components each); (3) and he finally assembled the whole watch from the 10 subassemblies produced in step 2. During the first two phases, any interruptions would sabotage only the one subassembly he had been working

¹⁵As the above discussion of the hierarchical structure of the atom may suggest, these two special cases of a near-decomposition (structural and time-scale hierarchies) often go together.

¹⁶Simon's tendency to decompose complex systems into weakly interacting subsystems is revealed in his efforts to decompose complex human problems into subproblems solved in AI models by simple heuristic rules. The systems of nature and the methods of science suffer similar decomposition strategies in his recent contributions to AI models of scientific discovery (Simon 1977; Langley, Simon, et al. 1987).

on. Interruptions during step 3 would require a repetition of only this last and relatively un-time-consuming phase. Clearly, if phone calls were required for business, Hora would be more likely to succeed.

Simon's watchmaking metaphor has become well-known in systems theory circles, as it dramatizes so well the unlikelihood that complex systems such as organisms could evolve from their elemental components—say atoms—one atom at a time. Simon rightly suggests that biological evolution has proceeded in steps from atoms (and ions) stable at earthly temperatures, to inorganic molecules, to biopolymers, to cells, to multicellular organisms, to societies, etc. The stability of the structures of each stage paves the way for the next.

Had Simon argued only for the hierarchical *origins* of biological structure, his view would have been unobjectionable. But Simon argued further that hierarchical systems are prevalent in nature, that the subsystems of any level are relatively autonomous, and that different levels of structure may be studied one at a time. Simon implicitly presumed that systems which begin as aggregations of weakly interacting components remain fixed in their primitive organization, and never have the opportunity to become more highly integrated through further evolution. This assumption, in the light of numerous biological illustrations and the fundamental principles of evolutionary biology, has been the subject of considerable philosophical criticism in the past decade and a half.

We have had the opportunity to review only a small sample of the formulations and illustrations of hierarchical organization that have filled the system philosophy literature. Yet this may be sufficient to suggest how captivating the hierarchical view of systems can be. Suffice it to say that many system philosophers assumed that some type of hierarchical organization characterizes every system of scientific interest, and that modeling or developing theories at different levels is generally justified. We argue in Chapter III that many systems of scientific interest—physical as well as biological and social systems—fail to meet criteria for hierarchical decomposition, and illustrate this claim in Chapter IV with a detailed consideration of the complexities of the climate system.

2.3 Theoretic Levels in Logical Positivism

Logical positivism is one of approximately six interrelated movements in this century's philosophy which have been considered to fall under the broad category of analytic philosophy.¹⁷ If an area of philosophy as broad as analytic philosophy can be characterized at all, it is minimally concerned with sufficient clarification of philosophical problems to reveal whatever sense or nonsense should properly be associated with them. Logical positivism in particular began as a movement committed to the discovery of formal rules for distinguishing between metaphysical speculation which cannot appeal to experience or logic for confirmation and proper claims of fact or logical necessity.¹⁸

Because science has presumably made an art of distinguishing fact from metaphysics, much of the logical positivist literature is devoted to the philosophy of science. The fundamental commitment of positivist philosophers of science was the development of a universal logic of justification, one which would apply to all contemporary areas of science and all theoretical advances in the history of science.¹⁹ Toward this end, mathematical logic was applied in the hope of developing an ideal language of science into which any theory could be translated so that its confirmation requirements would be revealed.

At the outset of the movement in the 1920's, most positivists were *logical atomists* after the fashion of Wittgenstein (1922) and Russell (1918, 1924). Only certain types of simple statement were taken to be directly verifiable by observation. More complex statements of belief or theory, if they were to have any verifiable empirical meaning, had to be translatable into conjunctions of such simple verifiable statements, or be logical consequences of such conjunctions. In order to make such translations possible, it seemed necessary to distinguish between *theoretical terms* and *observation terms*. Theoretical terms were presumed to

¹⁷Depending on how they have been aggregated, different numbers of analytic movements have been distinguished. In one anthology of analytic philosophy, Klempke (1983) has identified six: G. E. Moore's versions of realism and "common sense," logical atomism, logical positivism, conceptual analysis, logico-metaphysical analysis, and linguistic analysis. Another helpful anthology is that of French et al. (1981).

¹⁸See Ayer (1952) and Ayer (1959), respectively, for a book that became something of a positivist bible and an anthology of some of the best known papers which emerged from the Vienna circle. See Achinstein and Barker (1969) for a helpful anthology of critical essays on logical positivism.

¹⁹It is beyond the scope of this introductory discussion to present a detailed account of positivistic philosophy of science. The most comprehensive and insightful account of which I am aware may be found in Suppe (1977). A briefer and extremely helpful review written for scientists unacquainted with the literature may be found in Bechtel (1988). Reviews of the forms of reduction—one of the central problems of the positivistic philosophy of science, and the one that best reveals the significance of positivistic notions of theoretic levels—may be found in Sklar (1967), Schaffner (1967), Nickles (1973), and Wimsatt (1974).

be definable in terms of observation terms which in turn represented directly observable phenomena.

Originally, observation terms were taken to represent incorrigible *sense-data* such as privately perceived colors and shapes (e.g., Carnap 1928). By the 1930s, however, most positivists were committed to physicalistic (e.g., Carnap 1930-31, 1931, 1932, 1932-33) and probabilistic (e.g., Ayer 1936) notions of evidence, such that intersubjectively acknowledged physical occurrences provided probable confirmation for the general statements or laws of science.

Two further developments led to a notion of *theoretic levels* which is of central importance in the present study, as it presupposes a system theoretic conception of hierarchical organization. One followed from the recognition that the physical-thing language into which it was hoped all scientific theories could be translated was only a universal language in principle—not in practice—and that even if the terms of a social science theory or biological theory were translatable into physical terms, there would be no guarantee that the theory so translated would not be too complex to confirm. Positivists distinguished between a *unity of language* provided, for example, by a translation of all theories into physical terms, and a *unity of laws*, achieved through a *deduction* of all laws and theories from physical theories (Carnap 1934).

It was widely acknowledged that the unity of laws had not been achieved. The success of the biological and social sciences demanded some other explanation. It was generally conceded that reduction by a physical theory was not historically necessary for the confirmation of higher-level theories, and that theories could be confirmed at their respective *levels of observation*. The acknowledgment of such a hierarchy of confirmation contexts challenged positivists committed to the progressive unification of laws to develop analyses of theory justification that were general enough to account for scientific progress at multiple *theoretic levels* as well as the gradual reduction of higher-level to lower-level theories. This challenge led to an interest in the logical requirements for two types of reduction: the *successional reduction* of earlier by later theories at the same theoretic level and the *interlevel reduction* of higher-level to lower-level theories.²⁰

²⁰These terms are Wimsatt's (1974). Other names for the same distinction include *homogeneous* vs. *heterogeneous* reductions (Nagel 1961), and *domain-preserving* vs. *domain-combining* reductions (Nickles 1973).

As long as the ultimate possibility of the *unity of language* could be presupposed in analyses of reduction, the reducing theory in interlevel as well as successional reductions could be viewed as a theory of greater generality, which, together with the special boundary conditions associated with the reduced theory, logically implies the reduced theory as a special case. In his account of *heterogeneous* (interlevel) reductions, for example, Nagel (1961) shows how definitions of the terms of the higher-level theory in terms of the lower-level theory—definitions which in many cases describe the boundary conditions presupposed by the higher-level theory in lower-level terms—may allow the deduction of the former theory from the latter. Such definitions are facilitated, for Nagel, if the lower-level terms represent the constituents of the phenomena represented by the higher-level terms.

It must be observed here that in accounts of interlevel reduction such as the above, positivistic reductionists appealed to exactly the same types of structural and boundary condition hierarchies that antireductionist system theorists appealed to in their defense of the autonomy and justifiability of higher-level models. The robustness of the hierarchical world view is dramatized further if we consider a second development (as promised above) which led to a refinement in the positivistic conception of theoretic levels.

This second development was the growing recognition of the difficulty of translating all the theoretic terms of many theories into observation terms, even—and especially—in the areas of science considered to be most fundamental. Indeed, quantum mechanics and special and general relativity—paradigms, if any were, of successful science—dramatized that such translations represented more than a philosophical difficulty. Such theories were not deducible from empirical generalizations. They rather provided a framework for deriving and unifying their empirical laws and for predicting phenomena that mere empirical generalizations could not.

After it was appreciated that lower-level theories are not the logical consequences of observation statements, the quest for a general model of theory confirmation forced positivists to allow higher-level theories the same privileges. Not only did theoretic levels provide shelter to unreduced theories that awaited the proverbial unification of laws; they housed theories that went beyond observations at that level to postulate theoretical laws that were confirmable only in view of their contributions to the coherence and predictive power of the theory as a whole (e.g., Hempel and Oppenheim 1948; Kemeny and Oppenheim

1955).

The notion of theoretic levels which are underdetermined by evidence represented no little difficulty for the kind of positivistic reductionist who required that successful new theories imply their successful predecessors as special cases. When the confirmation of theories is conceived *instrumentally* in terms of their predictive power, there is no guarantee that the theoretical terms of a higher-level theory could be translated into observation terms at its own level, let alone into the terms of a lower-level theory. The underdetermination of both levels of theory by their respective levels of evidence would make the meanings of theoretical terms in the two theories difficult to compare.

A new notion of reduction had to be developed which allowed a theory to reduce another successful theory if the former proved to have greater predictive power than the latter. Kemeny and Oppenheim (1956) and Oppenheim and Putnam (1958), for example, considered a type of interlevel reduction in which a lower-level theory predicts the observable behavior of the constituents of the observable phenomena predicted by the higher-level theory. The lower-level theory is shown to have greater scope if it predicts the behavior of these constituents whether or not they are constrained by the higher-level systems context or boundary conditions. This instrumentalist view of reduction proved to be persuasive, and reduction through the expansion of the scope of theories came to supplement the older conception of reduction through increases in generality or unconditionality.

There seems to be a two-fold irony associated with such instrumentalist notions of reduction. First, in order for a lower-level theory to predict the observable consequences of the higher-level theory, a weak notion of the *unity of language* had to be maintained in which higher-level observation terms (not higher-level theories) were translatable into lower-level observation terms. It was generally presumed that lower-level observations were *constituents* of higher-level observations (e.g., Oppenheim and Putnam 1958). Thus philosophers who had reservations about the descriptive value of theories proposed a general *description* of nature to provide the basis for the theoretical unification of science. Nature was organized hierarchically, with constituents within constituents, so that constituent theories could reduce higher-level theories of their aggregate behavior.

Second, the structural hierarchy proposed by such positivists was identical to the structural hierarchy proposed by system philosophers who were trying to justify the

autonomy and adequacy of higher-level theories. While for system philosophers systems that were relatively insensitive to changes in lower-level structures could be modeled holistically at the higher level of *systemic* behavior, positivistic philosophers saw constituent stability as the guarantee that theories which predicted constituent behavior could also predict the behavior of aggregations of constituents at higher levels of observation.

Thus the hierarchical world view was espoused by instrumentalists for whom theories are justified through their *predictive value*, as well as by inductivists who believed (at the beginning of the positivist movement) that theories *describe* the world. And moreover, the hierarchical world view was espoused by positivistic reductionists and holistic system theorists alike. We now turn to the *Weltanschauung* movement in the philosophy of science to find the same exact commitment to the hierarchical picture of the world.

2.4 Levels of Research in *Weltanschauung* Analysis

There are perhaps two major lines of transition from positivism to *Weltanschauung* analysis.²¹ These are associated, respectively, with dilemmas surrounding the notion of theories which are underdetermined by evidence, and the notion of observations which are independent of theories.

If a theory is underdetermined by the appropriate observational evidence, could not any number of theories take its place as long as they made the same predictions? How could the respective scientific virtues of equally good instruments of scientific prediction be compared? Even if we allow appeal to notions such as simplicity and coherence, what guarantee is there that we could define them adequately, and even if we could, on what basis could we assign them relative weights in theory assessment?

Difficulties such as the above lead us to search beyond a logic of justification for additional constraints on theory construction which would render theories less arbitrary. The great invention of *Weltanschauung* analysis is that those constraints are imposed by the historical research context in which theories are developed. Various names were invented

²¹See Suppe (1977) for a detailed account of the development of *Weltanschauung* analysis.

Table 2.2: *Models of the Context of Discovery.*

Specialized Research Context	Philosopher
<i>Theory-laden context of discovery</i>	Hanson (1958)
<i>Paradigm</i>	Kuhn (1962)
<i>Research program</i>	Lakatos (1963-4, 1968, 1970, 1976)
<i>Domain</i>	Shapere (1969)
<i>Disciplinary matrix</i>	Kuhn (1970)
<i>Discipline</i>	Toulmin (1972)
<i>Field</i>	Darden (1974)
<i>Research tradition</i>	Laudan (1977)

or redefined to designate such specialized areas of relatively organized research. Some of the best-known are listed in *Table 2.2*.

The philosophers listed in *Table 2.2* differ greatly in their conceptions of the specialized area of research. Indeed, it is questionable whether most philosophers of science would want to classify all of them as Weltanschauung analysts. For example, neither the proliferation of competing research programs promoted by Lakatos (1968) nor the interfield cooperation analyzed by Darden and Maull (1977) appear to render the single specialized research context sufficiently isolated and exclusive to qualify it as a *monopolizing world view*.

In calling the philosophers in it *Table 2.2* Weltanschauung analysts, I simply want to highlight a commitment which they have in common which stands in direct contrast to the positivist approach to the philosophy of science. The commitment of Weltanschauung analysis is to explore the context in which theories are developed—the *context of discovery* as it is called. How do the background assumptions, theories developed to date, research priorities, requirements for adequate observation, experimental methods, standards of explanation, and other factors unique to a particular specialized area of research all constrain the development of theories in this area of science? Even if the success of new theories developed within such a discipline were acknowledged by a broader scientific community, how might the *direction* of research emerge, to some extent, from the organization of the discipline or program?

While positivists were primarily concerned with the universal standards of justification, Weltanschauung philosophers were concerned with the historical contexts of discovery. The underdetermination of theories by evidence which tormented positivists was cured with a vengeance by Weltanschauung analysts who appealed to the social context surrounding developing theories to give them direction. Weltanschauung analysis replaced the image of a runaway set of alternative and equally confirmed or corroborated theories with the image of a social enterprise committed to the development of theories of a certain style. To the extent that such stylistic commitments were maintained in spite of anomalies, it might be said that the objective underdetermination of theories under the positivist conception was replaced by the subjective overdetermination of theories under the Weltanschauung conception.

The second line of development from logical positivism to Weltanschauung analysis is more radical in the sense that it explicitly denies any theory-independent conception of evidence. Positivists always had a great deal of trouble defining the notion of a theory-independent observation language. Observations were at different times interpreted phenomenally (in terms of sense-data), physicalistically, or most liberally, in terms of levels of direct observation. Even levels of observation, however, could not account for new theories aimed at the *same* level of observation which depended for their confirmation on new *types* of observation or evidence.

Once it is allowed that the nature of observational evidence depends upon the theoretical context from which the need for such evidence emerges,²² the positivist program of theory reduction no longer makes sense. It was shown above that the underdetermination of theory by evidence permits two theories associated with the same class of observations to be *incommensurate*, thereby obstructing any strictly logical deductions from the reducing to the reduced theory. The theory-ladeness of observation, however, represents an even more radical incommensurability,²³ since two theories which have their predictive value defined with respect to different types of observables could not be compared at all. Instrumentally conceived notions of reduction are refuted,²⁴ and the only sense of reduction left is outright replacement of one theory by another.

²²This was the thesis put forward by Hanson (1958) at the outset of the Weltanschauung movement.

²³See the helpful discussion in Newton-Smith (1981).

²⁴Most of the interest of historicists in theory incommensurability centered on its implications for successional rather than interlevel reduction, but as we shall see, it had implications for both.

This is the take-off point of Kuhn (1962, 1970) in his well-known theory of *scientific revolutions*. Major shifts in theoretical commitment simply cannot be justified in any formal way. They involve shifts in the types of problem taken seriously, in the types of solution considered, and in the types of evidence allowed. While some general scientific values are certainly relevant to any comparative assessment of different theoretical orientations, they are not well-defined, and their relative weights are subject to change (Kuhn 1973). In retrospect, we observe progress to have occurred in the major paradigm shifts in the history of science (Kuhn 1970, Postscript), but there is no paradigm-independent basis—at least no formal one—for assessing the virtues of the shift at the time. (See *Figure 2.3.*)

Within a paradigm—or *disciplinary matrix*, as it was later called (Kuhn 1974)—the situation is very different. *Normal science* (nonrevolutionary science) does have standards and makes possible, in fact, a very organized and fruitful scientific enterprise. Kuhn was not at all committed to the irrationality of science. On the contrary, he considered the disciplinary matrix to be the epitome of the rational enterprise. Problems and feasible solutions are clearly defined.

The picture of a self-contained disciplinary matrix was intended to challenge all positivistic notions of a universal logic of justification and progress through theory reduction. It had the unintended consequence, however, of providing no basis for interdisciplinary cooperation. As Kuhn later acknowledged, there could not be an *interdisciplinary matrix* unless it met all of the criteria of normal science, i.e., of a disciplinary matrix (Kuhn 1978). Legitimate interdisciplinary research had to await, in other words, the emergence of a new discipline.

By artificially isolating his disciplinary matrix, Kuhn not only removed it from the continuum of historical scientific progress, he removed it from the *matrix*, as it were, of overlapping and interacting areas of contemporary science. The position is a consistent one. Not only did Kuhn's position claim a refutation of *successional* forms of reduction (reduction of earlier by later theories in the same area of science), but it implicitly claimed a refutation of *interlevel* forms of reduction (such as the reduction of chemistry to physics). Kuhn's model of disciplinary progress killed two positivistic birds with one stone.

Kuhn's position was a powerful one, but it created a dual crisis for many who saw the importance of analyzing the historical research context of scientific progress. First, how is

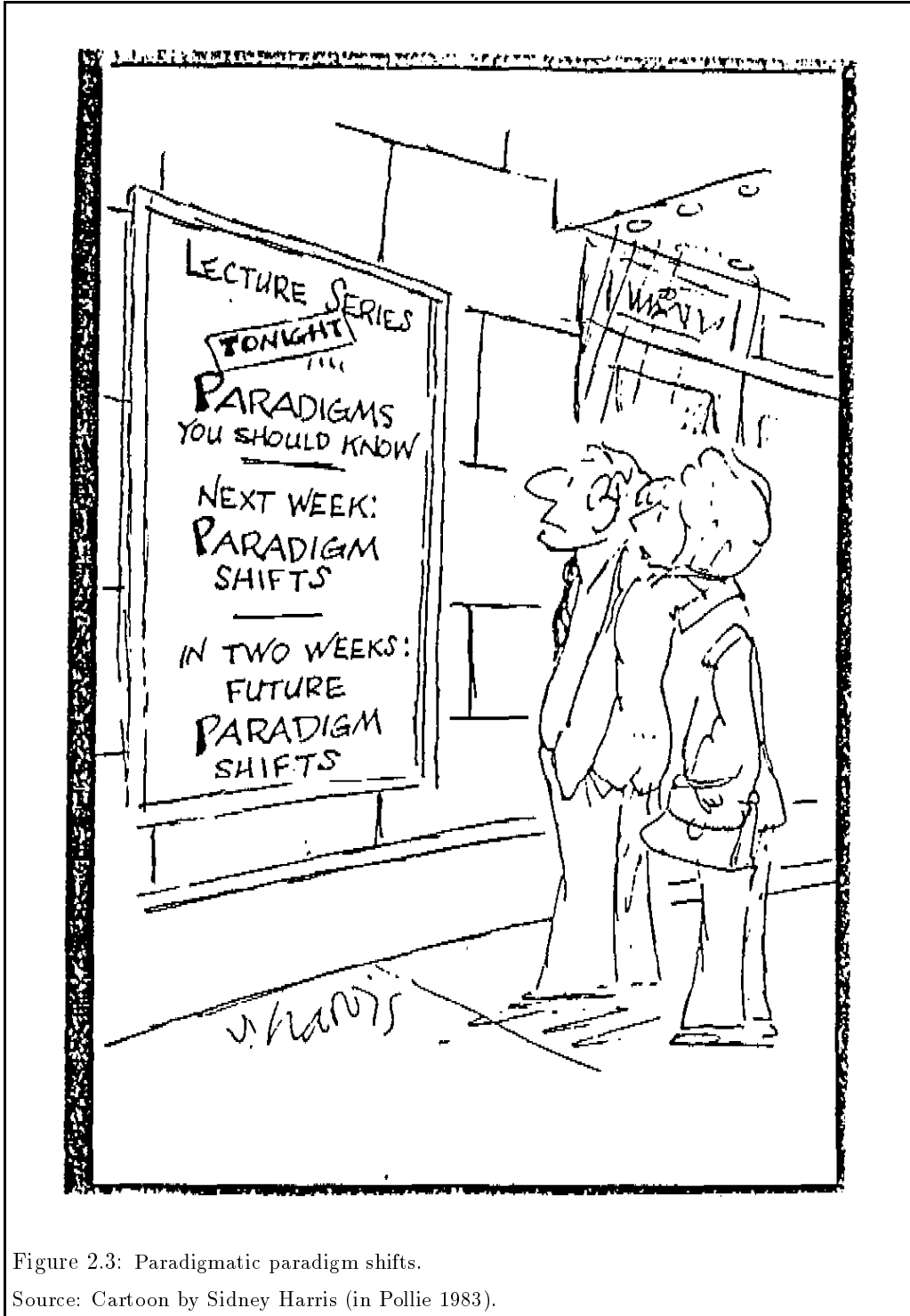


Figure 2.3: Paradigmatic paradigm shifts.

Source: Cartoon by Sidney Harris (in Pollie 1983).

it possible to steer a middle path between a logical interpretation of scientific rationality and a denial of rationality in the historical succession of major theories? Second, how is it possible to steer a middle path between a logical interpretation of scientific rationality and a denial of rationality in the interactions of contemporary areas of science?

Toulmin (1972) and Darden and Maull (1977; Maull 1977) were concerned, respectively, with answering the first and second of these questions. Their views have hierarchical structures, and from the point of view of this study, are significant for defining *levels of research* in which theoretical levels are embedded, much as genes are embedded in cells.

Toulmin invented the notion of an *intellectual ecology*—actually, a multidisciplinary ecosystem—to provide the selection pressure, in his evolutionary analogy, for mutating disciplines. While theoretical and methodological *innovations* which are sufficiently conventional do not strain the assessment resources of the disciplinary context, major innovations—those which change the *direction* of the discipline—do. The intellectual ecosystem is a broader scientific community than the discipline in question, and its average scientific values determine the acceptability of changes in disciplinary direction.²⁵

Actually, Toulmin’s concept of a discipline is defined loosely enough so that the intellectual ecology could be a broader discipline, and a discipline would function as an intellectual ecosystem for its subdisciplines. The picture is one of nested disciplines—subdisciplines within disciplines within fields or branches of science within the scientific community. Higher or more inclusive levels change more slowly, and thus provide boundary conditions for their more rapidly changing lower or more specialized levels. All disciplinary innovations, at whatever level, must be *selected*, either at that level or at some higher level. The level at which the selection occurs reflects the scientific impact of the innovation. The role of boundary conditions, on this reading of Toulmin, is similar to the role of boundary conditions invoked by system theorists in characterizing evolutionary explanation. This is no accident, as Toulmin’s view of science is based on an evolutionary metaphor.

It is to be observed that Toulmin’s nested hierarchy of research contexts cuts a middle path between the two extremes of successional reductionism and Kuhnian revolutionism.

²⁵Toulmin, in fact, also makes room for social factors to influence disciplinary direction without jeopardizing the scientific community’s role in certifying scientific progress. Science policy and funding pressures, as well as the sociological and political characteristics of the scientific community, can influence research priorities without affecting scientific standards. See *Figure 2.4*.

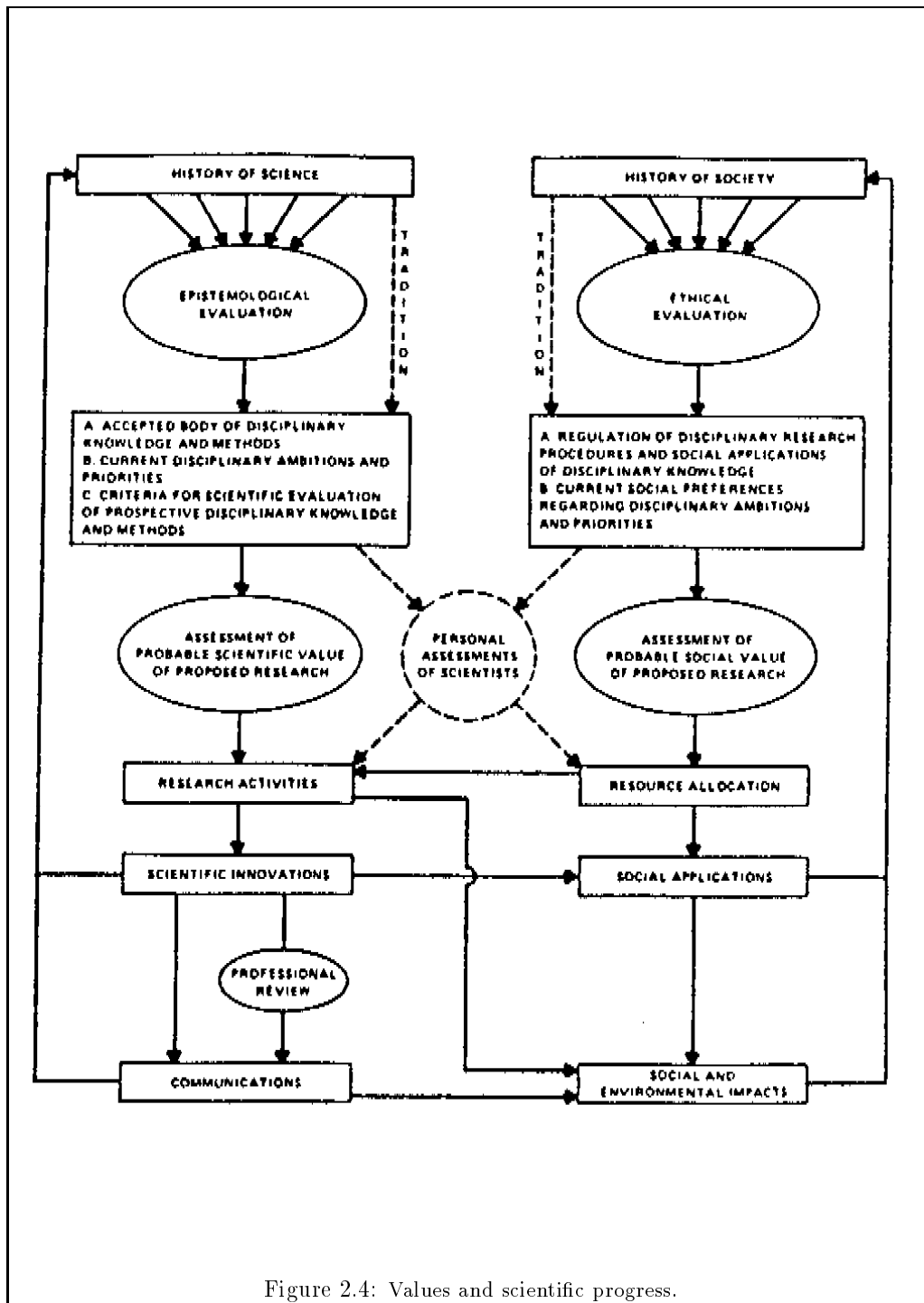


Figure 2.4: Values and scientific progress.

Rational scientific change is guaranteed for Toulmin by the scientific community which assesses the change; yet that scientific community—together with its *ideals of explanation* and other scientific values—is itself subject to change on longer time scales. Darden and Maull’s work on interfield theories (Darden and Maull 1977; Maull 1977) is intended to cut a middle path between the extremes of interlevel reductionism and the runaway fragmentation of science into noninteracting areas of specialized research. While Toulmin’s model of progress explicitly appeals to an analogy with the evolution of biological species, Darden and Maull’s model of progress appeals implicitly—but unmistakably—to a special type of biological evolution: coevolution.²⁶

Darden and Maull (1977; Maull 1977), in the context of several case studies of interdisciplinary cooperation in the biological sciences, developed a model of *interfield theories* which shows how one field may solve a problem which arises in a second field without replacing the second field or reducing any of its theories. In the special case of different fields which have their primary (but not exclusive) interests in different levels of organization (such as the levels in *Figure 1.1*), the deeper (lower-level) field may provide details regarding the components or constituent processes the aggregate behavior, systemic functions, or causal consequences of which are studied by the higher-level field (Maull 1977, p. 156).

While Toulmin considered a hierarchy of selection pressure imposed by different levels of the so-called intellectual ecology on innovations in disciplinary direction, Darden and Maull consider a hierarchy of fields defined by the primary levels of organization associated with their respective domains. Toulmin’s hierarchy is at the least a hierarchy of scientific values—ranging from the most specialized to the most universal. As we have suggested above, there is some reason to interpret this hierarchy as associated with a nested hierarchy of disciplines. Darden and Maull’s hierarchy is at the least a hierarchy of empirical concern—which they call *levels of description*.²⁷ There is reason to interpret their conception of the fields which participate in interfield theory development as levels of empirically related but methodologically autonomous areas of research. This interpretation is both in keeping with Darden’s original (1974) definition of fields and with Maull’s (1977) notions of *levels of description* and the *depth* of a field (the lower the level of description of a field, the greater

²⁶The coevolution of species may be defined as a reciprocal evolutionary change that involves the partial coordination of nonmixing gene pools (Thompson 1982).

²⁷This is to be distinguished from Pattee’s (1973a,b) notion of levels of description defined above.

its depth).

Maull's notion of a descriptive level is a scientific *domain* (Shapere's 1969 notion of an interrelated area of background knowledge and empirical interest defined by a central problem or problems) that has its primary *but not exclusive* focus at a level of organization. A field, on Darden's view, applies a specialized methodology to a domain. By employing a notion of fuzzy levels, Darden and Maull make room for the intersection of domains of several fields which study different descriptive levels of the same system. The lower-level field is presumed to be concerned primarily with the constituents of the systems which are of primary interest to the higher-level field. To the extent that the parts of the system are relevant to the functioning of the whole or vice versa, there is some reason to expect the domains of the respective fields to intersect. This intersection sets the stage for one of the fields to develop an interfield theory.

For Darden and Maull, one field adopts or addresses itself to a problem which arises in the second field, the former field solves the problem, and the latter field utilizes the result.²⁸ The theories of the two fields become mutually adjusted to one another, much as the phenotypes of coevolving species become mutually adjusted to one another. The methodologies of the two fields undergo transformations to make their emerging theoretical developments possible. This is analogous to the coordination of gene pools associated with biological coevolution. But the two fields do not become involved in cooperative research per se. This would involve an integration of methodologies which would be as antithetical to the notion of fields as an integration of the gene pools of several species would be antithetical to conventional notions of species.²⁹

To carry the analogy a step farther, Darden and Maull's conception of the development of an interfield theory is asymmetrical much as parasitism—a special kind of coevolution—is asymmetrical. Parasitic species at first adapt to the host species, rather than the other way around. And parasites are most successful if they do not kill their hosts. The field which Darden and Maull call the deeper field—i.e., the one concerned primarily with lower levels of organization—at first adapts to the theoretical context of the upper level field. And if the

²⁸The more recent consideration by Darden (1986) of less asymmetric forms of cooperation among several disciplines is reviewed in Chapter III below.

²⁹Note that the endosymbiotic theory of cell evolution suggests that bacterial species not only coevolved to create eukaryotic cells, but that they actually exchanged genes (Margulis 1981). Indeed, it is known that different bacterial species often exchange genes with the help of plasmid vectors.

deeper field is to find confirmed applications at the higher level, it does not reduce or replace the upper level field. Successful interfield theories, like successful parasitism, may set up a sequence of adaptive interactions which accelerates the evolution of both participants.

By allowing *levels of research*, as I shall call them, to be methodologically autonomous, Darden and Maull provided the same kind of basis for interdisciplinary progress that Weltanschauung analysts had provided for specialized progress: the specialized methodology. While models of reductionistic progress appeal to positivistic notions of universal forms of justification, Darden and Maull's model of interfield progress appeals to the historical context of discovery to provide a basis for rational scientific judgment. Unification is achieved without reduction. Cooperation in theory development is achieved without cooperation in research.

2.5 The Hierarchical Compromise

We have considered in this chapter how three schools or movements in this century's philosophy of science came to hold hierarchical positions. System philosophers, in defense of modeling systems at less detailed levels of description, promoted a hierarchical view of nature and a hierarchical interpretation of the organization of complex systems. The characteristics of various forms of hierarchical organization which have served such holistic purposes have been captured by Simon's (1962, 1973, 1976) notion of the nearly decomposable system.

Positivists were committed to analyzing the requirements for the progressive unification of all areas of science through a program of the reduction of theories by theories of ever greater generality or scope. Characterizing domain-combining reductions, as they were called, of theories in different areas of science was especially challenging as the theories generally appeared to be about different phenomena. Generally the terms of the two theories are too different to easily derive one theory from the other; and their observational consequences are too different to show that one theory predicts and explains more than the other. By assuming that these theories were about different levels of organization in nature, positivists were able to show how a theory about the constituents of phenomena addressed by another theory could imply the latter theory as a special case or at least imply its observable consequences under special boundary condition assumptions.

Table 2.3: *Hierarchy and Asymmetry.*

Philosophers	Problem	Relevant Hierarchy	Solution	
Simon	Incomplete description	Levels of organization	Boundary conditions ↓	Averaging or aggregation ↑
Nagel Kemeny & Oppenheim	Domain-combining reductions	Theoretic levels	Definitions ↓	Interlevel reduction ↑
Darden & Maull	Inter-disciplinary progress	Levels of research	Vocabulary problem, & context ↓	Interfield theory ↑

Weltanschauung analysts had characterized progress internal to a paradigm, discipline, or field so well that the rational basis for transitions between paradigms or for interdisciplinary forms of scientific progress remained central problems for the Weltanschauung view of science (except for those Weltanschauung analysts who neither believed in progress nor in interdisciplinary cooperation). A hierarchical conception of the scientific community was proposed to provide the basis for the assessment of changes in disciplinary direction (Toulmin 1972), and a hierarchical conception of scientific specialization was proposed to provide a shared systems context for interdisciplinary cooperation (Darden and Maull 1977; Maull 1977).

The hierarchical commitments of the three schools are summarized in *Table 2.3*. The table shows how the three schools are actually concerned with different problems, but that their solutions reveal analogous asymmetries in the interlevel relationship which they propose.

The arrows in *Table 2.3* indicate the direction of scientific information flow between levels of concern. In Simon's model of nearly decomposable systems, higher levels of organization change more slowly and thus provide boundary conditions for lower levels of organization (note downward arrow), and the processes internal to the subsystems of a level are summarized as subsystem outputs in some average or aggregate measure of their consequences on subsystem time scales (upward arrow).

In accounts of domain-combining reduction such as those of Nagel and Kemeny and Oppenheim, higher-level terms are translated into lower-level terms as explained above. This is indicated by the downward arrows between theoretic levels. The upward arrow indicates the logical deduction of either the higher-level theory or its observable consequences from the lower-level theory. And in Darden and Maull’s analysis of interfield theories, the problem, vocabulary, and background information are transferred (downward arrow) to the deeper (lower-level) field, and the latter’s solution is transferred (upward arrow) to the higher-level field.

One consequence—and perhaps one motivation—of the development of the above hierarchical perspectives was the relief of certain fundamental tensions between the three philosophical movements. While system philosophers differed from positivists in that the former tended to be *holistic* and the latter tended to be *reductionistic*, both holists and reductionists came to share their promotion of hierarchical conceptions of systemic complexity. Both groups were motivated to imagine systems to be as simple as possible; holists hoped to justify omissions of descriptive detail, and reductionists hoped to establish the cross-domain definitions or bridge-laws required for interlevel reduction.

While holism was at one time connected with metaphysical claims that the whole is *greater than*—that is, in principle irreducible to—the sum of its parts,³⁰ the development of system philosophy led it, ironically, to hold that the whole is *less than* the sum of its parts, that is, that all constituent processes need not be considered for many modeling purposes. Furthermore, less detailed descriptions were promoted by system theorists as practical necessities—not metaphysical necessities.³¹

Reductionists of the 1930s, on the other hand, in their commitments to a physicalistic observation language, preferred to neglect all but the lowest levels of evidence. The

³⁰See J. C. Smuts’ (1926) classic, **Holism and Evolution**.

³¹In fairness, it should be pointed out that many holistic system theorists—especially among those not chiefly concerned with mathematical modeling issues—remain committed to the existence of such “emergent” properties as biological functions and mental phenomena. Although emergent phenomena are not, by definition, just the average or aggregated behavior of lower-level phenomena, the evidence for them is not usually claimed to be derived from direct empirical observation. As long as theories of these emergent phenomena are construed instrumentally rather than realistically—to be valued for their predictive but not their descriptive value—arguments in their favor can remain perfectly consistent with a belief in a hierarchy of descriptive detail which provides the empirical context for the development and testing of theories at any level. Thus even theories of emergent properties can avoid radical incommensurability with lower theoretic levels and share a commitment to the descriptive hierarchy presupposed by positivists. As we shall see, however, this hierarchical compromise often fails to address legitimate scientific problems about interactions between levels.

development of positivism led reductionists to acknowledge the importance of various levels of evidence, and to agree with holists that higher-level science is, for the time being, a practical necessity. The difference between the two viewpoints is no longer fundamental. It is rather a difference of opinion as to the prospects for the eventual reduction of higher-level to lower-level models, theories, or branches of science.

A second tension overcome by what I shall call the *hierarchical compromise* is the tension between the *realism* of mathematically oriented system philosophers and the *historicism* of Weltanschauung analysts. System analysts never challenged the commonsensical belief in the outer world, nor did they have reason to be skeptical about the capacity of mathematical models of complex systems to describe features of that outer world—to capture, in approximate form, objectively existing patterns of systemic structure, behavior, and evolution. Models are realistic—if simplified—descriptions of how certain clusters of interrelated phenomena are (in fact) organized.

Weltanschauung analysts, on the other hand, were committed to the notion of a *theory-laden context of discovery* (Hanson 1958). Although observations could confirm, they could only confirm if they fulfilled the definition of observations implicit in the theoretical context. The joint commitment to the constraints of the historical context on what counts as observation and the objectivity of evidence has been called *historical realism* (Suppe 1977). What we can observe or measure in a given theoretical context depends upon what we are looking for, the technology employed in our observations, and how we interpret what we find. These observational interests, methods, and assumptions are constrained by the scientific domain of interest—i.e., by what we take to be background knowledge and what we take to be problematic about it. As theories grow and change, the domains of concern change with them, and new forms of observational evidence emerge. These forms of evidence are objective to the extent that they provide empirical information relevant to theoretical progress.³²

While many system theorists have been *naive realists* in the sense of presuming scientific description to represent the structure of the outer world, those Weltanschauung analysts who have been liberal enough in their historicism to acknowledge scientific progress have been committed to a historical form of realism which allows scientific observation to capture

³²A clear account of how scientific observation is constrained by background information but contributes to the growth of knowledge may be found in Shapere (1982).

only those features of the world which are historically considered to provide relatively direct evidence. The tension between the two views is associated with their respective interests in the constraints of the objective organization of systems on observable patterns, and the constraints of the historical organization of areas of scientific knowledge and research on the patterns which are observed.

This tension has been greatly relieved, in the opinion of this writer, by the recent work of Darden and Maull (1977; Maull 1977; Darden 1986). By connecting the domains of several fields in virtue of the relationships between levels of organization of a complex system which the respective fields are primarily concerned to address, Darden and Maull have established a connection between the structure of the world and the structure of scientific domains.³³ Even if the disciplines that study different levels of a system historically developed their respective theories or models independently, they are theories or models, after all, about different aspects of the same system. Their mutual relevance should not come as a surprise.

If the realism of Darden and Maull is still historical, it at least allows some structural features of the world to constrain the emergence of domains. Domains cut at the joints when they develop in nonarbitrary ways, and may find themselves at crucial stages of their development at different rungs of the same hierarchy. Interfield theories are not historical accidents, as it were, but hypotheses as to objective connections between historically constrained contexts of empirical concern. While system theory and historical realism continue to differ in their respective interests in systemic organization and the historical organization of science, they now seem to agree in their metaphysical commitments to a well-structured and theory-independent world.

Yet there is a sense in which the hierarchical adjustment of *Weltanschauung* analysis to system theory has not gone beyond the fundamental commitments of *Weltanschauung* analysis in any revolutionary way. Although domains (or levels of description, as Darden and Maull call them in the context of interfield progress) may intersect by virtue of the interfield relevance of part/whole, structure/function, and process/consequence relationships, and although interlevel theories link these domains in hybrid ways, the research of the respective fields is still conducted independently in accordance with different methodologies. If Darden

³³Together with Wimsatt's analysis of complexity, Darden and Maull's work on interfield theories in the seventies provided the foundations the *historically realistic* departure of the eighties that merged for the first time analyses of systemic complexity, interlevel explanation, and interdisciplinary cooperation. These developments are reviewed and extended in Chapter III.

and Maull liberate domains and theories from confinement within a paradigm or disciplinary matrix, they have not liberated levels of research (fields)—or had not in 1977—in the same way. The specialized context of discovery yields in its theoretic possibilities to structural features of the world, but the empirical procedures of research remain dependent upon specialized methodologies internal to fields. Interfield methodologies do not emerge with interfield domains.

Despite their historical realism, then, Darden and Maull have been classified as Weltanschauung analysts for the purposes of the present review. Since system theorists often have been concerned to justify the legitimacy of methodologically autonomous levels of research which study different levels of the same system, there does not seem to be any conflict between hierarchical views of systems and hierarchical views of the organization of specialized research.

A third tension overcome by the *hierarchical compromise* is the tension between the *context-independent* form of theory confirmation required by positivism and the *context-dependent* form of theory justification required by Weltanschauung analysis. Positivism was committed to the development of an ideal language of science and a logic of confirmation which would allow any scientific theory to be translated into a language which clearly distinguished theoretic and observation terms in such a way that the laws of the theory—or on alternative conceptions of confirmation, the theory as a whole—could be confirmed—on many accounts probabilistically—through direct observation. Weltanschauung analysis, on the other hand, maintained that no such universal language of science and logic of confirmation is revealed in the actual history of science, and that instead, the criteria of theory certification are to be found in the interrelated commitments to background knowledge, problems, methods, explanatory ideals, and the like which constitute the disciplinary matrix or specialized area of research.

Hierarchical perspectives led to a relief of this tension between *positivism* and *pluralism*, as it had to the respective tensions between *holism* and *reductionism*, and between *realism* and *historicism*, which have been discussed above. In view of the generally acknowledged progress of the biological and social sciences, positivism maintained commitments to a universal language of science and a universal logic of confirmation by: (1) presuming a unity of language, or at least a unity of observation languages, in the sense of presuming

observation terms in all scientific theories to be translatable, in principle, into a physicalistic language; (2) presuming that physical theories were about the constituents of chemical theories, physical and chemical theories about the constituents of biological theories, etc., so that the terms of higher-level theories—or at least the observable consequences of higher-level theories—could be defined in terms of constituents addressed at lower theoretic levels; (3) presuming the same logic of confirmation to apply at all levels of suitably direct observation.

The challenge for positivists to account for the success of a variety of areas of science was met by the postulation of theoretic levels. This postulation was shared, as we have explained above, by *Weltanschauung* analysts such as Darden and Maull who wanted to account for the interactions of different fields or branches of science. Both positivists and Darden and Maull acknowledged that higher-level and lower-level theories were connectable. The difference between their perspectives is a subtle one. Positivists generally believed the connection to be one of definition; Darden and Maull believed it to involve an interlevel theory which required confirmation. Even while requiring confirmation for their interfield theory, however, Darden and Maull clearly assigned the responsibility for that confirmation to the specialized research context of the so-called deeper field. Whether they were motivated by the requirements for confirmation or by the requirements for the progress of individual fields, both positivists and interfield theorists came to share their hierarchical conceptions of scientific progress.

The irony of the above agreement as to the hierarchical organization of science is that the parties to it were positivists who presumed a single logic of confirmation to apply to all levels, and *Weltanschauung* analysts who subsumed the context of justification under the context of discovery. What led Darden and Maull to deny reduction as an ideal of progress was their respect for the actual history of science. What led positivists to deny any fundamental pluralism as regards scientific method was their commitment to the potential unification of all science. Despite this implicit disagreement as to the degree of generality attributable to methods of scientific confirmation, the commitment of positivists and pluralists alike to levels of research which must appeal to appropriate levels of evidence represents a compromise which lessens the tension between generalized and contextual models of scientific progress.

That the three schools are more consistent with one another when they each espouse

certain hierarchical viewpoints is part of the explanation for the apparent resistance of each of these schools to more complex perspectives. It is as if a conceptual steady-state has made itself stable with respect to the perturbations of alternative viewpoints. Another reason for the scarcity of alternative perspectives is that science itself has probably proceeded in approximately hierarchical fashion for most of its history. Yet the degrees of systemic complexity considered today—with the advantages of observation and computing technologies peculiar to recent decades—far exceed any considered in the earlier history of science. Such complexities have invited more complex models and theories, and more complex forms of interdisciplinary cooperation. To the extent that the philosophy of science has been based on earlier forms of science, it perhaps has been justified in its hierarchical models of systemic complexity, scientific explanation, and interdisciplinary cooperation.

Whether the philosophy of science has kept pace with science adequately the author cannot say. It would seem, however, that it would be of interest to consider modifications of the conventional philosophical positions which may do greater justice to recent developments in science. Such alternatives are reviewed or developed in Chapter III and applied to climate modeling in Chapters IV and V.

Chapter 3

Interactional Complexity, Interlevel Explanation, and Interdisciplinary Cooperation: The Case of Climate and Climate Modeling

3.1 Introduction

The conventional hierarchical view of complex systems has served us well in the philosophy of science. Dividing up the world into variously conceived levels of reality, we have identified separate, idealized domains of scientific inquiry that have enormously simplified our analyses of science. Laws, theories, and disciplines are easiest to analyze when they are associated with well-defined empirical contexts—with the levels or subsystems of Simon's (1962, 1973, 1976) *nearly decomposable* systems (reviewed above). Laws and theories are presumed to enjoy determinate scope, simplifying the philosopher's job of characterizing the basis for their justification. Areas of legitimate inquiry within each discipline are similarly presumed to be restricted, simplifying the philosopher's analysis of scientific research and discovery. While scientific rationality and method pose motionless for philosophical cameramen, too often neglected are the more complex and perhaps the most interesting empirical concerns

and historical transitions of science.

However justified such a strategy has been in the analysis of early stages of scientific inquiry, the physical, biological, and social branches of science today enjoy frontiers of a more exciting nature. The nonatomistic theories of quantum mechanics;¹ the nonlinear mathematical models of the geophysical, atmospheric, oceanic, ecological, and astrophysical sciences;² the perplexities of far-from-equilibrium thermodynamics³ and chaos theory;⁴ the *interactional complexity*⁵ inherent in biochemical processes⁶ and in the positive synergisms that often drive biological evolution⁷ and social change⁸—all bear witness to the inadequacy of hierarchical presuppositions about the subject matter of science.

An alternative to the hierarchical view of systemic complexity, introduced to the respective literatures of philosophical biology and psychology by William Wimsatt (1972a, 1975), has profound implications for the interdependence of different levels of scientific explanation and for the structure of the interdisciplinary cooperation that may be required to explore that interdependence. Wimsatt's model of complexity points to the importance, in many highly organized systems (and, we might add, in many chaotic systems), of causal interactions between phenomena defined at different levels and/or between the constituent processes of different putative subsystems. Such *interactional complexity*,

¹Čapek's (1961) exploration of **The Philosophical Impact of Modern Physics** in his classic book of that title remains one of the clearest analyses of the breakdown of the atomistic world view implied by both quantum mechanics and relativity. See Appendix B below for a discussion of key features of this revolution in the metaphysical or ontological presuppositions of theoretical physics.

²Climate models, which integrate these and other areas of mathematical modeling, will be the subject of the case study in Chapters IV and V below.

³Prigogine and his school have done much to popularize the nondeterministic implications of self-organizing systems which can change steady-states in response to small perturbations. Of Prigogine's work, Prigogine (1980) and Prigogine and Stengers (1984) are perhaps the most accessible to nonmathematicians. Helpful reviews by sympathetic scientists in other fields may be found in Jantsch (1980), Laszlo (1987), and Davies (1988). An early philosophical review may be found in Denbigh (1975).

⁴An excellent popular review may be found in Gleick (1987). A review article by the pioneers of the field may be found in Cruthchfield, Farmer, Packard, and Shaw (1986).

⁵This term was coined by William Wimsatt (1972a, 1975) to represent systems with strong interactions between subsystems or levels. Wimsatt's model of complexity will be reviewed below.

⁶For a philosophical analysis of the interconnectedness among enzyme systems and its significance in the emergence of biochemistry as a discipline, see Bechtel (1986a)

⁷See Wimsatt (1972a, 1975) for mention of, and Appendix A below for a development of, the interactional complexity inherent in the evolution of eukaryotic cells, social amoebae, and social insects.

⁸See Corning (1983) for a survey of the variety of synergisms important in the history and politics of our species and a comparative analysis of the contributions of synergism to the *progressive*—i.e, complexifying—aspects of biological evolution and social change. Appendix C below contains a reprint of my own analysis of an important social synergism—between scientists and policy makers—inherent in successful policy analysis.

as Wimsatt calls it, may render the distinctions between different levels and between any subsystems associated with these levels obscure. As we shall see, the existence of interactional complexity may furthermore necessitate the integration of laws, theories, or models previously associated with different levels or subsystems in explanations of the structure, behavior, and evolution of the system, and the methodological integration of several disciplines or areas of science previously presumed to study independent aspects of the system.

The relevance of Wimsatt's view of complexity to issues regarding the reducibility of biological and psychological theories, the status of functional explanations, and the motivation for the progressive integration of cooperating disciplines has been recognized in the philosophy of science literature. Yet most would-be analyses of interlevel science ironically regress to hierarchical presuppositions and may fail to appreciate the full implications of interactional complexity for the integration of levels of explanation and the integration of cooperating disciplines. And while the importance of interactional complexity in biological and psychological science has been recognized, the significance of the notion for the physical sciences has evidently escaped notice. We shall review Wimsatt's rather formal definition of interactional complexity immediately below and go on to explore its applicability to physical as well as biological and social systems and its implications for interlevel explanation and interdisciplinary cooperation.

3.2 Interactional Complexity

Wimsatt (1972a) defines interactional complexity in the context of a *near-decomposition* (Simon 1962, 1973, 1976) which results in an unacceptably high error due to subsystem interactions. It will be recalled that a near-decomposition of a set of variables into relatively independent clusters of variables is based on the assumption that the average interdependence between the variables of a single subsystem (cluster) is far greater than the average interdependence between the variables of different subsystems.⁹ This assumption

⁹As noted in Chapter II above, Simon and Ando (1961) developed a mathematical definition of near-decomposability which relies upon a measure of variable interdependence, and upon which Simon's later discussions are based. Similar mathematical criteria for the appropriateness of neglecting certain variables or interactions of variables in models which predict or simulate certain aspects of complex systems have actually been developed in many areas of science. We will explore in Chapter V below a straight-forward measure of

justifies the omission of much of the (*lower-level*) detail of interactions between subsystems in the definition of *lumped* subsystem outputs which provide the inputs to the other subsystems. (See the review of Simon's model in Chapter II above.) When it is discovered that intersubsystem interactions are stronger than originally assumed, or when greater accuracy becomes desirable, the original subsystem analysis may become inadequate, and a more detailed examination of specific intersubsystem interactions may be appropriate.

Interactional complexity is defined, then, as a case of sufficiently strong interactions between subsystems to require—in the context of given modeling objectives and standards—a more detailed analysis of interactions between subsystems than provided for in the original near-decomposition. There is a spectrum of possible responses to interactional complexity. The simplest response—simplest in the sense that it allows modeling at a single level—is a reduction of the original system description to a lower level of description. In the case of such reduction, original subsystem boundaries would be dissolved and the system description would be replaced—or the system's behavior would be predicted—by a more detailed description that would include explicit reference to *all* of the constituent processes, structures, variables, etc. of one or more of the original subsystems. Even if a reduction were possible in principle, however, it is rarely possible in practice when the systems involved are extremely complex. To keep our perspective here, we might recall that theoretical dynamics has not yet solved the general three-body problem.

A more complicated response to the challenge of modeling interactions between subsystems in greater detail—more complicated in the sense that it must appeal to interlevel considerations—involves the development of models in which the outputs of the original subsystems are maintained as boundary conditions for lower-level processes, and selected lower-level processes are modeled as feedback to correct those outputs. We may, for example, correct the outputs of one or more of the original subsystems by a more detailed consideration of some—but not all—of the constituent processes, structures, or variables from which the original outputs were approximated by averaging, aggregation, curve fitting, theoretical consideration, etc. Strategically selected spatial, temporal, physical, or other forms of

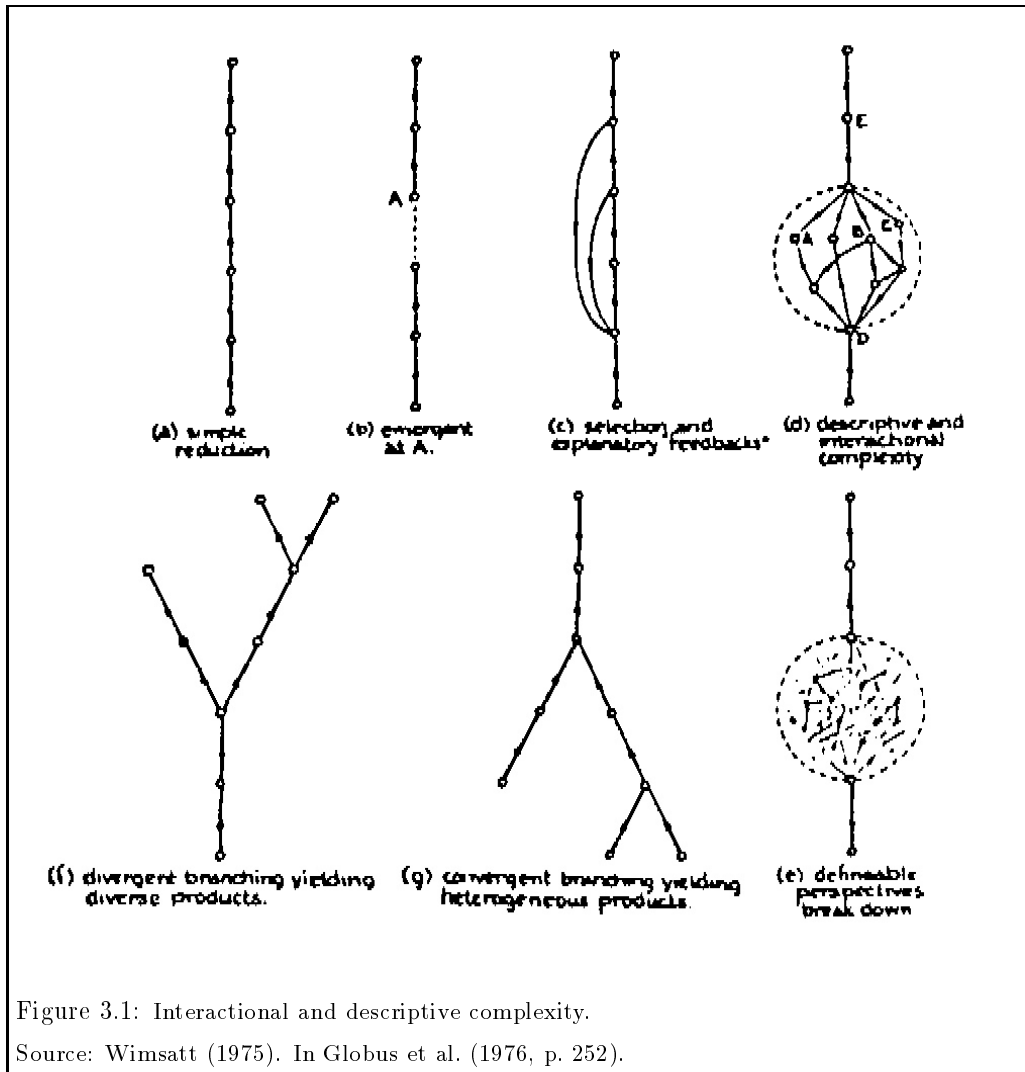
variable interdependence employed by climate modelers in the development of mathematical models of the earth's climate, namely the *sensitivity* of one variable of modeling interest to fractional changes in another variable ($\frac{dy}{dx} = x \frac{dy}{dx}$). The present discussion does not presuppose a particular interpretation of variable interdependence, save for our assumption that the measure employed facilitates the calculation of average degrees of variable interdependence within and between candidate subsystems for the purpose of justifying omissions of specific intersubsystem or interlevel interactions.

lower-level detail are modeled as corrections to higher-level subsystem outputs, which in turn constrain the lower-level detail. The net effect of such a modeling strategy is the mixing of spatial scales, frequencies, structural levels, etc. in the representation of *interlevel interactions*.

In view of the fact that different disciplines often study the processes conventionally thought to reside at different levels, Wimsatt emphasizes that the response to interactional complexity often involves the consideration of the theoretical perspective which generated the original description (subsystem analysis) together with supplementary theoretical perspectives which provide greater focus on those specific details of interactions between the original subsystems which are of special interest. In the most radical responses to interactional complexity—radical in the sense that the relationship between levels is obscured—the supplementary theoretical perspectives are associated with complementary subsystem analyses that include in single subsystems internal features of several of the original subsystems (as dramatically illustrated, in some interpretations of quantum mechanics, by wave-particle duality).

Wimsatt calls the historical fact of an apparently irreducible number of complementary subsystem analyses *descriptive complexity*.¹⁰ Relative to a given near-decomposition known to break down, descriptive complexity involves the definition of subsystems that cut across several of the original subsystems, often obscuring the contribution of the newly defined subsystem outputs to the original subsystem outputs. This kind of complementarity leads more immediately to alternative models than to master models that integrate selected lower-level detail with the original description. The challenge of managing multidisciplinary research programs which model different aspects of the same system in ways that are related unclearly is the nightmare of contemporary mathematical modeling. The way that even such chaotic programs can make progress toward integrated understandings of complex systems

¹⁰Wimsatt also develops (1972a, pp. 69-72) a definition of descriptive complexity which appears to be independent of his definition of interactional complexity. For a set of descriptions to qualify as *complex*, he requires that they partition the system in spatially overlapping ways. It would seem, however, that a spatial definition may not be sufficiently general to apply to biological and social systems, the functions of which are not necessarily organized by location. A more general definition of descriptive complexity is the existence of useful subsystem analyses that combine within single subsystems internal features of several subsystems identified in other useful analyses. In the context of the failure of any particular subsystem analysis to be cut at the joints by other relevant descriptions because the latter provide needed information about the interactions among the subsystems of the former, the system would be (relative to the former analysis and its permissible error in predictions of systemic behavior) interactionally complex. (By way of near-decomposition, this note may be ignored if it makes things too complex.)



will be considered for the case of climate modeling in Chapters IV and V below.¹¹

See *Figure 3.1* for a reproduction of Wimsatt's schematic representation of interactional and descriptive complexity, *Figure 3.2* for his illustration of what he calls the *biopsychological thicket*, and *Figure 1.1* in Chapter I above for an illustration of an analogous biochemical thicket of interactionally complex interlevel interactions between organelles and biopolymers (Bechtel 1986a).

¹¹See Schneider (1988) for a recent plea by a leading climate modeler that climate modeling programs need greater methodological integration of the participating disciplines if their cooperation is to succeed in doing justice to the complexities of the climate system.

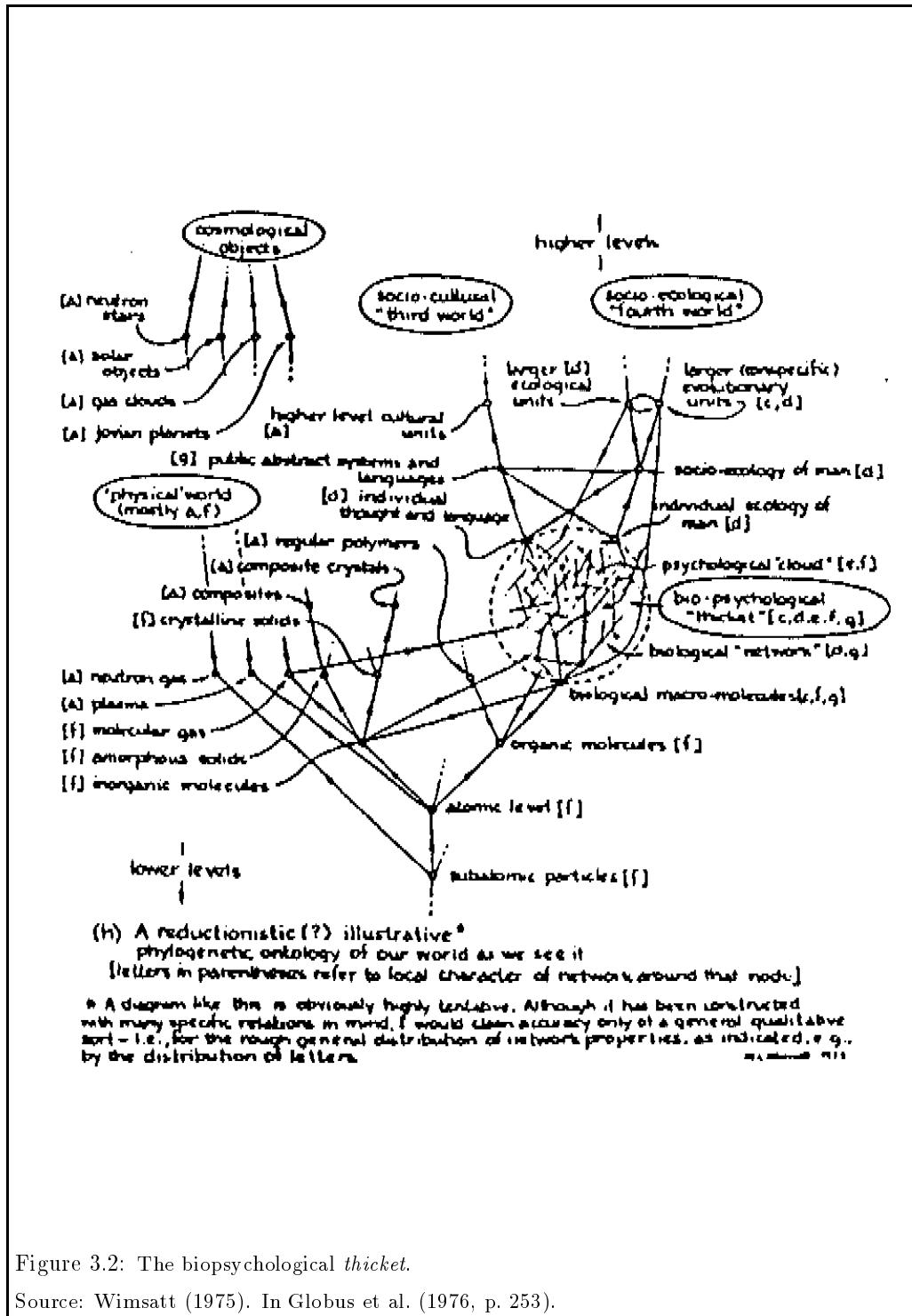


Figure 3.2: The biopsychological *thicket*.

Source: Wimsatt (1975). In Globus et al. (1976, p. 253).

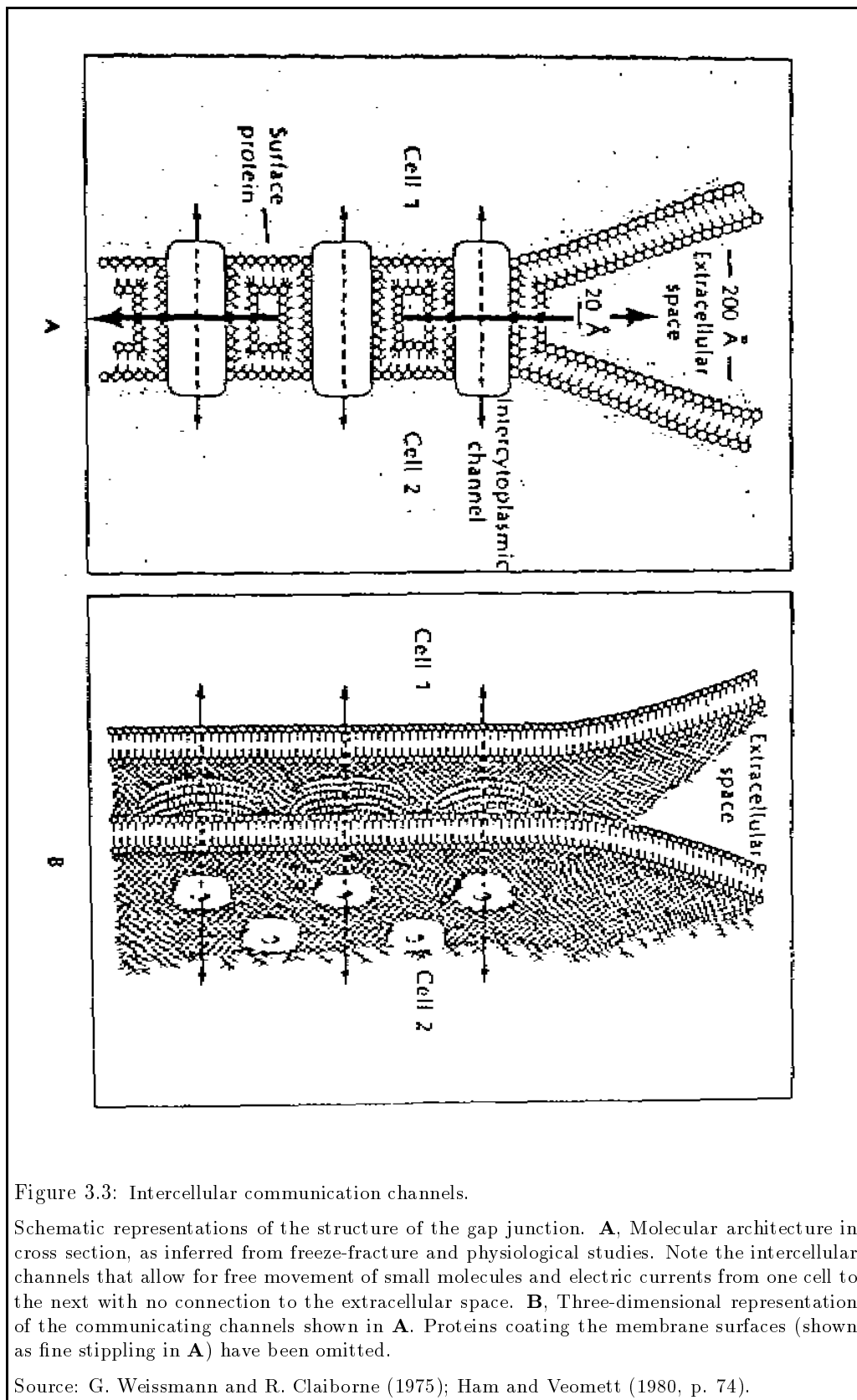


Figure 3.3: Intercellular communication channels.

Schematic representations of the structure of the gap junction. **A**, Molecular architecture in cross section, as inferred from freeze-fracture and physiological studies. Note the intercellular channels that allow for free movement of small molecules and electric currents from one cell to the next with no connection to the extracellular space. **B**, Three-dimensional representation of the communicating channels shown in **A**. Proteins coating the membrane surfaces (shown as fine stippling in **A**) have been omitted.

Source: G. Weissmann and R. Claiborne (1975); Ham and Veomett (1980, p. 74).

An interesting example of interactional complexity is illustrated in *Figure 3.3* by the case of gap junctions between cells, communication channels that allow the cells to exchange small molecules and electrical currents important in regulating embryogenesis in mammals. Gap junctions make specific internal processes in one cell extremely relevant to its neighboring cells, and bring into question our natural tendency to think of cells as separate subsystems. Similar remarks could be made about the way supercell thunderstorms spawn tornados or the interrelationships among cells in a multi-cell thunderstorm.

There are many other more or less obvious examples of interactional complexity. Explanation of the evolution of eukaryotic cells seems to require consideration of the coevolution of bacterial precursors of such organelles as mitochondria, chloroplasts, and possibly cilia with a larger bacterial host cell or its protist descendents. Since such coevolution involved the mutual adaptation of the host cell and its bacterial/organelular subsystems, explanations of their coevolution must consider the respective adaptations of whole and parts to have provided selection pressures for one another. Although the sense in which interlevel interactions were important in eukaryotic cell evolution may seem merely semantic, a careful consideration of some of the evolutionary transitions likely to have been involved suggests an authentic case of interactional complexity.

Wimsatt's (1972a) biological illustrations of interactional complexity include the evolution of eukaryotic cells, slime molds, multicellular organisms, and social insects, respectively, from symbiotic species of bacteria, periodically starving colonies of amoebae, unicellular protists, and solitary insects. Appendix A below develops three of these illustrations—the evolution of eukaryotes, social amoebae, and *eusocial* insects—in sufficient detail to dramatize the usefulness of Wimsatt's model of complexity in interpreting the origins and organization of diverse forms of life. In all of these cases relatively decomposable relationships among subsystems become progressively integrated over evolutionary time scales in the coordinated service of the whole.

In dynamic models of the atmosphere, motions at many different temporal and spatial scales (see *Figure 3.4*) interact with one another—and with motions within the oceans—to create the weather and climate. All models must neglect some of these motions or their spatial and temporal detail would surpass our abilities to observe the required initial conditions and to run the model in a reasonable time, and without excessive error, on a

computer.¹² Yet it is almost always the case that some details omitted from earlier modeling efforts prove essential to future modeling purposes. The art of climate modeling has much to do with the identification of such critical details. Since it is not always possible or advisable to respond reductionistically—i.e. to increase the spatial and/or temporal resolution of current models in order to better capture such critical details—other strategies are often employed to represent the *effects* of these details which are unmistakable examples of interlevel modeling.¹³

Similar remarks could be made about many areas of science that model complex systems. Interactions of motions or patterns at many different scales, for example, are important considerations in the study of phase transitions near critical points (Wilson 1979), Brownian motion (Lavenda 1985), chaotic attractors (Crutchfield et al. 1986), convection (Verlarde and Normand 1980), the nonlinear thermodynamics of rotational motion (Lavenda 1978), and the theories of turbulence, wave flow, subharmonic resonance, and generally, in the study of numerous nonlinear problems of physics.

Lest it should be thought that only areas of physical science that are considered *applied* encounter interactional complexity, let us reconsider our above interpretation of Table 2.1 in Chapter II. We should find it instructive to formulate nonhierarchical interpretations of the apparent structural levels represented. Spacetime foam—if Wheeler’s geometrodynamics conceptions of quantized gravity¹⁴ are correct—arises from the high frequency appearance and disappearance of small-scale wormholes that provide bridges between different locations in space-time. If from one point of view these are stable over the smallest time-scales, from another—as the term *foam* suggests—they create continual instabilities in space-time. Moreover, they represent the mutual interactions between global space-time and local point charges, and as such are theoretically, at least, the result of interactions between the largest

¹²When sets of differential equations that include one or more nonlinear equation are solved by numerical approximation on a computer, the state of the system at each of a number of small discrete time steps following the initial conditions is calculated from linearized approximations of the original equations and the state of the system at the previous time step. Errors introduced in the initial conditions, and those introduced by the numerical methods themselves, tend to grow at ever increasing rates as nonlinear functions of the number of time steps. Acceptable error levels necessarily impose constraints on the number of time steps over which the model can be run. Such constraints (together with complexities associated with interactions between the atmosphere and the other climate subsystems, as well as limited theoretical understanding of turbulence, clouds, and other phenomena known to contribute to the evolution of the atmosphere) seriously handicap efforts to model the atmospheric circulation over long periods of time.

¹³An analysis of the interactionally complex aspects of the climate system and interlevel explanatory strategies in climate modeling may be found in Chapter IV.

¹⁴See Graves (1971) for a remarkably lucid review of the insights and problems of geometrodynamics.

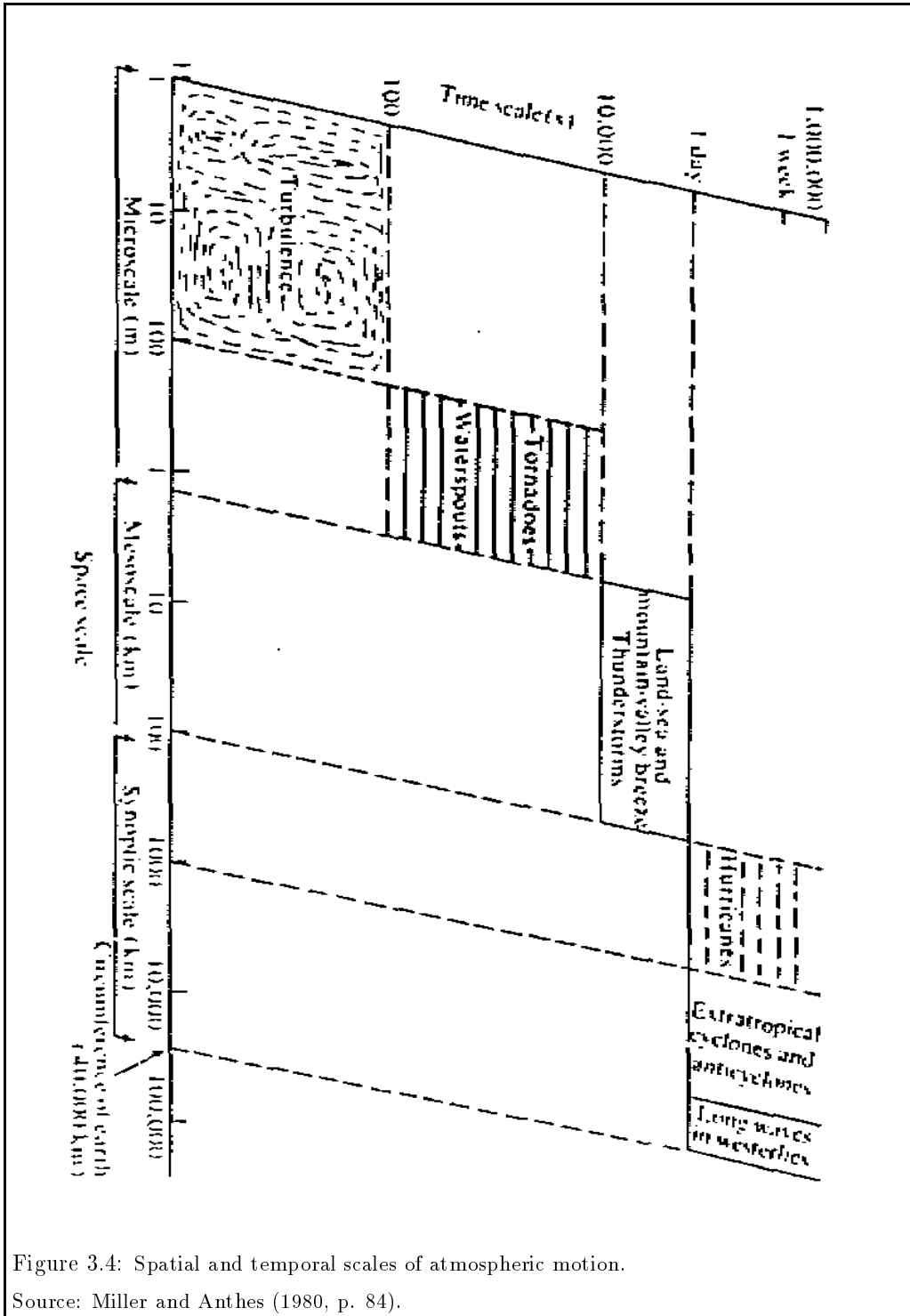


Figure 3.4: Spatial and temporal scales of atmospheric motion.

Source: Miller and Anthes (1980, p. 84).

and smallest scales in the universe.

Moving down the table (and up the levels of organization), quarks are assumed to be elementary structureless particles that are the lowest level components of such composite particles as protons, neutrons, and mesons. Yet in theory quarks do not exist in isolation, but only within composite particles where they continually exchange virtual gluons. Whether three quarks exchange gluons within nucleons, or quark-antiquark pairs exchange gluons within pi-mesons, their color charges are continually changing as a result of these interactions. And whether these color changes occur within three-quark or two-quark particles, the net color of the entire composite particle is always white. Thus it would seem as reasonable to describe quarks as oscillating patterns within a white steady-state as it would be to describe them as building blocks.

At the next putative level in Table 2.1, protons and neutrons are presumed to be the relatively stable constituents of atomic nuclei. Yet they do not remain protons and neutrons, but continually exchange identities with one another as they exchange pi-mesons. Protons and neutrons within nuclei are perhaps better described as oscillating patterns than they are described as hard-structured components.¹⁵

The reinterpretation of the rest of the table is left as an exercise. It should be apparent from the first few levels that intersubsystem and interlevel interactions are the rule rather than the exception in the so-called structures of our universe. Of course it is true that the statistics of intersubsystem interactions are stable enough at sufficiently low temperatures and pressures to create the next higher level of structure, so that hierarchical conceptions certainly capture important features of systemic organization. Yet it must be remembered that the stability of such statistics is entirely dependent upon energies available for interaction. (See *Figure 2.1*.) If we turn up the available energy sufficiently, any level of structure eventually breaks down.

Moreover, in those critical ranges of temperature and pressure within which both higher-level and lower-level structures have significant probabilities of temporary existence, interactional complexity is rampant and hierarchical assumptions are of little use, except in well-behaved situations where renormalization or other mathematical methods succeed

¹⁵See Appendix B for a detailed analysis of the implications of theoretical physics for the prevalence of interactionally complex systems.

in applying different laws or regularities to different ranges of scale. When we have the interest to look between levels, the contingency of even the most fundamental structures of theoretical physics (as in the proverbial quark-gluon soup of big-bang fame) is no different in kind from the contingency of fluctuations at a spectrum of scales in fluids near phase transitions or bifurcations in dynamic or thermodynamic evolution.

Interactional complexity is as common a complication of social science as it is of the biological and physical sciences. Whether we consider the interdependence of tribes or nations, institutions, sectors of society, organizations, families, or people—or their interlevel connections with one another—positive and negative synergisms are widespread and contribute to the explanation of social process and social evolution.¹⁶ The interactions among cooperating disciplines which so often transform the interests, methods, and organization of disciplines—most dramatically resulting in the emergence of subspecialties or the aggregation of several disciplines or areas of science into new research programs or disciplines—actually represent forms of social interactional complexity, forms which presumably enjoy the special constraints of historically progressive standards of scientific progress. The stronger forms of interdisciplinary cooperation are both a response to, and an illustration of, interactional complexity. A model of the cooperative study of complex systems will be developed later in this chapter.¹⁷

Having provided a sampling of interactionally complex physical, biological, and social systems, we may not be imprudent to suggest that they are more widespread than either reductionists or holists have been wont to admit. We postpone detailed consideration of the complexity of the climate system—which incorporates physical, biological and social subsystems—to Chapter IV, and turn now to a consideration of the general implications of interactional complexity for the structure of explanations of the structure, behavior, and evolution of interactionally complex systems.

¹⁶The analogy between the importance of synergisms in biological evolution and human history is explored systematically and convincingly in Corning (1983). Also see Berlinski (1976) for a systematic mathematical argument that the social sciences often oversimplify in their linearity assumptions in analyses of complex systems. The need for nonlinear system analysis has implications for the limitations of Simon's method of near-decomposition.

¹⁷See Appendix C for an analysis of the social interactional complexity involved in risk analysis, in which I argue that valuation processes of citizens and policy-makers and factual predictions of scientists ought to interact mutually.

3.3 Interlevel Explanation

Wimsatt's model of interactional complexity has caused no little excitement in the philosophy of science, as it provides a framework for the integration of several levels of theory or law without a reduction of higher by lower levels. Darden and Maull (Darden and Maull, 1977; Maull 1977; Darden 1986) have explored a variety of examples of what they call *interfield theories* in the biological sciences.¹⁸ In their earlier (1977) examples of interfield theories (reviewed above), one field solves a problem which arises in a second field without replacing the second field or reducing any of its theories. The lower-level field, in these illustrations, develops an interfield theory which identifies components or analyzes constituent processes the aggregate behavior, systemic functions, or causal consequences of which are studied by the higher-level field (Maull 1977, p. 156).

In Darden's recent (1986) example, several established fields (including genetics, mathematical population genetics, and experimental and field studies of populations), together with a new area of interest (in the ecology of selection pressure mechanisms), contribute to the development of interfield theories that propose explanations of the evolution of species. These interfield theories make reference to a new, higher level of *downward causation* (Campbell 1974) which is associated with isolating mechanisms and selection pressures on species. A new research program, if not a new field, embraces multiple levels and their interactions in its development of structural, causal, and functional explanations.

In other accounts of evolutionary explanation, Wimsatt (1972b) and Wright (1976) appeal to the (downward causation) role of selection pressures in favoring variants that are *functional*, while Machamer (1977) and Bechtel (1986b) consider the irreducible contribution which the biological organization resulting from variation makes, along with selection pressure, to survival and reproduction in their accounts of teleological functional analysis. And in still another case study of interfield theories, Bechtel (1984, 1986a) shows how the new field of biochemistry emerged from organic chemistry and physiology in order to treat the interactionally complex interdependence of biochemical reactions, which for Bechtel constitute a new level of integrated metabolic pathways intermediate between the respective

¹⁸Historically, Darden and Maull had been students of Wimsatt's when the interfield implications of interactional complexity were very much in the air (Wimsatt 1979).

levels treated in the chemistry of specific reactions and the physiology of the cell.¹⁹

What all of the above accounts of interlevel explanation have in common²⁰ is their integration of multiple levels of explanation in the development of interfield theories or in the theory development of a new field or research program that is justified by its consideration of a new level of explanation.²¹ It is ironic, perhaps, that all of these accounts uncritically accept the notion of levels, while it is one of the most interesting implications of Wimsatt's notion of interactional complexity (which all interfield theorists explicitly or implicitly invoke) that in many cases of complicated forms of intersubsystem or interlevel interaction (especially in the case of descriptive complexity), the very notion of levels may be in jeopardy.

Mutual interactions between the phenomena conventionally defined at different levels often preclude effective decomposition strategies, and make the development of more integrated models desirable. It is always possible to *define* the empirical scope of an integrated model—or a potentially integrated model—of interlevel interactions as a new *level*, but definitions of this sort do not tell us anything about the phenomena involved. Furthermore, hierarchical presuppositions may obscure the immediate need to cut across the boundaries of previously defined subsystems and/or levels, and cut across the empirical domains of established disciplines, in order to make progress toward the development of more integrated models.

The type of interlevel theorizing that I have in mind can most easily be illustrated by consideration of the requirements for modeling extremely complex physical systems, for these can be specified with mathematical precision without the complication of appealing to functional and other forms of uniquely biological explanation. Gaian theories of the atmosphere-ocean-cryosphere-lithosphere-biosphere system notwithstanding,²² the structure of the atmosphere may for our purposes be considered an illustration of an interactionally complex *physical* system which, except for its admitted biological and social boundary conditions, operates according to physical laws. Even so, the so-called laws that we find in models of any degrees of spatial and temporal resolution always include certain

¹⁹The significance of the two-way arrows between biopolymers and organelles in *Figure 1.1* should now be clear.

²⁰See Bechtel (1986c and 1988) for extremely helpful reviews.

²¹Similar analyses have been made of the emerging integration of the theories and models of neurophysiology, artificial intelligence, and cognitive psychology (e.g., P.M. Churchland 1988 and Bechtel 1988).

²²See the discussion of the *Gaia hypothesis* in Chapter IV below.

regularities which climate modelers call semi-empirical parameterizations. These generally contain parameters fit to the present climate, and are confirmed in three independent ways: Inductively, by consideration of data sets with degrees of freedom independent of those to which the parameterizations were fit; instrumentally, at the level of detail of the model, through model predictions or simulations which are compared with climate data; and through interlevel theories of the mutual interactions between the phenomena defined at the level of detail of the model and phenomena which, for practical reasons, are too confined in space and/or time to be resolved by the model.

We can identify three levels of physical and spatial detail that climate models or models of the atmospheric component of the climate system presuppose. The most detailed is the hypothetical description that is resolved enough in space and time to represent all processes relevant to the atmospheric evolution explicitly represented in the current model. Because of observational, computing, and mathematical constraints, this lowest level of (greatest) detail is not represented explicitly in the model, but only represented indirectly through the use of parameterizations. Second in degree of detail is the degree of spatial and temporal resolution of the model itself. And least detailed are those climate patterns to which parameterizations are originally fit. Note that parameterizations cannot be eliminated, as they implicitly represent the consequences at the level of detail of the model of interactions between explicitly modeled phenomena and unresolvable phenomena (such as clouds, small-scale turbulence, etc.), and because mathematical closure requires that parameterizations supplement the fundamental physical laws of the model.²³

Thus parameterizations are fit at the highest level (the level of least detail), confirmed both at that level and at the intermediate level of detail of the model, and ultimately justified theoretically by maximally detailed consideration of interactions between explicitly

²³Note that this role of parameterizations in supplementing fundamental physical laws to provide mathematical closure at a less detailed level of description generally qualifies climate models as holistic models in the sense defined in Chapter II above. In some cases, however, the parameterization and its constants can be derived theoretically from fundamental laws and given boundary conditions (as in the case of the parameterization of infrared radiation leaving the atmosphere). In such cases of reductive derivation, the parameterization does not imply holistic assumptions. To the extent that the parameterization is not derived from first principles, however, and its mathematical structure is suggested by considerations of the organization of the system being modeled while its constants are fit to system data, the use of the parameterization renders the model holistic. Yet its use is holistic with a catch, as the parameterization, as explained above, implicitly represents the consequences at the level of detail of the model of generally mutual interactions between unresolved and explicitly modeled phenomena. We therefore prefer to consider it an interlevel rather than a holistic explanation. This is implicitly understood by climate modelers who speak of parameterizing clouds, subgrid-scale turbulence, and so forth when it is only their effects or more properly, their interactive contributions to explicitly modeled phenomena, that appear in the model.

modeled and unresolved phenomena. The last theoretical justification generally involves several cooperating disciplines, as the phenomena involved are associated with the mutual interactions of phenomena defined at different ranges of spatial and temporal scale, and different disciplines generally study the different levels.

What appear, then, to be laws defined at the level of detail of the model, applicable to the proven empirical scope of the model, turn out to be reflections at the model's level of detail of interlevel phenomena. In the general case, the regularities or patterns proposed by parameterizations are contingent. Under different boundary condition assumptions (such as the location of the continents), in different climatic circumstances (such as whether the earth is in an ice age), or as a result of random fluctuations (as are believed to occur in small-scale turbulence), other regularities or patterns might emerge which would be better captured—in a given modeling context—by different parameterizations.

Parameterizations are interlevel and conditional explanations, then, which constrain, and in theory are contingent upon, the evolution of the modeled system. The climate modeler must systematically explore, with the help of the disciplines that study relevant “subgrid-scale” phenomena, the circumstances under which a given parameterization is likely to be sufficiently accurate for the purposes of a given model, and the circumstances under which an alternative modeling strategy would be more appropriate. If it seems that parameterizations are not lawlike because they depend on history, and that their role in explanation can only be circular, their status may not be different from that of fundamental laws of physics which may be similarly dependent on the history of the universe.

The way that particular parameterizations, such as those associated with the contributions of clouds, turbulence, or vegetation, contribute to climate models will be reviewed in Part II of the case study below. We here wish to suggest that the apparently higher-level regularities employed in so many areas of science that model complex systems often have the interlevel and conditional features of the parameterizations employed in climate modeling—and to explore the modifications in conventional philosophical views of explanation that are required to accommodate such interlevel explanations. This extended philosophical view of explanation will in turn provide the framework for analyzing the explanatory status of climate models in Chapter V.²⁴

²⁴It will be observed that consideration of climate modeling has influenced the author's view of explanation, while he applies his model of explanation to the case of climate modeling. The interaction here between

Although there are many approaches to modeling complex systems, the ones that involve the development of mathematical models generally apply sets of equations under specified boundary condition assumptions to the states of systems defined at specific levels of spatial, temporal, physical, or other forms of detail. It is only in special cases that the equations thought to generate the evolution of such systems from initial conditions can be solved analytically as general functions of time. Often the equations are nonlinear, and the numerical approximations required to solve the equations can best be calculated by computers. The growth of the computer technology over the past decades has understandably provided a great stimulus to the development of nonlinear models of complex systems; and equally important, it has made it feasible to develop models that employ a great many equations.²⁵ Computers, together with modern observation technologies such as satellite monitoring systems that continuously collect terrestrial, solar, or astrophysical data, have brought about revolutions in the numbers and interdependence of components or variables treated in models of complex systems in many areas of physical, biological, and social science.

For purposes of prediction and simulation, mathematical models of interactionally complex systems generally must appeal, as in the case of climate models introduced above, to equations that are not derived from first principles but which cannot be confirmed at the level of detail of the model alone. The methods used to develop and confirm such equations vary, but in the general case their constants are fit to data and they are confirmed not only in the context of additional data, but also as instruments of model prediction and as theories of the consequences at the level of the model of interlevel interactions that are not explicitly modeled. Although it is not always recognized in all areas of mathematical modeling with the clarity of the climate modeling methodology literature, the apparent higher-level laws of these models are no less contingent and unawful than the parameterizations of climate models.

The horror that interactional complexity creates for conventional philosophical views

philosophy and climate modeling thus reveals *interactional complexity*, or in the language of last century's pragmatist Charles S. Peirce (1901), it reveals the use of *abductive logic*.

²⁵ Contrary to popular belief, larger numbers of degrees of freedom do not necessarily result in greater stability. For discussions of the mathematical relationship between model complexity and stability in the case of ecological models, see May (1972, 1974), and May and Oster (1976). However, even three variables are sufficient to produce instabilities in nonlinear systems. Since instabilities are associated with major structural changes, they are closely related interactional complexity, i.e. to the breakdown of any hierarchical analysis associated with the stable system.

of explanation is that laws, theories, or models that are aimed and confirmed at particular levels—even several different levels—generally fail to predict the evolution of the system in all circumstances of interest. In systems that evolve in interesting ways over time, patterns which emerge temporarily at different scales—or more often, across a range of scales—mutually interact with other such patterns. The ideal of explaining and predicting conditional history from given boundary conditions and unconditional laws fails to provide an effective modeling strategy. Instead, progress in the understanding of evolving systems—systems which change their structures, states, or behavioral repertoires in significant or improbable ways over time—requires that we provide mutual explanations of mutually interacting patterns, that we analyze the mutual interactions of the phenomena that truly cause, or causally influence, the evolution in question. Physics and biology find themselves facing similar challenges of coevolutionary explanation, and must contend alike with perplexing mixtures of regularity and randomness that contribute to the evolution of the universe and earthly life.

The deductive-nomological model of explanation which emerged from the positivistic tradition—and dominated the philosophical literature on explanation until recently—was better designed to analyze the explanation from covering laws of simple interactions than it was to analyze the explanation of complex systems. Note the structural similarity between the atomistic philosophies from which the positivistic tradition first emerged and the higher-level atomism, as it were, of hierarchy theorists.²⁶ Whether we are concerned with interactions among particles, or among nearly decomposed subsystems, simplicity yields relatively uncomplicated predictions, whether from fundamental laws or their holistic analogs. Complex systems either have too many components, or too much interdependence among components, to lend themselves to straight-forward predictions from laws. (See note 12 above.)

As explained in Chapter II above, positivists were motivated to analyze explanation in terms of theories or laws at levels by their hope of embracing in a single methodology successional and interlevel forms of reduction. Yet when the apparent laws of a higher level are not really laws in that they neither come with guarantees of unconditionality under specified boundary condition assumptions nor guarantees of predictive power for a specified empirical scope, it would do little good to derive them or their observational implications

²⁶See Appendix B below for a systematic interpretation of atomism in the history of physics in terms of interactional complexity.

from theories of greater generality or scope. Deriving uncertain and contingent patterns with logical certainty from theories or laws that boast a broader confirmation space doesn't really lend support to those laws or theories. Furthermore, the latter laws or theories, to the extent that they explain only phenomena defined at particular levels, do not generally have the resources to analyze the implications of interlevel interactions for the patterns in question. Yet to christen the patterns in question *lawlike* would require a specification of the interlevel circumstances under which they hold and do not hold, and a demonstration that the boundary conditions of the model and the perturbations at unresolved scales to which the real system may be subject, result in robust patterns that are justifiably employed in explaining other systemic behavior.

Contingent laws, at least when they are intended to contribute to explanations of the larger-scale or higher-level behavior of complex systems, are indeed more properly called patterns than laws. And these patterns are sustained, in general, by intersubsystem and interlevel interactions which must be studied in their own right if we are to analyze the circumstances under which the patterns in question provide boundary conditions for other phenomena and the circumstances under which they are unstable to perturbations.

Positivists generally were so preoccupied with the logic of theory reduction and the associated logic of deductive explanation from covering laws that they neglected to explore the relationships between explanatory laws and theories and the causal interactions on which their legitimacy presumably depends. Philosophical interest in causal explanation emerged from conventional positivist views of explanation in several steps, from Hempel's (1962, 1965a, 1968) work on statistical explanations of so-called high probability,²⁷ to Salmon's (1965, 1971) work on statistical explanations that may provide partial explanation despite their low probability or predictive power, to the more recent interests of Salmon (1975, 1977a,b, 1978, 1982, 1984), Fetzer (1974a,b,c,d, 1977a,b, 1981, 1982, 1987), and others in truly causal statistical explanations.

Salmon (1971, p. 44) has proposed a distinction between two types of reference classes (or statistical samples employed to establish statistical laws) that may have some application to our present concerns. He defines an *epistemically homogeneous reference class* as the broadest or most representative statistical sample available which, as far as we know, cannot

²⁷Hempel and Oppenheim (1948) were the authors of the classic paper that formalized the deductive-nomological model of explanation that was so pivotal in positivist thinking about science.

be partitioned fruitfully to yield subsamples which generate different statistics. In contrast, he defines a *practically homogeneous reference class* as one which we have reason to believe can be partitioned in statistically relevant ways, but which, for practical reasons, we treat as if it were homogeneous.

If we may illustrate both classes with the structure of climate models, a practically homogeneous reference class is defined by the grid-size of a model, i.e, the smallest volume of fluid resolved by the model, together with the time-step of the model, i.e, the shortest period of time resolved by the model. All smaller-scale processes that are unresolved by the model are treated by the model as if they were homogeneous, that is, as a whole without the explicit representation of internal structure. The statistics of these smallest volumes for the duration of the time-step define the state of the modeled system. Even though many types of smaller-scale process, such as cloud processes or millimeter-scale turbulence near the earth's surface, are *known* to be statistically relevant to processes resolved by the model, it is not practical, for reasons explained above, to represent them explicitly in a model. In Salmon's language, it is not practical to *partition* the reference class defined by the spatial and temporal resolution of the model in such a way that we distinguish subgrid-scale processes. Yet for theoretical purposes, epistemically homogeneous reference classes may be defined which are sufficiently resolved in space and time to allow reference to all of the processes known or believed to be relevant to explicitly modeled phenomena. Both types of reference class are relevant to the development and confirmation of models.

From a slightly different point of view, Fetzer (1981) has defined a *requirement of strict* (causally relevant) *maximal specificity* (*RSMS*). This requirement points to the importance of defining, to the extent possible, a space (again, for the establishment or confirmation of statistical/causal laws) in which all causally relevant dimensions or variables are distinguished. *RSMS* is similar, in ways, both to Salmon's *homogeneous reference classes* and to Hempel's *requirement of maximal specificity*. It is sufficient for our purposes to point out that both Hempel's (1962, 1965a, and 1968) model of statistical explanations of high probability, and Salmon's earlier views (1971) on statistical explanations of low probability, failed to explore the distinction between causal relevance and statistical relevance emphasized in Fetzer's (1981) work.

The state spaces, or levels of description, as we have called them above (following

Pattee 1973a, 1973b), of mathematical models of complex systems generally are designed to meet, as far as limitations in data, computing, and predictability will allow, Fetzer's requirement of strict maximal specificity. There has been some debate in the literature as to whether to construe the variously conceived notions of maximal specificity as ontically (really), epistemically (as far as we know), or practically (as far as we are able) maximal, but this literature has been abstract with unclear implications for the actual conduct of science, and need not concern us here. It would seem helpful, however, to distinguish between a state space or degree of modeling resolution which is maximally specific in the practical sense, and the ideal state space of the theoretician which is maximally specific in the epistemic sense.

Thus, whether we contrast the modeler's homogeneous interpretation of subgrid-scale phenomena with the theoretician's nonhomogeneous interpretation, or we contrast the spatial, temporal, and physical detail of models aimed at a level and theories which embrace multiple levels, philosophical vocabulary does seem to be available to assist our comparison. Yet if the philosophical language is there, the understanding quite possibly is not. Most of the relevant questions for a philosophy of explanation that addresses the problems of the mathematical modeling of complex systems have not even been asked, let alone answered.

Under what circumstances can we be comfortable with our parametric approximations, and under what circumstances need we instigate interlevel and often interdisciplinary research to improve our parameterizations? What does it take to supplement the curve fitting and predictive confirmation of models with interlevel considerations of causal factors that may impact the stability of parametrically represented patterns? Or are parameterizations or other higher-level modeling principles nothing more than predictive instruments whose value is established by their pragmatic contribution to models? What would it tell us about the explanatory status of models if we discover that there is no single master model that predicts the evolution of a system under all circumstances and for all purposes? Could the same universe be described by alternative laws? If evolving patterns are invoked to explain one another, is our explanation circular and ultimately ungrounded? If causes explain laws, and laws explain causes, is explanation not again circular and ungrounded? If we don't stick to causes at a level in our confirmation efforts, couldn't our quest for causally relevant phenomena lead to an endless network of interconnected causes? Can the organization of a system explain laws rather than the other way around?

Does inherent unpredictability, as seems to occur around bifurcation points in the evolution of nonlinear systems, imply inexplicable behavior? Does irreducible randomness, whether it arises from deterministic equations that generate chaos or stochastic forcings, again imply inexplicable behavior? When do we have a good model, anyhow?

Questions such as the above need to be addressed in any adequate philosophy of explanation. Rather than answer them in the abstract, as is so common in the philosophy of explanation, it would be more illuminating to first look seriously at cases of science that confront such problems. This is what we do in Chapters IV and V. Several points may be made in this context, however, that may go to the heart of evolutionary explanation, as I shall call it, and provide a framework and motivation for the analysis of explanation in climate modeling in the Chapter V.

First, it may be helpful to challenge the long-standing bias among scientists as well as philosophers that presumes that the fundamental laws and theories of theoretical physics are of greater explanatory status than the contingent regularities and patterns associated with complex systems. It would seem that models of complex systems are better confirmed, in a way, than fundamental laws and theories. Consider fundamental theories to model the organization and evolution of the universe—a complex system if there ever were one—and fundamental laws to operate like parameterizations in fundamental theories. Indeed, fundamental laws often employ parameters that are fit to data. There would seem to be no reason not to expect the level of fundamental particles or wave functions at which the universe model is defined to be subject to influences from subgrid-scale or otherwise unrepresented phenomena. Bohm (1957), for example, has spoken of the *qualitative infinity of nature* in his suggestion that even the most fundamental of theories may neglect consideration of phenomena which could be causally significant at the level of the theory.

The difference between the universe model and models of everyday complex systems is that universe modelers do not have independent empirical access to the smaller scales and otherwise irreducibly theoretical phenomena neglected when the universe model is employed as a predictive instrument at a level of descriptive (and observable) detail. Climate models and other models of complex systems always neglect causally relevant phenomena in the explicit representations of their models, but the modelers know something about these phenomena and are in communication with the specialists who study them. This provides

the opportunity to explore systematically the circumstances under which the model may be expected to hold and not to hold, and to improve it accordingly for applications in the latter circumstances. To the extent that we have no reason to believe that the fundamental laws and theories of physics are not contingent upon boundary condition assumptions or stochastic forcings, it would seem that modeling methodology ought to serve as the paradigm for theoretical physics rather than the other way around.

It would furthermore seem, as we have suggested above, that theoretical physics no less than applied physics confronts the challenge of modeling interactionally complex phenomena. Indeed, wave-particle duality seems to involve descriptive complexity, the most radical form of interactional complexity considered above. The variety of forms of interdependence treated in quantum mechanics and special and general relativity is indeed mind-boggling, and has been the subject of detailed studies by philosophers and practitioners of physics such as Čapek (1961) and Graves (1971). These forms of interdependence are reviewed from the perspective of Wimsatt's theory of interactional complexity in Appendix B below. It would seem that the models of complex systems that treat the more empirically accessible forms of interactional complexity could provide insights into the way so-called fundamental phenomena are organized, and the way models of the higher-level consequences of complex interactions can best be confirmed. The universe may be organized in interesting ways at many scales, and it would seem pure bias to assume that it is only the smallest scales of quantum mechanics and the largest of general relativity that are of fundamental scientific interest.

Having at least suggested the possibility that modeling methodology may provide invaluable insights into the nature of scientific explanation, we postpone further conclusions about explanation until the case study of climate modeling in Chapters IV and V. The following section of this chapter will consider the structure of the interdisciplinary research often required to develop interlevel explanations.

3.4 A Model of Interdisciplinary Scientific Progress

If hierarchical assumptions have handicapped the philosophical understanding of the organization of complex systems and of the interlevel forms of explanation required to

understand that organization scientifically, they have equally handicapped our philosophical understanding of interdisciplinary cooperation. The disciplines that study different aspects of the same complex system ought to interact with as much complexity as the phenomena which they study. If the subsystems or levels of specialized interest to different disciplines interact in strong or interactionally complex ways, scientific progress would seem to demand analogous complexity in the interactions of cooperating disciplines. Too often interdisciplinary research programs are multidisciplinary in disguise in that the participating disciplines do not integrate their research efforts. And too often philosophers who seem to be promoting interdisciplinary cooperation are promoting multidisciplinary research in disguise.

For example, Darden and Maull, whose work (Darden and Maull 1977, Maull 1977, Darden 1986) has been reviewed above, took the first step toward a model of interdisciplinary progress in their demonstration that the theories of different fields or areas of research can become progressively integrated in the context of multidisciplinary theory development. It is questionable whether Darden and Maull's analysis of interfield theories truly revealed the integration of theory across theoretic levels or whether the part-whole connections they considered preserved the respective levels of parts and wholes (in the fashion of the reductionists they wished to refute). Even if we allow interfield theories to achieve high degrees of interlevel integration, however, it is clear that Darden and Maull made no claims of methodological integration among the fields which contribute to interfield theories. Rather, their analysis of the research context presupposes a hierarchy of distinct methodologies with different (primary) levels of empirical concern.

We have also reviewed above Bechtel's (1984, 1986a) demonstration that interactionally complex interactions among metabolic pathways necessitated the methodological integration of chemical and physiological approaches to enzymology in the emergence of biochemistry as a field. While Bechtel's work is unique in clearly connecting interactional complexity and the methodological integration of several areas of science, it presupposes that biochemistry defines a new level of concern. But does this notion of level help at all to define the emergent focus of biochemistry? Could any integration of chemical and physiological domains be called a new level for lack of a better term? Are chemical and physiological concerns really homogenized at a new level, or are they selectively integrated in ways that partly preserve the conventional levels and partly break them down? Don't physiological and chemical disciplines (such as cell biology and organic chemistry) continue to contribute to

progress in biochemistry? Does a distinctly biochemical methodology imply an isolated level of biochemical concern, or must the former continue to selectively integrate our understanding of chemical processes and physiological structures and functions? The significance of interactional complexity is that the connectedness of levels lies somewhere between relatively isolated levels and relatively integrated levels. Even when new disciplines emerge which specialize in interlevel relationships, such new disciplines generally continue to have (complex) relationships with their mother disciplines.

It is to correct such limitations of the literature on cooperative science that the following model—or sketch—of cooperation is proposed. The development of a more complete model—and an adequate general theory of interdisciplinary cooperation—must await the comparative study of many areas of science. The preliminary characterization of interdisciplinary scientific progress outlined here has been motivated by the case of climate modeling, and has been tested (informally) to the author’s satisfaction only for the mathematical modeling sciences. Its illustrative application to climate modeling may be found in Chapter V.

As we have perhaps demonstrated above, analogies borrowed from evolutionary biology have been employed liberally in the philosophy of scientific progress, and an evolutionary analogy may be helpful here as well. We have mentioned above and review in detail in Appendix A how different species of bacteria coevolved and became progressively integrated in eukaryotic cells and later in plant cells. Mitochondria, chloroplasts, cilia, and the endoplasmic reticulum and its nuclear membrane and Golgi apparatus derivatives all probably have their origins in parasitic species of bacteria that coevolved with their originally bacterial host cell. In contemporary organisms, these organelles and cell structures are thoroughly integrated in the eukaryotic cells of protists, plants, fungi, and animals. The once symbiotic organelles have swapped genes with their host, and can no longer live apart. Yet mitochondria and chloroplasts still reproduce separately inside cells and presumably continue to coevolve with their permanent host. It is significant, for example, that mothers pass on their mitochondria with the cytoplasm of their egg cells, while the sperm cells of fathers do not carry this substantial genetic information. Chloroplasts have had less time to become integrated than mitochondria, and they reveal more of their original genome. It is not unthinkable that we could genetically engineer chloroplasts to live outside the cytoplasm of cells and provide humanity with their efficient and nonpolluting solar energy. Mitochondria and chloroplasts, even if they are integrated in the cells of advanced life forms,

are still very much alive.

Likewise, it would seem that the scientific progress of our future will depend upon the progressive integration of scientific disciplines in interdisciplinary research programs and eventually in complex superdisciplines. This is not to say that scientific progress will not also depend upon the differentiation of new disciplines, competition among disciplines, and the extinction of outdated or fully accomplished disciplines. Biological evolution certainly requires continued speciation, competition, and extinction despite its countless experiments in coevolution, symbiosis, and positive synergism. But if there is a direction to biological evolution, it seems to lie in its progressive integration of molecules into bacterial cells, bacteria into larger sexually reproducing cells, eukaryotes into multicellular life forms, and all of these into a working biosphere in partially regulated interaction with the atmosphere, oceans, and earth.²⁸ It is at least reasonable to expect that a successful scientific institution will similarly progress from separate disciplines of narrow focus to increasingly coordinated and integrated efforts at understanding the organization of our natural and artificial worlds.

If we may exploit a second analogy with features of the endosymbiotic evolution of eukaryotes, the relationship between the scientific community and society is similar, in ways, to the relationship between the parasitic bacterial precursors of mitochondria and their hosts. Scientists who do not grow food but depend upon grants and university positions to buy their food bear a certain resemblance to their parasitic bacterial ancestors. Yet just as mitochondria were able to utilize oxygen in respiration and provide their hosts with high-energy chemical bonds, the scientific community provides society with high-information theories, models, and laws. While mitochondria provide the energy for the highly technological reproductive capacities, so to speak, of eukaryotes and the multicellular organisms to which eukaryotes contribute, scientists provide the information necessary for modern technological societies to operate and to make rational environmental and international policy decisions. It would seem coadaptive for the scientific community and society to improve the quality of available scientific information, and one way of doing this is to encourage interdisciplinary problem-solving that leads to an understanding of the systems upon which human, animal, and indeed, biospherical well-being, if not survival, clearly depend.

²⁸See Chapter IV below for a discussion of, and perhaps a demythologizing of, the *Gaia hypothesis*.

This is not to argue for a reduction of science to a pragmatic exercise that mistakes the inherent value of knowledge for its usefulness, nor to presume an evolutionary epistemology that minimizes the importance of scientific creativity and insight in the innovative developments of science. It is only to suggest that an institution which has the mission of reconciling theory with evidence has to bring us to an understanding of the complex systems that wherever we look seem to integrate the various forms of evidence we seek. If different disciplines have specialized experience with the different forms of evidence, yet the evidence which they respectively prefer to gather is reciprocally contingent upon the evidence gathered by other disciplines, success—even in gaining useless knowledge—would seem to depend upon appropriate forms of positive synergism, cooperation, and coevolution. And it is unthinkable that the success of many cooperating disciplines could prove anything but useful in our highly interconnected world.

Mathematical modeling programs have much to teach us about interdisciplinary cooperation because the mathematical and computing languages that many—if not all—of the contributing disciplines share serves to facilitate—even if it does not necessitate—unified methodology in the approach to interdisciplinary modeling problems. Such programs are often the recipients of significant public funding, and as a result, their scientific progress often proceeds at a remarkable pace. Since our society’s investment in science is turning increasingly toward such programs, opportunities for philosophical case studies are easy to find. And hopefully, the comparative approach to scientific methodology in which philosophers specialize may make some contribution to the methodological awareness of the scientific participants of these programs. We consider in Chapters IV and V an example of such a modeling program which is beginning, at least, to reveal the methodological unity and rapid scientific progress of a very significant scientific enterprise.

Chapter 4

Gaia Revisited: Part I. Interconnectedness in Climate

4.1 Introduction

In 1969, inventor James Lovelock published his now classic book on the Gaia hypothesis, which extended and popularized the theory he had developed with atmospheric chemist James Lodge and cell biologist Lynn Margulis.¹ The theory boldly proposed that the biosphere has developed the capacity to maintain climatic and chemical conditions in its environment which are conducive to its own survival.

Lovelock considers the contemporary concentrations of such atmospheric gases as nitrogen, oxygen, carbon dioxide, methane, nitrous oxide, ammonia, sulphur gases, methyl chloride, and methyl iodide, and argues that they are present in much higher concentrations than they would be if there were no biosphere, that they serve vital functions in the biosphere, and that the capacity to regulate their environmental levels therefore may be an evolutionary adaptation of the biosphere itself (Lovelock, 1979).²

¹See Lovelock (1972), Lovelock and Lodge (1972), Margulis and Lovelock (1974, 1978), Lovelock and Margulis (1974), and Lovelock (1979, 1988).

²Most recently, Charlson, Lovelock, Andreae, and Warren (1987) have shown that dimethylsulfide produced by algae is rapidly oxidized in the atmosphere to create sulfuric acid aerosols that serve as nuclei for cloud formation, resulting in what appears to be a highly adaptive climate modification associated with the cooling effects of cloud top albedo (reflectivity). Also see the discussion in the new Gaia book (Lovelock

Empirical evidence for or against Lovelock's *Gaia hypothesis*, the view that the atmosphere-ocean-cryosphere-lithosphere-biosphere system is self-regulating in the service of life, is unfortunately difficult to obtain.³ Yet the reason for the scarcity of evidence is not that earthly conditions which favor life are scarce; it is that the significance of such conditions for the existence of Gaia is unclear.

Consider, for example, the upwelling of warm, salty, circumpolar deep water in the Southern Ocean which contributes significantly to the global distribution of excess heat and salinity from the tropics and subtropics to the poles. Heat is exchanged between relatively warm upwelled water and the colder atmosphere, and the salinity of relatively salty upwelled water is reduced through the accumulation of precipitation and ice melt. The resulting cooler water of lower salt content circulates equatorward. Remarkably, the upwelling circumpolar water also releases excess carbon dioxide to the atmosphere and absorbs excess atmospheric oxygen, thereby functioning as something of a global lung for marine life. It is interesting to observe that fortuitously located holes in the Southern Sea ice called *polynyas* control the exchange of heat and biologically vital gases between upwelled water and the atmosphere, the accumulation of precipitation and ice melt responsible for diminishing the salinity of upwelled water, and the thermal gradient responsible for upwelling itself (Gordon and Comiso 1988). Polynyas, in short, strongly influence the contributions of circumpolar upwelling to the global ecology and climate.

What does the existence of a such a global pulmonary circulation and the improbable but essential location of polynyas imply about the adaptive mechanisms of the biosphere? Actually, very little. Even if mechanisms were discovered whereby the biosphere might help to bring about the coordination of deep water upwelling and polynya formation (say by somehow increasing cloud cover over the polynyas, thereby cooling the surface and enhancing upward convection of the relatively warm circumpolar water), this would not necessarily involve biological adaptation at a global scale. It could be argued that the aggregate effect of all species on the global environment at any time is accidental, that it naturally has

1988). Note that there is more recent evidence that puts the original results of Charlson et al. (1987) into question (Warren 1989).

³As noted above, it may be easier to show that individual species alter the environment to their own advantage. Indeed, as many cases of symbiosis illustrate, there is often selection pressure to be useful to neighboring species. It is unclear, however, how selection pressure might operate to make it adaptive to serve the biosphere as a whole (except in the case of humans, who might never have the opportunity to populate the galaxy if they remain too homocentric on earth).

certain stable features, and that it is necessary to life only because life adapted to it.⁴

In spite of the hypothetical nature of the Gaia hypothesis, and its failure to date in generating anything like an interdisciplinary research program to seriously investigate its explanatory value, the idea of a planetary organism or global cybernetic system has been so provocative that it has by now inspired a decade of debate as to its implications for environmental policy, environmental ethics, and our general world view. Are we to value sentient creatures, species, or the biosphere as a whole? Do nonliving natural environments (as on Mars) have any value in themselves? If the biosphere is self-regulating, need we worry about the effects of pollution and deforestation? Or are there Gaian organs (such as rainforests, wetlands, and coral reefs) the damage of which could jeopardize the future of the biosphere? What is the role of human civilization in Gaia? Should we protect Gaia from ourselves, use Her for our own purposes, or manage Her to maintain stable conditions for life and/or for ourselves?

Needless to say, such questions about Gaia's implications have been difficult to answer. This is not only because of the unprecedented value issues often involved; we do not even know whether Gaia exists, or by what trick of coevolution, design, or self-organization she may have come to exist. Does this mean that rational international environmental policy must await the verdict of a Twenty-first Century research program investigating Gaia's existence and origins? By then, of course, it may be too late.⁵

Fortunately, the environmental sciences are far more sophisticated than their ignorance about Gaia may suggest. We already know a great deal about the interdependence of the atmosphere, oceans, cryosphere, lithosphere, and biosphere, and about the interdependence

⁴The possible climate control considered here is fanciful, of course, as the deep circulation of the oceans varies considerably with the distribution of sea ice and other factors which affect ocean salinity, with the global climate and its thermal forcings of the deep circulation, and over millions of years, with continental drift; and in any case the oceans would exchange gases with the atmosphere. This is not to say, however, that atmospheric carbon dioxide levels are not relevant to climate, or that carbon dioxide levels are not altered by the biosphere. Changes in atmospheric carbon dioxide levels indeed seem to be associated with glacial-interglacial transitions, and there is some evidence that the advance of forests in the carboniferous period were responsible for lowering atmospheric carbon dioxide levels and for the great ice age beginning at the end of the carboniferous period some 280 million years ago. Although there may have been some advantage of this development for marine life, it would hardly have served the forests that created it or the terrestrial ecologies with which their evolution was most directly linked.

⁵This is not to say that the question of whether there is selection pressure for contributions to the general interspecies welfare, or whether some other organizing principle is at work, is not an interesting one or that it has no scientific merit. It is only to say that for the present we will have to motivate our own interest in the welfare of the planet independently of conclusive evidence that Gaia exists.

of processes within these subsystems of the climate system.⁶ Climate models have predicted the greenhouse effect, and with less certainty, the likelihood of a *nuclear winter* or *nuclear fall* occurring as a side effect of nuclear war.⁷ Such predictions have evident policy implications, implications which need not await perfectly confirmed models or the eventual demonstration that life has value because it controls the biosphere for its own purposes. Presumably, life is already known to have value, and even uncertain models about the state of life and its environment can make invaluable contributions to our environmental policy decisions.

This is not to say that there is no risk in interpreting uncertain models too liberally or in misunderstanding their limitations. Indeed, recent nuclear winter predictions generated no little controversy when it was learned that they were first offered to the public before assumptions which may have led to exaggerated climate impact predictions were adequately assessed. Questions of the ethics of public communications by scientists aside, when do we know if we have an adequate environmental model, say an adequate climate model? This question is complicated by the fact the climate system is so complex that all models must make simplifying assumptions—indeed, different ones for different predictive purposes. Furthermore, many different disciplines contribute to the development of climate models, and there is no little challenge in integrating their methodologies to do justice to the actual causal interactions among the phenomena that fall within their respective spheres of concern.

Whether or not we believe in Gaia, the interconnectedness among the great variety of processes which contribute to our planetary environment is undeniably abundant, with profound implications for the type of interdisciplinary cooperation required to understand the factors that contribute to environmental stability and change. Even if the ethical implications of climate models are not as dramatic as those of Gaian metaphysics, it would

⁶For U.S. climatologists, the biosphere is one of the climate subsystems and is taken to include the biota exclusive of their atmospheric, oceanic, and lithospheric environments. For many ecologists, environmentalists, and Soviet climatologists (e.g., Budyko 1980, 1986), the biosphere is more inclusively defined to contain the atmosphere, oceans, cryosphere, and lithosphere, as well as the biota, as subsystems. We will use the term biosphere in its former, more limited sense, and use the terms climate system or global ecosystem (following Budyko 1980) for the more inclusive system. In ways that should be clear from the context, we will call the climate system Gaia either in Lovelock's sense or to emphasize its partial determination by the biosphere.

⁷The nuclear winter hypothesis was first proposed (in modern form) by Crutzen and Birks (1982). The first nuclear winter model was developed by TTAPS (i.e., Turco, Toon, Ackerman, Pollack, and Sagan 1983, 1984). Methodological considerations such as those of Covey et al. (1985) motivated more careful consideration of model parameterizations (representations of the spatially and temporally resolved effects of unresolved phenomena as functions of resolved model variables). The nuclear fall alternative was proposed by Thompson and Schneider (1986); the most recently published nuclear fall figures include those of Thompson et al. (1987) and Ghan et al. (1988). A recent review of nuclear cooling findings may be found in Schneider and Thompson (1988).

seem that the former implications are immediate and involve the energy, agricultural, and military industries upon which so much of the world economy—for better or worse—seems to depend. Moreover, it is difficult to see how the international cooperation required to manage the planet could ever hope to succeed if scientists, who belong to that sector of society most committed to—and presumably successful at—rational social organization, cannot cooperate amongst themselves to provide ongoing accounts of our global environment that educate the public and policy-makers alike, and provide the objective context from which genuinely international policy can emerge. Perhaps the scientific community is the best candidate for the function of global mitochondria, producing the information, if not the energy, necessary for a sustainable global ecology.

This chapter and the following one will explore the special features of the climate system which make it difficult to understand, and their implications for the explanatory status of climate models and the structure of the interdisciplinary cooperation essential to climate model development. As a case study, this consideration of climate modeling may have general implications for the scientific requirements, at least, for a better understanding of our planetary environment and the opportunity, at least, for better informed environmental policy.

4.2 The Climate System: Nearly Decomposable and Interactionally Complex

The climate system is most often defined to include the atmosphere (for most purposes limited to the troposphere and stratosphere), oceans, cryosphere (continental ice sheets, seasonal snow cover, glaciers, seawater and freshwater ice, and permafrost), lithosphere (the earth's crust and solid upper portion of the mantle conventionally thought to constitute the plates that move over the partially molten asthenosphere, or weaker region of the mantle, in sea floor spreading and continental drift),⁸ and biosphere. Over millions of years,

⁸There is substantial evidence that the oldest continental cores penetrate deeply into the mantle rather than riding on top. See Jordon (1979) for a review of the original discovery. Recent evidence also points to a recycling of subducted ocean crust through its deep descent into the lower mantle and its return to the lithosphere via the convective upwelling of hot plumes of crustal remains through the lower and upper mantle and out hot spot volcanos. See Courtillot and Besse (1987) for a review in connection with polar wandering and magnetic field reversals. Our understanding of the nature and causes of tectonic phenomena is in a period of rapid change.

changes in the circulation of the mantle may contribute (in ways explained below)—with possible feedbacks from the above climate subsystems—to climatically significant variability in continental drift, the subduction of ocean crust and sediment, and volcanic emissions. As our understanding of mantle processes improves, its mutual interactions with the other climate subsystems may make it appropriate to consider it the sixth climate subsystem.

The above subsystem divisions are not the only ones possible. The totality of the earth's carbon (the carbonsphere) and the totality of its water (the hydrosphere), for example, are critical components of the climate system, yet cut across (or include in the case of the oceans and cryosphere) all of the above-distinguished climate subsystems. The contribution of these two alternative climate subsystems to the climate would alone account for almost all of the planet's reflection of incoming solar radiation, greenhouse trapping of emitted infrared radiation, and storage of heat—three of the most important determinants of the earth's climatic response to the sun. Clearly, whether we choose to focus on the planet's carbon and water or we choose to focus on the organization and interactions of the atmosphere, oceans, cryosphere, lithosphere, biosphere, and mantle among which carbon and water are exchanged will depend upon our scientific interests.

Climate modelers have found it natural to think in terms of the primary subsystems because they identify (on the average) very different types of matter (gas, liquid, solid water, solid rock, organisms, and if the mantle is included, hot creeping solid) that are spatially separated and conventionally studied by different disciplines. Meteorology, oceanography, glaciology, geology, ecology, and geophysics are all disciplines in their own right, and have only recently been called upon to contribute to global climate models. Moreover, the climate subsystems often contribute to the climate on different time scales, on the average, so that the slower subsystems can often be held fixed as boundary conditions or represented in simplified form—while the faster subsystems are modeled in more detail; or alternatively, time averages of faster subsystem behavior can be integrated with models of slower subsystems for long-term simulations or predictions.⁹

Thus for time scales under, say, 1 million years, it appears justified to neglect changes

⁹As we shall see below, both strategies have been employed together in asynchronous couplings of ocean and atmosphere models.

in the mantle,¹⁰ and simply to employ fixed locations¹¹ as boundary conditions and, if relevant, empirically determined levels of volcanic activity (currently estimated at 0.09 g Carbon per year).¹²

Similar considerations of time scale apply to the other climate subsystems. The thermal relaxation time of large ice sheets, and the residence time of the water in them, are both of the order of 100,000 years (Barry 1979; Gill 1982), while significant variation in glacier size is generally assumed to be of the order of 1000 years (Schneider and Dickinson 1974);¹³ the turnover time of water in the deep oceans is of the order of 100 to 1000 years;¹⁴ the extent of seasonal sea ice and snow cover and the heat storage (due to downward mixing) of the ocean mixed layer vary significantly over decades;¹⁵ and ocean eddies last months as compared with the life cycles of days to weeks of transient eddies in the atmosphere. The considerably larger heat capacity of the upper mixed layer of the oceans than that of the atmosphere and land surface makes sea surface temperatures lag months behind the solar forcing of surface temperatures on land.¹⁶

¹⁰Whether we have the knowledge to include them is another issue.

¹¹Continental speeds appear to be one to several centimeters per year, or tens of kilometers per million years. (India is an exception; see below.)

¹²Significant variations in the volcanic outgassing of CO₂ appear to have time scales of the order of 10,000 years; but the levels are low enough to contribute to climatically significant atmospheric levels only cumulatively over several million years. See the discussion in Arthur (1982). Volcanic dust and aerosol precursors may also have significant climatic effects. The possible importance of volcanic dust in climate was first explored quantitatively by climate modelers in Schneider and Mass (1975). Recently, it has become clear that the formation of sulfuric acid aerosols in the stratosphere from precursor gases emitted by volcanos can trap IR radiation emitted at lower altitudes, heating the stratosphere and cooling the surface, as has been demonstrated in the case of a 1982 eruption of El Chichón (Ramanathan 1988). In the past few years, the possible contribution of unusual volcanic activity to the mass extinctions of 65 million years ago has been among the numerous explanations considered. Although volcanic output of aerosol precursors presumably varies with volcanic carbon dioxide output on 10,000 year time scales, we do not know whether it can impact the climate on these time scales. For lack of evidence, I have conservatively lumped the time scales of climatically significant aerosol-producing emissions with the likely time scales of climatically significant carbon dioxide emissions. All of this is not to say that single volcanic events might not perturb climate on interannual time scales. But such events are not likely to be predicted from the study of the slow creep of mantle circulation.

¹³100,000 years is the length of the primary Milankovich cycle of ice age of glacial-interglacial periods associated with the earth's orbital parameters. Interglacial periods last about 10,000 years. Deglaciations seem to occur much more rapidly than glaciations, and are associated with sudden sea level increases and changes in the deep circulation which require study at minimal temporal resolutions of 1000 years. (See Berger 1982.) Although there is growing evidence that glacial surges and ungroundings may occur within the course of a century (see, e.g., Hollin and Barry 1979; Hollin 1980), their occurrence has not been conclusively confirmed.

¹⁴It is estimated that the deep circulation of the contemporary Atlantic takes some 500 years to turn over, and the deep circulation of the Pacific some 2000 years. The average is generally taken to be 1000 to 1200 years. These figures could decrease in an ice age due to enhanced thermal and salinity forcings. (The deep circulation is often called the thermohaline circulation.)

¹⁵See the discussion in Harvey and Schneider (1985a).

¹⁶The heat capacity of the entire atmosphere is less than that of the top 3 m of the oceans (Bryan et

Apparent differences of orders of magnitude in time scale seem to provide some justification for the omission of slower subsystem detail in models primarily concerned with faster time scales, and in cases of time integrations on slower subsystem time scales, of also allowing short runs to presumed equilibrium of the fast subsystem to represent longer periods for economic couplings with the slower subsystem.¹⁷ It will be recalled from the discussions in Chapters II and III above that these strategies are associated with *near-decompositions* of a system into subsystems with disparate time scales so that slower subsystems provide boundary conditions for faster ones, and the faster subsystems provide temporally averaged or integrated outputs as feedbacks to the slower subsystems. For certain modeling purposes, the climate system is treated as nearly decomposable.

Simulations of the 100,000 year cycle of glacial and interglacial periods of the past several million years, for example, are unlikely to be sensitive to continental drift involving motions of the order of meters per century (unless there are continental surge mechanisms associated with magnetic pole shifts). Likewise, since the contemporary deep circulation of the oceans turns over in 500 to several thousand years, climate models concerned with next century's greenhouse have often found it justified to neglect the details of the deep circulation.¹⁸ It is not necessary to model continental drift to predict next century's climate, and unless we are very unlucky, predictions of glacial surges are not relevant to predictions of the climate of the next few years.¹⁹

Near-decompositions of the climate system into subsystems (and subsystems) presumed to vary significantly on different time scales have allowed climate modelers to make the best of the historical lag of upper mixed layer ocean modeling behind atmospheric modeling, modeling of the deep oceans behind mixed layer modeling, glacial modeling behind deep ocean modeling, and geophysical models of continental drift behind glacial modeling.

al. 1988). This contrast results from the high relative density of water and air (about 800 at the density of surface air), the resulting relative mass of the oceans and atmosphere (about 270), and the relative heat capacities of water and air (about 4) (Gill 1982). An early energy balance analysis of the delaying effect of the ocean mixed layer on a CO₂ warming may be found in Cess and Goldenberg (1981).

¹⁷ See the discussion of asynchronous coupling schemes below.

¹⁸ Unfortunately, most models have also neglected the exchanges of heat between the upper mixed layer and deeper oceans. The transient (time dependent) response of the atmosphere-ocean system, however, has been shown to be quite sensitive (on decadal time scales) to coupling assumptions between the upper mixed layer of the oceans and the thermocline (intermediate water with temperatures rapidly decreasing with depth), and between the thermocline and the deep oceans. See Harvey and Schneider (1985a, 1985b).

¹⁹ As suggested above, there is growing evidence that glacial surges and ungroundings (by the sea) and the associated catastrophic increases in sea level can occur within one century (Hollin and Barry 1979, Hollin 1980).

By treating slower subsystems as fixed boundary conditions, or as slowly changing boundary conditions in models of low temporal resolution, and by considering additional detail for particular modeling purposes as appropriate and possible, climate modelers have been able to develop a hierarchy of climate models that treat the climate system with different degrees of detail.²⁰

Thus climate modelers have gradually attempted—as particular climate problems may have required—to incorporate progressively more detail of the slower subsystems into their models. Beginning with *near-decomposition* assumptions dictated as much by incomplete knowledge, economic considerations, and the nonlinear growth of error with model time²¹ as by hopefully justified considerations of time scale, model development often has proceeded with an implicit strategy of filling in the *interactional complexities* (Wimsatt’s notion reviewed in Chapter III above) that were neglected in earlier models, of progressively increasing the detail, when warranted by the problem under consideration and permitted by the state of the art, with which intersubsystem interactions were treated.

Gradual complexification is revealed, for example, in the progressive treatment of oceans and sea ice by general circulation models. Many early general circulation models (GCMs)²² (which model atmospheric and/or oceanic motions by dynamical consideration of the forces that produce these motions) were essentially models of the atmosphere’s response to fixed or highly simplified ocean boundary conditions, as in atmosphere models coupled with so-called *swamp* models which represented oceans by areas of wet land with infinite evaporation potential, zero heat capacity, and no horizontal or vertical heat transport (e.g., Manabe and Wetherald 1975, Wetherald and Manabe 1975, Manabe and Wetherald 1980). Later GCMs²³ often incorporated simplified models of the upper mixed layer of the oceans

²⁰See Schneider and Dickinson’s (1974) review of the hierarchy of climate models of different degrees of spatial, temporal, and physical detail. Note that the hierarchy of climate models refers to atmospheric or oceanic models of different degrees of spatial, temporal, and physical detail, as well as to the more inclusive hierarchy of climate system models. As we shall see below, considerations of temporal (as well as spatial) scale are also relevant to strategies of modeling the atmosphere and oceans, although the near-decomposability of the atmosphere and of the oceans is perhaps more questionable than the near-decomposability (for some modeling purposes) of the climate system as a whole.

²¹See the note 12 in Chapter III above.

²²For reviews of general circulation modeling, see Chang (1977), Saltzman (1978), Haltiner and Williams (1980), and Washington and Parkinson (1986). For a critical review of the variety of climate models (including GCMs) see Schneider and Dickinson (1974).

²³An exception to the historical progression suggested here is the asynchronously coupled atmosphere-ocean model developed as early as 1969 by Manabe and Bryan. The model (Manabe and Bryan 1969) calculated the equilibrium climate approached by the atmosphere and oceans under the assumption of very different relaxation times for the atmosphere and oceans. This model was a forerunner of later coupled

which were intended to provide more realistic, seasonally varying, boundary conditions for coupled models of the atmosphere. The fixed depth mixed layer models of Manabe and Stouffer (1980) and Washington and Meehl (1984), for example, calculate seasonal sea surface temperatures and albedos (reflectivities) from simplified assumptions about sea ice distribution and thickness and calculations of the vertical heat exchange between the atmosphere and oceans. These models neglect horizontal transports of heat within the mixed layer (largely driven by winds) and exchanges of heat between the mixed layer and the deeper ocean, as well as seasonal and horizontal variation in the depth of the mixed layer and in the associated structure of the thermocline (intermediate water between the upper mixed layer and the deep ocean characterized by temperatures rapidly decreasing with depth)²⁴; and they neglect the dynamics of sea ice.²⁵

Bryan et al. (1982)²⁶ coupled full-blown atmospheric and ocean GCMs, but failed to consider realistic lithospheric boundary conditions, i.e., the land/sea fraction at each latitude (a deficiency identified by Thompson and Schneider 1982). Schlesinger et al. (1985)²⁷ employed realistic geography in a similarly coupled atmosphere-ocean model. In a recent model, Bryan et al. (1988) employ realistic geography and asynchronously coupled atmosphere and ocean models.²⁸

Although the different *average* time scales of the climate subsystems suggest the appropriateness—for some modeling purposes and at some historical stages of model development—of neglecting slower subsystem detail, scientific progress in climate modeling demands that those details are gradually filled in to the extent they make a difference to

models (e.g., Bryan et al. 1982) intended to calculate transient (time-dependent) responses of atmosphere and oceans to imposed boundary conditions (such as CO₂ increases or solar output changes).

²⁴See the critical discussions in Harvey and Schneider (1985a and 1985b) and Gallimore and Houghton (1987).

²⁵See the critical discussion in Semtner (1987). A review of many aspects of air-sea-ice modeling may be found in Herman (1986).

²⁶Also see Spelman and Manabe (1984) and Bryan and Spelman (1985) for further discussions of Bryan et al. (1982).

²⁷Also see Schlesinger and Jiang (1987) for further discussions of Schlesinger et al. (1985).

²⁸Asynchronous couplings appeal to the longer thermal relaxation times of the ocean mixed layer as compared with that of the atmosphere to allow short atmospheric runs to represent the longer-term response of the atmosphere to slow changes in sea surface temperature calculated by the ocean model. Note the assumption of a near-decomposition of the atmosphere-ocean system into atmospheric and oceanic subsystems. In the case of dynamical models, asynchronous coupling strategies are motivated by the economics of computer time and the nonlinear growth of errors with model time. For critical analyses of the limitations and justifiable uses of asynchronous coupling schemes, see Ramanathan (1981), Schneider and Harvey (1986), and Harvey (1985).

the faster subsystems (or aspects thereof) which may be of more direct climatological or social interest.²⁹ This need for greater detail emerged, for example, in the context of modeling the transient response (the time-dependent response in approach to a presumed equilibrium) over the coming decades to centuries (or longer) of the atmosphere-ocean system to feasible increases in atmospheric carbon dioxide levels. Despite the longer time scales of the mixed layer as compared with the atmosphere, and of the deep ocean as compared with the upper mixed layer of the oceans, the interactions of the former and of the latter have proven quite relevant to the rate at which the atmosphere will approach its equilibrium response to, say, a CO_2 doubling.³⁰ On shorter time scales, the devastating consequences for fisheries and the widespread climatic shifts (such as monsoon failure) associated with El Niño (the periodic failure of upwelling along the coast of Peru) has made it imperative to explore the couplings of atmospheric winds and tropical cyclones with the horizontal circulation and vertical mixing of the Pacific mixed layer on the fast time scales of the atmosphere.³¹

In addition to a progression from nearly decomposed subsystems to interactionally complex treatments of climate subsystem interactions, respective models of the atmosphere and oceans reveal a similar order of development. In model experiments of the effects of increases in the levels of carbon dioxide, for example, it has proven important not only to treat interactions between the atmosphere and oceans in greater detail, but also to consider the feedbacks that operate within the atmosphere that have the potential for amplifying or diminishing the greenhouse warming. Clouds, for example, are too small to be resolved in climate models, and average values for cloud amount, height, and albedo (reflectivity) at model scales generally are prescribed. Yet the greenhouse warming could alter cloud amounts, heights, optical density, and distribution in ways that affect both the flux of incoming solar radiation reflected back to space and the flux infrared radiation emitted at lower altitudes that is trapped by clouds. Such effects

²⁹Scientific progress in our understanding of any of the climate subsystems obviously also has inherent, if more specialized, scientific value.

³⁰See, for example, Schneider and Thompson (1981), Thompson and Schneider (1982), Harvey and Schneider (1985a and 1985b) and Gallimore and Houghton (1987).

³¹See the review by Ramage (1986) which emphasizes the (mutual) interdependence of sea (surface temperature and currents) and tropical cyclones, an empirical study of the ocean response to a hurricane by Sanford et al. (1987), and a discussion of the response of an ocean-atmosphere GCM to wind bursts associated with tropical storms by Latif et al. (1988). Note the atmospheric time scales on which all these concerns are defined. When it proves necessary to explore slower subsystem processes defined on the time scales of the faster subsystem to better account for faster subsystem behavior that results from interactions with the slower subsystem, it suggests that the system in question is believed to be interactionally complex (relative to the systemic behavior of interest). See the detailed discussion of interactional complexity in Chapter III.

could in turn enhance or diminish the warming due to carbon dioxide, depending on their relative importance at different latitudes. Climate modelers have become increasingly aware of the urgency of treating clouds in greater detail (e.g., Schneider 1972; Schneider and Dickinson 1974, 1976; Schneider et al. 1978; Washington and Meehl 1984; Wetherald and Manabe 1986, 1988; Ramanathan et al. 1989). Since clouds affect incoming solar and outgoing longwave (infrared) radiation fluxes that distribute temperature vertically (i.e., transport heat with consequences for vertical temperature) and thereby force both vertical and horizontal atmospheric motions, and atmospheric motions alter the formation and distribution of clouds, the interscale treatment of cloud feedback—i.e., the representation of mutual interactions between clouds at unresolved spatial scales and resolved atmospheric motions—can be classified as interactionally complex.³²

For some purposes the climate system has been nearly decomposable into subsystems or phenomena modeled at different temporal or spatial scales, into subsystems or processes whose couplings have been simplified in various ways. For other purposes selected intersubsystem and intrasubsystem details have been brought into focus as missing links in climate problems that are not easily solved. The question is not, therefore, whether the climate system is nearly decomposable or interactionally complex. It is whether a particular problem approached by a given state of the art requires and allows the integration of several subsystems or treatment of single subsystems in greater in detail than earlier decompositions allowed.³³

³²Ocean and cloud feedbacks also have proven critical in nuclear winter (or nuclear fall) simulations. See the discussion in Schneider and Thompson (1988).

³³My distinction here between intersubsystem interactions and interactions between different phenomena within a subsystem is a concession to the ordinary use of the word subsystem, and intended to be consistent with the above identification of the primary climate subsystems. It will be recalled from the discussions of Simon's notion of near-decomposability in Chapters II and III, however, that subsystems are distinguished by the relatively high degrees of interdependence among their variables. Thus variables distinguished by different degrees of temporal or spatial resolution are often relatively independent, and are taken to define different subsystems. Cloud variables, or variables associated with microscale turbulence, mesoscale winds and storms, or any other subgrid-scale or unresolved phenomena would, as a first approximation, identify subsystems different from those defined by resolved model variables. If a near-decomposition is assumed, then it is only the average or aggregate consequences of faster, smaller, or lower-level subsystems that are relevant to slower or higher-level subsystems. Yet when strong feedback from smaller to larger scales exists, the mutual interconnectedness between scales demands that at least some of the neglected detail regarding smaller scales be coupled with larger-scale variables. Likewise, when slower subsystems interact on time scales much shorter than their average time scales with faster subsystems, it becomes important to directly represent these interactions. Whether we are considering the importance of representing neglected feedbacks from smaller temporal or spatial scales to larger ones or from slower subsystems to faster ones, the situation is interactionally complex (Wimsatt's notion reviewed in Chapter III). As we shall see below, parameterizations (or the improvement of parameterizations) in climate models generally can be interpreted as responses to perceived interactional complexity.

Modeling strategies and priorities are not only the outcome of system science. They emerge from the physical, biological, and social processes, the temporal and spatial scales and historical periods of scientific and social interest; the degree of accuracy required; the state of our theoretical understanding; the availability of data; the tractability of the modeling required; and even the availability of grants and the personal interests of the scientists involved. Historically, for example, limitations in knowledge, data, tractability, and interest—as well as considerations of contrasting time scales—have caused ocean modeling to lag behind atmosphere modeling, with the result that the interactive contribution of the oceans to atmospheric processes and climate have been represented in models with less than ideal detail for many modeling purposes. But models with simplified atmosphere-ocean couplings have defined new challenges for oceanographers and climate modelers, and have provided the context for progressively more detailed treatments of atmosphere-ocean couplings. What was once a premature activity has become increasingly appropriate as theoretical knowledge and data availability have grown, as new problems have become clarified, and as professional interests have shifted over the years. Perceptions of interactional complexity mature over time.

Yet in the long run, it is the complexity and organization of the system itself that will determine how effectively research in a field has captured its structure, behavior, and evolution. It may help to highlight the importance of *eventually* considering relevant interactions between climate subsystems, as well as interlevel interactions within these subsystems, if we review, in very broad and introductory outline, some of the major contributions of the climate subsystems to climate.³⁴

4.2.1 The atmosphere

The atmosphere intercepts about a billionth of the sun's energy,³⁵ although this fraction varies seasonally and over predominant cycles of 100,000 years, with the combined effects

³⁴Our short review of the climate system will necessarily be highly selective. It is designed to clarify the types of complexity (i.e., degrees of interdependence and multiplicity of relevant processes) revealed in the climate system. Standard reviews of the climate system may be found in National Research Council (1975), Lockwood (1979), Saltzman (1983), and Monin (1986). Reviews that integrate professional and public education concerns include those of Schneider with Mesirov (1976), Kandel (1980), and Schneider and Londer (1984).

³⁵The sun currently radiates energy at the rate of 3.8×10^{26} Watts, of which the earth's disk intercepts some 4.5×10^{-8} percent (Schneider and Londer 1984).

of the earth's orbital parameters (i.e., the wobble and tilt of its axis and the eccentricity of its elliptic orbit).³⁶ The sun's output is also known to vary, most dramatically over billions of years of stellar evolution, but also, for example, over days (*r*-Mode oscillations³⁷ and solar-sector boundary passages), years (the quasi-biennial oscillation), tens of years (the 11- and 22-year solar magnetic cycles), and hundreds of years (Maunder-minimum type cycles) (Siscoe 1978).³⁸ At particular locations, the solar radiation received at the top of the atmosphere will depend upon the time of day, as well as the time of year and astronomical orientation and boundary conditions.

The solar flux (energy per unit time per unit area) intercepted by the spherical shell at earth's average distance from the sun (1 astronomical unit or A.U.) is given by the radiative output of the sun divided by the surface area of the spherical shell. Since the radiative output of the sun is known to vary, this amount is not constant, but is nevertheless called the solar constant. Its current value is about 1380 Watts per m^2 . The solar energy per unit time intercepted by the earth's disk is given by the solar constant (S) times the area (πR^2) of the earth's disk. The daily global average flux received by the planet (averaged over sunny and dark times of day and over the entire planet) is given by the energy per unit time received by the planet divided by the surface area of the planet. The *earth-averaged solar constant* (now known to vary with solar output) is thus given by $S_{av} = \frac{\pi R^2 S}{4\pi R^2} = \frac{S}{4}$; its value (currently 345 Watts per m^2) is a major determinant of climate. Let us consider some of the effects of this *insolation* (incoming solar radiation, or flux) as it enters the climate system through the atmosphere.

³⁶The eccentricity of the earth's orbit (its degree of ellipticity) cycles through its highest order response to gravitational perturbations in about 100,000 years, affecting the seasonal closeness of the planet to the sun. The angle of obliquity between the earth's equatorial plane and that of its ecliptic or orbital plane (or equivalently, the tilt of its axis relative to the normal to the ecliptic) varies in a period of 41,000 years between 22.1 and 24.5 degrees (it is presently 23.5 degrees). In an average period of 22,000 years the earth's axis gradually precesses (wobbles) through a full circle, cyclically affecting the annual timing of seasons for the two hemispheres. Despite the stronger solar forcings associated with the two shorter cycles, Hays, Imbrie, and Shackleton (see the review in Covey 1984) have shown that the combined effects of the three orbital parameters may better explain the observed 100,000 year cycle of ice ages over the past several million years than the effects of any one of the orbital parameters acting alone. The cause of the climatic optimum of 9,000 to 5,000 years ago, however, seems to have been a result of the combined effect of tilt and wobble (Kutzbach and Guetter 1984), so that it seems appropriate to consider the orbital parameters to have climatic effects on time scales of the order of 1000 years. Also see the discussions in Kandel (1980) and Schneider and Londer (1984). Early modeling of the possible connection between orbital variations and ice ages may be found in Schneider and Thompson (1979).

³⁷See the discussion of various correlations between *r*-modes (recognized oscillations most often with periodicities between 13 and 85 days) and solar output in Wolff and Hickey (1987).

³⁸For discussions of possible sun-weather/climate relationships (other than the obvious ones), see Markson (1978) and Herman and Goldberg (1978a, 1978b). Also see the classic discussion of the connection between the Maunder Minimum and sunspots in Eddy (1976), and an anthology of the new solar physics in Eddy (1978).

For current atmospheric composition and surface conditions, about 25% of the earth's insolation is absorbed in the atmosphere (about 3% is the ultraviolet radiation absorbed by the ozone layer in the stratosphere, about 17% is absorbed by water vapor, dust, and haze, and 5% by clouds);³⁹ of the remaining 75%, about 30% is reflected back to space (about 19% off of clouds, 6% by dust and haze, and 5% off the earth's surface) (Schneider and Londer 1984; Schneider 1987a).⁴⁰ The remaining 45% is absorbed by the earth, divided approximately equally between land and sea (Gill 1982).

We assume that the planet as a whole behaves like a blackbody in radiative equilibrium and stabilizes in globally and annually averaged temperature by emitting radiant energy (in the case of the earth, infrared or longwave energy) at a rate equal to the rate at which solar energy is absorbed. (This assumption works well only when the climate isn't changing in association with interannual changes in heat storage in the oceans and cryosphere.) The radiative component of vertical heat transports may be summarized as follows. The net infrared radiation leaving the planet is equal to the difference between the insolation (incoming solar radiation) and the reflected radiation. Virtually all of the radiation absorbed at short wavelengths is reemitted at long wavelengths.⁴¹ For our above figures, this comes to 70% of the incoming radiation (about 4% of which is emitted by the earth's surface, 63% by water vapor, carbon dioxide, and clouds in the troposphere, and 3% by carbon dioxide and water vapor in the stratosphere). Vertical convective transport of heat represents about 29% of the incoming solar radiation (24% latent heat and 5% sensible heat).⁴²

³⁹The climatic significance of the reflective and absorptive properties of aerosols was first explored systematically by Rasool and Schneider (1971) in an article that helped to awaken an awareness of the potential impacts of human activity on the global climate.

⁴⁰As Schneider and Dickinson (1976) have pointed out, a considerable portion of the planetary reflected radiation is actually multiply reflected between earth and sky (mostly clouds). We shall explore some of the implications of multiple reflections below.

⁴¹The wavelenths of terrestrial radiative emissions peak at 10 mm in the longwave infrared (IR), while incoming solar radiation peaks at 0.5 mm in the visible; their respective spectra overlap hardly at all.

⁴²Note that vertical convective transports of heat disrupt radiative equilibrium assumptions for the surface and atmosphere treated separately, but not when they are lumped together for consideration of the planetary radiation balance. Furthermore, as discussed below, vertical transports of dry air transport heat upward adiabatically, so that all of the heat absorbed at the surface is expended, in principle, in the work of expansion. When the vertically transported air eventually returns to lower altitudes, the atmosphere around it does work on it to compress it, and the resulting heating in principle returns the originally absorbed heat to lower altitudes. Thus for a global and annual average, vertical transports of sensible heat affect neither the radiation balance of the surface nor the radiation balance of the atmosphere. The situation is very different, however, for latent heat release associated with the condensation of humidity into clouds as rising cooling air becomes saturated. Some of this heat may be transformed into the kinetic energy of atmospheric circulations (such as that of the Hadley cells), and the balance is radiatively emitted (as a first approximation, equally) upward toward space and downward toward the earth's surface. (For more detailed accounts of radiative transfer in the atmosphere, see Wallace and Hobbs (1977, Chapter 6), Paltridge and Platt (1976), and Liou (1980).) Thus wet convection redistributes heat vertically and disrupts the radiative equilibrium of both

Although the atmosphere has a negligible heat storage ability (associated with its heat capacity and with the latent heat of vaporization of water) as compared with the heat storage abilities of the oceans and cryosphere (associated with the heat capacity of the oceans and cryosphere and with the cryosphere's latent heat of fusion), radiative and dynamical processes in the atmosphere are so important to climate that certain feasible atmospheric distributions of carbon dioxide, water vapor and clouds, or aerosols possibly could result, in principle, in a runaway greenhouse and boil the oceans away, or cause a complete glaciation of the planet. In general, variations in atmospheric carbon dioxide, absolute humidity, cloudiness, and aerosol content are responsible for the major part of the variations in the earth's albedo (reflectivity) and in the greenhouse effect.

As the above figures indicate, in the current climate the absorption of incoming solar energy (per unit time) is divided about equally among the atmosphere, oceans, and land; and the redistribution of heat from the warmer tropics and subtropics to polar regions is divided about equally between atmospheric and oceanic circulations (Gill 1982). The atmosphere distributes seasonal temperatures between land and sea, and its winds are the driving force of the ocean circulation. The atmosphere is also essential to the biosphere, of course, and the biosphere plays a major role in determining the chemistry and associated climatic behavior of the atmosphere and oceans (Lovelock 1988). And the very patterns of humidity, cloudiness, precipitation, temperature, wind, and pressure that contribute so much to the distribution of heat, mass, and momentum in the climate system themselves constitute what we usually mean by climate—and likely will until we populate the seas.

If we average horizontally over the entire globe and temporally over the seasons of a year, consider just the two vertical levels of the atmosphere and surface, and assume that over the year there is no net change in the storage of heat by the oceans and cryosphere, it is possible to develop simple radiation models of the atmosphere-surface system that explore the mathematical relationships among the processes of reflection, absorption, transmission, and emission of radiant energy that are the major determinants of the globally averaged climate in periods too short (say years) for significant climatic change associated with changes in heat storage. A brief, and necessarily simplified consideration of just such a model may help us to appreciate both the value and limitations of *near-decomposition* strategies

the atmosphere and the surface. Energy balance is achieved, but without radiative equilibrium.

that separate vertical from horizontal and radiative from dynamical processes.⁴³ The energy per unit time absorbed by the planet is given by

$$S(1 - \alpha_p) \pi R^2,$$

where S is the solar constant, as defined above, α_p is the planetary albedo (i.e., the fraction of incoming radiation which is reflected back to space, whether directly off cloud tops or other atmospheric constituents or off the surface of the earth, or indirectly in connection with multiple reflections between earth and sky); $1 - \alpha_p$ is the fraction of incoming radiation which is absorbed (whether in the atmosphere or by the surface), and R is the earth's radius (so πR^2 is the area of the earth's disk). For the atmospheric radiation figures considered above, $0.30S\pi R^2$ is reflected back to space by clouds, and $0.70S\pi R^2$ is the energy per unit time absorbed by the planet, of which $0.25S\pi R^2$ is absorbed by the atmosphere and $0.45S\pi R^2$ is absorbed by the earth's surface. (Multiple reflections are neglected in these figures, but will be considered below.)

It should be emphasized that the net effect of solar radiation absorbed in the atmosphere is complicated, in that the atmosphere's outgoing emissions of infrared radiation in association with directly absorbed incoming solar radiation results in a loss of approximately half the absorbed energy per unit time to spaceward emissions;⁴⁴ while the atmospheric absorption of indirect (reflected and multiply reflected) solar radiation results in a lower effective planetary albedo (associated with less indirect, i.e., reflected solar energy per unit time transmitted out of the planet and with correspondingly more energy per unit time absorbed by the planet), as well as with the emission of half the absorbed radiation to space. For a given planetary albedo, however, the total energy absorbed per unit time is also given (by $S(1 - \alpha_p) \pi R^2$, as indicated above). Therefore, if the planetary albedo and associated planetary absorption of energy are given, approximately half of all of the (direct

⁴³The model developed here is a variant of the one developed by Schneider and Mass (1975) as a teaching aid, although some aspects of the interpretation are my own. This basic radiation model will be extended and considered in more detail in Chapter V in a discussion of the explanatory functions of parameterizations in climate models.

⁴⁴Radiative equilibrium is assumed here, whereby the atmosphere emits infrared radiation at the same rate it absorbs solar radiation. (Radiative equilibrium is invoked again below when we assume that the atmosphere emits infrared radiation to balance its absorption of the major part of the infrared radiation emitted by the surface in the greenhouse effect.) We suppose—in this simple, intentionally idealized, two-level radiative climate model—that half of the radiation emitted by the atmosphere goes upward to space, and half downward, to be 100% absorbed by the surface. Vertical convective transports of energy are neglected. (Their role in climate will be considered below.) For more detailed and realistic considerations of radiative transfer, see, for example, Wallace and Hobbs (1977, Chapter 6), Paltridge (1976), and Liou (1980).

or indirect) solar energy absorbed in the atmosphere is lost to space through IR emissions, and is unavailable to heat the surface. This surface cooling effect is to be distinguished from the *greenhouse effect* (discussed below), which is associated with the atmospheric absorption of a large percentage of the IR radiation emitted by the surface and the radiation of approximately one half the energy per unit time absorbed in the atmosphere back to the surface where it is 100% absorbed, thereby heating the surface over and above its heating due to the direct and indirect solar radiation it absorbs and the downward emission of IR associated with solar radiation absorption in the atmosphere.

The net thermal consequences of the absorption of energy by the planet are derived from the assumption of equilibrium between the rates at which the planet absorbs and emits radiation. The total longwave (terrestrial IR) energy per unit time emitted by the planet is given by

$$\sigma T_p^4 4\pi R^2,$$

where σ is the Stefan-Boltzmann constant, T_p is the planet's effective temperature of radiative equilibrium (i.e., the temperature which the planet would have if it were a homogeneous blackbody in radiative equilibrium with the absorbed solar flux), and $4\pi R^2$ is the surface area of the planet. For the observed contemporary outgoing longwave radiation of $237 \frac{W}{m^2}$ and a Stefan-Boltzmann constant of $5.67 \times 10^{-8} \frac{W}{m^2 \cdot K^4}$, the planet's effective temperature of radiative equilibrium is approximately 254 K. This compares with our current equilibrium surface temperature of 288 K corresponding to observed surface longwave emissions of $390 \frac{W}{m^2}$, which as we shall see below, is warmer than planet's equilibrium temperature by 34° C due to the combined effect of the greenhouse warming of the surface and the net relative surface cooling associated with the capture of significant solar radiation in the atmosphere—rather than its direct absorption by the surface.

From the assumption of radiative equilibrium, the incoming solar energy per unit time absorbed by the planet is equal to the longwave energy per unit time emitted by the planet.⁴⁵ Therefore,

$$S(1 - \alpha_p) \pi R^2 = \sigma T_p^4 (4\pi R^2),$$

⁴⁵This equilibrium assumption would not be expected to hold in particular seasons, but only for a suitable averaging period, say a year; and once again, radiative equilibrium does not hold when the heat storage of other climate subsystems (e.g., the oceans and cryosphere) changes during climatic change.

$$\text{and } \sigma T_p^4 = \frac{S}{4}(1 - \alpha_p),$$

where $\frac{S}{4}$ represents the daily global average solar flux at the top of the atmosphere (incoming energy per unit time divided by the earth's surface area). If we represent $\frac{S}{4}$ by, say, S_{av} (as above), then

$$\sigma T_p^4 = (1 - \alpha_p)S_{av},$$

$$\text{or } T_p = \left[\frac{1}{\sigma}(1 - \alpha_p)S_{av}\right]^{\frac{1}{4}}.$$

This equation is actually the simplest of all possible models of the climate. Technically, it is an energy balance model, since it doesn't contain any dynamical calculations of atmospheric or oceanic motions, but only treats transports of heat (in this case, only radiative transports and no annual changes in heat storage by the hydrosphere). It expresses the planet's effective temperature of radiative equilibrium as a function of daily and globally averaged incoming solar flux and the earth's aggregated albedo.

The planetary albedo α_p can be calculated directly from a consideration of the destiny of solar radiation as it is reflected, absorbed, and transmitted through the climate system. We need only be given α_S , A_S , α_E , and A_D , which respectively represent the aggregate albedo (the fraction of solar radiation reflected) of the sky (largely due to clouds), the fraction of incoming solar flux directly absorbed in the atmosphere, the aggregated albedo of the surface of the earth, and the fraction of diffuse solar flux (associated with solar radiation that has been reflected one or more times) that is absorbed in the atmosphere. Note that we have made the simplifying assumption that the sky reflects direct and diffuse solar flux equally. The four quantities defined here are among the simplest parameters that assign, for certain modeling purposes, constant values to properties known to vary with climatic change itself. Thus the model attains mathematical closure at the expense of long-term realism, but as a step toward such realism it allows us to explore the dependence of the planetary temperature of effective radiative equilibrium upon insolation, the albedo of earth and sky, and the atmospheric absorption of direct and diffuse solar radiation.

Incoming solar flux S_{av} is either reflected (α_S) off cloud tops and other atmospheric constituents before it can reach the earth's surface, absorbed (A_S) in the atmosphere, or transmitted ($1 - \alpha_S - A_S$) to the surface. The reflected flux ($S_{av}\alpha_S$) contributes to the

planetary albedo. At the surface the transmitted solar flux ($S_{av}(1 - \alpha_S - A_S)$) is either absorbed ($1 - \alpha_E$) or reflected (α_E). The reflected solar flux $S_{av}(1 - \alpha_S - A_S)\alpha_E$ is either reflected once again off the sky (α_S), absorbed (A_D), or transmitted out of the atmosphere ($1 - \alpha_S - A_D$). The transmitted part ($S_{av}(1 - \alpha_S - A_S)\alpha_E(1 - \alpha_S - A_D)$) contributes, along with the incoming radiation originally reflected off of clouds, to the planetary albedo. The part of the diffuse solar radiation reflected back to the surface from the aggregated cloud and sky bottom is given by $S_{av}(1 - \alpha_S - A_S)\alpha_E\alpha_S$. This multiply reflected radiation is now either absorbed at the surface ($1 - \alpha_E$) or reflected back upward (α_E) again. The portion of this flux that is reflected upward again is given by $S_{av}(1 - \alpha_S - A_S)\alpha_E\alpha_S\alpha_E$. Once again, this radiation is either reflected back to the surface (α_S), absorbed in the atmosphere (A_D), or transmitted out of the atmosphere ($1 - \alpha_S - A_D$) to space. The fraction of radiation transmitted back to space is given by $S_{av}(1 - \alpha_S - A_S)\alpha_E^2\alpha_S(1 - \alpha_S - A_D)$. The latter flux contributes, along with the previously reflected flux, to the planetary albedo.

Thus, if we consider the infinite series of multiply reflected flux as well as the incoming solar flux originally reflected back to space off the top of the sky, the planetary albedo is given by the following, where S_{out} is the total solar radiation reflected out of the atmosphere:

$$\begin{aligned}\alpha_p &= \frac{S_{out}}{S_{av}} \\ &= \alpha_S + (1 - \alpha_S - A_S)\alpha_E(1 - \alpha_S - A_D)[1 + \alpha_S\alpha_E + (\alpha_S\alpha_E)^2 + \dots] \\ &= \alpha_S + \frac{(1 - \alpha_S - A_S)\alpha_E(1 - \alpha_S - A_D)}{1 - \alpha_S\alpha_E}.\end{aligned}$$

Note that multiple reflections can contribute significantly to planetary albedo over and above the initial sky and surface reflections of $\alpha_S + (1 - \alpha_S - A_S)\alpha_E(1 - \alpha_S - A_D)$. The denominator $1 - \alpha_S\alpha_E$ is less than one, and can substantially increase the contribution of surface reflections to the planetary albedo.⁴⁶ If α_S and α_E are prescribed, we can calculate the effective temperature of planetary effective radiative equilibrium directly as a function of the solar constant and planetary albedo. Thus

$$\begin{aligned}\sigma T_p^4 &= (1 - \alpha_p)S_{av} = S_{av}\left[1 - \alpha_S - \frac{(1 - \alpha_S - A_S)\alpha_E(1 - \alpha_S - A_D)}{1 - \alpha_S\alpha_E}\right] \\ T_p &= \left(\frac{S_{av}}{\sigma}\left[1 - \alpha_S - \frac{(1 - \alpha_S - A_S)\alpha_E(1 - \alpha_S - A_D)}{1 - \alpha_S\alpha_E}\right]\right)^{\frac{1}{4}}\end{aligned}$$

⁴⁶See the discussion in Schneider and Dickinson (1976) of the importance of including multiple reflections in climate models.

Although the above expression does allow us to explore the sensitivity of T_p to different assumptions about insolation and sky and surface albedo, it gives us no insight into processes internal to the climate system that could affect such sensitivities, and as it stands, it gives us no information about surface temperature, which is generally of greater climatic interest. If we consider the conditions for radiative equilibrium, however, the outgoing longwave (infrared or *terrestrial*) radiation emitted by the planet must equal the radiation absorbed. The radiation that is absorbed is simply the radiation that is not reflected by the earth-atmosphere system. The longwave radiation emitted by the planet comes from three sources: surface emissions that are not captured in the atmosphere but escape to space, the emissions of the atmosphere that balance its absorption of (direct, reflected, or multiply reflected) solar radiation, and the emissions of the atmosphere that balance its absorption of infrared radiation emitted by the surface.

Thus we can write

$$(1 - \alpha_p)S_{av} = \sigma T_E^4(1 - A_T) + \frac{F_S}{2} + \frac{1}{2}\sigma A_T T_E^4,$$

where the left side expresses total planetary absorption of solar energy as calculated above, the first term on the right side represents the emissions of the surface that are not absorbed in the atmosphere (where T_E is the surface temperature, A_T is the atmospheric absorptivity of terrestrial radiation and we presume that IR radiation is not reflected, so that whatever fraction is not absorbed is transmitted), the second term on the right represents 50% of the atmospheric emissions due to the absorption of solar energy (the other 50% being radiated downward), and the last term represents 50% of the atmospheric emissions due to the absorption of longwave emissions from the surface (the other 50% being radiated downward to warm the surface in the greenhouse effect). The planetary albedo α_p in the above expression is given by $\alpha_S + \frac{(1 - \alpha_S - A_S)\alpha_E(1 - \alpha_S - A_D)}{1 - \alpha_S\alpha_E}$, as above, and global reflected radiation is simply $S_{av}\alpha_p$. F_S can be calculated from an infinite series similar to the one considered for reflected radiation and albedo. We simply replace all the relevant atmospheric reflection terms with absorption terms. Thus $F_S = S_{av}[A_S + \frac{(1 - \alpha_S - A_S)\alpha_E A_D}{1 - \alpha_S\alpha_E}]$.

Solving for σT_E^4 , we get

$$\sigma T_E^4 = \frac{(1 - \alpha_p)S_{av} - \frac{F_S}{2}}{1 - \frac{A_T}{2}},$$

where $\frac{F_S}{2}$ represents the apparent IR loss of 50% of the energy that would have heated the surface (neglecting the positive feedback of the greenhouse for the moment) had the atmosphere not absorbed solar radiation, and the denominator on the right side represents the positive feedback surface warming due to the greenhouse effect. I say the *apparent* 50% surface cooling since the absorption of diffuse (reflected or multiply reflected) solar radiation in the atmosphere not only results in spaceward IR emissions, but it also reduces planetary albedo by trapping radiation that otherwise would have been transmitted out of the atmosphere. For the purposes of our simple model, half the diffuse radiation absorbed in the atmosphere will be radiated downward to heat the surface. Thus the comparative effects of the surface cooling due to the trapping of incoming direct solar radiation in the atmosphere and the surface warming associated with indirect solar absorption must be considered to estimate their combined effects on surface temperature. The effects of solar radiation absorption, as well as of the greenhouse effect, can be appreciated in more detail if we make the substitutions indicated above.

$$\begin{aligned}\sigma T_E^4 &= \frac{(1-\alpha_p)S_{av} - \frac{F_S}{2}}{1 - \frac{A_T}{2}} \\ \sigma T_E^4 &= \left[\frac{S_{av}}{1 - \frac{A_T}{2}} \right] \left[1 - \alpha_S - \frac{(1-\alpha_S - A_S)\alpha_E(1-\alpha_S - A_D)}{1 - \alpha_S\alpha_E} - \frac{1}{2} \left(A_S + \frac{(1-\alpha_S - A_S)\alpha_E A_D}{1 - \alpha_S\alpha_E} \right) \right] \\ &= \left[\frac{S_{av}}{1 - \frac{A_T}{2}} \right] \left[\left(1 - \alpha_S - \frac{A_S}{2} \right) - \frac{(1-\alpha_S - A_S)\alpha_E(1-\alpha_S - \frac{A_D}{2})}{1 - \alpha_S\alpha_E} \right]\end{aligned}$$

The above equation shows that $\frac{A_S S_{av}}{2}$, the radiation emitted spaceward due to the direct absorption of incoming solar radiation, does indeed diminish the solar flux initially *arriving* at the surface, as distinguished from the solar flux absorbed at the surface, by $\frac{A_S S_{av}}{2}$ (neglecting, for the moment, the greenhouse effect represented by the denominator $1 - \frac{A_T}{2}$). Yet the second of the two major terms on the right essentially represents all of the fluxes which are not available to the surface that are derived from the direct solar radiation initially arriving at the surface, so that the IR loss to space due to direct (incoming) solar absorption in the atmosphere represented by $\frac{A_S S_{av}}{2}$ must be understood to work together with IR losses to space due to *indirect* solar absorption in the atmosphere and the transmission of unabsorbed indirect solar out of the atmosphere (associated with the portion of the planetary albedo not due to reflection of incoming solar radiation off the sky) to deprive the surface of incoming flux that was not initially reflected back to space off of the sky.

The above derivation of surface temperature as a function of surface and sky albedo

and atmospheric absorptivities clarifies the comparative surface warming and cooling effects of the respective absorption of diffuse and direct solar radiation in our globally averaged (nonconvective) atmosphere. Diffuse solar absorption diminishes the contributions of surface reflections to outgoing solar transmissions (and hence to planetary albedo) by a factor of A_D . But of the energy it traps, only 50% is made available to the surface through downward emissions. The other 50% is radiated to space. Thus the absorption of indirect solar has an $\frac{A_T}{2}$ warming effect. When multiple reflections (and emissions) are considered, the last equation above shows that diffuse solar absorption contributes $[\frac{(1-\alpha_S-A_S)\alpha_E\frac{A_D}{2}}{(1-\frac{A_T}{2})(1-\alpha_E\alpha_S)}]S_{av}$ to surface temperature, where $\frac{(1-\alpha_S-A_S)\alpha_E\frac{A_D}{2}S_{av}}{1-\alpha_S\alpha_E}$ is the downward IR emission and the $1-\frac{A_T}{2}$ greenhouse denominator amplifies the thermal consequences for the surface. This effect is to be compared with the $\frac{A_S}{2}S_{av}$ cooling effect due to direct solar absorption and the resulting spaceward IR emissions.

The net surface cooling due to the total absorption of solar radiation in the atmosphere is therefore given by the difference between the $\frac{A_S S_{av}}{2}$ cooling effect and the net surface heating due to reflected solar radiation absorption in the atmosphere: $(\frac{1}{2}S_{av})[A_S - \frac{(1-\alpha_S-A_S)\alpha_E A_D}{1-\alpha_S\alpha_E}]$.⁴⁷ Such a net cooling effect of solar radiation absorbed in the atmosphere is associated, for example, with nuclear winter scenarios and with volcanically derived sulfuric acid aerosols. We have seen here, however, that the extent of these effects is diminished by the absorption of indirect solar radiation.

We point out these complexities only to dramatize that neglected considerations of the multiple significance of certain processes can lead to inaccurate results. In climate modeling it is important to look at the interactions among feedbacks, as well as to consider them individually. Planetary albedo and the solar absorption cooling are not independent.

The greenhouse effect is perhaps easier to understand. No matter what the sources of solar radiation that the surface receives, the radiative response of the surface (under the assumption of radiative equilibrium) results in the equal upward emission of longwave radiation. A fraction of these upward emissions is absorbed in the atmosphere, one half of which (for the purposes of the present two-layer model) is radiated back downward where it is 100% absorbed at the earth's surface, resulting in an equal upward longwave

⁴⁷Note that we are neglecting the vertical convective transport of latent heat, and are therefore introducing error into the surface energy balance, as noted above. We will review below how climate modelers integrate radiative and convective processes in radiative-convective models.

emission, a fraction of which is absorbed in the atmosphere, and so forth. If F_o in direct and indirect solar flux is initially received, surface emissions approach an equilibrium given by: $F_o[1 + \frac{A_T}{2} + \frac{A_T^2}{2} + \frac{A_T^3}{2} + \dots] = \frac{F_o}{1 - \frac{A_T}{2}}$, where A_T is the fraction of longwave radiation absorbed by the atmosphere, as considered above. This may help to explain the *greenhouse denominator* encountered above. Note that the maximal greenhouse effect for 100% IR absorption (as on Venus) is a doubling of surface emissions. Absolute temperature would, of course, respond with a fourth root of 2 effect. Note the important difference between the warming effect of the greenhouse and the probable net cooling effect of solar radiation absorption in the atmosphere. This distinction is important, as certain contributions to the greenhouse effect, such as those of carbon dioxide, make no contributions to the surface cooling due to incoming solar absorption in the atmosphere (carbon dioxide largely absorbs longwave), while others, such as the contributions of water vapor, clouds, and ozone, absorb both incoming solar and outgoing IR (ozone absorbs solar radiation in the UV range, while clouds and water vapor are reflective as well as absorptive of solar radiation).

Note that we have taken sky and earth albedos, direct and indirect solar absorptivities, and longwave absorptivity as independent of one another and independent of temperature. In practice, such assumptions do not reflect the complex interactions in the climate system. A warming, for example, would increase absolute humidity and possibly the extent, distribution, and reflective and absorptive properties of clouds, the extent of the planet's seasonal sea ice and snow cover, and over sufficiently long periods, the extent of its ice sheets; and there is evidence that a warming (whether or not it is initially caused by carbon dioxide increases) may often increase atmospheric levels of carbon dioxide (possibly due to release by the oceans and other carbon cycle effects reviewed below). These effects would in turn affect albedo (especially by changes in the reflection of solar radiation off of cloud tops and off the planet's snow and ice cover), the degree of the planet's greenhouse (especially through IR absorption by humidity, clouds, and possibly carbon dioxide), and the absorption of incoming solar radiation in the atmosphere (especially by water vapor and clouds). Moreover, the extent to which any of these changes occur will generally depend on processes not considered under the assumption of radiative equilibrium, including, for example, the vertical convection that redistributes atmospheric heat from low to high altitudes and the horizontal circulation of the atmosphere and oceans that redistributes heat from low to high latitudes and between the continents and seas.

In horizontally resolved models, it is furthermore necessary to distinguish among the albedos at different horizontal locations on the surface and in the atmosphere,⁴⁸ and therefore it is necessary to have some basis for calculating changes in snow and ice cover, in the distribution and optical properties of clouds, and possibly for calculating climatically induced changes in vegetation that could significantly alter albedo. The care with which such details are treated can make significant differences in the outcomes of model simulations and predictions. Over periods longer than decades, the deep circulation of the oceans and its interactions with the upper mixed layer may contribute to significant changes in the heat stored by the oceans, which in turn would remove heat from or introduce heat to the atmosphere in ways that would have further feedbacks on the processes that cause radiative exchange. It therefore should be understood that the above radiation balance considerations are most helpful in estimating globally averaged radiative processes for short (say interannual) periods for which we can presumably neglect the feedback effects of altered circulation patterns and changes in planetary heat storage on the properties of the atmosphere and surface that determine values of their albedos and the shortwave and longwave absorptivity of the atmosphere.

It will be observed that the partition between radiative processes on the one hand and dynamical processes and long-term thermodynamic processes associated with changes in heat storage on the other hand amounts to what we have described above as a near-decomposition. Indeed, it is a total decomposition, since dynamical and storage processes are neglected in radiation models, so that not even slow feedbacks can be represented. This is appropriate for educational purposes, and for first order approximations of the behavior of the system in response to stipulated changes in solar radiation, albedo, or the absorption of solar or terrestrial radiation in the atmosphere. To make more fruitful predictions when several of these change at once, it is necessary to connect these factors with each other, either through presumed relationships of dependence of the three optical indexes on surface temperature, or through modeling their interactions with dynamical processes that redistribute the mass and heat of the atmosphere and possibly with processes of heat, mass, and momentum exchange between the climate subsystems. Indeed, for any climate

⁴⁸This is not only because albedos vary horizontally with snow and ice cover and cloudiness (and additionally with land-sea contrasts and with vegetation) but also because the seasonal angle at which the sun's radiation falls on the surface (the zenith angle) is different at different latitudes, resulting in greater effective albedos at high altitudes.

change of significance, predictions of greater detail would in any case be of interest.⁴⁹ We explore some of these atmospheric processes below.

The absorption of incoming solar radiation by atmospheric components and by the earth's surface results in heating, the emission of longwave (infrared) radiation, and the continued reabsorption, heating, and emission of radiation until, in principle, radiative equilibrium is reached to yield vertical distributions of temperature (and pressure) that force atmospheric motions.⁵⁰ The thermal effects of radiative processes drive processes of vertical convection, as well as the horizontal circulation of the atmosphere; and along with salinity contrasts, they drive the circulation of the deep oceans.⁵¹ Small-scale vertical convection in the atmosphere, as well as vertical transports of heat by cyclones and other transient disturbances, serve to transport heat from the relatively hot surface to higher altitudes. The large-scale circulation of the atmosphere works together with ocean circulations to redistribute heat from the warmer tropics and subtropics toward the poles. And land-sea circulations redistribute heat seasonally between thermally contrasting oceans and continents. Heat is stored by the oceans, and in latent form (with the potential to release heat during the formation of clouds and ice), respectively by humidity and water.

A spectrum of scales of distinguishable forms of motion contribute to the redistribution of heat by atmospheric (as well as oceanic) motions. In the atmosphere, microscale (mm to 100 m) turbulent diffusion mediates evaporation and heat exchanges (as well as momentum exchanges) in the boundary layer at sea and land surfaces in time scales of minutes. Small-scale (100 m to 10 km) convection—which transfers heat (sensible heat) and water vapor (latent heat) to higher altitudes if the vertical temperature gradient is sufficiently steep—has a relaxation time of minutes to an hour.⁵² Mesoscale (intermediate spatial scales of tens to

⁴⁹There are numerous discussions of climatically significant feedback processes in the climate modeling literature. Among the earliest analyses of feedback mechanisms include those of Gal-Chen and Schneider (1975), Cess (1976), Coakley (1977), and Ramanathan (1977).

⁵⁰At particular locations (within latitude bands, columns of air, etc.) transports of mass and heat by atmospheric motions continuously change the vertical distributions of mass and heat, so that radiative transfer and vertical motion actually have interactive effects on the vertical distribution of heat in the atmosphere. We speak loosely, therefore, of the “vertical distributions of temperature (and pressure) that force atmospheric motions.”

⁵¹Technically, the three forces that accelerate atmospheric and oceanic motions are pressure gradients (differences in force per unit area, sometimes easiest to interpret as transfers of momentum, as in the forcing of surface ocean currents by winds, or as diffusive processes, as in the turbulent transfer of heat near the surface), the coriolis force (the apparent force associated with the spin of the earth), and the force of gravity (as in the effects of density gradients associated with temperature and salinity gradients on the deep circulation of the oceans).

⁵²Sensible heat is transferred vertically through the expansion of each parcel of warm air as it in principle

hundreds of km) phenomena, such as thunderstorm complexes and land-sea and mountain-valley breezes, synoptic scale (100 to 1000 km) anticyclones, depressions, and tropical storms, and large-scale (1000 km to 10,000 km) planetary waves, Hadley cells, and subtropical cyclones and anticyclones contribute significantly to vertical as well as horizontal transports of heat over their respective life-times of hours, days to weeks, and weeks to decades.⁵³ In the midlatitudes, subtropical cyclones dominate small-scale convection in transports of heat from the surface to higher altitudes.

When air rises in association with any of the above scales of vertical motion (horizontal motions will be considered separately below), it cools with increasing altitude, and if sufficiently cooled to saturation with the water vapor it contains (and condensation nuclei are available), releases latent heat as water vapor condenses into clouds. In the tropics, the transport of latent heat from the surface to high altitudes far exceeds the transport of sensible heat. Latent heat released in the tropics is essential in driving the Hadley cell circulations toward the poles at high altitudes, as discussed below. The vertical temperature distributions resulting from vertical convection and cloud formation cause continuous radiative exchange toward radiative equilibrium. When air is not entrained in larger-scale motions, small-scale convection and radiative exchange interact until, in principle, radiative-convective equilibrium is achieved in which there is radiative equilibrium at a vertical temperature gradient which is no longer sufficiently steep to cause small-scale convection.

Radiative-convective processes can be modeled as a functions of latitude and longitude when incorporated into general circulation models.⁵⁴ Convective adjustments (which restore

ascends adiabatically (without exchanging heat with its environment) to higher altitudes until its density is equal to that of its environment. The heat it had absorbed at the surface is expended in the work of expansion, but it is theoretically returned when the atmosphere around it does the work of compression to return it to a lower altitude, as when horizontal transports at high altitudes eventually descend to lower altitudes. Latent heat, on the other hand, is released at high altitudes when the unsaturated air from lower altitudes cools with increasing altitude sufficiently to saturate it, and cloud nuclei (such as sulfuric acid aerosols) are available for the condensation of water droplets. The heat released is partly radiated to space, partly radiated back to the surface, partly carried by horizontal atmospheric motions to different locations, and partly transformed into the kinetic energy of atmospheric motions (eventually to be dissipated as heat).

⁵³Synoptic scales—or the scales of organized systems—are sometimes defined more inclusively to embrace both the 100 to 1000 km scales of tropical storms and the large scales of extratropical cyclones and anticyclones (1000 km to 10,000 km) and planetary waves (approximately 10,000 km to 40,000 km (the circumference of the earth)).

⁵⁴See the classic radiative-convective model of Manabe and Strickler (1964) that has been incorporated into numerous general circulation models to provide thermal inputs to their larger scale dynamical calculations (which in turn provide inputs to the radiative-convective model). A more recent review of radiative-convective models may be found in Ramanathan and Coakley (1978).

a critical lapse rate⁵⁵ if vertical temperature gradients make the atmosphere unstable to convection)⁵⁶ provide the larger-scale motions resolved by the model with vertical temperature distributions as inputs, and horizontal and vertical motions resolved by the model—as well as any heat exchange with other climate subsystems—provide atmospheric heat inputs to the radiative-convective model. The radiative component of the radiative-convective model calculates the reflection, absorption, and transmission of incoming solar radiation and the emission, absorption, and transmission of longwave radiation by the atmosphere, oceans, land, cryosphere, and biosphere, to the extent they are represented in the model; any changes in ocean heat storage associated with the exchange of radiative fluxes between the atmosphere and oceans; the release of latent heat in the condensation of clouds and the removal of latent heat of fusion by melting snow and ice; and the longwave emissions to space (outgoing longwave radiation) at the top of the atmosphere. Any resulting vertical temperature gradients steeper than the presumed critical gradient are switched to the critical gradient (i.e., the critical lapse rate) in the convective adjustment to represent the effects of small-scale convection in restoring vertical stability. The resulting vertical temperatures provide inputs to the explicit dynamical component of the model.

It will be observed that radiative processes and small-scale convection are *nearly decomposed* in radiative-convective models in that each is allowed to reach equilibrium (or brought instantly to equilibrium) independently of interactions with the other. Likewise, small-scale convection is nearly decomposed from larger-scale atmospheric motions in that it is only the spatially and temporally averaged or integrated outcome (a subcritical lapse rate that is left unchanged or a critical lapse rate) that provides the inputs to the model dynamics at resolved scales, and only the thermal and mass distribution effects of resolved atmospheric motions considered on longer time scales (or changes in sea surface temperature on still longer time scales) that provide feedback to the radiative-convective model.

From another point of view, much of the contribution of atmospheric motions to the *horizontal* transport of heat is achieved through scales of motion larger than small-scale convection. Such disturbances generally distribute heat vertically as well as horizontally, so that they are not responses, properly speaking, to more locally defined convective

⁵⁵The lapse rate is the negative (or absolute value) of the vertical temperature gradient (change of temperature with altitude), making the critical lapse rate positive.

⁵⁶Note the implicit release of latent heat during cloud formation when the moist adiabatic lapse rate is employed for the convective adjustment. The critical lapse rate (above which a column of air is unstable to convection) is typically 3 to 4 K/km for moist air, as compared with 10 K/km for dry air.

adjustments. Larger-scale motions are *organized* at larger scales than small-scale convection. Given that large-scale subtropical cyclones contribute much more to vertical heat transports in the midlatitudes (on the average characterized by descending air or high pressure zones, deserts, etc.) than small-scale convection (Stone and Carlson 1979), it is natural to wonder whether models could calculate vertical radiative exchanges as responses to transports of energy associated with the mean poleward circulation and its large-scale transient disturbances (as well as transports of heat by the oceans which make major contributions to surface temperatures), rather than using calculations of the radiative-convective adjustment to constrain the calculations of large-scale motions, as in most general circulation models.⁵⁷ Indeed, the former strategy was the basis of the first modern horizontally resolved energy balance model in the U.S. (Sellers 1969). While general circulation models generally employ the convective adjustment, horizontally resolved energy balance models⁵⁸ generally parameterize the infrared radiation leaving the top of the atmosphere, as well as horizontal transports of heat, as functions of surface temperatures.⁵⁹ The explanatory status of such parameterizations will be considered in the discussion of parameterization and explanation in climate modeling in Chapter V below.

In the present climate, the motions of the atmosphere are responsible for approximately 50% of the distribution of excess solar energy received at lower latitudes toward the poles. The ocean circulations, including the wind-driven circulation of the surface waters above the thermocline and the thermohaline (temperature and salinity gradient-driven, i.e., density gradient-driven) deep circulation, are responsible for the other 50%. The forces that drive these atmospheric and oceanic circulations are pressure differences, gravity (or density differences), and the coriolis force (a so-called apparent force associated with the spin of the earth).

The atmospheric circulation is organized—statistically speaking—in three large-scale convective cells in each hemisphere which are deflected seasonally toward the warmer

⁵⁷ Another alternative is to employ two critical lapse rates, the moist adiabatic for the tropics and a critical lapse rate for baroclinic adjustment associated with midlatitude baroclinic eddies (e.g., cyclones). This suggestion is motivated not only by empirically observed differences between low-latitude and midlatitude vertical convection, but also by theoretical considerations of the coupling of vertical and horizontal processes. See Stone and Carlson (1979).

⁵⁸ Both pure radiation models (considered above) and radiative-convective models are energy balance models, but lack horizontal resolution.

⁵⁹ This provides constraints on albedo and greenhouse feedbacks that allow them to change with temperature; some such constraints are needed for mathematical closure, as explained above.

hemisphere. The tropical Hadley cells are driven, to a great extent, by the latent heat released at high altitudes by vertically expanding air near the equator, which spills downward and northward or southward to the horse latitudes (30 degrees north or south), descends, and returns along the surface toward the equator and the lower pressures associated with the rising arms of the tropical Hadley cells. The two Ferrel cells are most difficult to discern in the midlatitudes, where transient large-scale motions dominate the mean flow. In the two polar Hadley cells, polar air moves downward due to its density and moves toward approximately 60 degrees north and south where it rises, releases latent heat, and returns to the poles. The Ferrel cells link the subsiding air of the tropical Hadley cells with the rising air of the polar Hadley cells. All of these circulations are deflected by the coriolis force to form the westerlies, trade winds, and the jet streams aloft, and are both reinforced and perturbed by smaller-scale storms, pressure systems, and eddies that travel along their paths. These large-scale circulations, and the smaller-scale disturbances with which they mutually interact, are responsible, along with the ocean circulations, for the efficiency with which earth transports heat from the tropics and subtropics to the poles. This efficiency is dramatized by the contrast between the much greater solar flux received in low as compared with high latitudes, and the much more equally distributed emission of terrestrial radiation across latitudes, as measured by satellite. The atmospheric circulation is also responsible, through seasonal land-sea circulations that make both summertime and wintertime continental temperatures more moderate than they would be otherwise, for redistributing heat between the continents and oceans. (See the discussion of the oceans below.)

Many different scales of motion interact to produce the atmospheric circulation. As suggested in Chapter III, larger-scale flows break down into smaller and smaller-scale motions, and are eventually dissipated through surface friction. The resulting surface heat is convected and radiated upward, to some extent reabsorbed by humidity, clouds, ozone, carbon dioxide and other greenhouse gases, to some extent reabsorbed and reemitted by the earth's surface, and eventually radiated back to space. At the same time, smaller-scale vortices (created by the relative motion of the atmosphere and the earth's spin) have a tendency to aggregate into larger eddies, storms, and pressure systems, and contribute to the large-scale atmospheric circulation. Mass, energy, and momentum are transferred from larger to smaller scales and from smaller to larger scales of atmospheric motion, complicating efforts to calculate larger-scale outcomes of smaller-scale motions. A case in point is so-called

blocking patterns.

Blocking patterns are stable configurations of transient eddies that can change the mean flow around them. They are one candidate for climatic change mechanisms that originate within the atmosphere, as distinguished from those forced by other climate subsystems or caused by external (extraterrestrial) forcings, although they are insufficiently understood to determine whether they have one or several causes.⁶⁰ One type of blocking pattern involves the coupling of a high latitude anticyclone with a lower latitude cyclone. (The latter are typically far more transient but can stay put for quite a few weeks if the conditions are right.) The fact that cyclones are not resolved in zonally averaged models (models that don't resolve longitude), yet contribute to the mean flow on a unique instance basis, has been one of the challenges of global modeling. Even in higher resolution models that resolve cyclones, these transient eddies occur differently in each of a number of experimental runs (on climate time scales) from realistically variable initial conditions.⁶¹ It is only simulations or predictions of the mean flow and the more stable quasi-stationary features of the general circulation in which we have confidence at the present time. Much more work needs to be done to understand the extent to which blocking patterns (and other anomalies) are the result of random fluctuations in high frequency atmospheric phenomena (which may mean that they can only be treated stochastically),⁶² or whether the circumstances under which they arise can be modeled, in principle if not in practice, from phenomena resolved by models. They represent one of the most interesting cases of dynamical interactional (interscale) complexity in the atmosphere. If blocking patterns do prove important in changes of atmospheric state, they may ultimately contribute to predictability rather than the other way around—if we can single them out for more resolved treatment. This amounts, of course, to developing an

⁶⁰Some blocking patterns may be caused by sea surface temperature anomalies rather than by factors internal to the atmosphere.

⁶¹For statistical analyses of the significance of multiple climate model runs, see Chervin (1978, 1980a, 1980b).

⁶²See Hasselmann (1976) for a discussion of the importance of treating low frequency atmospheric variability stochastically, Frankignoul and Hasselmann (1977) for applications of stochastic modeling methods to ocean variability, and Lempke (1977) for applications of stochastic methods to zonally averaged energy balance models. The three articles form a three-part series of considerable historical significance. More recent literature on stochastic modeling may be found, for example, in Hasselmann's (1981) review, in Section 2B of Berger and Nicolis (1984), and in Lempke (1986). From the point of view of our present concerns, stochastic models dramatize the importance of treating (in some fashion) the interactionally complex feedbacks from high frequency to low frequency phenomena. (Note that stochastic representations of unresolved phenomena that allow for their variability are very different from statistical representations that only employ averages.) See Charney and DeVore (1979) and Charney and Strauss (1980) for theoretical analyses of the possible contributions of smaller-scale processes to the production of blocking patterns through nonlinear resonance. An alternative theoretic approach (McWilliams 1980) involves the analysis of blocking patterns as modons. See the introductory review in Leith (1984).

interactionally complex model. See the discussions of the limits of predictability in Leith (1973, 1978a, 1978b, and 1983) and the review of current understanding of the causes of low-frequency atmospheric variability in Wallace and Blackmon (1983).

The interactional complexity associated with the variety of feedbacks and interlevel interactions in the atmosphere will be discussed in a more formal way in Chapter V in the context of an analysis of the explanatory value of parameterization. The couplings of the atmosphere with the other climate subsystems are explored immediately below.

4.2.2 The oceans

The atmosphere is interactively coupled to the oceans.⁶³ Winds and storms drive surface circulations and contribute to vertical mixing of the ocean mixed layer with consequences for sea surface temperature as well as for the deep circulation, and seasonal land-sea circulations (such as those that bring the Monsoons) and mesoscale land-sea breezes distribute heat between land and seas, and strongly couple sea surface temperatures and the weather and climate over the land. Evaporation from the sea surface (associated with sea surface temperature as well as winds) is important in the genesis of tropical storms, in the latent heat release which (along with evapotranspiration in tropical forests) drives the Hadley cells and transports heat toward the poles, and in humidity and cloudiness that contribute so much to the earth's albedo and greenhouse (and consequently to the surface temperatures of the seas).

The oceans account for about one half of the transport of heat from the tropics and subtropics to the poles. (The atmosphere carries the other half.) While the oceans lag months behind the atmosphere in their response to solar forcings, they have a longer memory, so to speak; their larger heat capacity allows the upper mixed layer—and on time scales longer than approximately one decade, the deep oceans—to accumulate surpluses or deficits of heat.⁶⁴ The upper mixed layer of the oceans is so-called because temperature changes at

⁶³See Gill (1982) for a comprehensive review of atmosphere-ocean dynamics. Introductory reviews of atmosphere-ocean interactions may be found in Perry and Walker (1977) and Woods (1984). For a discussion of atmosphere-ocean-ice interconnections as relevant to climate models, see the review on pp. 46-56 in Washington and Parkinson (1986). For an introduction to ocean climate modeling, see Bretherton (1982).

⁶⁴The atmosphere does not have a great heat capacity, so that unless its general circulation can assume alternative steady states in response to the same boundary conditions, climatic change would either have

its surface are quickly distributed throughout the layer as a result of the action of waves, wind, and thermal and salinity gradient-driven convection. The mixed layer circulation is primarily horizontal, and is driven by winds. This stands in contrast with the deep circulation, called the thermohaline circulation because vertical gradients of temperature and salinity cause relatively cold and/or salty water to descend—in the present configuration of continents, primarily in the Weddell Sea and the North Atlantic—thereby driving the circulation. Water has a residence time of approximately 500 years in the Atlantic and 2000 years in the Pacific, which provides a measure of the life cycle of the deep circulation. The deep circulation exchanges heat with the upper mixed layer through upwelling at coastal sites caused by the divergence of the surface current flow from the shore due to winds and the coriolis force.

Feedbacks between the oceans and the atmosphere, as suggested above in the case of El Niño, are often complex. An increase in net radiation absorbed at the surface, for example, warms the mixed layer and results in increases in evaporation and humidity, and an associated increase in the humidity feedback that enhances the warming. (The effects on clouds may involve negative cloud albedo feedbacks and/or positive cloud greenhouse feedbacks, as suggested above.) Yet the mixed layer has a much greater heat capacity than the atmosphere, and serves to buffer climatic change.⁶⁵ The heat capacity of the mixed layer is determined by its depth. The depth of the mixed layer is determined both by temperature and salinity (warm or fresh water is less dense so has less of a tendency to mix downward and extend the depth of the mixed layer) and by winds (most dramatically by tropical storms). The mixed layer depth therefore varies seasonally, with latitude, with changes in surface wind stress, and with changes in net incoming radiation. The greater the depth of the mixed (isothermal) layer for a given heat content, the greater its heat capacity and the lower the surface temperature. Thus although warmings and coolings are buffered by the mixed layer heat capacity, this effect may be diminished by altered mixed layer depth for perhaps as many as ten years. Beyond this time, mixing with the deep water becomes

to renew itself each year through external forcings such as changes in solar output or orbital parameters, or accumulate from year to year through changes in the heat stored in the oceans and changes in the (latent) heat of fusion stored in the cryosphere. The climate system accumulates climatic changes which it can amplify through positive feedbacks, even as it delays climatic change due to the thermal inertia of the oceans and cryosphere.

⁶⁵The importance of including mixed layer models in calculations of the transient (time dependent) response to atmospheric carbon dioxide increases has been explored, for example, by Schneider and Thompson (1981) and Thompson and Schneider (1982).

relevant (Harvey and Schneider 1985a).⁶⁶

Another factor that could diminish the negative feedback of the mixed layer heat capacity, again, on interannual time scales, is the effects of winds (e.g., those associated with Monsoons) that redistribute heat between oceans and the continents, thereby diminishing their temperature contrasts. Such effects would be the greatest in the season that enhances the warming or cooling trend, and could considerably retard the short-term response of the climate system to radiative forcings (Harvey and Schneider 1985b). For periods beyond 10 years, the far greater heat capacity of the deep oceans comes into play, damping climate trends (through diffusive mixing with the mixed layer as well as upwelling and downwelling) and providing the climate system with thermal stability on 10 to 1000 year time scales.⁶⁷ At the same time, the ocean is the greatest reservoir of carbon dioxide, and the solubility of carbon dioxide (as everyone knows from observing the behavior of soda pop) depends on temperature. Thus the upwelling of warm, saline circumpolar deep water in the Weddell Sea,⁶⁸ which accounts for most of the ocean discharges of carbon dioxide to the atmosphere in the current configuration of continents, conceivably could amplify climatic change through its control of the carbon dioxide greenhouse. The extent of such effects are unknown, but there is growing evidence that carbon dioxide feedbacks have often been associated with glaciation and deglaciation.⁶⁹

The ocean is responsible for many other processes that may affect climate. The strength of the deep circulation, for example, in the North Atlantic is derived, in part, from the salinity of water remaining after seasonal sea ice formation, and so may be sensitive both to the inflow of freshwater from river runoff and the inflow of highly saline waters from sequestered seas. Changes in the strength of the deep circulation could cause rapid adjustments in the meridional (poleward) atmospheric circulation. (Feedbacks involving the cryosphere are considered below.) Small-scale eddies in the oceans may be long-lived enough

⁶⁶Similar considerations may be found in Hansen et al. (1985).

⁶⁷The turnover time of the deep oceans is 500 to 2000 years.

⁶⁸The circumpolar deep water, which carries heat and salt derived (by diffusion) from surface waters in the tropical and subtropical oceans, circulates in an intermediate layer between the Antarctic intermediate and bottom waters. Its upwelling in the Weddell Sea (Southern Ocean around Antarctica) is incompletely understood, but there is evidence that Polynyas, or openings in the sea ice, expose the Antarctic surface water to cold Antarctic temperatures and create the temperature gradients that drive the upwelling of the circumpolar deep water. See the review in Gordon and Comiso (1988) and the discussion earlier in this chapter above.

⁶⁹See Kerr (1984) for a review of recent findings regarding the role of carbon dioxide in ice ages and Nance et al. (1988) for a discussion in the context of ice ages and the continental drift cycle.

to trigger vertical mixing events across hundreds to thousands of kilometers (McWilliams 1985), suggesting one of the many ways that interscale interactional complexity plays an important role in the oceans. The oceans provide the warm water important to coral reefs, the upwelling cold waters that support marine ecosystems and fisheries, and the dissolved oxygen and nutrients that make marine life possible. Some of the feedback effects of marine life on climate are considered below.

4.2.3 The cryosphere

The cryosphere⁷⁰ affects climate directly, in that snow or ice cover is a climatic variable along with temperature, humidity, cloudiness, wind speed and direction, and storm activity. Indirectly, changes in the extent of continental ice sheets, seasonal snow cover, and sea ice have a feedback effect on warming and cooling trends. Increases (or decreases) in the surface area of the cryosphere increase (or decrease) the reflectivity or albedo of the earth's surface, thus amplifying cooling (or warming) trends through positive feedback.⁷¹ Seasonal sea ice increases ocean albedo, thermally isolates the sea beneath, releases saline waters that—together with polar cooling of surface water—drive the deep circulation, and can block the flow of upper mixed layer currents.⁷²

Glaciation and ice sheet growth remove water from the oceans. This decrease in ocean mass has numerous climatically significant consequences. These include changes in the rate of sediment formation (see discussion of lithosphere below); in ocean salinity and its effects on sea ice formation and the deep circulation;⁷³ in the temperature contrasts between low and high latitudes that drive the deep poleward circulations of both the atmosphere and (together with salinity contrasts) the deep oceans; and in the length of day as the spin of the planet adjusts to the vertical redistribution of water mass. The weight of ice sheets can cause subsidence of continents which effectively raises sea level, partly compensating for the

⁷⁰For an introductory discussion of the cryosphere, see Untersteiner (1984).

⁷¹This feedback effect of cryosphere extent on climate trends was dramatized by the first modern energy balance models of Budyko (1969) and Sellers (1969).

⁷²See the recent anthology on the geophysics of sea ice edited by Untersteiner (1986) and the discussion of sea ice as it relates to climate models in Washington and Parkinson (1986).

⁷³See the discussion in Schneider et al. (1987a).

sea level decreases due to loss of mass.⁷⁴ This could cause an end to ice ages if regional effects outweigh the global ones, and sea level increase surrounding a continent causes the ungrounding of its ice sheets. Glacial surge mechanisms and ungroundings may have general importance in the rapid transitions to interglacial climates and associated increases in sea level. It has also been suggested that glacial surges may, by cooling the seas rapidly both directly, and indirectly through sea ice formation, drive the climate into ice ages.⁷⁵

4.2.4 The biosphere

The biosphere contributes to climate so strongly that many have wondered whether the biosphere as a whole has somehow adapted to create and maintain the atmospheric conditions that it needs to survive. While theories of Gaian self-organization remain highly speculative,⁷⁶ it is fairly certain that photosynthesizing microbes (blue-green algae and cyanobacteria) were responsible for dramatic increases in atmospheric oxygen and decreases in atmospheric carbon dioxide billions of years ago,⁷⁷ and it is well-known that photosynthesizing forests are major sources of atmospheric oxygen and sinks of atmospheric

⁷⁴The direct effect of ice sheet growth is to lower sea levels by removing water from the oceans. The weight of ice sheets on continents, however, causes them to subside into the seas, and the removal of ocean weight from the ocean crust may result in rebound effects, both of which contribute to relatively higher sea levels (Schneider and Londer 1984). The empirical evidence seems to suggest that the net effect of both factors is lower sea levels during ice ages. During ice ages that recent speculation suggests may occur in 440 million year cycles in connection with continental drift, there is some evidence that sea level is low due to the high elevation of minimally drifting, maximally separated continents which are subject to increasing pressure from mantle upwelling over time, and that such sea level effects may indeed contribute, in complicated ways, to the onset of the ice ages themselves (Nance et al. 1988).

⁷⁵See the classic proposal of the theory of glacial surges in Wilson (1964), the subsequent paper by Hollin (1964), and the review of Wilson's theory by Hollin (1965). See Haq et al. (1987) for a review of the magnetostratigraphic, chronostratigraphic, biostratigraphic, and sequence stratigraphic evidence for the numerous documented sea level changes since the Triassic. For discussions of sea level data in support of surges, see Aharon et al. (1980) and Winograd et al. (1988). Other empirical evidence for surges is discussed in Hollin (1969, 1977, and 1980). A review of early empirical and theoretical evidence for surges may be found in Hollin and Barry (1979). See Budd and McInnes (1975), Schilling and Hollin (1981), and Budd et al. (1984) for discussions of glacial surge mechanisms and modeling methods. See Kamb et al. (1985) for a report on a recent surge (1982-1983) of Variegated Glacier of Alaska. See Radok et al. (1987) for a recent review of the surge literature, a helpful bibliography, and skeptical conclusions about the likelihood of Antarctic surges. A general discussion of cryospheric responses to global warmings may be found in Barry (1979), and a general discussion of cryospheric variability as a contributing factor to climate may be found in Barry (1987). See Paterson (1981) for a standard glaciology text. For reviews of polar climate mechanisms and research needs, see National Research Council (1978, 1984), The Polar Group (1980), and the University Corporation for Atmospheric Research (1988). See Washburn and Weller (1986) for a discussion of the Arctic Research and Policy Act of 1984 and its implications for appropriate research priorities.

⁷⁶See Lovelock (1972), Lovelock and Lodge (1972), Margulis and Lovelock (1974, 1978), Lovelock and Margulis (1974), and Lovelock (1979, 1988) for the classic formulations of the Gaia hypothesis, as noted above.

⁷⁷See the helpful review in Cloud (1983).

carbon dioxide today. Whether, as some have argued (e.g., Lovelock and Margulis 1974), the warming of the early sun would have caused conditions inhospitable to life were it not for the uptake of carbon dioxide by the biosphere, we do not yet have enough evidence to say.⁷⁸

The biosphere alters surface winds and microscale turbulence, as well as surface albedo, surface heat capacity, and the resulting processes of vertical radiative exchange.⁷⁹ Vegetation breaks up the land and facilitates its erosion by rains and winds, and thereby accelerates the carbon cycle, and organic sediment is a major link in the carbon cycle, as explained below. Cess (1978) concluded from a comparison of vegetative albedo data for the recent ice age (18,000 years ago) and current surface albedos that biosphere-albedo feedback might double the sensitivity of surface temperatures to the causes of climatic change. This suggests that not only can portions of the biosphere be damaged or assisted by climatic change, but the biosphere's response can, in some circumstances, accelerate climatic change. Transpiration makes a large contribution, through evapotranspiration, to the levels of humidity, cloudiness, rainfall, and the transport of latent heat from the surface to high altitudes, and once again, to the resulting processes of vertical radiative exchange. We are only beginning to assess the climatic impact of contemporary rainforest destruction.

Charlson et al. (1987) have shown that the production of dimethylsulfide by algae may be an adaptation whereby mid-ocean clouds are provided the condensation nuclei (e.g., sulfuric acid particles from the oxidation of DMS) needed for clouds to form and cool the subtropical ocean climate.⁸⁰ in association with the large fluctuations in atmospheric carbon dioxide produced by seasonal photosynthesis. The Southern Hemisphere ice age of the late Carboniferous and early Permian Period beginning some 280 million years ago may have been precipitated by the great success of forests and their uptake of carbon, which diminished the greenhouse. The biosphere also contributes a significant portion of other greenhouse

⁷⁸For an alternative view on the possible importance of cloud-IR feedback for the early earth, see Rossow et al. (1982). Climatic conditions prior to the emergence of the biosphere also would have implications for the contribution of the early biosphere to the earth's climate. There is apparent conflict, however, between theoretical predictions of an ice-covered prebiotic earth and the empirical evidence which seems to indicate a liquid ocean. See the discussions in Henderson-Sellers (1983) and Holland (1984). For a comprehensive treatment of the chemical evolution of the oceans and atmosphere and for further clues as to the role life might have played in earth's history, see Holland (1984).

⁷⁹Radiation is also emitted sideways but the ultimate effects of horizontal processes are considered in the vertical radiation balance.

⁸⁰More recent evidence suggests that if DMS production by algae is altered in response to warmings at all, it may actually decrease, providing fewer condensation nuclei for cloud formation. If such a mechanism indeed exists, it may represent a maladaptation to climate change rather than the opposite (Warren 1989)!

gases, such as methane produced by termites and ruminants and chlorofluorocarbons produced by man. (The latter are powerful greenhouse gases as well as ozone depleters). And humans are agriculturally, transportationally, economically, medically, and recreationally influenced by climate, while their aerosols, fuels, nuclear wars, agriculture and rain forest destruction, dust, and pavement all have their impacts. Climate models themselves actually contribute to a form of interactional complexity in the climate system by inviting planet management feedback effects on climate based on the knowledge climate models provide.⁸¹

The interactions of climate and the biosphere occur on every time scale, and parameterizations of the influence of the biosphere on climate is one of the most important and uncertain components of general circulation models. See Sellers et al. (1986) for a recently developed biosphere model for use with GCMs. For the results of several workshops on biosphere-climate interactions, see University Corporation for Atmospheric Research (1985) and Rosenzweig and Dickinson (1986).

4.2.5 The lithosphere

On time scales of millions of years, continental drift plays an important role in climate. A full cycle of sea floor spreading, continental separation, and then subduction of ocean crust, the closing of the sea, and the aggregation of supercontinents is thought to go through one cycle in approximately 440 million years (Nance et al. 1988). Continents generally move about 10 km in 1 million years (India moves some 10 times faster), so variability in continental motions on these time scales could presumably affect the world climate through the opening or closing of seas. Recent evidence from the Ocean Drilling Project, for example, suggests that Baffin Bay and the Labrador Sea surrounding Greenland began opening up some 55 million years ago and stopped expanding by about 36 million years ago. Their formation allowed cold Arctic water to flow into the Atlantic, and undoubtedly had a major effect on climate.

Nance et al. (1988) have suggested that the widest spread of continents (as we seem

⁸¹See Budyko (1986) and Lovelock's new (1988) book on Gaia for a review of what is known of the interactions between the biosphere and its environment through evolutionary history, and Schneider and Londer (1984) for a popular review of interactions between the human component of the climate system and climate.

to have now) may trigger ice ages through feedback processes associated with sea level and the carbon cycle. Continents seem to settle over upwelling regions of the mantle, and the accumulation of heat and pressure is thought to cause stationary continents to rise out of the sea, effectively lowering sea level. Lowered sea level exposes the continental shelf to erosion and runoff, providing nourishment for marine life. Silicates transported by rivers to sea are oxidized to carbonates (e.g., limestone) by the dissolved carbon dioxide in the sea. Flourishing marine life also increases its deposition of carbonate shells. Both sources of limestone sediment accumulate, effectively removing carbon dioxide from the atmosphere and diminishing the greenhouse.⁸² The cooling could contribute to an ice age.

This process might be enhanced by ice sheet-mediated feedback, as discussed above. The following scenario is speculative, but may serve to dramatize the complexity of the interactions that could be involved. The growth of ice sheets lowers sea levels, exposing continental shelf to erosion and runoff.⁸³ The increased contrast between polar and tropical temperatures associated with ice sheet growth also enhances average winds and contributes to erosion (Nance et al. 1988)⁸⁴ and to wind-forced upwelling of deep water (and the associated enhancement of air-sea gas exchange) along continental shelves. Increased latitudinal temperature contrasts also increase the strength of the deep circulation, as does the increased salinity associated with the removal of fresh water to ice caps and sea ice. The intensified deep circulation works together with increased winds to increase deep water upwelling along continental shelves. The enhanced upwelling of deep water enriched in mineral content from increased runoff (resulting from the exposure of continental shelves and any increases in erosion) increases the mineral content in shelf water and sustains a greater mass of marine life, which in turn results in increased deposition of carbonate sediment. Carbon removal results in a diminished greenhouse and enhances the cooling. Such a mechanism embraces all the climate subsystems (including the mantle, as we shall see below), and something like it may well be important in amplifying initial glaciations in 100,000 year Milankovich ice ages as well as in the major glaciations that may be associated

⁸²Carbon in sediment is eventually recycled to the atmosphere through the ocean circulation, and over longer time scales, by subduction into the mantle and eventual outgasing in volcanos and through the mid-ocean rift.

⁸³See the discussion above of the competing effects of subsidence.

⁸⁴It has also been suggested that carbon dioxide losses from the atmosphere during periods of ice sheet growth (see below) might result in decreased carbonic acid levels in rain, decreased soil carbon dioxide, and as a result, decreased—rather than increased—erosion (Arthur 1982). Many feedbacks may be involved in climatic change, and they all do not go in the same direction. For a simplified accounting system of multiple positive and negative feedbacks to determine the net feedback direction, see Kellogg (1975).

with the cycle of continental drift. The myriad of feasible intersubsystem feedbacks—of which we have only mentioned a few—dramatizes how the near-decomposition of the climate system into subsystems for specialized study at different average time scales must eventually be reintegrated if we are to understand and model the most interesting cases of climatic change.

4.2.6 The mantle

If we include the mantle as a climate subsystem, it completes the carbon cycle we have implicitly woven through all of the subsystem discussions above.⁸⁵ Atmospheric carbon dioxide is removed from the atmosphere by rain, photosynthesis, the weathering of rocks, and uptake by the oceans; organic and inorganic carbon is removed from the land (along with silicates) by erosion and river transport to the seas, where, together with inorganic limestone deposits derived from the oxidation of silicates and organic limestone and carbonaceous sediment derived from marine life, it is stored in the rock and ooze at the ocean bottom; and carbon deposits are either dissolved in the oceans and recycled back to the atmosphere through the deep circulation and upwelling or diffusion into the mixed layer, or subducted (with ocean crust) into the mantle and recycled back to the atmosphere through volcanic emissions, outgasing from the mid-ocean rift, and mineral springs. Since the removal of limestone and carbonaceous sediment into the mantle along with the subduction of ocean crust makes the carbon unavailable for recycling to the atmosphere through the seas, changes in the rates of subduction certainly would affect the climate, presumably on the time scales of continental drift. Thus there would seem to be a feedback between climate and the rate of carbon recycling through the mantle.

Volcanic activity, as indicated above, may also vary on time scales of millions of years, with potentially profound effects on climate associated with aerosol precursors and dust. The possible importance of volcanic dust in climate was first explored quantitatively by climate modelers in Schneider and Mass (1975), whose methods provided a foundation for further modeling studies of the climatic impacts of volcanos. Recently, it has become clear that the formation of sulfuric acid aerosols in the stratosphere from precursor gases emitted by volcanos can result not only in increased albedo with cooling consequences, but also

⁸⁵For reviews of the implications of the carbon cycle for climate, see Revelle (1982) and Arthur (1982).

in the trapping of IR radiation emitted at lower altitudes, heating the stratosphere and cooling the surface, as has been demonstrated in the case of a 1982 eruption of El Chichón (Ramanathan 1988). In the past few years, the possible contribution of unusual volcanic activity to the mass extinctions of 65 million years ago has been among the numerous explanations considered. Although volcanic activity does vary significantly on 10,000 year time scales, we do not know whether it can impact the climate on these time scales. The carbon dioxide emissions of volcanos may also contribute to climate. As noted above, however, variations in the volcanic outgasing of CO₂ occur over 10,000 years, but the levels are low enough to contribute to climatically significant atmospheric levels only over several million years. (See the discussion in Arthur 1982). Over the longest time scales the volcanic emission of water has been the likely source of our oceans, with possible supplementary contributions from the newly discovered phenomenon of ice comets.

Although the mantle flows in two layers of convective cells to depths of 2900 km at the core-mantle boundary, it is a hot solid, so its flow is technically called *creep*. Its circulation determines the irregular motions of continents, which, as we have illustrated above in the case of the sudden opening and closing of Baffin Bay and the Labrador Sea, may result in dramatic climate changes associated with changes in the circulation of the seas in time scales of millions of years. Mantle circulation equally determines the subduction of sea crust into the mantle, along with carbon sediment that it sequesters for millions of years and recycles through volcanic emissions of carbon dioxide (as well as methane).

Since the consequences of mantle processes are so important in climatically significant continental drift and volcanic activity, it is important that we make progress in modeling its activity. It is known that the mantle is not undisturbed by the lithosphere. As noted above, there is substantial evidence that the oldest continental cores penetrate deeply into the mantle rather than riding on top (Jordon 1979). Recent evidence also points to a recycling of subducted ocean crust through its deep descent into the lower mantle and its return to the lithosphere via the convective upwelling of hot plumes of crustal remains through the lower and upper mantle and out hot spot volcanos (Courtilot and Besse 1987). The ocean crust cycle undoubtedly affects the upper mantle circulation and its consequences for continental drift.

The cryosphere may also have feedback effects on mantle process. The reciprocally

related weights of ice sheets and oceans cause a seesaw effect of pressure on the mantle due to subsidence as water mass shifts between the cryosphere and oceans in the glacial-interglacial cycle. Redistributions of pressure over the mantle and associated redistributions of heat at the top of the mantle circulation conceivably could alter mantle circulation and affect tectonic processes. Increased weight over the ocean crust, whether due to increases in the mass of an ocean or the mass of sediment at its bottom, conceivably could alter the thermal forcings of the circulation of the upper mantle. Our study of the mantle, and the possible feedbacks from climate subsystems above the surface, is (so to speak) in a state of rapid and exciting flux.

With all the mutual interactions between and within climate subsystems that we have just begun to suggest, the reasons climate modelers cannot model everything at once should be clear.⁸⁶ Indeed, climate modelers have to be quite selective, as limitations in computing and economic resources, observational opportunities, knowledge, and theoretical predictability preclude master models that consider all climatically significant processes. What is included in a model will depend upon the physical, biological, spatial, and temporal details of interest and the relevance of particular processes to the climatic conditions defined over the time scales and at the historical times of interest. In many cases, the climate problem will allow for simplifying assumptions that amount to decompositions by time scale, but in many other cases perceived feedbacks, whether from generally slower subsystems to faster, or unresolved processes to resolved processes, will motivate treatments—often interdisciplinary treatments—of the interactional complexity involved.

What types of process can be held fixed, which should be treated parametrically, and which must be modeled explicitly? We consider in the next chapter how climate modelers decide what couplings and what processes to parameterize or hold constant, and which to model explicitly; and how climate models can provide adequate explanations, despite the acknowledged complexity of the climate system and the simplifications necessary for practical modeling.

⁸⁶See Figure 2 of Bretherton (1986) for an illustration of many of the complex interactions that contribute to climate, and Schneider (1987b) on the inappropriateness of master models.

Chapter 5

Interconnectedness in Global Science

The interconnectedness of the processes that contribute to the earth's climate—whether within the climate subsystems or between and among them—has been reviewed in Part I of *Gaia Revisited* above. As limited as this review has been in detail, it perhaps has been suggestive of the great multiplicity of processes that contribute to the earth's climate across many spatial and temporal scales, and of how difficult it often is for scientific specialists to model the contributions to climate of specific phenomena at characteristic spatial and/or temporal scales independently of interdependent phenomena generally studied in other areas of science. Indeed, in recent years a growing number of researchers in climatology, climate modeling, and related disciplines have recognized the imperative of communicating closely with other disciplines; of developing global data management programs that facilitate the exchange of data and model outputs among global science disciplines; of exchanging such data and model outputs more efficiently in highly coordinated multidisciplinary programs; of integrating the results of many contributing disciplines in global system models that exploit the most sophisticated global monitoring and data management programs, computing capabilities, interinstitutional communications, and mathematical modeling methods; and of conducting cooperative interdisciplinary research to study and model the interactive processes that cross the boundaries of specialized disciplines.

The major United States agencies involved in the funding of the biogeosciences—e.g., the National Science Foundation (NSF), the National Oceanic and Atmospheric Administration

(NOAA), and the National Aeronautics and Space Administration (NASA)—have responded to the growing need for cooperative global research efforts through the development of a variety of interagency programs that are unprecedented in their focus on multidisciplinary problems, their coordination of contributing disciplines, and their planetary and—if we consider the importance of remote sensing by satellite, the study of the sun, and comparative climatology—interplanetary scope. The most visible of these, presently in the planning stage, is the Earth System Science Program as proposed by the Earth System Sciences Committee of the NASA Advisory Council in 1986.¹ At an international level, the Ad Hoc Planning Group on Global Change of the International Council of Scientific Unions (ICSU 1986) has proposed what is undoubtedly the most ambitious international global research program ever conceived: *The International Geosphere-Biosphere Programme: A Study of Global Change*. Its proposed coordination of disciplines in participating developed and developing countries, to begin in the 1990s and continue into the next century, embraces process studies, monitoring programs, global system modeling, the study and simulation of earth's history, and the development of a global data and communication system. The latter global data and communication efforts are already under way at NOAA's National Geophysical Data Center and World Data Center (Kineman, Hastings, and Colby 1986; Kineman and Clark 1987).

Occasionally the new global research vision is expressed in terms of *Gaia*: To whatever extent the biosphere regulates the climate and biogeochemical cycles, planetary biogeochemical processes are certainly highly interconnected; and as science progresses in modeling this interconnectedness, our growing ability to manage our global environment can be thought to give birth to Gaia, even if she was only potential or embryonic before man. (See, for example, the writings of Kineman et al. cited above.)

Others are more skeptical, not only about Gaia, but about the ability of large, bureaucratically managed, programs—at least as they are presently conceived—to foster the development of interdisciplinary models of the greatest scientific value. System science—however sophisticated its computing, communications, and observational resources—is not an adequate substitute for the scientific achievements only won by the integration of theory and data in research at the leading edge of science. Traditional scientific skeptics

¹See the report of the Committee published in 1988. The report, and additional literature and information, are available at the Office for Interdisciplinary Earth Studies, University Corporation for Atmospheric Research, P.O. Box 3000, Boulder, CO 80307.

recognize only the role of specialized research in the most valuable scientific achievements. An alternative criticism of big multidisciplinary science, justified perhaps only in view of the hard-earned interdisciplinary progress of the past decade, is that multidisciplinary progress, even if it enjoys the blessings of system science expertise in the transdisciplinary coupling of specialized outputs with inputs in the development of master models, may still fall short of interdisciplinary success in understanding and modeling processes that cross disciplinary boundaries. Multidisciplinary is not interdisciplinary. There is no substitute for the scientists who learn enough of each other's fields to work together in integrated research efforts that are designed to model and explain the phenomena that involve mutual interactions across conventional disciplinary domains of concern.²

From the perspective of the present (interdisciplinary) effort to explore the mutual relevance of the philosophy of science and climate modeling, we need every level—or type—of research, ranging from the most specialized empirical and theoretical studies, to interdisciplinary communication, to multidisciplinary coordination, to interdisciplinary research, to the emergence of a new—transdisciplinary—discipline. One challenge for the philosophy of science in contributing to such research efforts is to help to distinguish among the various forms of research, to analyze the methodologies which may be involved, to suggest ways of evaluating research priorities and of assessing the appropriateness of pursuing one or another type of research at the various crossroads of scientific progress.

The particular challenge of the present study has been to distinguish—in the light of empirical evidence from climatological and related sciences—between multidisciplinary and interdisciplinary research in the study of complex systems, and to explore the relevance of such philosophical insight to emerging research challenges in climate modeling. We will outline below how considerations of causation and explanation are relevant to whether multidisciplinary or interdisciplinary research is appropriate in climate modeling, and how the methodology developed by scientists originally involved in modeling the atmosphere and/or oceans is sufficiently general to provide a basis for the integration of participating disciplines in the interdisciplinary development of global climate system models.

²An editorial reminder that multidisciplinary efforts cannot replace bona fide interdisciplinary focus in the global change movement may be found in Schneider (1987b).

5.1 Sensitivity and Feedback

Two very different approaches to climate modeling have developed—relatively independently of one another—during the past several decades. These two climate modeling traditions may be distinguished from each other by whether or not atmospheric and oceanic motions are explicitly calculated through an application of some form of Newton’s Second Law ($\mathbf{F} = m\mathbf{a}$ or the conservation of momentum). The older tradition—*explicit-dynamic modeling* or *general circulation modeling*—emerged in the 1950s as an extension of early efforts at numerical weather prediction.³As the name *explicit-dynamic modeling* suggests, atmospheric or oceanic motions are calculated explicitly in this type of modeling from a consideration of the forces that cause these motions, namely pressure gradients, gravity, and the coriolis force (an apparent force associated with the earth’s spin).

Energy balance modeling, on the other hand, emerged in mature form in the late 1960s⁴ as an application of certain theoretical developments in physical climatology. These developments were concerned with the mathematical relationships between fluxes of energy and the distribution of temperatures, and allowed models to calculate horizontal and vertical energy fluxes without calculation of the forces that caused these motions. Energy balance models may calculate velocities as well as energy fluxes as functions of temperature (e.g., Sellers 1976). But whether or not velocities are calculated along with energy flows, these calculations are based on thermodynamic and empirical considerations rather than on considerations of the accelerations and forces that produced the motions.

Climate models are used for a variety of purposes, including the simulation of past and current climates, the prediction of future climates, the identification of the possible causes of climatic change, and the estimate of the response or sensitivity of the climate to realistic or feasible changes in causally relevant phenomena. Sensitivity studies and the analysis of feedbacks that contribute to climatic change are extremely important components of climate modeling methodology, in that they help to determine the variables and processes represented in future modeling efforts, they can provide information relevant to environmental policy (as in estimates of the sensitivity of climate variables to increases

³Actually, the first partially successful applications of fundamental dynamical and thermodynamic laws to the problem of atmospheric motion were those of Richardson (1922). Since his work we have found ways of simplifying his mathematical assumptions to facilitate computational feasibility.

⁴There were several pioneering studies in the late 1950s and early 1960s.

in atmospheric carbon dioxide), they contribute to the theory and explanation of climatic change, and they help to motivate interdisciplinary research needs.

It will be helpful for our present purposes if we show how sensitivity and feedback determinations can help to motivate multidisciplinary and interdisciplinary research needs. As we shall see, just which is needed (multidisciplinary or interdisciplinary) will generally depend upon the status of the current parameterizations employed in the contributing disciplines. The explanatory role of parameterizations, and their relevance to the motivation of multidisciplinary and interdisciplinary forms of research, will be explored in the section below. In the present section, we will outline the role of sensitivity and feedback analysis in motivating cooperation generally, without regard for whether this cooperation ought to be multidisciplinary or interdisciplinary.

Sensitivity studies have been performed by climate modelers to determine the response of climate variables such as globally and temporally averaged surface temperatures to changes in the so-called solar constant (see discussion in Chapter IV above), volcanic eruptions, atmospheric aerosols of industrial origin, atmospheric carbon dioxide, and other candidates for the causation of climatic change. (Sensitivity studies can also estimate the response of the model climate to feasible variations in internal variables and in parameter values in model parameterizations, as discussed below.) Schneider and Mass (1975) were among the first to emphasize the importance of such studies in climate modeling. Although the significance of sensitivity analysis first became clear in the mathematically simpler context of energy balance models, both energy balance modelers and general circulation modelers employ sensitivity studies today.

Sensitivity has most conveniently been defined in terms of the sensitivity of one variable to a fractional change in another. Although climate modelers often are interested in the responses of other variables, such as those associated with wind speed and direction and atmospheric pressure (in GCMs) and precipitation and vegetation (when such variables are calculated rather than fixed as boundary conditions), the most general indication of the state of the climate, and the most obvious implications for seasonal snow cover, the length of the growing season, and in general, for society, are provided by variables associated with temperature.

The sensitivity of planetary or surface temperature (see the definitions above) to

changes in the solar constant are considered paradigmatic of sensitivity measures. (It is perhaps easiest to view the level of the sun’s radiative output as independent of climatic conditions on earth, and it certainly has a great deal to do with how hot it is down here.) The solar sensitivity parameter, or β , then, is defined as the response of globally averaged temperature (whether planetary or surface) to fractional changes in the solar constant:

$$\beta = \frac{dT}{\frac{dS}{S}} = S \frac{dT}{dS}.$$

It is a matter of calculus (not climate modeling) that β is given by the following:

$$\beta = S \frac{dT}{dS} = S \left[\frac{\partial T}{\partial S} + \sum \frac{\partial T}{\partial X_i} \frac{dX_i}{dT} \frac{dT}{dS} \right],$$

where $\frac{\partial \dots}{\partial \dots}$ represents the partial derivative of the first variable with respect to the second, and the “ X_i ”s are the variables other than S upon which the temperature T depends. Solving for $S \frac{dT}{dS}$, we get

$$\beta = S \frac{dT}{dS} = \frac{S \frac{\partial T}{\partial S}}{1 - \sum \frac{\partial T}{\partial X_i} \frac{dX_i}{dT}}$$

It will be observed that the numerator $S \frac{\partial T}{\partial S}$ is essentially the response that the temperature variable would have if temperature depended only on variation in the solar constant S, while the summation term $\sum \frac{\partial T}{\partial X_i} \frac{dX_i}{dT}$ in the denominator represents the contributions of variables (such as albedo and absorbance variables) that both respond to and influence—i.e., have *feedback* effects upon—changes in temperature. Climate modelers think of the numerator $S \frac{\partial T}{\partial S}$ as the temperature sensitivity (β_0) without *feedback* ($\sum \frac{\partial T}{\partial X_i} \frac{dX_i}{dT} = 0$), and define feedback as the ratio of sensitivity with feedback to sensitivity without feedback. Thus feedback is given by:

$$\frac{\beta}{\beta_0} = \frac{1}{1 - \sum \frac{\partial T}{\partial X_i} \frac{dX_i}{dT}}$$

The following remarks may help to highlight the significance of the above measures of sensitivity and feedback. How can we establish whether a candidate external forcing (variable thought to vary independently) is important in climatic change as compared with other internal and often interdependent sources of variation? Answer: (1) Find out whether the climate (model) is sensitive to the candidate forcing over feasible ranges of variation; (2) Determine the relative contributions of feedback and sensitivity without feedback (their

product is the sensitivity) to a suitably defined sensitivity parameter; (3) Consider whether the sensitivity parameter itself is sensitive to changes in the candidate forcing (i.e., calculate the second derivative with respect to the forcing variable).

Now we are in a position to ask how we determine whether some form of cooperative research—such as interdisciplinary or multidisciplinary research—is required. If the output (climate) variables of one discipline’s models are sensitive to the dependent (i.e., calculated) variables of another discipline’s theories or models, then the former discipline would be well-advised to take an interest in the results of the latter, and the latter can find applications to the problems of the former. If the dependent variables of several disciplines are mutually sensitive, interdisciplinary communication would be in order. When would interdisciplinary as distinguished from multidisciplinary research be appropriate? The answer to this question depends on how routine the contributions of the contributing discipline or disciplines are. If the model outputs in question are thought to be reliable, multidisciplinary coupling of results may be sufficient. It may only be necessary to coordinate the formats of the outputs and inputs of the respective disciplines. But if the parameterizations of the *donor* disciplines are in question, and they are furthermore sensitive to results of the recipient discipline, interdisciplinary research may be in order.⁵We explore this possibility in the context of a discussion of parameterization immediately below.

5.2 Parameterization and Explanation in Climate Modeling

Both energy balance models and general circulation models employ parameterizations. We shall generalize from a consideration of the simplest energy balance models to show that parameterizations indirectly approximate the effects of unresolved or otherwise omitted phenomena in models of any degree of complexity, that they can have explanatory value if their confirmation requirements are understood in terms of interactional complexity, and that the interdisciplinary methodology often required for the development and confirmation of adequate parameterizations can integrate the participating disciplines without eliminating

⁵Even when multidisciplinary research suffices to solve global system problems, interdisciplinary abilities are often involved in weaving these results together into an integrated climate model.

them,⁶ and is therefore an appropriate vehicle of authentically interdisciplinary scientific progress.⁷

Climate models have been classified at levels of spatial, temporal, and physical detail in what has been called *the hierarchy of climate models* (Schneider and Dickinson 1974). The most obvious ordering of model complexity is the progression from globally averaged energy balance models, to radiation models which treat the vertical radiation budget but not the horizontal transfer of energy, to models which treat the former and one dimension of the latter (such as zonally averaged energy balance models resolved in latitude but not longitude), to models which treat the former and both dimensions of the latter (such as energy balance models that calculate interactions of land and sea). Further degrees of detail are associated with general circulation models, which most often (but not always) require higher degrees of spatial and temporal resolution than energy balance models, and explicitly represent wind velocity, atmospheric pressure, and other physical variables generally omitted from energy balance models in order to calculate atmospheric motions (as much as possible) from first principles.

The simple radiation model (of less than one dimension) developed as a teaching tool in Chapter IV above will be extended here to illustrate parameterization methodology and explore its implications for the explanatory status of climate models as well as for the distinction between multidisciplinary and interdisciplinary research. See the discussion in Chapter IV for an introduction to the model.

We have seen how the effective temperature of planetary radiative equilibrium (T_p) is given by $F_{IR} = \sigma T_p^4 = \frac{S}{4}(1 - \alpha_p)$, where F_{IR} is the thermal (IR) radiation emitted by the planet, α_p is the effective planetary albedo, and $\frac{S}{4}$ is the globally and temporally averaged solar flux (represented by S_{av} in Chapter IV) entering the top of the atmosphere. The sensitivity of the effective planetary temperature to fractional changes in the solar constant ($\beta_p = S \frac{dT_p}{dS} = \frac{S \frac{\partial T_p}{\partial S}}{1 - \sum \frac{\partial T_p}{\partial X_i} \frac{dX_i}{dT_p}}$, as derived above), can be calculated from the planetary energy balance by equating the derivatives with respect to S of outgoing IR and incoming solar

⁶This is in contrast to the positivistic unification of scientific disciplines through theory reduction. See Chapter II for a review.

⁷For the classic review of the variety of climate models, see Schneider and Dickinson (1974). For a detailed review of the variety of statistical-dynamical models, see Saltzman (1978). Other reviews of general circulation models include those of Chang (1977), Haltiner and Williams (1980), and Washington and Parkinson (1986). Brief introductions for the nonspecialist may be found in Bergman et al. (1981), Mitchell (1976), and Schneider (1987a).

radiation absorbed:

$$\frac{d(\sigma T_p^4)}{dS} = \frac{d[\frac{S}{4}(1-\alpha_p)]}{dS}.$$

Differentiating, substituting $\frac{S}{4}(1-\alpha_p)$ for σT_p^4 , and solving for the sensitivity $\beta_p = S \frac{dT_p}{dS}$, we get:

$$\beta_p = S \frac{dT_p}{dS} = \frac{\frac{T_p}{4}}{1 + \frac{\frac{T_p}{4}}{1-\alpha_p} \frac{d\alpha_p}{dT_p}},$$

where $\frac{T_p}{4} = S \frac{\partial T_p}{\partial S} = \beta_0$, the sensitivity without feedback; $\frac{T_p}{1-\alpha_p} = -\frac{\partial T_p}{\partial \alpha_p}$ (the sensitivity of temperature to changes in albedo, without feedback); and feedback is measured by $\frac{1}{1 + \frac{\frac{T_p}{4}}{1-\alpha_p} \frac{d\alpha_p}{dT_p}}$.

It should be observed that the mathematical interdependence T_p and α_p does not permit solutions of the above equations which express T_p and α_p , respectively, as functions of S alone. We cannot hope to achieve mathematical closure, but only to determine temperature and its changes in terms of the other variables. The sensitivity of albedo to temperature $\frac{d\alpha_p}{dT_p}$, upon which temperature and its changes depend, remains an undetermined quantity. This should come as no surprise, as the processes that establish changes in planetary albedo, such as how cloudiness and the optical (reflective and absorptive) properties of clouds and sky respond to changes in temperature and related changes in atmospheric composition and motion, are not represented explicitly in the model.

We can, of course, express α_p as a function of the albedos of sky and earth and the atmospheric absorptivity of direct and diffuse solar, as derived in Chapter IV. But then we will have the same problem determining the sensitivities of these albedos and absorptivities to changes in planetary temperature. At some point we must make a choice, therefore, as to whether to undertake empirical studies to explore, possibly with little theoretical insight, the sensitivities of such feedback-related quantities to temperature; to develop theories from first principles, possibly with little empirical confirmation, of the sensitivities that are involved; or to develop semiempirical parameterizations, as they are called in climate modeling. These are equations that provide mathematical closure, are physically motivated in structure, but the constants of which are fit either to empirical data or to a more highly resolved model. Knowledge of how sensitive model outputs are to changes in the constants in parametric

representations indicates how much effort should be expended in their confirmation. As reviewed above, parameterizations are confirmed as empirical generalizations from data sets independent of (or with degrees of freedom independent of those of) the data used to fit them; as instruments of model predictions and simulations of past or current climate; and as theories which link contributing disciplines that specialize in the study of the *parameterized* (indirectly represented) processes with the field of climate modeling.

We may appreciate how parameterizations contribute to the explanation of climate despite the implicit nature of their reference to causally responsible or relevant processes, and explore the implications of parameterizations for the structure of interfield cooperation, if we return to our illustrative climate model. How shall we generate additional equations so that our model can be solved for temperature responses to solar forcings? It has been possible, here, to kill two birds with one stone. We are much more interested in surface temperature than the effective (vertically, horizontally, and temporally averaged) temperature of planetary radiative equilibrium anyhow. Schneider and Mass (1975) have outlined the strategy involved in parameterizing albedo and terrestrial infrared emissions in terms of surface temperature, and the role of sensitivity studies in a brief but frequently referenced methodological discussion. We now consider the contribution of such parameterizations to our above illustrative model.

If we could express the planetary sensitivity, and in order to do so, express albedo and other feedbacks, as functions of surface temperature and derive surface temperature, in turn, from the planetary energy budget, we might obtain the mathematical closure required. We observe that at least on the surface of the earth, there is a positive albedo feedback with respect to solar constant changes since warmings cause retreats of the seasonal snow line to higher latitudes and hence decrease the contribution of surface to planetary albedo while coolings have the opposite effect. We also know that absolute humidity increases with the surface warmings that drive increased evaporation, thereby increasing the albedo of the sky, and that any increases in global cloudiness associated with such absolute humidity increases would also increase sky albedo. Although such humidity and cloudiness feedbacks would be negative in the global average, zenith angle and multiple reflection effects at particular locations, combined with the uncertainty as to changes in cloudiness, make it difficult to predict the strength of such negative albedo effects. The positive surface albedo feedback is thought to predominate. On the other hand, the surface warming associated with the

greenhouse is controlled by (the fourth power of) surface temperature and the associated IR emissions from the surface and by the atmosphere's absorptivity (A_T) of terrestrial (IR) radiation (see the discussion in Chapter IV above), while A_T would depend on cloudiness and the composition of the atmosphere (i.e., its absolute humidity, carbon dioxide content, methane content, etc.). Absolute humidity, once again, increases with surface warmings, and carbon dioxide may be released into the atmosphere from warmer seas or soils). Absolute humidity increases, any associated increases in cloudiness, and any carbon dioxide increases would all contribute to a net positive IR-feedback resulting from increased IR absorption and the resulting intensification of the greenhouse.

As a first approximation, it is inviting for reasons of mathematical convenience to consider linear dependencies of planetary albedo and planetary IR emissions upon surface temperature.⁸ Empirical observations also seem to suggest linear relationships. Thus we explore the modeling implications of the following parameterizations:

$$F_{IR} = a + bT_E = \sigma T_p^4 = \frac{S}{4}(1 - \alpha_p) \text{ and } \alpha_p = c + dT_E$$

(where a, b, c, and d are constants).

It will be recalled that we could not calculate $\frac{d\alpha_p}{dT_p}$ in the equation

$$\beta_p = S \frac{dT_p}{dS} = \frac{\frac{T_p}{4}}{1 + \frac{\frac{T_p}{4}}{1 - \alpha_p} \frac{d\alpha_p}{dT_p}},$$

considered above. Now we can calculate $\frac{d\alpha_p}{dT_p}$ from surface temperature changes and surface temperature changes from planetary considerations as follows:

$$\frac{d\alpha_p}{dT_p} = \frac{d\alpha_p}{dT_E} \frac{dT_E}{dF_{IR}} \frac{dF_{IR}}{dT_p} = \frac{\frac{d\alpha_p}{dT_E} \frac{dF_{IR}}{dT_p}}{\frac{dF_{IR}}{dT_E}} = \frac{(d)(4\sigma T_p^3)}{b} = \frac{4d\sigma T_p^3}{b}$$

The planetary sensitivity, β_p , is therefore given by:

$$\beta_p = S \frac{dT_p}{dS} = \frac{\frac{T_p}{4}}{1 + \frac{\frac{T_p}{4}}{1 - \alpha_p} \frac{4d\sigma T_p^3}{b}} = \frac{\frac{T_p}{4}}{1 + (\frac{d}{b}) \frac{\sigma T_p^4}{1 - \alpha_p}}.$$

Substituting $\frac{S}{4}(1 - \alpha_p)$ for σT_p^4 in the above expression, we get

⁸Note that planetary IR emissions to space, which are largely emitted by the atmosphere and only to a small extent directly by the surface, depend on surface temperature less strongly than surface emissions do (see Chapter IV). The dependence of terrestrial emissions on surface temperature is mediated by changes in atmospheric composition, in the same way as the downward emissions associated with the greenhouse are mediated by such changes, as explained above.

$$\beta_p = \frac{\frac{T_p}{4}}{1 + \frac{Sd}{4b}}, \text{ with feedback given by}$$

$$\frac{\beta_p}{\beta_o} = \frac{1}{1 + \frac{Sd}{4b}}.$$

It will be fruitful to compare the above planetary sensitivity with the surface sensitivity under the same parametric assumptions:

$$F_{IR} = a + bT_E = \sigma T_p^4 = \frac{\frac{S}{4}}{1 - \alpha_p}$$

$$\alpha = c + dT_E$$

$$\begin{aligned} \beta_E &= S \frac{dT_E}{dS} = S \frac{dT_E}{dT_p} \frac{dT_p}{dS} = \left(S \frac{dT_p}{dS} \right) \frac{dT_E}{dT_p} = \beta_p \frac{4\sigma T_p^3}{b} \\ &= \frac{\frac{T_p}{4}}{1 + \frac{Sd}{4b}} \frac{4\sigma T_p^3}{b} = \frac{\sigma T_p^4}{b + \frac{Sd}{4}} = \frac{F_{IR}}{b + \frac{Sd}{4}} \end{aligned}$$

The ratio $\frac{\beta_E}{\beta_p} = \frac{4\sigma T_p^3}{b}$ is seen to vary only with planetary temperature, so that as the temperature rises, feedback effects have an increasing impact on surface temperature as compared with planetary temperature. At today's temperatures, and at commonly assigned values of b , the surface temperature is $2\frac{1}{3}$ times more sensitive than planetary temperature to fractional changes in the solar constant (Schneider and Mass 1975). This relatively strong surface response may be an artifact of the model, however, since the model does not independently parameterize or otherwise calculate the sensitivities of surface albedo, cloud and clear sky albedos, IR absorptivity, and direct and diffuse solar absorptivities. It is essential to compare the relative contributions of these feedback processes if conclusions are to be drawn as to their relative importance in climatic change.

Modelers have also calculated sensitivities of surface temperature to changes in their parameter assignments, and have indeed discovered the sensitivity of surface sensitivity, so to speak, to parametric assumptions regarding the temperature dependence of the reflective and absorptive properties of clouds, atmospheric composition, and the earth's surface. In addition to conducting sensitivity studies, modelers more or less systematically confirm their parameterizations as empirical generalizations and predictive instruments and coordinate their efforts with specialists in parameterized processes, as outlined in Chapter III, in order to improve their models over time and motivate further research.

The above rather elementary mathematical discussion perhaps prepares us to appreciate better the role of sensitivity studies and parameterization in the motivation of multidisciplinary and interdisciplinary research in climate modeling. Discovery is iterative in climate modeling. Parameterizations are employed to represent indirectly the effects of processes that vary too much in space and time to be represented explicitly in the model and/or the theoretical understanding of which is inadequate for a derivation from first principles of the contribution of the processes in question to explicitly modeled processes. If we discover that our sensitivity parameter (usually the surface sensitivity since it is more sensitive and a more direct indication of socially relevant climate) is itself sensitive to parametric assumptions about feedback processes, or we find that other model outputs of interest are sensitive to these assumptions, we know (if immediate solutions are beyond the state-of-the-art in other disciplines) the following: More research needs to be done to improve our model's indirect representation of the phenomenon, or to allow explicit representation (if mathematically, computationally, and observationally feasible) of these processes in future models. If the areas of research that specialize in the phenomena in question can find solutions independently of considerations of the outputs of climate models, then the research required is clearly hierarchical and multidisciplinary, not interdisciplinary. If it happens that the contributing disciplines in question themselves need to consider carefully the outputs of the model or models in question in order to contribute to those models—either because the models are unfamiliar or because the model outputs have a feedback effect on the parameterized processes in question—closer forms of interdisciplinary cooperation may be useful.

We should distinguish between cooperation between modelers and theoreticians and cooperation between two modeling disciplines or subdisciplines. Modelers often work closely with specialists, as when modelers become specialists in other fields, or when specialists become modelers. Glaciologists (e.g., Claire Parkinson), radiation physicists (e.g., V. Ramanathan), geophysicists (e.g., Eric Barron), and dynamicists (e.g., Starley Thompson), for example, have all become climate modelers, and many climate modelers have made themselves specialists in contributing fields such as oceanography (e.g., Kirk Bryan, Danny Harvey), micrometeorology or ecology (e.g., Robert Dickinson, Y. Mintz), and even economics and political science (e.g., Stephen H. Schneider). (The social sciences not only study climate impacts, but contribute to environmental policy and science policy in ways that affect climate—both directly in altering environmental impacts of human activities, and indirectly through the funding of climate modeling and global sciences

programs that inform environmental policy.) Many climate modeling teams (e.g., the group at the Goddard Institute for Space Studies) have included diverse groups of specialists such as meteorologists, oceanographers, glaciologists, ecologists, radiation physicists, and atmospheric chemists, while many climate modelers have pursued training in such related disciplines. Climate modeling is inherently interdisciplinary, in fact, since it has gained its professional practitioners from numerous fields.

When the cooperating disciplines are or include mathematical modeling disciplines or subdisciplines, the coupling between disciplines is more formal, and easier to motivate and achieve. Models are built with inputs and outputs, and often can be coupled with models of other fields. How much cooperation between ocean modeling and atmospheric modeling, for example, is required to predict the response of surface temperatures to feasible increases in atmospheric carbon dioxide over the coming decades to centuries? Climate modelers have argued, employing sensitivity studies more complicated in detail but not in principle—that models of the oceans (in particular, the deep oceans) are relevant to predictions of surface temperature responses to carbon dioxide increases over decadal and longer time scales, and need to be coupled with atmospheric models with greater attention to detail.⁹

The arguments employed in justifying such recommendations often are derived from sophisticated versions of the types of sensitivity studies outlined above. Atmospheric variables and parameters alike are sensitive to heat storage changes in the mixed layer, which are in turn sensitive to the interactions of the mixed layer and deep oceans. The depth of the mixed layer, at specific locations and times of year (consider the case of El Niño reviewed in Chapter IV above), is furthermore sensitive to the effects of winds on surface currents, mixing, and heat exchange with the atmosphere, while evaporation, cloudiness, atmospheric pressure and temperature, cyclogenesis, and atmospheric chemistry and motion all depend—in their interactions with one another—on sea surface temperatures and outgassing from the mixed layer, which are in turn sensitive over decadal and longer time scales to interactions with the deep oceans. The type of ocean detail needed by atmospheric modelers or atmospheric detail needed by ocean modelers will, of course depend on the climate problem and modeling context in question, but it is fair to say that the success of parameterizations in models of both subsystems will ultimately depend, for many modeling purposes, upon the more broadly defined success of coupled ocean-atmosphere

⁹See the work of Harvey and Schneider (1985a and 1985b) reviewed in Chapter IV above.

models. Similar remarks could be made about the couplings of ocean-atmosphere models with models of the other climate subsystems.

It is fortunate that the methodology of climate modeling is inherently transdisciplinary in that it can embrace and ultimately integrate any number of modeling subdisciplines. In suitably coupled models it can test the sensitivity of component model outputs to coupling schemes (see the discussions of asynchronous coupling schemes in Chapter IV above), the sensitivities of ocean variables (or parameters) to atmospheric variables (or parameters) or vice versa, the sensitivities of variables to parameters or parameters to variables, as well as atmospheric (or oceanic) variables or parameters to other atmospheric (or oceanic) variables or parameters. As we gain more confidence in glacial, biosphere, and social models, it will become equally routine to study the cross-sensitivities (or mutual impacts) among all the climate subsystems. The reason for this is simply that causes are causes, and it does not matter what climate subsystem they belong to.

The methodology of climate modeling allows us to gradually improve our models to directly or indirectly represent with increasing detail, to the extent this detail is relevant to our particular modeling purposes, the causal interactions that actually determine the evolution of climate. It should not be assumed, however, that we will ever develop a definitive master model¹⁰ that tells us everything we want to know about climate.¹¹ As long as we have a variety of climate problems, limitations in computing time and the capacity of individuals or teams to know or consider everything, and continuing interests in the more specialized areas of global science, we will have a multiplicity of climate models for different and often overlapping purposes. (Recall the discussion of Wimsatt's notion of descriptive complexity in Chapter III.)

Furthermore, parameterizations—even when they adequately serve certain modeling purposes—are not unconditional laws. They may work in some circumstances (e.g., over certain time scales) but not in others. Our parameterizations of the global evaporation and precipitation cycle, for example, and the associated cloudiness, may work well in future models under some assumptions, but become increasingly inadequate as our rain forests are destroyed or continental drift changes the distribution of land and oceans. Yet this is not

¹⁰Diagrams of the components of potential master models, such as that of Bretherton (1986), may be viewed, with some prudence, as heuristic.

¹¹There may also be limits to predictability which preclude the possibility of such a master model.

necessarily a shortcoming of parameterizations. If we are interested in modeling complex systems, we will always have the difficulty of anticipating undiscovered or yet-to-occur circumstances which alter the feedback processes or boundary conditions relevant to our predictive or simulative concerns. The best we can do is to try to identify the circumstances under which the parameterizations of specific models are adequate for our purposes, and our opportunities to develop better models. The methodology of parametric representation, inductive (descriptive) confirmation of the regularities proposed by the parameterizations (independently of models), instrumental testing of model predictions against data, the testing of model outputs as inputs to coupled models, and the progressive consideration of candidate processes for their causal relevance,¹² will yield both a growing variety of models suited to different assumptions and purposes and a growing integration of our models as the gaps between our areas of knowledge are filled. If parameterizations in models are not unconditional laws, they more or less accurately describe more or less regular features of the climate system and provide testable explanations of explicitly modeled processes. It would seem that the explanatory status of the fundamental laws of physics are different only in their degree of generality and scope, not in kind, from parameterizations in climate models.¹³

Parameterization and sensitivity analysis, then, constitute a methodology of climate modeling that is adequate to integrate progressively, to the extent that physical interconnect-edness demands and the multiplicity of processes allows, and to the extent that scientific and social research interests justify, a growing number of disciplines, subdisciplines, and areas of specialized science by any other name.¹⁴ The Global System Science initiative mentioned early in this chapter is an example of the growing realization by climate modelers, related disciplines, funding agencies, and legislators that cooperation among specialists is essential

¹²See Schneider (1979), Schneider and Thompson (1980), North et al. (1981), and Schneider (1986) for detailed reviews of climate modeling methodology.

¹³See the favorable comparison of parameterizations (or conditional regularities) with theoretical laws in Chapter III above and in David Bohm's classic (1957) discussion of the qualitative infinity of nature. For less favorable considerations of the risks of overparameterizing (that is, fitting data to too many free parameters in a single model), see Larimore and Mehra (1985). Note that overparameterizing is as much a problem in areas of theoretical physics such as quantum mechanics as it is in areas of applied physics such as climate modeling.

¹⁴I would argue that the variety of intersubsystem coupling strategies, some aspects of which have been reviewed in Chapter IV above, may serve to integrate disciplines which develop submodels, but that the need to integrate subsystems with more or less detail needs to be justified by sensitivity studies of the mutual relevance of the processes (or parameterizations) of different subsystems. Thus, I have not included coupling strategies in my analysis of interdisciplinary methodology in climate modeling, but consider the former to follow from the latter.

for efficient solutions of pressing global system problems.

Yet it must be understood that an interdisciplinary or transdisciplinary methodology is no guarantee of interdisciplinary expertise.¹⁵ It is not system science—or more futuristically, artificial intelligence—that will solve our global science problems. Even if mathematical modeling evolves into a discipline that interacts with contributing disciplines in highly organized international and multidisciplinary programs, the individuals involved, whether in empirical, theoretical, modeling, or strictly methodological disciplines, will only know as much about each other's fields as they do. This tautology has the unsurprising implication that methodology is no substitute for insight and hard work in exploring the physical, chemical, biological, and social interconnectedness of our planet. Mutual causal interconnections among the phenomena conventionally studied by different (nonmethodological) disciplines often have to be studied with as much expertise as the interconnections within the scope of specialized disciplines. Methodology and content must come together, within or across disciplinary boundaries, if science is to progress without unnecessary blind spots. We can integrate many areas of knowledge in global programs only to the extent, ultimately, that empiricists, theoreticians, specialized modelers, and generalist modelers work closely together, and that at least some individuals become multidisciplinary themselves. We can successfully manage the planet as a whole only to the extent that we have had the scientific and ethical vision to study and appreciate it as a whole, and to recognize what we have not understood.

¹⁵One of the reasons that interdisciplinary cooperation in research is so difficult to achieve is that quality review standards are most often exclusively disciplinary. See the analysis of this problem in Schneider (1977).

Chapter 6

Conclusion

We have shown how interconnected the climate system is, and how perfectly it reveals—for many modeling purposes, at least—an example of an interactionally complex system. We have shown how sensitivity studies can probe for these interconnections, and how parameterizations can help to explain them—as long as it is understood that parameterizations indirectly represent the causal consequences of interactions between inexplicitly and explicitly represented phenomena, so that even a model that performs well in some circumstances may, because of conditions not adequately represented, not work well for all modeling purposes. The challenge of employing such conditional explanations is not to make them unconditional; this may not be possible. It is to investigate as systematically as possible the conditions under which they may or may not explain and predict enough of the variability of the climate system under specified circumstances to be helpful in a given modeling context. And finally, we have shown how the methodology of climate modeling seems adequate as an interdisciplinary methodology, as long as it is understood that methodology itself is no substitute for interdisciplinary collaboration in model development.

The implications of these results for the philosophy of science are dramatic, in that we can at once deduce from the counterexample of climate modeling that hierarchical levels of systemic organization, theoretic levels or levels of lawlike explanation which require confirmation only at those levels, and methodologically isolated levels of disciplinary research are all suspect philosophical notions if thought to apply to all areas of science at all times.

Many areas of interdisciplinary study of complex systems are emerging in contemporary science which invite philosophers to catch up with the science they aspire to analyze and assist.

The old philosophical schools do not need to abandon their research programs in the study of systems, explanation, and disciplinary or multidisciplinary research. They can join forces, in fact, in developing unified philosophical perspectives on the systems context and methodological basis of areas of contemporary science. The old schools only need to abandon the exaggerated paradigms that launched their research traditions. We can no longer maintain, for example, the positivist quest for a confirmation methodology that guarantees successful explanations in all fields of science. Methodologies are around to unify broad areas of science, as our case study makes clear. But adequate explanations must be sought by scientists prepared to explore candidate explanations for their relevance. Until heuristic modeling succeeds in simulating such a creative process, we need not worry that a single method will guarantee scientific success. Methodology makes disciplines compatible. It is for individual scientists to integrate their theories, models, and research, and to specialize in new directions as new ways of slicing up the universe emerge. Methodology does not hover above science as a measure of success. It provides an opportunity to cooperate in the quest for knowledge, with no guarantees that nature has been exhausted in its infinitude of processes and interconnections. The unity of science is as tentative as the unity of Gaia. There are always new challenges in store.

If methodology is less totalitarian than positivists imagined, it is also less pluralistic than historicists imagined. Scientists have many opportunities to cooperate across disciplinary boundaries, and their contemporary progress in cooperative global science, cognitive science, health science, and so forth are an indication of the growing methodological compatibility of the participating fields. Moreover, if the systems in which so many disciplines often share an interest were organized as step ladders from atoms to molecules, and so forth, to cosmos, there would be little need to explore the vertical interconnections that integrate levels.

We must indeed abandon, on empirical evidence, oversimplified or overgeneralized notions of levels of organization, explanation, and research. Yet the philosophical research traditions that analyze the systems, methods, and disciplines of science have a broader

purpose than the promotion of idealized and outdated models of science. These philosophical models are but first steps to models of nature and science in motion and transition, in continual reorganization of nature's subsystems and the disciplines of science. If there is little to hold onto in all this process, let philosophers hold on to each other's traditions, and bring the realism of system theory together with the analysis of scientific justification and the study of scientific history. Let us observe science as empiricists, model it as theoreticians, integrate our observations and theories as scientists, and communicate our findings as philosophers, in ways that have social force. In this way we sacrifice only our indulgence in extreme forms of positivism, pluralism, or realism; we gain a professional cohesion that finds its place dynamically among the disciplines whose progress we wish to support.

As interdisciplinarians involved in the comparative study of science, philosophers can help to bridge areas of science. We can catalyze interaction among disciplines by holding them simultaneously in view; we can carry information across their boundaries as plasmid vectors carry genetic information between species of bacteria. We can promote the coevolution of scientific disciplines toward eukaryote-like integration in higher-level cells of the scientific community, toward a greater understanding of our world.

Appendices

Appendix A

Interactional Complexity in Biological Evolution

Wimsatt's theory of *interactional complexity* (Wimsatt 1972a, 1975; reviewed in Chapter III above) emerged from his concerns in the philosophy of biology. The theory was motivated by the apparent failure of conventional forms of hierarchical system analysis to represent adequately the high degrees of interdependence among the various molecular and cellular structures, physiological and behavioral functions, and physical, biological, and social environments of living or evolving organisms. In particular, Wimsatt's analysis of interactionally complex systems provided an interpretation of the high degrees of biological integration that emerge from the mutual adaptation of coevolving species or from natural selection for structurally and functionally integrated genomes, organisms, or communities.

Wimsatt defined interactional complexity in terms of the strength of intersubsystem and interlevel interactions. Three of his illustrations of interactionally complex systems—eukaryotic cells, slime molds, and social insects—all reveal high degrees of interdependence between *subsystems* (organelles and nucleus, amoebae, or castes) and between *levels* (organelles and cell, differentiated amoebic cells and slime mold, or caste and insect society) that emerged from the progressive integration of components and levels over evolutionary time scales. These three illustrations are developed in detail below.

A.1 Interactional Complexity and the Endosymbiotic Theory of Cell Evolution

According to the widely accepted endosymbiotic theory of cell evolution (see especially Margulis 1981, 1982), eukaryotic (*truly nucleated*) cells emerged from the mutual adaptation and progressive integration of two species of bacteria: an (oxygen) respiring parasite and a fermenting host cell. There is evidence that the parasite was similar to the modern *paracoccus denitrificans* (Dickerson 1980, Margulis 1981), and that the host was similar to certain modern *thermoacidophiles* (Margulis 1981).¹

It is unclear how the guest and host coevolved. On one view, the host was killed or disadvantaged until it evolved a nuclear membrane to protect its genetic material against the toxic oxygen required for its guest's metabolism (Margulis 1982). On another view, the mutualistic takeover of most of the guest's protein synthesis by the host was virtually complete by the time the nuclear membrane evolved and prevented further genetic transfer from guest to host (Grivell 1983).

Whether the transition from parasitism to mutualism followed or preceded the evolution of the membrane- bounded nucleus, the general outcome of the coevolution of guest and host is clear. The former benefited from the products of the host's glycolysis and from its protection, and the latter benefited from the guest's efficient respiratory production of energy-rich ATP. The former evolved into an organelle—the mitochondrion—which became progressively dependent on its host not only for nutrients, but for much of its original genetic burden. The latter, by progressively taking advantage of increasing quantities of ATP provided by one or more mitochondria, was able to evolve such energy-intensive capabilities as the coordination of intracellular movement through the ionically controlled action of protein filaments, mitotic and meiotic forms of reproduction, phagocytosis (the capacity to engulf prey or large particles), motility through the movement of cilia or flagella, characteristic eukaryotic organelles (such as golgi bodies), and gigantic sizes (compared with their bacterial ancestors) to contain all this activity. These same eukaryotes were to give rise to the multicellular plants, animals, and fungi.

What began as a symbiotic society became—through a process of mutual adaptation

¹See Woese (1981) for a discussion of the possible role of Archaeobacteria in the origins of eukaryotes.

and the progressive coordination and integration of the contributing genomes in the service of that society—a single organism. Genetic redundancy was selected against, biological structures and functions were coordinated, and new biological structures and functions—impossible for either of the symbionts in isolation—evolved.

From the point of view of Wimsatt's model of interactional complexity, the following interpretation seems appropriate. If the original symbiotic bacteria are considered to be subsystems, and their mutual adaptation society—the evolving eukaryotic cell—is considered to be the system in which they are contained, the coevolutionary development of the eukaryotic cell represents a transition from a *near-decomposition* (in Simon's 1962, 1973, 1976 sense of the term; reviewed in Chapter II above) to interactional complexity.

At first the subsystems are loosely or externally coupled. One supplies energy and the other supplies food (or substrate). They may be viewed as two organisms adapting to each other. The cytoplasm of the host represents the environment of the guest, and the behavior of the guest represents part of the environment of the host. The gene expression of each organism is the output which generates the inputs of the other organism; the outside environment is assumed to be a slowly changing boundary condition. Genetic variation in either organism is assumed not to affect the genome of the other organism directly, but only indirectly through gene expression and its ultimate consequences for the survival and reproduction of either or both symbionts. The subsystems, to use Simon's term, are nearly decomposed.

Over evolutionary time scales, however, the situation is very different. Since the survival of each symbiont quickly becomes dependent upon the survival of the other, both symbionts experience *group selection*, i. e., those subsystem variations have survival value which contribute to the survival of the system. What is interesting about this situation is that the usual Darwinian near-decomposition of high frequency genetic variations from low frequency changes in environmental selection pressure breaks down. The eukaryotic cell—host plus guest—is the environmental selection pressure imposed on the evolving mitochondrion, and the mitochondrion contributes significantly to the environmental selection pressure imposed on the eukaryote of which it is a part. By contributing positively or negatively to the coordination of their biological structures and functions, host and endosymbiont variations have direct consequences for the social environment which they share. The time

scales of variation and selection become more closely synchronized, and variation shapes selection as much as the other way around.

Whenever lower-level variation has direct consequences for higher-level change, Wimsatt calls the situation interactionally complex. The rapid feedback from higher to lower levels results in the possibility of rapid structural change, i. e., an acceleration of evolution. The remarkable evolutionary transfer of genetic material from mitochondria to the nucleus is a case in point.

Whether plasmids were trained to go back and forth carrying DNA² or mitochondria and nuclei learned to conjugate with their cell mates,³ no one knows.⁴ Regardless of the mechanism, however, the exchange of genetic material represents strong subsystem interactions which Simon could hardly classify as a nearly decomposable relationship. Moreover, the consequences for the rate of evolutionary change are obviously profound.

By the time the mitochondrion was well integrated—with other organelles of both endosymbiotic and nuclear origin—in such precursors of multicellular life as protozoa and algae, the eukaryotic cell had become more than the sum of its parts. A near-decomposition of the eukaryote into weakly interacting organelles, nucleus, and cytoplasm obviously breaks down whenever complex integrations of the contributions of the different cellular components are involved in particular biological functions. The evolution of the eukaryote from its symbiotic ancestors dramatizes how an interactionally complex system can evolve from nearly decomposable parts.

A conventional holist might be inclined to classify the eukaryote as a higher level, and its organelles, nucleus, and cytoplasm as lower levels—in keeping with a hierarchical perspective. Yet, however useful, such a classification could easily miss the point of the above discussion.

²Plasmids are virus-like particles which are known to carry DNA between closely related species of bacteria. Whether such massive transfers as found within the eukaryotic cell could be explained by such a mechanism is not known.

³Chloroplasts, which also have endosymbiotic origins and which, in some species, have transferred much of their DNA to both nuclei and mitochondria, have been observed conjugating with mitochondria! See Lewin (1984) for a discussion of the promiscuous relationships among the genes of nuclei, mitochondria, and chloroplasts.

⁴See Grivell (1983) for a discussion of the structure and evolutionary origins of the mitochondrial genome.

The eukaryote—at least in an evolutionary context—is not one thing. It has evolved in dramatic ways since its endosymbiotic origins, and this evolution has hardly depended only weakly on the evolution of its acquired organelles. Coevolution is a helpful term because it suggests mutuality. When whole and part interact that closely, one connotation of *hierarchical organization* becomes problematic. This is the connotation that the whole is a highly aggregated consequence of lower-level phenomena, and that the structure, behavior, and evolution of the whole may be exhaustively modeled at its own level independently of consideration of the internal workings, evolutionary transformations, and changing significance of its parts. In the case of the evolution of eukaryotic cells, this is not the case.

In the present analysis of interactional complexity we are indeed promoting a type of holism, but it is not the kind which is a halfism in disguise. It is natural to distinguish subsystems from one another and levels of analysis from one another. Without distinctions we recognize only chaos, and distinguishing parts from parts and parts from wholes will always have its place. If we assume, however, that parts do not change significantly in response to other parts and in response to wholes, and that wholes do not change significantly in response to parts, then we are dealing with frozen history—not evolutionary change. The organization of parts and wholes alike—and indeed, their identity—is subject to change in evolutionary contexts. New parts and new wholes may emerge.

Simon did not argue that radical structural change was not possible, only that it is not prevalent. Perhaps he might acknowledge our above example but deny the prevalence of coevolution beyond less integrated forms of interaction. Yet bacteria have apparently done it again and again. The precursors of chloroplasts were similar to modern cyanobacteria, and only gradually have become integrated into their eukaryotic hosts as obligate organelles. Like mitochondria, chloroplasts reproduce independently of the eukaryote, and give every appearance of semiautonomous existence. While it is true that algae and plants have had less opportunity to evolve than eukaryotes, so that chloroplasts have more of their original DNA than mitochondria, a close examination would undoubtedly reveal the profound forms of interactional complexity in algae and in plant cells, and in their evolution from eukaryotes with photosynthesizing guests.

Margulis (1981) has argued that the undulipodia (cilia or flagella) of eukaryotes also

have endosymbiotic origins. Spirochete bacteria which attached themselves to eukaryotes for food provided, through their waving motions, opportunities for eukaryotes to swim. Spirochetes may also have been taken into eukaryotes as endosymbionts, where they contributed to the evolution of mitosis. If this has been the case, nearly decomposable origins have resulted in such thorough integration of parts with whole that the very identity of the parts has become obscured.

While eukaryotes evolved from symbiotic societies of different species of unicellular organism, many multicellular and social forms of life arose from the progressive differentiation and integration of different members of the same species. We now consider the interactional complexity inherent in the evolution of the most primitive of multicellular forms, the cellular slime mold, and the most integrated of social forms, eusocial insect colonies.

A.2 Biological Complexification by Progressive Differentiation: Slime Molds and Social Insects

If eukaryote evolution is indebted to bacterial cooperation, multicellular life and social life are equally indebted, respectively, to the cooperation of cells and animals. An explication of two more of Wimsatt's illustrations of interactional complexity—slime molds and social insects—may serve to satisfy any hesitant Simon longing for simplicity that complexity is less an exception in biological evolution than it is a rule.

Cellular slime molds are part time multicellular organisms which lend themselves to important speculations about the origins of full time multicellular life. In the species which will concern us here (primarily those of the *Dictyostelium genus*),⁵ haploid amoebae live as individuals feeding on bacteria, until their food in that region of the soil runs out. Through the exchange of chemical signals called acrasins, starving amoebae are attracted towards a central mass of amoebae, where they begin to aggregate into a multicellular organism. The creature which forms looks like a small slug, and is called a grex. It crawls towards the light, where its anterior cells (about $\frac{1}{3}$ of the cells) move backwards through the grex to form a stalk, and the remaining cells form a fruiting body. During the formation of the fruiting

⁵Helpful reviews of these and related species may be found in Ashworth and Dee (1975) and Bonner (1982, 1983). For a more extensive review of the cellular slime molds, see Bonner (1967). A beautiful pictorial review of the life cycles of social amoebae may be found in **Science** **84**, pp. **72-77**.

body, haploid cells combine to form diploid cells, which in turn divide into haploid spores. The spores complete the life-cycle when they germinate in appropriate conditions to form free-living haploid amoebae.

Two aspects of the life cycle of social amoebae suggest evolutionary steps towards multicellular life. One is the differentiation into stalk cells and spores (in the ratio of approximately one to two). Since stalk cells die without the opportunity to reproduce, their altruism in favor of fruiting body cells indicates one of the Darwinian obstacles to multicellularity which they have overcome.

Secondly, the formation of spores in the fruiting body anticipates sexuality in that genetic variation is produced through crossing-over of portions of chromosomes and the random assortment of homologous chromosomes. Since crossing-over occurs in mitosis rather than meiosis, however, it does not give rise to as much recombination as plants, animals, and fungi⁶ have achieved. Moreover, the transition from diploid cells to haploid spores is inefficient, since it goes through an intermediate phase in which chromosomes are lost. Clearly, however, the parasexual cycle exhibited by many social amoebae represents an advance towards the sophisticated mechanisms of variation required of a minority of cells in the context of their participation in a multicellular organisms which consumes considerable energy to sustain its size and extended life cycle.

How did such a remarkable social agreement evolve? It seems unlikely that it evolved solely through selection for the advantages of an experiment in mechanisms of variation and spore distribution. Macrocysts form in many of the same species which form spores in fruiting bodies. Mitosis and meiosis are known to occur in macrocyst development (from a giant diploid amoeba which engulfs other amoebae), and would have provided adequate opportunities for experiments in variation mechanisms. And a stalk which provides spores with the advantages of height would be of value only in the context of an existent community of amoebae for whom established forms of reproduction proved inadequate.

Bonner (1958) has suggested that a multicellular phase could have arisen from a nutritional synergism. Individual amoebae that were deficient in certain nutrients would have found it advantageous to approach the membranes of their friends. To the extent that

⁶Protoctists include the unicellular protists (e. g., protozoa, algae, and Euglenas) and colonial or primitive multicellular forms difficult to classify in the other kingdoms (e. g., slime molds and sea weeds).

such a need were mutual (perhaps for different nutrients), the advantages of a multicellular phase are obvious.

Multicellularity could have led to the evolution of sharing mechanisms, then to experiments in nuclear combination and genetic exchange, and next to experiments in spore formation and motion towards the air. It is but a step further to the evolution of mechanisms of attaining height off the ground, such as the production of stalk material (e. g., cellulose). Selection for efficiency in the production of stalk material could have led to the evolution of specialized—and sacrificial—stalk cells. The reproductive phase could easily have been postponed through gene regulation until the air had been reached.

The story told here is just a likely or perhaps merely a feasible story, but some such sequence of advantageous intermediary evolutionary steps had to precede the evolutionary achievement of a stalk and fruiting body. For our purposes in analyzing the evolution of interactional complexity, it seems appropriate to classify the early nutritional synergism as a near-decomposition of amoebic subsystems, and the latter achievement of the slime mold life cycle as a clear case of interactional complexity. When cytoplasm and genes are shared, when cells work together in crawling behavior, when the differentiation of different cell types involves the sacrifice of some cells for the reproductive advantage of others, it is difficult to say that the slime mold is the behavioral sum of amoebic subsystems in external interaction.

An important contrast between the evolution of slime molds and the evolution of eukaryotic cells should be pointed out. While the bacterial precursors of mitochondria ultimately lost their autonomy and became inseparable components of their hosts, free-living amoebae have continued their bacterial feeding phase—complete with reproduction through mitotic division—much as before. In the evolution of eukaryotes, both higher and lower levels—the eukaryote, and say, its protomitochondrial guest—were progressively transformed as explained above. In the evolution of slime molds, however, the interlevel interactional complexity was not as complete, since the higher level changed identity but the lower level—the amoebae—did not (save for the genetic changes required for their aggregation, migration, differentiation, and fruiting body formation).

The situation is very different if we continue the story of evolution from part time multicellularity to full time multicellularity—as it occurred independently (and probably a number of times) in each of the nonprotocist eukaryotic kingdoms (plants, animals, and

fungi).⁷ The gradual shift of feeding behavior from the unicellular to multicellular phase, and the limitation of unicellular functions to germ cells, represents an extreme form of interlevel interactional complexity. Selection for the coordination of biological structures, functions, development, and behaviors at the multicellular level interacted with selection for reproductive fitness at the unicellular level, and both levels were so completely transformed that the multicellular species which emerged bore little trace of their protist ancestors.

The evolution of multicellular species from aggregating protists is only one of various feasible pathways from unicellular to multicellular life. Yet multicellularity must have arisen often enough from cooperative colonies to reveal the bias towards earlier stages of coevolution inherent in Simon's claim of widespread near-decomposability. While near-decomposability may capture the near-autonomy of cooperating protists, it fails to capture the degrees of integration characteristic of multicellular life.

If nearly decomposable colonies of bacteria and protists evolved, respectively, into interactionally complex protists and multicellular organisms, nearly decomposable societies of multicellular organisms on occasion evolved—most notably in the cases of social insects and humans—into interactionally complex societies. Human interactional complexity will be considered in Appendix C. A discussion of the evolution of social insects from solitary insects—Wimsatt's (1972a) third illustration of biological interactional complexity—will conclude the present appendix.

The *eusocial* (truly social) insects have been defined (Wilson 1971, 1975) as those groups (species, genera, families, etc.) which have attained at least three features of social organization: cooperative brood care, multi-generational communities, and behaviorally (and usually morphologically) distinct castes (including, at the least, reproductive and worker castes). These requirements have been met by almost all species of ants and termites and many species of bees and wasps.

The morphological similarities of social termites and solitary cockroaches, and the unusual dependence of both termites and cockroaches upon intestinal protozoans for assistance in digesting wood, suggest that termites evolved from cockroachlike ancestors. Contemporary species of wasp reveal a full spectrum between solitary and eusocial

⁷See Valentine (1978).

forms, and suggest what the evolutionary steps to wasp eusociality might have been like. Morphological considerations, and the existence of contemporary wasplike species (and in the case of primitive ants, occasional fossils), suggest the evolution of ants and bees from different superfamilies (scoliodea and sphecoidea, respectively) of wasp (Wilson 1975).

All eusocial insects have at least two castes: queens which have exclusive or major reproductive privileges and workers which care for their mother's brood and (in most species) contribute to the nest work and hunt or gather food. Most ants and termites also have soldier castes. Intermediate castes often exist, and temporal castes—which assume their roles at different stages of development—may contribute further to the complexity of insect societies.

Advanced forms of communication—as mediated, for example, by pheromones, the exchange of food or oral or anal liquids, touch, or symbolic movement—make remarkable social enterprises possible. The farming of fungi or yeasts by ants of the *Attini* genus, the massive migrations, bivouac formations, and marching behaviors of army ants, and the waggle dance of honeybees are among the best-known illustrations of the cultural fruits, as it were, of eusocial cooperation.

Because of the high degrees of social integration required for the more advanced eusocial developments, group selection—i. e., selection for the functional coordination and fitness of the colony as a whole—clearly superseded individual selection in evolutionary significance. Yet eusocial insects evolved in steps from solitary insects, for whom individual selection was of primary significance. Moreover, the transition to eusociality required that workers sacrifice reproductive privileges in favor of the queen. How did the transition from the individual selection associated with solitary life to the group selection characteristic of eusocial stages of evolution occur?

Wilson (1975) has suggested that two alternative types of evolutionary sequence—which he has called the *subsocial* and *parasocial* sequences—have been of major importance. The former route moves from intergenerational forms of community to cooperative brood care (with one generation of offspring assisting their mother in the care of one or more additional broods) and finally to the differentiation of reproductive and worker castes. The latter begins with members of the same generation cooperating in the care of a single brood, and is followed directly by the differentiation of reproductive and worker castes, and only finally

by intergenerational societies in which daughter workers assist their mothers (rather than their sisters) in brood care.

Although the above sequences are theoretical, there are convincing illustrations of each. Termites must have evolved from cockroaches along something of the first, or *subsocial* route. In order to digest cellulose, young termites must receive their intestinal protozoans from adult termites through anal feeding. Those adults were originally their parents, but to the extent that mature offspring remained with their parents long enough to witness additional broods of their parents, it would have been efficient for the first brood to assist in the anal feeding of the second. The success of this arrangement would have led naturally to the differentiation of queen and working castes. (Soldier castes no doubt evolved as specialized workers.)

The living species of wasps represent a full spectrum of degrees of sociality, and provide suggestive evidence of the likely *parasocial* sequence followed in the evolution of many species of wasps and bees.⁸ Paper wasps (genus *Polistes*) suggest an early stage in the sequence. Nests generally are founded by single females in late spring. By summer, from two to six additional females who have been unable to found their own nests have been observed to join the original foundress. The auxiliary foundresses often attempt to lay eggs, but the head foundress will try to obstruct their efforts, and will even eat any eggs that they manage to lay. Eventually, the ovaries of the subordinate females will regress.

The auxiliary foundresses are generally sisters of the foundress. They maximize their inclusive fitness (the probability that their genes will survive in their near relatives if not in their own offspring) by contributing to the survival of the foundress through such activities as gathering her food and later contributing to the care of her brood and the foraging tasks of the colony. Evidently this arrangement proved so useful that foundresses evolved the capacity to produce several broods which, like her sisters, were characterized by undeveloped ovaries and a willingness to contribute to the general work of the colony. Only toward the end of the summer do reproductive paper wasps emerge, some of whom—given the competition for nesting opportunities—are likely to assume secondary roles in the following nesting season.

By definition, eusociality requires a special reproductive caste the members of which

⁸Primitive bees are essentially wasps that collect pollen rather than hunting prey.

express—through their behavior and usually their morphology—different genes from their nonreproducing sisters. Such reproductive specialization was achieved by yellow jackets (or hornets), which are distributed widely through the temperate regions of the northern hemisphere as well as in tropical Asia.

It may seem as if the story we have told of the evolution of insect eusociality is more suggestive of evolutionary development toward greater near-decomposability than toward greater interactional complexity. Specialized castes, after all, seem to be far more independent than many of the same behaviors integrated in single solitary insects. Indeed, most of the traits found in the specialized insects are also found in their solitary ancestors (Oster and Wilson 1978). How, then, are the above conceptions of likely subsocial and parasocial evolutionary sequences intended to illustrate progressions from near-decomposability to interactional complexity?

The subsocial sequence which was illustrated above by an account of termite evolution began with an intergenerational anal feeding community from which specialized nurse and reproductive castes evolved. The parasocial portrayal of the evolution of wasp eusociality began with a phase of cooperative brood care and evolved into intergenerational forms of care. In both cases, forms of cooperative behavior preceded the evolution of highly differentiated worker and reproductive castes.

It is likely that caste evolution did not require—at least at first—vast accumulations of variability which were not present in the gene pools of the original solitary insects. It is more likely that the control of gene expression through selective feeding and orally or anally administered pheromones could have guided the development of larvae towards the size and hormonal features appropriate to their tasks. Insects were perhaps the first molecular biologists in their inadvertent but highly adaptive control of the physiological and behavioral development of their offspring.

Once the differentiation of castes had been given a head start through differential gene expression, there is no doubt that genetic changes followed which provided more effective and complete caste differentiation. Differentiated castes were only the beginning, however, of new evolutionary sequences which coordinated the behavior of such castes in service of the adaptations of particular species. Clearly, these later evolutionary sequences would have been guided by group selection more than by individual selection.

We propose, then, to identify two phases of development from near-decomposability to interactional complexity. Different generations of solitary insects are nearly decomposable in that the genotypes of offspring are determined by the reproductive *outputs*—i. e., fertilized, or in some cases unfertilized, eggs—of parents. The control of gene expression by the behavior of parents or nurses represents an increase in interactional complexity, in that phenotypes associated with different gene expressions may be viewed as a new level of nearly decomposable types which provides the context for the progressive coordination of caste behaviors in the service of specific adaptations. Waggle dances, agricultural, and disciplined armies are among the more dramatic—and interactionally complex—results.

A discussion of social insect evolution would be incomplete without mention of the importance of the haplodiploid system of inheritance shared by wasps, ants, and bees. Females are diploid—born from fertilized eggs—and males are haploid—born from unfertilized eggs. Because females are related more closely to their sisters than to their offspring, some inclusive fitness may be gained by workers who, rather than reproducing, assist in the care of subsequent generations of sisters. Yet only a small fraction of their sisters will ever become queens and reproduce, so that the calculation of inclusive fitness may not be straight forward. It is also possible that the queen benefits most from the arrangement at first, and only later is inclusive fitness maximized for the colony as a whole.

While it is beyond the scope of this review to consider any of the mathematical details (see especially Oster and Wilson 1978), we may observe that the importance of kin selection (selection for or against behavior which contributes or fails to contribute to the reproductive fitness of self or genetically related kin) in social insect evolution is another example of the evolutionary significance of interactional complexity. The total fitness of a group is not the sum of the fitnesses of individuals; it is approximated more closely by the sum of the inclusive fitnesses of individuals—the extent to which everybody, so to speak, is helped by everybody else. Group selection is perhaps the logical limit of kin selection; in a highly integrated group it makes sense to ask how all behaviors contribute to the survival, and perhaps to the reproduction, as it were, of the group. As a rule, we would expect group selection to emerge from the interactionally complex coupling of the behavior of some individuals and the survival and reproduction of others.

Our discussion of the importance of interactional complexity in social insect evolution

completes our review and explication of Wimsatt's three main illustrations of interactionally complex systems. There are many other biological illustrations of interactional complexity which we could have explored, including, for example, the complexity of intercellular interactions in embryonic development; between the hypothalamus and pituitary, or between numerous other functional areas, or between the neurons, of the human brain; or among species and their environments in ecosystems. The general pattern of biological complexity may by now be clear enough, however, to clarify the challenge of extending Wimsatt's model of complexity to physical and social systems.

We shall have to show how physical systems which neither reproduce nor evolve with the benefit of genetic memory, and social systems which evolve by choice as much as by variation and selection, are fruitfully analyzed in terms of strong subsystem and strong interlevel interactions. Physical and social interactional complexity will be treated, respectively, in Appendixes B and C below.

Appendix B

From Atomism to Interactionism in Twentieth Century Physics

The present appendix explores how the atomistic models or interpretations of theories espoused by many classical physicists and chemists gave way, in the twentieth century, to the nonmechanistic models of special and general relativity and quantum mechanics. (We also consider similarly complex astrophysical models of galaxies.) Although Wimsatt's model of interactional complexity (Wimsatt 1972, 1975; reviewed in Chapter III above) was developed with biological systems in mind, it applies equally well to complex physical systems. We shall explore the similarities between strong intersubsystem and interlevel interactions in physics and biology, and consider, in particular, how forms of physical complexity may be viewed as analogs of biological integration. This comparison will serve to extend Wimsatt's theory of interactional complexity to cover physical as well as biological systems.

B.1 Atomistic Presuppositions of Classical Physics

It is important to emphasize at the outset of this discussion that not all physicists and chemists of the seventeenth through nineteenth centuries were atomists in the classical sense of the term. Leucippus and Democritus are credited with founding the Greek school of atom-

ism in the fifth century B.C. They hypothesized that impenetrable corpuscles—constituted by varying amounts and shapes of a single homogeneous substance—were responsible for all change and macroscopic order through their continual collisions, combinations, divisions, and chaotic motion in a passive void.

In contrast, many classical physicists held alternative views, such as the dynamist belief in unextended point particles, the existence of noncontact forces and action-at-a-distance, the energetist espousal of various forms of potential energy and the reality of fields, and theories of continuous mechanical or electromagnetic ethers. These views respectively denied the atomistic principles of action by contact, the association of energy with corpuscular motion, and the passivity of space.¹

Not only did many classical physicists and chemists pull themselves away from the grip of atomism; often the same scientists entertained both atomistic and nonatomistic views. Graves (1971) has suggested that in such cases the contrasting views may have played different roles in interpreting (or motivating) formal theories. Graves borrowed Hesse's (1966) distinction between a *modell* and a *model2* to argue that many physicists who were informally committed to atomistic interpretations of their theories by way of familiar analogies which did not perfectly fit their theories (*models2*) were obliged—if they were to provide their theories with precise ways of applying to observations—to follow formal dynamistic idealizations (*models1*) which facilitated their calculations. Point particles and action-at-a-distance were mathematically tractable assumptions even for metaphysical atomists. Thus Newton consistently maintained both points of view.

Having acknowledged some issue as to the historical prevalence and centrality of atomistic commitments, we wish here only to make the following five claims: (1) that atomistically conceived building blocks are near-decompositions² par excellence, (2) that alternatives to atomism, such as dynamism, often bear a strong family resemblance to atomistic models and share their near-decomposability, (3) that the extended family, as it were, of nearly decomposable atomistic or quasi-atomistic models functioned as a world

¹For a review of dynamism, energetism, and fluid ether theories from the point of view of a philosopher of physics committed to their historical inadequacy as compared with atomistic perspectives, see Çapek (1961). Another philosopher of physics (Graves 1971) has argued for the historical superiority of dynamistic perspectives. A more even-handed review by a historian of physics of many of the conceptual developments in nineteenth century physics may be found in Harman (1982). A helpful historical outline of atomistic and related thinking from the Greeks to the mid-twentieth century may be found in Whyte (1961).

²See Chapter II for a review of Simon's theory of nearly decomposable systems.

view in the context of which the directions of research programs in chemistry, statistical mechanics, and atomic physics often were conceived, (4) that developments in special relativity, general relativity, and quantum mechanics early in this century clearly had contradicted the fundamental presuppositions of the entire family of atomistic and quasi-atomistic perspectives, and finally, (5) that recent developments in many areas of theoretical and applied physics appear to have established an interactionally complex world view within physics.

Strictly speaking, atomism presupposes extended but structureless particles which interact on the stage of empty space only by mutual contact, and influence only one another's motion. All complex systems or processes are constituted by such interactions. It follows tautologically from the assumption of *structureless* building blocks that they contribute through their combinations and motions to systems and processes as nearly decomposable subsystems, i.e., as subsystems that affect one another's states but not the details of one another's internal structures and processes. Whether such building blocks stick together in virtue of Democritean hooks or in virtue of gravitational or electrical forces, and whether their transfer of motion is understood intuitively or in terms of the conservation of momentum, neither the particles nor the space in which they move are altered through their interactions. Not only are atomistically conceived systems nearly decomposable; they are entirely decomposable into their atomic or elemental constituents. Position and its time derivatives are at once the only inputs and the only outputs of these elemental subsystems.

As we have indicated above, Newton was philosophically committed to an atomistic interpretation (model2) of both his mechanics and his theory of gravitation. His mathematical theory of gravitation, however, was not adequate to treat any transmission of gravitational disturbances through the collisions of imponderable etheric corpuscles which he presumed to fill the intervening space between massive bodies. Newton's official attitude was a positivistic denial of the impulse to fill the void imaginatively with unobservable causes in the form of etheric collisions and transmitted forces. He affirmed only the universal correlation between the gravitationally induced acceleration of a body and the mass of the gravitational source divided by the square of its distance; he did not *feign hypotheses* about the empirically inaccessible particles and forces which he imagined to cause the correlation. Moreover, since the calculus allowed him to consider separately the action of each differential portion of the mass of a source, and to superpose their consequences for a distant test body, in-

finitesimal masses replaced finitely extended masses in the model or formal interpretation of his theory.

Newton's dynamism, then, denied two principles of atomism. His mathematically tractable atoms were infinitesimal or point masses rather than extended; and his forces were propagated instantaneously across distances without a theory, at least, of mechanical mediation. Yet from the point of view of our interest in near-decomposability, distant point masses interacting instantaneously—at least in the context of Newton's linear law of gravitation—are conceptually similar to impenetrable masses interacting by contact.

The differential equation associated with $F = \frac{Gm_1m_2}{r^2}$ is Poisson's equation, which, for the special case of the empty space outside a set of gravitational sources, reduces to Laplace's equation. The latter is a linear differential equation such that any linear combination of solutions is also a solution. Thus the gravitational field produced by any set of masses at the location of a test mass will be equivalent to the vector sum of the fields which would have been produced by each of the masses acting separately. This is the case for a set of finite spherically symmetric masses acting effectively at their centers of mass, but it is also true for the set of differential masses within a finite mass which it may be necessary to consider in the case of complex spatial distributions of density.

The implication here is that in the general case of a distribution of mass density, the infinitesimal masses which constitute the source do not alter one another's contributions to the field produced at a location external to the source. If the gravitating source is considered to be the system, and its infinitesimal contributing masses its subsystems, the system is nearly decomposed in that interactions among the differential constituents of the gravitating system do not alter the field produced by that system over differential time scales. Clearly, however, if the source redistributes its mass over time as a result of internal gravitational interactions, the net field produced by the source at an outside location will also change over time. But such changes in external field intensity occur in the larger time scales over which the action of the source must be integrated. It remains that at any particular instant, the field produced by the source is the vector sum (calculated by a spatial integral) of the fields produced by the contributing infinitesimal masses.³

³For a helpful philosophical analysis of the implications of the linearity of Newton's law of gravitation, see Graves (1971).

Similar interpretations of the near-decomposability or quasi-atomicity of the dynamistic commitments of other physicists have been made in the philosophy of science literature.⁴ Regarding ether theories, Čapek has pointed out that those that were corpuscular preserved the solidity and contact requirements of atomism, and ironically presupposed an empty euclidean space in which etheric particles could move. Continuous ether theories on the other hand, such as those of Descartes and Maxwell, often contained ingenious but farfetched assumptions—such as Descartes' vortical transmission of motion and Maxwell's permanent electromagnetic vortices and mediating friction particles. The difficulties of characterizing material inhomogeneities, motion, and the transmission of momentum or energy in a space or ether that was presumed to be incompressible were insurmountable. Thus, while corpuscular ethers were quasi-atomistic, continuous ethers didn't work (Čapek 1961).

Other ontological commitments were clearly opposed to quasi-atomistic models of physical theories. Faraday's view of lines of force as material substance, for example, suggested a continuous ontology which had little resemblance to atomistic models, and provided no basis for natural near-decompositions. Natural decompositions depended, at the least, on discrete views of nature. Continuous ethers and fields are ruled out, but dynamistic models of point particles interacting at a distance, and models of corpuscular ethers which mediate or constitute gravitational and electromagnetic interactions, clearly meet our ball park requirements. Whenever particles affect one another's motions, but not one another's essential characteristics (such as mass or charge) or internal structures or processes, we may be safe in presuming near-decomposability.

Nearly decomposable views were not only important in the interpretation of physical theories, they were important in motivating research. Dalton's conception of atomic weight preceded by more than a century Rutherford's scattering experiments which revealed the distinction between atomic weight and atomic number. Dalton's prophetic clarity did much to provide the context for the intervening century of chemistry. Classical atomism lay at the foundation, of course, of the kinetic theory of gases and statistical mechanics.⁵ And despite Planck's early denial of atomism, and Einstein's later misgivings about quantum mechanical

⁴See, for example, Čapek's (1961) discussion of Boscovich's theory of point sources of force which repel one another up close and gravitationally attract, instantaneously, at a distance. Čapek argues that while force seems to replace solid mass on Boscovich's view, his point sources act just like atomistic corpuscles in maintaining their isolation within certain radii.

⁵For a comprehensive review of the coevolution of statistical physics and the atomic theory of matter, see Brush (1983).

uncertainty, Planck and Einstein respectively explained discrete black-body radiation and the photoelectric effect in terms of a universal quantum of action (h) and quantized photons. These later developments, together with the work of Rutherford, Bohr, and others in atomic physics, represent the culmination of the atomistic research program foreseen by the ancient Greek atomists, and at the same time, initiated (together with Einstein's special and general relativity) the decline of the atomistic world view.

Before proceeding to consider the interactionally complex ontologies inherent in twentieth century physics, it may be helpful to draw attention to the twofold irony which motivates the present comparison of the complexification of the respective world views of physics and biology. First, hierarchical holists—who were found more often among biologists than physicists—often lumped positivists and atomists together (in characteristic holistic fashion) as interlevel reductionists, and therefore, as the enemy. We have shown in Chapter II above how positivists came in the end to acknowledge a hierarchy of nearly decomposable theoretical levels. Atomists—whose *ontological* reductionism was more uncompromising than the *epistemological* reductionism of positivists⁶—were not only hierarchicalists, their hierarchies were of the most completely decomposable kind. The hard impenetrable spheres of classical atomism were subsystems which had no internal structure and provided all the information required—in principle and not only in practice—for calculations of their interactions. And when matter was conceived as continuously divisible without limit, as in the formalism of Newtonian mechanics, the calculus guaranteed that subsystems defined as differential masses and integrated as linearly combinable sources provided maximal information in the context of a given distribution of density. The irony here is that in justifying their hesitation to look inside their subsystems, holists played a game of near-decomposition at which they had long ago been beaten by their atomistic enemies.

The second irony is that holists were so intent on justifying the irreducibility of the biological to the physical sciences that they neglected the importance of strong interlevel interactions until perhaps the 1970's; yet far earlier in the century, physicists and their positivistic advocates had been forced by developments in special and general relativity and

⁶To the extent that *atoms* or any other ultimate building blocks were unobservable, early positivists denied their proper representation in formalized theories, and later positivists considered such representations to be of only instrumental value. The epistemological reductionism of positivists is associated with models of the reduction of theories (i.e., our knowledge) to theories of greater generality or scope. The ontological reductionism of atomists was the claim that systems—quite independently of our theories—are composed of fundamental building blocks.

quantum mechanics not only to avoid the pitfalls of atomistic realism, but to acknowledge such interactionally complex couplings as those of matter and energy, space and time, mass-energy and spacetime, position and momentum, energy and time, and the observer and the observed. Thus, even as atomistic physicists were hierarchicalists before holistic biologists, positivistic physicists had outgrown hierarchicalism—at least *within* physics⁷—before their holistic critics.

The above two ironies suggest a fundamental perspective of the present thesis. The tension between those who would model the universe from the bottom-up and those who would model the universe from the top-down is less significant, in retrospect, than the stages of development which they have in common. Hierarchical analysis predominates at earlier stages, and eventually gives way to more sophisticated analyses of interlevel and intersubsystem transactions.

We now consider how special and general relativity and early developments in quantum mechanics challenged the hierarchical world view.

B.2 Interactional Complexity in Special and General Relativity

In special relativity, space and time are mixed, contradicting the atomistic assumption of their independence. The interconvertibility of rest mass and kinetic energy in special relativity contradicts the atomistic assumption that there is no feedback from the external interactions of particles to the internal structure or identity of particles. Without such feedback neither nuclear synthesis in stars and supernovae nor a nuclear holocaust would be possible. The near-decomposition between the temporal and spatial scales of social evolution and of cosmic evolution, such that the latter is taken to provide boundary conditions for the former, would be broken, according to special relativity, by speeding space travelers who could experience hundreds of thousands of years of galactic evolution as measured from earth in the course of their lifetimes. Special relativity permits, in principle, strong interlevel

⁷In contrast to their interactionally complex perspectives within physics, positivistic physicists became increasingly committed to the hierarchy of theoretic levels, as explained in Chapter II above.

interactions between the galaxy and its human constituents.⁸

General relativity also implies forms of interactional complexity. Objects orbiting a strong gravitational source, such as a black hole, would experience time dilation and space contraction effects similar to those associated with high velocities. More generally, masses would not interact in a passive void, but rather would warp the very spacetime in which they move. More precisely, the nonlinearity of the field law in general relativity implies that the field produced by a system of masses is not equivalent to a linear superposition of the fields which would have been produced by each of the contributing masses acting separately. Thus, general relativity points to strong interlevel interactions between spacetime and its component masses.⁹

B.3 Interactional Complexity in Quantum Mechanics

During the late 1920's, the rapid sequence of revolutionary developments in quantum mechanics was responsible for a progressive deterioration of the atomistic world view. Shortly after the development of matrix mechanics by Heisenberg, Born, and Jordon (Heisenberg 1925; Born and Jordon 1925; Born, Heisenberg, and Jordon 1926), the mathematics of Schrdinger's (1926a) wave mechanics proved more suggestive of physical interpretations or models. Schrdinger (1926b) was the first to propose a physical interpretation (or model²) of his wave equation as a distribution of matter throughout a region of space. In contrast with Schrdinger's wave interpretation, Born (1926a,b,c) proposed a particle interpretation of wave mechanics in which particle behavior was taken to be irreducibly probabilistic. Heisenberg's (1927) indeterminacy principle or uncertainty principle established a lower limit on the respective binary products of the uncertainties in position and momentum, energy and time, and angular position and angular momentum. And finally, Bohr's (1928) complementarity principle, which was to provide the basis for the Copenhagen interpretation of quantum mechanics, attempted to integrate the strict determinism associated with the evolution in time of Schrdinger's (1926a, part I) time-dependent wave function (in the context of given initial conditions and a known Hamiltonian operator for the entire system) and the unavoidable

⁸For a clear discussion of the implications of special relativity for space travelers, see Martin (1985).

⁹See Graves (1971) for helpful discussions of the implications of the nonlinear field law in general relativity.

uncertainty associated with observations of particular spacio-temporal realizations of the wave function.¹⁰

From the point of view of our concern with the decline of the atomistic or hierarchical world view within physics, we offer the following interpretation of the above developments in quantum mechanics. Schrödinger's realistic interpretation of his wave function preserved Laplacian determinism, but not the atomism which Laplace presupposed. Atomistically conceived matter is homogeneous, while the probability amplitude of the wave function may vary with position. Moreover, because of interference effects, the probabilities associated with a system of particles—or more dramatically, perhaps, with a set of alternative wave functions for a single particle—are not the sum of the probabilities (or amplitudes squared) associated with the *component* wave functions. The wave function of a system is not nearly decomposable into the wave functions of its parts.¹¹

Born's probabilistic interpretation of quantum mechanics and Heisenberg's uncertainty principle deprive particles of the simultaneous specifiability of their momenta and positions and energies and times presupposed by atomism and Laplacian determinism. And Bohr's complementarity principle assigns to the observer the responsibility of determining whether the system in question must be understood as a deterministic propagation of probabilities or a probabilistic (or fuzzy) event. The observer has no such active role in the atomistic tradition, nor in the realistically conceived decompositions of system science.

In order to characterize convincingly the development of authentic interactionally complex perspectives in representative areas of physics, and not to give the impression of simply exposing the downfall of hierarchical perspectives, it will be helpful to consider a number of more recent developments in this century's physics. While work in these areas remains speculative, sufficient results have been obtained, in the opinion of this writer, to make it unlikely that the degrees of complexity which have been identified will be eliminated in any subsequent theoretical developments.

In quantum chromodynamics, hadrons (either two-quark mesons such as pions or three-quark baryons such as protons and neutrons) are not nearly decomposable into their

¹⁰For helpful reviews of the revolutionary developments within quantum mechanics, see Čapek (1961), Jammer (1966), Pais (1982), and Brush (1983).

¹¹For discussion of the properties of wave functions, see (Feynman et al. 1965).

constituent quarks. Unlike the exchange of virtual photons between electrically charged particles in electromagnetic interactions—which alters the momentum but not the electric charges of the participating particles—the exchange of gluons between quarks in strong interactions does alter the color charges of the participating quarks. While the virtual photons exchanged in electromagnetic interactions do not carry any electrical charge, the gluons exchanged in strong interactions do carry color charge (actually combinations of a color charge and an anticolor charge). Moreover, quarks reabsorb many of the gluons which they emit, allowing but a fraction of their emitted gluons to be absorbed by other quarks. Yet remarkably, the exchange of gluons among the two or three quarks of hadrons is instantaneously coordinated to result in a white or neutral color charge for the hadron as a whole. Thus interactionally complex interactions of the quark subsystems of hadrons alter the very identities (or at least the essential attributes), and not simply the momenta, of quarks; and *strong* interlevel interactions between hadrons and their quark subsystems coordinate the identities (i.e., the color charges) of quarks to produce colorless hadrons.

The weak force, which plays an essential role in beta decay and in supernovae, is the mediator of a more dramatic form of interactional complexity than the strong force between quarks. In beta decay, for example, a neutron decays into a proton, electron, and antineutrino through the creation and subsequent decay of a virtual W particle. In the initial interaction, the neutron is transformed into a proton when a down quark decays into an up quark and a W particle. In general, the weak force mediates fundamental transformations of identity within the quark and lepton families, altering the flavors, electric charges, and masses of quarks and the electric charges and masses of leptons. (The lepton family includes, for example, electrons, positrons, neutrinos, and antineutrinos.) The weak force is responsible for interactionally complex interactions between the quark subsystems of baryons and between different types of lepton.

In the more recent and as yet unconfirmed theories of quantum mechanics, quarks and leptons become transformable into one another (in GUTs or grand unified theories) and fermions and bosons become unified (in supersymmetric theories). In the simplest GUT, X particles convert quarks and leptons into one another, and are hypothesized to mediate the rare decay of a proton into a positron and pion. Two quarks must approach each other within a very small distance (approximated at 10^{-29} cm) for the exchange of so massive a boson as the X particle (estimated at 10^{14} times the mass of the proton). At

almost vanishing temporal and spatial scales, quantum mechanical uncertainty permits the spontaneous appearance of the mass and momentum of the X particle (of the appropriate electric and color charges) required to jeopardize the presumed near-decomposition between quarks and leptons.

At still smaller temporal and spatial scales, all the fermion building blocks of the universe (quarks and leptons) and the boson exchange particles—which carry the gravitational, strong, weak, and electromagnetic forces—are unified in the supersymmetric theories. In such theories building blocks and exchange particles are interconvertible, and the possibility of feedback effects between exchange particles and the very identity of the particles responsible for their emission brings into question any near-decomposition between the particles interconnected by any particular force.¹²

Of course, the transformational events predicted by GUTs and supersymmetric theories are extremely unlikely events, and it would be natural to wonder whether such effects could contribute significantly to the structure of our universe. It happens that the most exciting applications of the emerging unified theories are to the early evolution of the universe in which the small spatial and temporal scales allow the transformations predicted by the unified theories to have profound effects on the evolution of the structure of the universe. For a fascinating account of both the new unified theories and the theories of the strong, weak, and electro-weak forces which preceded them, see Davies (1984). Other helpful reviews of recent developments in quantum mechanics may be found in t'Hooft (1980) and Quigg (1985).

If even the most fundamental subsystems discovered or hypothesized by physical science are likely to suffer changes in their fundamental properties and in their identities in the appropriate circumstances, it is possible that the atomistic, hierarchical, or nearly decomposable view of the world requires revision. Yet it is conceivable that transformational complexities at subatomic levels might average out to produce statistically stable molecular and macroscopic subsystems into which the larger systems of our biological, social, geophysical, and cosmic environments might be nearly decomposed. We have shown in

¹²Note that general relativity—even if it is neither quantized nor unified with quantum mechanical theories of the nongravitational forces—requires that the gravitational field exhibit both active and passive gravitational properties. Unfortunately, the energy associated with the gravitational field has never been defined adequately, so that it has not been possible to calculate directly the contributions of gravitating gravitational fields to the gravitational interactions of a system of masses.

Appendix A above how this is not necessarily the case for biological systems, and we shall show in Appendix C below how it is not necessarily the case for social systems. A treatment of the interactional complexity of spiral galaxies will complete this appendix below. An analysis of the interactional complexity of a single system (the climate system) with (interactionally complex) physical, biological, and social components may be found in Chapter IV above.

B.4 Interactional Complexity in Spiral Galaxies

Both the birth of stars in the spiral arms found in many younger galaxies, and the mechanisms which prevent the gravitational collapse of spiral arms about the dense central regions of the galactic disk, are likely to have explanations which involve forms of interactional complexity. As atomic hydrogen and dust particles such as graphite and silicates revolve in roughly circular orbits in the galactic disk, the areas of relatively dense spiral arms are intersected. The gas is condensed and molecular hydrogen—together with trace amounts of at least 53 other types of molecule (Blitz 1982)—is formed and provides the material from which new stars are born. Interactional complexity may be associated with the role which stars may play in the birth of other stars in molecular clouds (a strong intersubsystem interaction) and the role which stars may play in the maintenance of spiral arm stability (a strong interlevel interaction).

There are a number of theories of star formation, not all of which assign catalytic roles to other stars. On one view, for example, several molecular clouds may collide, propagating waves of density (shock waves) which trigger the formation of new stars (Scoville and Young 1984). Other views presume spontaneous star formation in areas of sufficient density, such as in globules of gas which have relatively dense cores (Bok and Bok 1981). On two other views, however, the ultraviolet emissions of young massive stars and the shock waves produced by supernova explosions of massive stars respectively may trigger the condensation of molecular gas to form one or more protostars.¹³

The former of the above interactionally complex views is derived from the observed

¹³Helpful reviews may be found in Bok and Bok (1981), Blitz (1982), Kippenhahn (1983), and Scoville and Young (1984).

proximity of H II regions and star formation in adjacent areas of the surrounding molecular cloud. The ultraviolet emissions of massive stars dissociate molecular hydrogen (H_2) into neutral atomic hydrogen (H) and subsequently ionize molecular hydrogen into its constituent protons and electrons. When protons and electrons recombine (temporarily until they are again disassociated), they emit radiation which makes the nebula visible. That same radiation, combined with the particle wind of the massive star, produces a shock wave in the surrounding molecular cloud which is thought to trigger star formation.

The latter theory assigns a catalytic role to dying massive stars rather than to newborn massive stars. When a sufficiently massive star exhausts its nuclear fuel and its core collapses to such great densities and temperatures that protons and electrons combine to form neutrons, the energy released throws off the outer shell of the star in a supernova explosion. This dramatic event produces a shock wave which is thought to trigger the formation of new stars.

The above four views—and other possible explanations of star formation—are not necessarily mutually exclusive. Different mechanisms of star formation may operate in different circumstances. If we may assume for the sake of our argument the likelihood that star formation resulting from both *nebular* and supernova shock waves does occur, we may conclude that such radical subsystem changes as star birth and star death can be significant in producing another radical subsystem change—the birth of a new star or stars. When changes in subsystem identity interact with other changes in subsystem identity, the interactions between subsystems are of maximal *strength*, and the system (in this case a molecular cloud region of a spiral arm) is anything but nearly decomposable into its constituent subsystems (say stars and gas).

When we consider the mechanisms which might explain the longevity of spiral arms, interactional complexity again seems to be involved. Two of the current views respectively ascribe spiral arm structure to density waves produced by gravitational perturbations arising from the galactic nucleus or from a neighboring galaxy or giant cloud, and to the opposition to the contraction of molecular clouds achieved by supernova explosions. If (lower-level) stellar events such as supernovae in galactic cores, or alternatively, in spiral arms, serve to maintain a higher-level feature of galaxies—namely spiral arms—interlevel interactional complexity is involved. And if two galaxies interact to maintain the spiral arm subsystems of

one of them, the feedback between intergalactic interactions and internal galactic structure—presumably on the same time scales—represents strong interlevel interactions between galactic and subgalactic levels of organization.¹⁴

Other astronomical interactional complexities could be considered, including the interaction between binary stars which results in nova explosions of material received by a white dwarf from its more massive companion, and interactions between galaxies which replenish the interstellar gas of a depleted galaxy. Our above considerations may be sufficient, however, to dramatize the importance of strong subsystem and strong interlevel interactions in galaxies. We may observe in conclusion that just as worker insects do not reproduce but contribute to the reproduction of the caste structure of social insect colonies, massive stars do not reproduce, but they contribute—whether through their energetic early lives or their energetic self-destruction—to the reproduction of characteristic patterns of star formation and galactic structure. Star development is at least an autocatalytic process;¹⁵ and if star death triggers star birth, the autocatalysis involved bears an interesting resemblance to the so-called altruism analyzed by sociobiologists. Stars might not have survived to populate our entire cosmos were it not for the continual violent demise of a minority of their population. Perhaps universes in which stars don't catalyze their own formation don't survive.

Biological analogies aside, many physical systems that are thermodynamically open to substantial sources of matter or energy exhibit self-organizing behavior similar to that of galaxies. The interdependence of phenomena defined at different spatial or temporal scales, at different levels, or internal to different subsystems, seems to be as necessary for the evolution, behavior, and structure of physical systems as it is for that of biological systems. While strictly physical systems do not, by definition, encode their structures in genes passed on to subsequent generations, they often do metabolize in the sense of dissipating energy to perform work, and they often must achieve high degrees of interconnectedness in order to survive. If we are insightful enough to identify the structures or patterns that codetermine one another's evolution, interactionally complex interactions among these structures and patterns are easy to find.

¹⁴For reviews of possible explanations of spiral arm structure, see Bok and Bok (1981) and Scoville and Young (1984).

¹⁵See Eigen and Schuster's (1979) theory of role of autocatalysis in precellular molecular evolution on earth.

Appendix C

Interactional Complexity in Science-Society Interactions

One of the more tempting forms of near-decomposition (Simon 1962, 1973, 1976; reviewed in Chapter II above) is associated with the assumption that factual and value judgments are relatively independent, and that in policy-making contexts, different groups—say scientific and technical experts on the one hand and policy-makers or citizens on the other—are or ought to be responsible for making the different judgments. The expert subsystem generates outputs in the form of judgments of probable impact (e.g., estimates of benefits and risks) associated with the various policy options; these outputs provide inputs to the policy-making or voting subsystem; and the latter valuational subsystem chooses among the policy options in the light of their likely consequences as predicted by experts.

Such a near-decomposition is not only an academic exercise. Policy analysts often develop procedures to help policy-makers distinguish carefully between the judgments of knowledge and value associated with the development of accountable policies.¹ Unfortunately, such procedures often obscure how value judgments can be presupposed by experts and how the familiarity of policy-makers and citizens with the importance of various types of impact can influence the impact dimensions for which they solicit scientific or technical judgments.

¹For a philosophical review of many of the fact-value problems associated with applications of social science to public policy procedure, see Rein (1976). These and related issues are often treated in detail in the journal *Social Epistemology*.

The inadequacy of near-decomposition assumptions can be remedied in policy analysis situations by a more realistic interpretation of the relationship between the expert and valuational components of decision-making. Wimsatt's model of interactional complexity (Wimsatt 1972, 1975; reviewed in Chapter III above) dramatizes the importance of intersubsystem and interlevel interactions. A policy analysis in terms of interactional complexity would draw attention to the need for interactions between the decision-making processes of experts and policy-makers or citizens.

The paper reprinted below (with the kind permission of the publisher) analyzes the interdependence of facts and values in risk analysis, with special attention to the challenge of assessing the risks of Denver's air pollution. Together with the reviews, respectively in Appendixes A and B above, of interactional complexity in biological evolution and theoretical physics, the following paper on interactional complexity in risk analysis should make it clear that Wimsatt's theory of interactional complexity has broad applications in the physical and social, as well as the biological sciences. A case study of a single system that integrates—in an interactionally complex way—physical, biological, and social forms of interactional complexity—namely the climate system—may be found in Chapter IV above.

C.1 The Risks of Predicting Risks: The Case of Denver's Air Pollution^{*†}

C.1.1 Introduction

A creature which behaved in the same way in all circumstances would not live very long. Life is made possible by the variety of responses (genetic, learned, or societal) which organisms have at their disposal. The organism must *predict* the types of challenges and opportunities it might have, and must be prepared to respond appropriately. Looked at in this way, the information stored in genes, brains, and cultures makes it possible for living things to hedge against the ever-present threat of injury or extinction. *Prediction* helps the organism to minimize environmental risks.

Humans have always been especially gifted in their predictions. We not only learn from our own experience, but also from the experience of others. Communal experience is accumulated and reorganized from generation to generation. It is recorded and communicated over distances of time and space. And it gains especially precise reference and reliable predictive ability in the various sciences. The result is the unprecedented predictive ability of our species to anticipate possible futures and to respond with sensitivity to their differences. Scientific prediction is insurance against environmental, social and economic risk.

One special case of human predictive ability has backfired, however. Although we have been very successful at predicting the utility of many technological innovations, we have often failed to anticipate the adverse side-effects of these innovations. Automobiles provide an excellent means of transportation, for example, but take many lives in crashes and pollute the air with emissions which may endanger human health. Agricultural technology increases agricultural productivity; but when such modern technology is transferred to less developed countries, it often creates tragic social and economic disruption. Or perhaps most

^{*}Reprinted from Goldberg, L. P., 1978: *The Risks of Predicting Risks: The Case of Denver's Air Pollution*. In Risk Studies Foundation (ed.): **The Future of Risk**. Risk Studies Foundation, New York, pp. 157-184.

[†]Originally prepared as the final report of a collaborative project involving the National Center for Atmospheric Research, the Center for Research on Judgment and Policy at the University of Colorado, and the Colorado Air Pollution Control Commission.

dangerously, the carbon dioxide pollution from the burning of fossil fuels (i.e., coal, oil and gas) may absorb large quantities of heat which would have radiated into space, and thereby cause a global warming.

While our technological activities are growing exponentially, our ability to predict the adverse impacts of these technologies remains very limited. Our scientific knowledge of the consequences of our activities often lags far behind the scientific and technological knowledge which made these activities possible. It was decades after we began using chlorofluorocarbons in aerosol spray cans and as refrigerants, for example, that we discovered the serious danger of releasing these chemicals into the atmosphere. Chlorofluorocarbons may accumulate in the stratosphere where they result in decreases in the concentration of ozone. This could result in increases in the incidence of skin cancer due to the increased quantities of ultraviolet light transmitted through the ozone layer, to say nothing of possible adverse effects on climate. It is only in the last few years, after the recent *predictions of risk*, that the use of chlorofluorocarbons in aerosol spray cans has been restricted (Kates 1978).

Even when a possible hazard has been identified, however, we are rarely certain of the exact degree of impact to be expected. Air pollution undoubtedly affects the health of people with cardiovascular or pulmonary disease. But just how much it does is a very controversial question (Leung et al. 1977). One of the greatest challenges of risk assessment is the interpretation of scientific uncertainty. Just how certain do scientists have to be before we take their predictions seriously? And just how terrible does a *possibility* have to be before we take serious steps to prevent it?

The first of the above questions regarding the risks associated with uncertain predictions has a traditional answer. Risk is usually defined as the probability of an adverse impact times its degree of severity (Lowrance 1976). Problems arise, however, when scientists disagree in their probability estimates. It is here that a variety of techniques, such as science courts (Bross 1973) and Delphi methods (Delbecq et al. 1975), have stirred up no little controversy. The second question, regarding what to do about risks, cannot be answered without a value judgment. The prevention of risks often has costs and requires sacrifices of benefits. Whether or not the risk-prevention in question is *worth it* is a value judgment which must be made in the light of the predicted impacts associated with each of a

variety of feasible policy options. A variety of methods has been developed to assist policy-makers or citizens in making such value judgments. Some of the better-known methods are reviewed in Kaplan and Schwartz (1977) and Hammond (1978). But none of these methods can substitute for the evaluation of alternative futures. Nor do they solve the problem of who is responsible for the evaluation. Whenever risks to minority populations are different from risks to the general population, problems as to how to represent these minorities on the *evaluation committee* are unavoidable.

The *risks of prediction* result from the problems of knowledge and values considered above. Uncertain knowledge and unclear responsibility for the evaluation of alternatives set the stage for bias. For example, scientists will often hesitate to offer professional judgments when large uncertainties are involved. The result is that very substantial risks (probability times severity) are often ignored. When this happens we are, in effect, substituting the values of scientists—who in this case believe that *certainty* is more important than *severity*—for the values of the society. Another type of bias results from democratic representation in the evaluation of risk predictions in the context of especially severe risks to minority populations. Here we are substituting the values of a majority for those of the minority which is severely affected. The inequities which may result are a *risk of prediction* in that predictions often do not attempt to specify impacts on sensitive minorities.

These two examples of potentially misleading prediction (or failure to predict) suggest the general position of this paper regarding the risks of prediction. *It is what experts fail to predict, but could have predicted, which constitutes the major risk of prediction.* Since scientists could easily overwhelm the public with irrelevant information, however, the solution to our problem is not to predict everything under the sun. Rather, it has to do with the quality of communication between those who make the predictions and those whose plans depend upon them. In the long run, minimization of the risks of prediction requires the scientific education of the public and the public support of needed research. In the context of particular policy-making contexts, minimizing the risks of prediction requires that we maximize our awareness of the issues in choosing among alternative prediction efforts.

The reader may at this point wonder whether I have neglected a *risk of prediction* which has made headlines in recent years. There has been much concern about whether it

is wise to make earthquake predictions in the light of the possibility that loss of life and property resulting from people's response to the prediction could be greater than the losses that would have resulted from the earthquake itself. The problem here may seem to be one of people's *response* to prediction rather than one of what to predict. But if we look more closely, the risk of earthquake predictions results from inadequate predictions of the social response to the prediction and of the social and economic consequences of this response. This case dramatizes the risks of making some predictions to the exclusion of others. It also indicates that risk assessment is an interdisciplinary affair. This may suggest further risks of prediction associated with the difficulty many scientists have in cooperating across disciplines.

What follows is a study in some depth of problems arising in the prediction of the health effects of Denver's air pollution. It is necessary, I think, to examine the details of at least one complicated assessment context in order to appreciate fully how much choice there is in what to predict, and how much risk may result from making such choices carelessly.

C.1.2 Predicting the Health Effects of Denver's Air Pollution

Denver's air pollution is primarily due to automobile exhaust. (There is not much polluting industry in the Denver area.) Low winds, mountains to the west, a river valley, and severe wintertime temperature inversions all help to trap automobile emissions in the Denver area while they accumulate. Denver's high altitude results in less efficient combustion of gasoline than at sea level, and therefore in greater emissions of pollutants per vehicle-mile traveled. The high altitude may also increase the severity of health damages from air pollution suffered by people with cardiovascular or pulmonary diseases.

Wise policy making regarding Denver's air pollution requires: (1) the identification of feasible policy options; (2) the predictions of the air quality consequences of the respective policy options; (3) the prediction of the likely *health impacts* of the air pollution levels associated with the respective policy options; (4) the prediction of other social or economic impacts, such as the convenience or economy of transportation, of the respective policy options; (5) the prediction of the acceptability of the various policy options (in the light of their respective impacts as predicted in (3) and (4)) to Denver's citizens, and (6) the

judgments of policy-makers regarding the acceptability of the various policy options (in the light of (3), (4), and (5)).

Assessment of policy options for air pollution control is thus a very complicated process. Note that (2) through (5) above require formal prediction. In this section, we will consider in depth the prediction of the health effects of Denver's air pollution ((3) above). It may serve to illustrate many of the general problems of *choice* in prediction. An awareness of these problems may suggest ways of minimizing the risks of prediction—or maximizing the *rationality* of our choices of what to predict.

C.1.3 General Limitations in Toxicological, Clinical and Epidemiological Research

The three basic areas of research which investigate the health effects of air pollution are toxicological, clinical and epidemiological. The standards of scientific acceptability in these areas vary with the goals of the respective types of research. Toxicological research is physiologically the most rigorous, as it seeks to trace the physiological consequences of pollution exposure in a causal series beginning with the site of original contact and ending with developments that manifest themselves as *acute* or *chronic* health effects. As most toxicological studies must be conducted on laboratory animals, however, contribution of this research to the understanding of human health effects of pollutants is often limited. Even in cases of similar physiological mechanisms in experimental animals and humans, the doses required to trigger the respective mechanisms would, in general, be quite different.

Clinical research treats the subject more like a black box of unspecified workings than like a physiological mechanism. Because clinical research doesn't have to poke around inside the box too much to get its results, it can ethically use human subjects. It can administer pollutant doses which are as precisely controlled as those administered in toxicological studies, although it cannot, of course, administer doses which would seriously endanger the health of the subjects. It looks for *acute responses*, such as respiratory symptoms or blood carboxyhemoglobin levels, which are readily measurable and indicate health status. For practical and ethical reasons, clinical research cannot, of course, investigate the effects of years of laboratory doses on the incidence and prevalence of chronic disease. It does permit,

however, the investigation of pollutant effects on people who are already suffering from chronic disease. This is especially important, as people with pulmonary or cardiovascular diseases are often those most highly at risk of health damage from air pollution.

Epidemiological research is not concerned with individual black boxes or with physiological mechanisms. It is rather concerned with large numbers of black boxes. Epidemiology is the comparative study of human populations. In the context of the investigation of the health effects of air pollution, epidemiological studies either compare the health effects of different pollution levels at different locations, or compare the effects across time at a particular location. They have the advantage of investigating the consequences of actual pollution situations, rather than the consequences of artificial laboratory atmospheres. As we shall see later, experimental results based on synthetic atmospheres can be very misleading. Epidemiological studies have the disadvantage, however, of often not allowing adequate control of variables. It is a possibility, for example, that a correlation between days of high pollution and the number of heart attacks has more to do with the air temperature or with the stress of driving cars than with the exhaust from the cars. Other conceivably confounding variables include unmonitored pollutants which affect health more than those monitored, and synergisms among pollutants or between pollutants and various commonly used drugs (e.g., tobacco, alcohol, or barbiturates). In epidemiological studies of pollution and heart disease in several cities, a correlation between high pollution and prevalence of heart disease may, once again, have a variety of explanations. Smoking habits, life style, medical practices, genetic factors, or any of a number of other circumstances, may contribute more to heart disease than high pollution contributes. Because of the complex ways in which populations in different cities differ, or the variable ways in which people may behave when they are making different levels of pollution, it is very difficult to establish causal connections between pollution and health on the basis of epidemiological studies alone. It should also be observed that epidemiological studies can be no more accurate than the public or institutional records of death or illness on which they are based.

It can be seen that a modeler attempting to provide an accurate prediction of the health effects of air pollution will have three very different types of scientific resources to draw upon, each with distinctive merits and limitations. Furthermore, just which research the modeler should draw upon depends heavily upon the purposes for which the model is developed. Several contexts in which choice among alternative types of research is required

of the modeler are considered below.

C.1.4 Synergisms and Laboratory Atmospheres

There is fairly good agreement in the clinical literature that the threshold dosage for acute human response to ozone (an automobile-related pollutant of significant concentrations in Denver, especially in the summer months) is approximately 0.37 ppm. This seems to be the case even for sensitive populations such as asthmatics, and for exposures of up to four hours (Hackney et al. 1975). It is tempting to conclude that Denver concentrations of ozone—which have so far been below 0.20 ppm (Air Pollution Control Commission 1977), and are expected to remain near or below 0.20 ppm in the foreseeable future (Stedman 1978)—are not a threat to human health. Are we justified, then, in assuming that ozone does not constitute a health hazard in Denver?

We cannot, unfortunately, come to this conclusion. We must always consider the possibility that several pollutants affect health synergistically (i.e., more (or less) than additively). In particular, there is some reason to believe that in the human body ozone interacts synergistically with sulfur dioxide (Hazucha et al. 1974 and Hazucha and Bates 1975), sulfates (Weiser 1977), or other particulates of respirable size. Thus, while neither ozone nor sulfur dioxide, for example, would represent a health hazard in Denver if considered in isolation, they may together, or in the presence of various particulates, represent a significant health hazard. Asthmatics, emphysematics, and smokers might be among the most likely victims of such synergisms. More research is required, however, to investigate the possibility of synergistic interactions of other pollutants with ozone.

The possibility of synergistic health effects of pollutants points to the general limitations of research involving controlled laboratory atmospheres. Although these studies permit the derivation of verifiable *dose-response* functions which relate the concentration and exposure of a particular pollutant to the degree of acute response, they often fail to achieve realistic simulations of all atmospheric conditions significant for the type of health response under study. It is conceivable that a more comprehensive clinical study might simultaneously administer doses of several pollutants and several allergens, and even manipulate meteorological variables such as temperature, pressure, and humidity. However,

the number of subjects and the number of trials required to derive a statistically valid dose-response function (where $\text{response} = f(\text{dose}_1, \text{dose}_2, \dots, \text{dose}_n, \text{weather variable}_1, \dots, \text{weather variable}_m)$) might easily multiply unmanageably. Furthermore, individual differences, even within the same disease or age group, might make the derivation of a single function impossible.

Regardless of the above technical problem, clinical studies can play fundamental roles in the detection of synergisms. If several pollutants have more than additive effects on most subjects, a synergistic interaction is indicated. Toxicological studies could provide the physiological explanation for the synergism, and in some cases, even a mathematical expression for the respective contributions of the several pollutants. If large numbers of trials and subjects are feasible, clinical studies could also pursue a set of mathematical functions relating the dosages of the several pollutants to the percentage of subjects who have each degree of response. Such functions would provide valuable information, while avoiding the statistical problems associated with the use of a single dose-response function to represent any individual's response.

If interactions of pollutants prove too complicated to analyze in the types of studies described above, clinical research does have the option of using city air in its experiments. Although it may not be able to establish just which of many atmospheric variables are responsible for observed health effects, it may be able to correlate some aggregate measure of high pollution with certain health effects. Such studies would be similar to epidemiological studies, with several important differences. Among them are the opportunities of clinical studies to use laboratory-certified clean air on experimental subjects for purposes of control and to make sophisticated and standardized health measures, as compared with the opportunities of epidemiological research to study very large populations and to investigate correlations between high pollution and the incidence or prevalence of chronic disease.

An understanding of the incompleteness of present knowledge regarding possible interactions of ozone and other pollutants, and an awareness of various research options for the investigation of synergisms, are essential to assessment of the health damages of Denver's air pollution. Although clinical research would stand behind a modeler who claimed an *ozone threshold* of 0.37 ppm, the interpretation that ozone is not a health hazard in the Denver area would be the modeler's, and quite possibly mistaken. *It is the responsibility*

of the modeler not only to model what is known, but also to point out clearly what is not known. Depending on the interests of the user, it may also be appropriate for the modeler to indicate the prospects for future gains in knowledge. Policy-makers, for example, may often be in a position to postpone premature decisions and await promising research; indeed, they may often be in a position to fund or arrange for the funding of such studies. Or, if answers are not immediately forthcoming, they may have to plan for the *possible*. In this case, it may be appropriate for them to consider *worst possible* outcomes as well as the *best judgment* of experts. Once again, the model would have to be tailored to the needs of the user. In this way the *risks* of inappropriate modeling may be minimized.

C.1.5 Adaptation and Differences in Group Sensitivity

The excellent work of Hackney et al. (1975) suggests another problem for the prediction of the health effects of Denver's air pollution. If Los Angeles residents have ozone thresholds of at least 0.37 ppm for acute pulmonary responses, does it follow that thresholds for Denver residents are also at least 0.37 ppm? The answer to this question is no, but the reason may not be obvious.

The same researchers have since discovered that Los Angeles residents may have higher ozone thresholds than people who live in less polluted areas (Hackney et al. 1977). The implication is that high levels of ozone pollution may bring about a physiological adaptation. Indeed, there is strong toxicological evidence that physiological adaptation does occur (JAMA 1975).

The above evidence for adaptation to ozone suggests the following possibility. Denver residents may not be subjected to high enough levels of ozone on enough successive days to bring about adaptation. This could mean that when ozone concentrations approach the highest Denver levels, Denver residents would suffer acute health damages. The insensitivity of Los Angeles residents to those levels may indicate adaptation, and may not be generalizable to residents of Denver. Further research is required to establish the actual sensitivity of Denver populations.

Another adaptation problem arises in predictions of the health impacts of Denver's

carbon monoxide pollution. Most of the people in Denver can be considered to have adapted to its altitude of 1600 meters. But significant numbers of people arrive in the area daily for vacations or on business. These people will temporarily experience, in general, lower levels of oxyhemoglobin. Carbon monoxide pollution, which binds to hemoglobin to form carboxyhemoglobin, also has the effect of reducing levels of oxyhemoglobin in the blood. Since the dangerous effects of carboxyhemoglobin may be largely due to the resulting lower levels of oxyhemoglobin, the effects of altitude and carbon monoxide pollution may be additive (at least) for visitors (Cobb 1974). This situation may be particularly dangerous for people with emphysema and certain forms of cardiovascular disease, who are in continual danger of serious complications from oxygen deficiency (Cobb 1974).

The above two examples of possible adaptation complications in the prediction of health effects of air pollution in Denver suggest the importance of caution in extrapolating from dose-response functions developed for one group of subjects to dose-response functions for people who may differ in critical ways from the subjects studied. We have already considered the importance of caution in extrapolating from results in artificial atmospheres. In both cases, what is required is caution in generalizing from *experimental* to *actual* health effects.

C.1.6 Individual Differences and Causal Explanation

The above example of the possible need to identify different groups in accordance with their levels of physiological adaptation (whether to the pollutant itself or the altitude at which the exposure in question takes place) suggests the criteria for group identification. It is appropriate to study the response characteristic of a particular population when (1) the health effects of pollution exposure upon group members are likely to be perceived by the model's users as significantly different from health effects upon members of other groups and (2) some sort of physiological difference is indicated which provides a convincing explanation of the difference in group sensitivity.

The above two criteria are met, respectively, by (1) clinical or epidemiological research and by (2) toxicological or epidemiological research. Clinical research has been characterized above as a *black-box* analysis which measures dose-response functions for individual subjects. As such, it would suggest distinct group sensitivity if it discovered

a clustering of dose-response relationships among the members of an identifiable group of subjects. Epidemiological research has been characterized above as averaging pollution levels and aggregating health consequences for large numbers of individuals, and seeking correlations between mortality, or the incidence or prevalence of acute or chronic diseases, and levels of air pollution. As such, it would suggest distinct group sensitivity if it found that distinct values of the pollution-morbidity or pollution-mortality correlation coefficients were associated with distinct populations. Toxicological or physiological evidence would not, in general, reveal either dose-response functions characterizing members of a group, or pollution-morbidity correlations characterizing particular groups. Rather, it would reveal underlying physiological mechanisms, i.e., a causal explanation, for any functional or correlational index of group sensitivity. It would shine some light, as it were, inside *black boxes*, and reveal any similarities of internal mechanisms of response to pollution insults which might characterize the members of an identifiable group.

Thus, groups with distinct sensitivity are identified by virtue of two general types of similarity among members. These might be designated as the *convergence of input-output relations* or the *resemblance of inner workings*. In practice, we generally confront, as it were, tentatively defined groups of gray boxes. Partially understood mechanisms, and partially convincing convergence of dose-response functions or pollution-morbidity/mortality correlations, tentatively suggest the need for special modeling consideration. It is understood that future evidence may indicate the need for revisions in the list of *populations at risk*.

What group divisions have been suggested by research to date? Distinct sensitivity has been associated with (1) age; (2) chronic disease status; (3) acute disease status; (4) exercise levels during exposure; (5) aerobic fitness; (6) smoking status; (7) history of exposure; (8) altitude adaptation status; and (9) possibly the use of drugs such as alcohol and barbiturates. Risk, it must be added (as distinguished from sensitivity), is also a function of likelihood of exposure. This is to say that behavior patterns, as well as the physiological states associated with the above nine categories, can be relevant in the modeling of health impacts.

Chronic disease status is by far the most significant classification for establishing the range of possible health damages. People with various forms of cardiovascular disease (Goldsmith 1977) and pulmonary disease (Leung et al. 1977), for example, are far more vulnerable than most to serious health consequences from exposure to carbon monoxide

pollution. This may be especially true at Denver's high altitude (see below). Sensitivity to ozone, on the other hand, is especially great in people with asthma or emphysema.

The importance of identifying sensitive populations has been suggested above. Failure to do so may mean that the most severe health impacts are not distinguished, but are rather *averaged* with the health impacts of a larger, less sensitive, population. The chances of failing to identify sensitive populations are great in the case of Denver's air pollution because the significance of Denver's altitude is not adequately understood. For example, although evidence is now lacking, people with cardiovascular disease who live at high altitudes may be more sensitive to carbon monoxide than people with cardiovascular disease who live at sea level. If this is the case, health impact figures for sea-level residents with cardiovascular disease should not be used to approximate the health impact for Denver residents with heart disease. Rather, the Denver population should be identified as a distinct population of risk.

Because it is so important, I will try to suggest some reasons for suspecting that high altitude residents who have emphysema or cardiovascular disease are at higher risk of health damages than sea-level residents with these diseases. This is not the analysis of a medical expert, but it may help to encourage medical research in the area. Furthermore, it seems appropriate to think seriously about this problem in the light of the failure of the Environmental Protection Agency to establish special air quality standards for Denver's high altitude.

The problem of possible increases in risk due to residence in Denver is not readily amenable to epidemiological analysis, as a low altitude city with identical pollution, meteorological conditions, and so forth, is not to be found for comparison. Clinical research, within ethical limits, could investigate carboxyhemoglobin and oxyhemoglobin levels as functions of altitude and disease status. This research unfortunately has not yet been done for the diseases most likely to be sensitive to altitude. Coburn et al. (1965) have developed a complicated equation for calculating carboxyhemoglobin levels as a function of exposure time and such variables as barometric pressure, alveolar ventilation rate, diffusion capacity of the lungs, production rate of carbon monoxide within the body, and the mean pulmonary capillary pressure of oxygen, as well as the ambient concentrations of carbon monoxide. The required data are seldom available, however, and even if they were, the equation cannot be solved without making simplifying assumptions (National Research Council 1977).

Physiological considerations must be brought to bear systematically in order to provide the perspective from which to generate more precise definitions of populations at special risk in the Denver area. Clearly, it is not sufficient to rely upon generalized conclusions for all people with cardiovascular disease. According to the American Heart Association, 14the United States population has some form of cardiovascular disease (American Heart Association 1978). Great variability is undoubtedly associated with the sensitivity of this many people. Goldsmith (1977) and Cobb et al. (1977) have reviewed evidence which suggests that people with coronary heart disease, who have limited capacity to increase the blood supply to their hearts, are especially vulnerable to decreases in oxyhemoglobin. Leung et al. (1977) have identified people with congestive heart failure as at especially high risk. The National Research Council emphasizes the complexity and variability of the cardiovascular response to oxyhemoglobin deprivation. Clearly, any relation of high altitude to health effects would have to reflect such complexity and variability of disease condition.

Emphysematics (and people with chronic lung disease in general) likewise would be expected to exhibit a complex and variable response to carbon monoxide pollution. Their effective alveolar partial pressure of oxygen is low due to the inelasticity of their alveoli, and tends to result in lowered oxyhemoglobin levels. Various adaptive mechanism, including cardiovascular adaptation similar to altitude-adaptation, increased pulmonary effort, and short-term increases in the production of 2, 3-diphosphoglycerate are known to compensate partially (Stryer 1975). Each of these compensatory abilities is limited, however, and one would expect the decreases in alveolar partial pressure of oxygen due to altitude (in conjunction with the emphysematic decreases) to hasten the encounter with these limits. More research is required to determine the conditions critical in the sensitivity of people with chronic lung disease, as well as the sensitivity of people with cardiovascular disease considered above.

The above suggestion of the importance of causal explanation for distinguishing groups at risk is not intended to place a special value on maximal group differentiation. The importance of differentiating finely among groups at risk is entirely relative to the interests of the users of the model. It may for certain purposes be far more useful to provide aggregated information for populations as large as that of an entire city. Indeed, such aggregation is difficult to avoid in the context of air pollution impacts upon the incidence and prevalence of chronic disease. The relationship between modeling options and user interests is thus a

subtle one, and is considered in some detail below.

C.1.7 The Selection and Presentation of Health Impacts

The users of a health impact model would not, in general, be health experts. They wouldn't be expected to have an interest in studying a detailed catalogue of scientific achievements. The modeler has the responsibility of summarizing and otherwise selecting from the current knowledge-base. Needless to say, he must attempt to provide a fair and intelligible representation of our current knowledge. He must also attempt to fulfill particular user requirements. Possible dimensions of user concern include those suggested by the following pairs of polar concerns: (1) exclusion of relatively uncertain or speculative knowledge/inclusion of available best guesses; (2) inclusion of effects upon sensitive minorities/limitation to effects upon significant (as designated) numbers of regional population; and (3) inclusion of future chronic consequences of long-term exposure/limitation to acute impacts. The significance of each of these dimensions of user concern for the selection of research findings for inclusion in the model, and for the manner of presentation of those findings, is considered below.

User problem I: Certainty vs. relevance

It is interesting to observe that the rankings (in general) of toxicological, clinical and epidemiological research for *certainty* (in the reliability of the result) and for *relevance* (to the types of health impact of public concern), respectively, are opposite. Toxicological research, because it provides a causal physiological account of the effects of pollutants, yields, of the three forms of research, the most certain results. Because it is most often conducted on laboratory animals, however, the implications for human consequences and sensitivities are not always clear. Clinical research is often more *relevant*, as it investigates human responses, but less certain in that the dosage-response functions relate black-box inputs and outputs, without an investigation of causal intermediaries. This leaves the causal relationships between dose and response unclear. Clinical research is also of limited relevance in that it measures only acute responses, rather than the incidence or prevalence of chronic disease. Epidemiological research is often still more relevant in that it can reveal morbidity, mortality

(which for obvious reasons clinical studies had better not reveal), and consequences of long-term exposure for the incidence or prevalence of chronic disease. Clearly, disease and death are consequences of greater human concern than are acute symptoms which may or may not later result in more serious health damages. In addition, epidemiological studies describe consequences in real atmospheres, and thus avoid problems of extrapolating to actual health effects from response to idealized exposures, concentrations, and combinations of pollutants. Epidemiology remains handicapped, however, in that it cannot control doses, and usually cannot differentiate finely among different populations at risk (Briese et al. 1974). Because doses and populations cannot be controlled, correlations between aggregated health effects and spatially and temporally averaged pollution levels are, in general, found to be quite variable. Both high and low correlations must be received with limited confidence.

Another issue of certainty versus relevance arises in the context of incomplete knowledge. Often the only way to get predictions of the health consequences people are most concerned about is to solicit expert opinions. The Delphi Method (Leung 1977) and Judgment Analysis (Hammond and Adelman 1976) are among the techniques which can facilitate systematic solicitation. The advantage of consulting experts is that we can ask them exactly what we need to know. We can guarantee, this is to say, the relevance of their answers to our concerns. We cannot, however, be certain that they are right.

How much confidence should we have in the judgments of experts? This would depend, of course, on how much agreement their judgment exhibits. In cases of great variability in expert judgments, it would be appropriate to report some estimates of uncertainty regarding their judgments. It is often possible to ask the experts themselves to make judgments of confidence regarding their own opinions. According to one interpretation of confidence judgments (Leung et al. 1977), a correspondence between the variability in expert opinion suggested by the confidence judgments and the variability actually found in the expert judgments of health impact provides a confirmation of the accuracy of the confidence judgments. This circumstance would presumably justify a report of uncertainty along with the report of expert opinions in the impact model.

Unfortunately, even the greatest accuracy regarding uncertainty does not diminish the uncertainty. The worst implication of this uncertainty is dramatized by the following situation. Let us say we discover that the health impacts modeled are more sensitive to

perturbations in the impact judgments (within the confidence interval, or alternatively, within the actual range of expert opinions) than they are to perturbations in the air quality inputs to the model (within the range of feasible air quality consequences of policy options). This would suggest that our model is not an effective predictor of health impacts. On the other hand, it may be found that the health impacts modeled are not as sensitive to expert opinion as they are to air quality. In this case, the model may be adequate in spite of the uncertainty of the expert opinions on which it was based. It should also be mentioned that adequate certainty can often be attained regarding probabilities of impact. The usefulness of probability estimates in impact modeling is discussed by Keeney and Raiffa (1977).

Despite the usefulness of sensitivity analyses in revealing the modeling implications of uncertainty, they cannot be depended upon to make decisions for us regarding the acceptable levels of uncertainty. User judgments must be made, in general, as to the relative importance of certainty and relevance.

The significance of this section's title, *The Selection and Presentation of Health Impacts*, may now be appreciated. The judgment as to the relative importance of certainty and relevance has been seen to be a responsibility of the user. But the modeler has two options in dealing with this user responsibility. He can find out before he completes the model how much (or what kinds of) certainty and relevance, respectively, are required. He can then tailor his model (i.e., make research *selections*) with these needs in mind. Alternatively, he can model (i.e., *present*) relatively certain and irrelevant impacts along with relatively uncertain and relevant impacts, and allow the user to place relative weights (explicitly or implicitly) upon certainty and relevance. Note that both the abbreviated model and its more complete alternative presuppose that the modeler is aware of which impacts are *relevant*. The following discussions of two additional user problems will consider in further detail the types of user judgments and modeling options associated with the choice of health impacts to be modeled.

User problem II: Equity vs. utility

The problem of *equity vs. utility*, in the context of modeling the health effects of air pollution, can be thought of as equivalent to the issue of *severity vs. frequency*. Are

the users more concerned with severe effects upon small numbers of people, or mild effects upon large numbers of people? If they are more concerned with the former, they would be interested in the consequences of the full range of feasible doses (not just averaged pollution levels) upon finely differentiated groups. This would show up the effects of short-term (and perhaps sudden) elevations in dose upon the most sensitive populations at risk. If, on the other hand, users are more concerned with the numbers of people affected, they would be interested in learning the percentage of all people affected to various specified degrees by averaged levels of pollution. Such information could be supplied by epidemiological studies. Epidemiological studies seldom have access, however, to detailed medical classifications of the people affected, nor to records of the air quality associated with residential, work, and travel patterns. Thus it is not, in general, possible for epidemiological studies to provide accurate figures for the number of people affected in a given sensitive population. Their usefulness lies in providing total figures for several stereotypical degrees of health effects, and in permitting some estimate by users of the total *disutility* of certain average levels of air pollution. They do not provide an adequate basis, however, from which to estimate *inequities* suffered by particular groups. (An exception may be epidemiological studies of excess mortality, which may indicate levels of the most severe form of inequity: death of the very few. See discussion of acute versus chronic impacts below.)

The modeler has options associated with the equity versus utility user problem analogous to his options associated with the certainty versus relevance problem considered above. He may supply the user with both types of output (epidemiological and clinical-toxicological, let us say), and allow the user to place relative weights upon the distinct types of impact revealed. Alternatively, the modeler may confine his output to the type of major interest to the user.

User problem III: Acute vs. chronic impacts

The acute/chronic distinction cuts across the certainty/relevance and equity/utility contrasts discussed above. Epidemiological studies which associate mortality with air pollution, for example, indicate quite *relevant* acute impacts which provide a basis for estimates of the frequency of the most severe form of *inequity*. Most often, however, mortality studies do not associate any excess mortality with specific populations at risk, and

thus would not necessarily contribute maximally to inequity judgments. Epidemiological and toxicological studies may investigate chronic impacts, with possibly opposite implications for both certainty/relevance and equity/utility concerns. Clinical studies of acute impacts, as explained above, are likely to fall in the middle of both certainty/relevance and equity/utility scales. Toxicological studies of acute impacts would, in general, provide relatively *certain* results, but, as explained above, often remain relatively *irrelevant* to estimates of human sensitivities.

Are the users more concerned with acute or chronic effects? The answer to this question may depend on the type of planning or policy-making under consideration. Long-term and preventive concerns must be contrasted with short-term and ameliorative concerns. Modelers may want to show both acute and chronic consequences and allow the users to evaluate their usefulness. Or, if the type of concern (say long-term or short-term) is clear, the modeler may want to simplify his model accordingly. Note that decisions as to the significance of acute versus chronic impacts may require consideration of the certainty versus relevance and utility versus equity issues. It should not be assumed that the value issues discussed in this section are independent of one another.

We are perhaps now in a position to appreciate the *risks* of predicting the health impacts of Denver's air pollution. Predicting health impacts is a complex modeling job which requires many decisions regarding the selection and presentation of scientific judgments or findings. These decisions will have implications for the impressions the model will make on its users. If those impressions are biased, because of the model, in the direction of lower health risks, much health damage may result from the use of the model. On the other hand, if exaggerated impressions of health risk result from the use of the model, much economic loss may result from air pollution prevention activities. The modeler must always be aware of the range of interests and values of prospective users, and make every effort to minimize inadvertent bias in the development of his model.

C.1.8 Conclusion

The above case study reveals, I believe, many of the typical *risks of prediction*. We have seen how an emphasis on the prediction of the relatively certain acute impacts of air

pollution may result in a biased neglect of the less certain, but more serious, chronic effects. We have also seen how sensitive minorities may be overlooked, how laboratory conditions may be mistaken for real ones, and how the significance of altitude may be ignored. All this says nothing, of course, about how estimations of the *aggregate*, or total, health impact on a community may be professionally avoided. It also says nothing, I might add, about how the expense of rectifying all of the above prediction shortcomings—as compared with the expense of cleaning up the polluted air—may never get predicted.

In all of the above-mentioned failures to predict, the risk of the failures is associated with the conclusions that people draw from the information made available. If people were well informed and not easily fooled, they would not draw premature conclusions from abbreviated assessments of risk. Prediction of how well-informed citizens or policy-makers are likely to be, and how easily they are fooled, may reveal a serious *risk*. It would be prudent if risk assessment were conducted with this *social risk* in mind.

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