



SFI Bulletin

Santa Fe Institute • Summer 1997 • Volume 12 • Number 2



**"WHEN IT COMES TO ENERGY TRANSPORT SYSTEMS, EVERYTHING IS A TREE."
GEOFFREY WEST ON THE ORIGIN OF QUARTER-POWER SCALING**

SFI Bulletin

SUMMER 1997 • VOLUME 12 • NUMBER 2

The *Bulletin* of the Santa Fe Institute is published by SFI to keep its friends and supporters informed about its work. The *Bulletin* is free of charge and may be obtained by writing to the managing editor at the address below.

The Santa Fe Institute is a private, independent, multidisciplinary research and education center founded in 1984. Since its founding, SFI has devoted itself to creating a new kind of scientific research community, pursuing emerging synthesis in science. Operating as a visiting institution, SFI seeks to catalyze new collaborative, multidisciplinary research; to break down the barriers between the traditional disciplines; to spread its ideas and methodologies to other institutions; and to encourage the practical application of its results.

Published by the **Santa Fe Institute**

1399 Hyde Park Road
Santa Fe, New Mexico 87501, USA
Phone (505) 984-8800
fax (505) 982-0565
home page: <http://www.santafe.edu>

Note: The SFI Bulletin may be read at the Web site:

www.santafe.edu/sfi/publications/Bulletin/.

If you would prefer to read the Bulletin on your computer rather than receive a printed version, contact

Patrisia Brunello at 984-8800, Ext. 269 or pdb@santafe.edu

EDITORIAL STAFF:

Ginger Richardson
William Clark
Pat Reed
Andi Sutherland

CONTRIBUTORS:

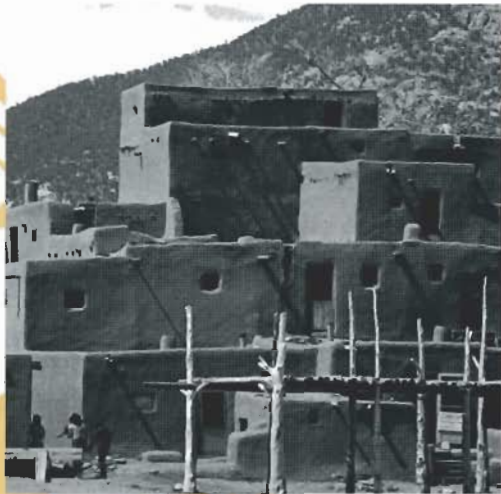
Diane Banegas
Barry McMullin
Peter Wills
Frank Zoretich

DESIGN & PRODUCTION:

Patrick McFarlin
Patrick McKelvey



Paths Not Taken



JONATHAN HAAS STUDIES PUEBLO POLITICAL LIFE BEFORE THE SPANISH

by Frank Zoretich

Tinkering, tinkering, tinkering—five hundred years ago, the Pueblo people of northern New Mexico were following paths of cultural evolution that included tinkering with new ways of relating to each other politically. But because their progress along those paths was suddenly halted in 1540 by the arrival of the Spanish, it's impossible to know exactly what kind of unification the Pueblos might have achieved if left alone.

Imagine northern New Mexico's mostly empty Galisteo Basin, for example, as a new center of civilization.

Didn't happen. But it might have.

Jonathan Haas, a Chicago-based anthropologist working with the Santa Fe Institute on the evolution of cultural complexity, says Francisco Coronado “encountered thriving communities of Pueblo people. The trajectory of their world changed from that point on. If we look at where they were going, that was altered unequivocally when the Spanish first arrived. The question is: what was life like before the Spanish got there, and then how did it change?”



Jonathan Haas







Haas, who is MacArthur Curator of North American Anthropology at Chicago's Field Museum of Natural History and an SFI external faculty member, says about sixty-five pueblos, each with three hundred to three thousand rooms, were occupied in the northern Rio Grande region at or shortly prior to the Spanish arrival. Today, there are only sixteen pueblos within the same area, and they are all politically autonomous.

On July 16 at the James A. Little Theater at the New Mexico School for the Deaf, Haas will discuss his work, including recent excavations at sites across northern New Mexico, in a Community Lecture titled "New Mexico in 1500: Pueblo Life Before European Contact."

Elsewhere throughout the New World, diseases brought by Europeans—including smallpox, chicken pox, mumps, and measles—killed up to 90 percent of local tribal populations. Haas wanted to establish whether a similarly severe depopulation occurred in the northern Rio Grande region because of diseases that may have arrived with the Spanish or moved along ancient trade routes decades in advance of the Spanish.

Contacted at the Field Museum recently, Haas said two theoretical models have been proposed for what might have happened to the Pueblo population.

"One model says the Rio Grande Pueblos went through the same decline—if not 90 percent depopulation, then 50 percent," he said. "The other model is that there never was a population decrease."

His research and fieldwork have led him to agree

The living pueblo at Taos (pages 1, 2-3); ruins (page 4, top), entrance to ceremonial kiva (page 5) and kiva interior (page 6) at Pecos National Historical Park, photos by William Clark. Anthropologist Jonathan Haas in the field (page 4, bottom), photo courtesy Jonathan Haas.



with the no-depopulation model. There are no accounts of such a devastation in the historical records and no archaeological evidence either.

Maybe it was the region's climate—relatively cool and dry, at elevations of five thousand feet and up—that prevented fomentation of Europe-spawned disease among the Pueblos, Haas suggested. “That’s the air-is-good argument,” he said.

Haas starts with demography, because numbers elsewhere have been shown to be linked to the sorts of political options open to a culture. “Different population densities,” he said, “are going to give you different opportunities for political organization.”

Sizes of populations and changes in their rates of growth are factors known to have helped determine the forms of confederacy eventually created by other societies. A larger population, rather than a smaller one, is more likely to be bound together in larger political groups, perhaps even a centralized governing system.

Even though some of the pueblos in what Haas called the “ethnographic present” are related linguistically, they are “all independent of one another, politically, socially, economically,” Haas said. “That’s a very exceptional, almost unique phenomenon if you look around the world. In similar areas with settled horticulturists, you have larger political bodies—confederacies.”

Is the present-day autonomy an “artifact of colonialism,” Haas wanted to know, “or is it a manifestation of an ongoing pattern that precedes the Spanish?”

Of course, from the viewpoint of the Spanish authorities, it was better to have the pueblos be politically separate from each other. “If each pueblo is independent, you can control each of them better than if everybody is united—divide and conquer,” Haas said. “The Pueblo Revolt of 1680 showed what can happen when everyone *is* united.”

With his wife Winifred Creamer, an anthropologist who teaches at the Northern Illinois University, Haas has conducted excavations for the last 10 years at the ruins of abandoned pueblos in the Galisteo Basin, south of Santa Fe, and examined other sites there and elsewhere in the region. Haas and Creamer are co-authors of a number of papers and a 1993 book, *Stress and Warfare Among the Kayenta Anasazi of the 13th Century A.D.*

From historical records, Haas said, eight big pueblos in the Galisteo Basin are known to have been occupied at some time during the 1500s. Altogether, they contain enough rooms for a population of about fifteen thousand—more than the number of people who live there now. If there had been a depopulation of the villages in the years just before Coronado arrived, Haas would have expected to find evidence of the calamity in the long-buried rooms of Pueblo San Marcos and Pueblo Blanco.

“I wanted to see the condition of the rooms when they were abandoned,” Haas said. “You’re looking at the floor, seeing what’s left. If you’ve got a pueblo ravaged with disease, you should see signs that the people did not have time to clean them out. If there had been warfare, you should see signs of burning, dead bodies on the floor, smashed artifacts. What we’re finding is that people just packed up and left—with no signs of rapid abandonment due to warfare and disease.”

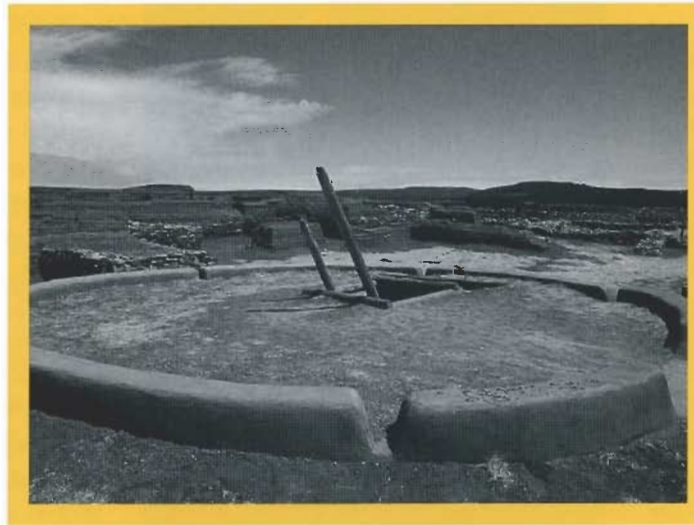
Haas and Creamer said the sites in the Galisteo basin were used sequentially by populations that moved periodically from one to another in a cycle of abandonment and reoccupation. The people would leave the village after being there for a generation of so, perhaps because of the depletion of firewood and other resources in the immediate area. Then, a generation or so later—fifty

to seventy-five years after the initial abandonment, when the land would have had time to replenish itself—the descendants of the original residents would return and build new rooms on top of the old, ruined ones.

So abandonment doesn’t necessarily reflect a 50 percent population decline, Haas said. “Only 50 percent of the sites (in the Galisteo Basin) were occupied at one time—and that was the normal pattern.” Pueblo San Marcos, for example, has more than three thousand rooms in several layers. But Haas said probably no more than 750 people lived there at one time.

So instead of fifteen thousand people living in the basin at or just prior to the arrival of the Spanish, a more realistic population figure for the time would be four thousand.

If this pattern of cyclical occupation was repeated throughout the northern Rio Grande region—and evidence of it has been seen in ruins at Arroyo Hondo south of Santa Fe, for example, as well as at several



other sites—then it is likely that Pueblo populations never reached a critical mass for more centralized government.

Evolution in human culture, including political development, is fundamentally different than evolution in the purely biological realm, Haas noted. In biology, variations are randomly produced. It's Darwinian. But in cultural evolution, variations are not randomly produced. They are generated, he said, "by deliberate selection—by thinking agents trying to solve problems."

In bio-evolution, he said, "an animal with longer tail feathers might find itself more competitive in an environment—but it didn't grow those longer feathers to become more competitive. And an animal might find itself more competitive because it has grown bigger teeth—but it didn't grow those teeth because it needs them to eat a new species of dik-dik that has just entered the area. The process of natural selection is more random than that."

In human cultural systems, variations are not random, Haas continued. "Humans are tinkerers. They tinker to make things better." A human may plant two or three seeds in a hole instead of one, for example, just to see what happens. If he gets more crop, then he will continue planting more than one seed—and the agricultural system has evolved another step.

Haas has joined with several other anthropologists and archaeologists in the SFI working group on Culture as an Adaptive System. "To model human systems—which are so vastly more complex than complex biological systems—you can't use random variations," Haas said. "You have to have thinking agents directing variation." You have to build tinkerers—thinking agents that act like humans—into any computer program that attempts to model cultural evolution.

The program should be designed so the agents are under stress, picking solutions to address that stress, and then learning from generation to generation what works and what doesn't work, said Haas.

"So far, modeling of real human systems has not been wholly successful," he said, "but at SFI, there's the computer power to do it. We want to see how we can get a whole lot of complexity through fairly simple agent-based rules. We're trying to put humans into the system. We're just at the baby stages, but we're getting close to figuring it out. I estimate a ten-year window for doing that."

If the modeling is successful, it could help researchers better understand how choices are made as a population evolves in political and other forms of cultural complexity. And it might help explain why the Pueblos in the northern Rio Grande did not follow the path that has generally been taken by other peoples—a path that would have led from small familial bands to tribes, then to confederacies, and finally to some form of centralized statehood.

Although the tribes did act as a confederacy in the Pueblo Revolt, Haas noted that the confederacy was formed to deal with an enormously unanticipated external problem. "And as soon as the Spanish were gone, the confederacy fell apart." The confederacy had not evolved, after all, as a way to address organizational problems within Pueblo culture.

"The post-Revolt period was a time of tremendous conflict among the Pueblos themselves," Haas said. "When the Spanish came back into the area, they reported that many Pueblo people welcomed them, because it would bring an end to the conflict that was ongoing."

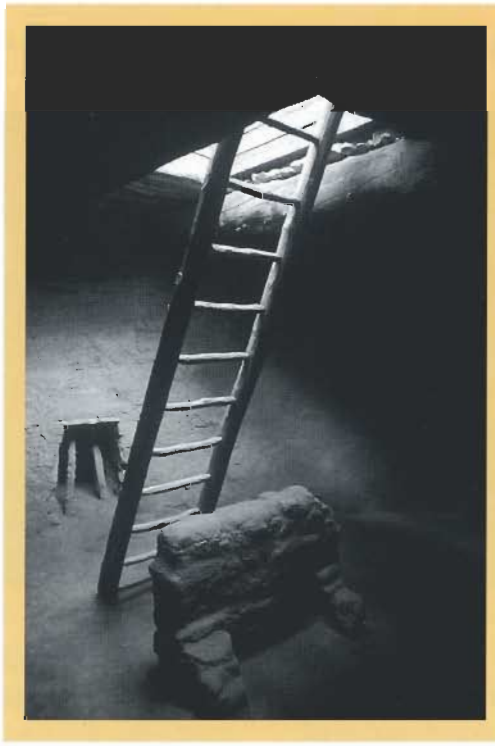
From his own work and other studies, Haas now believes that the Pueblo populations had been moving along several paths of political evolution, not just one.

Some of the Pueblos in 1500 were big, powerful, and relatively isolated, like Taos (which has survived) and Pecos (which did not). They had "always been off by themselves," he said, "and not always in good relationships with their neighbors."

But in the Galisteo Basin, "it looks like there was much more of a confederacy model," Haas said. That network of villages "was in a tight cluster, and we know they spoke the same language, Tanoan, and they were moving around in that valley in a coordinated way. They were united into something more than independent, autonomous villages," Haas said.

"But no one model had emerged among the Pueblos as consistently better and stronger or more competitive. There wasn't a single mode of organization—there were multiple modes. Each one of these systems was an experiment. The Pueblos were tinkering with political organization."

Frank Zoretich is a free-lance writer based in Albuquerque.



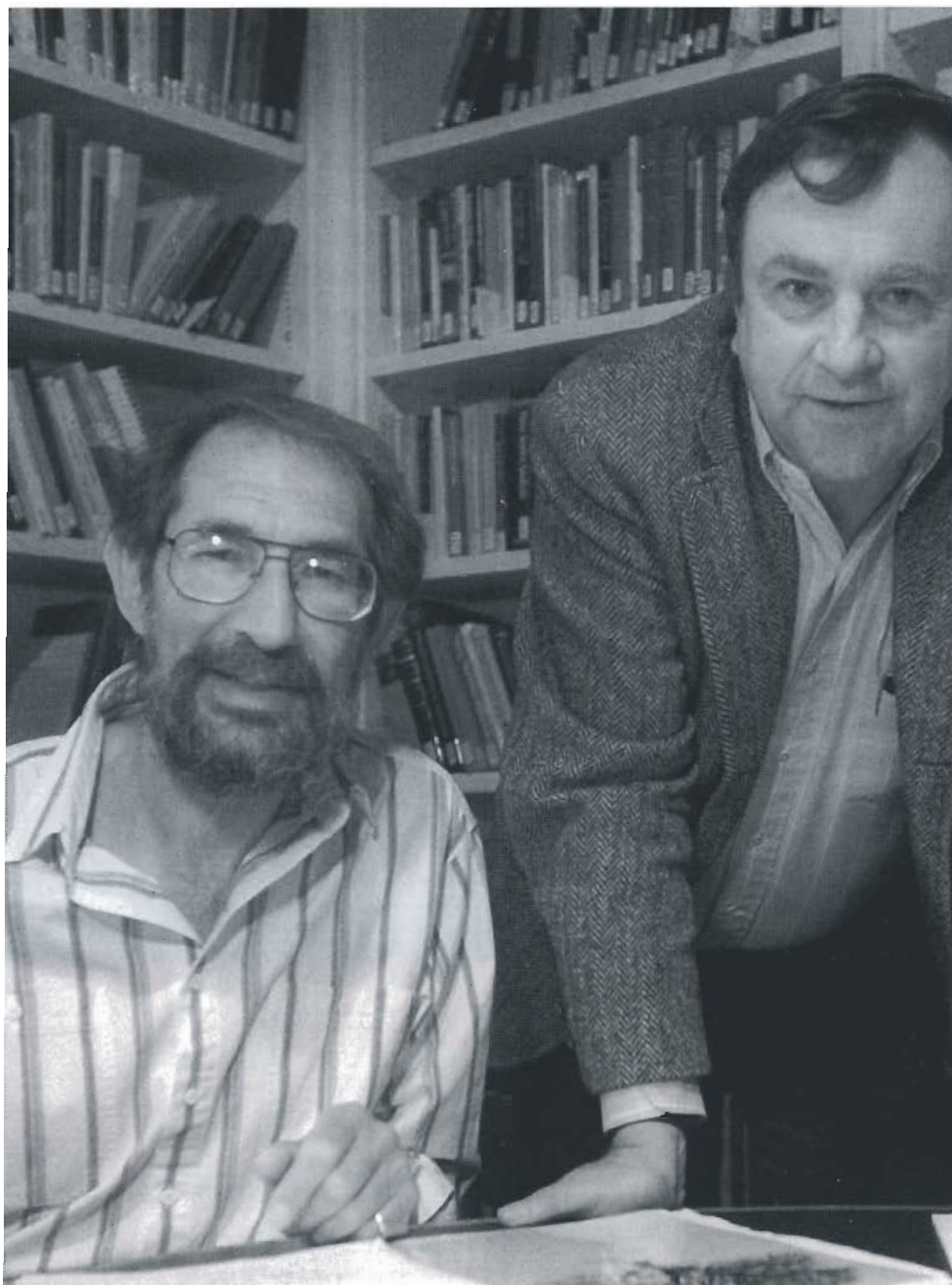
THE TREE OF LIFE

Scientists Model Nature's System of Fractal-branching Networks

by Diane Banegas

The words “drawn to scale” are familiar to nonscientists and scientists alike and their meaning is easily understood: the proportions of the original structure are preserved in the artist’s scaled-down representation.

Scientists have observed for more than fifty years that nature also preserves proportions in adapting biological features from species to species. Curiously, such features as metabolic rate, heart rate, and lifespan scale with size as some simple multiple of the one-fourth power, rather than as a multiple of the one-third power as expected from simple geometric scaling. In recently published research coming from Santa Fe Institute collaborations, Jim Brown, Brian Enquist, and Geoffrey West have presented, for the first time, a general model that explains the origin and prevalence in nature of quarter-power scaling. They propose that the origin of this distinctive biological scaling arises from the fact that cardiovascular systems, respiratory systems, plant vascular systems, and insect tracheal tubes all exhibit the same fractal-like branching structure.



Geoffrey West (left) and Jim Brown collaborate on their scaling research at SFI.

Two years ago, West, a Los Alamos National Laboratory physicist, and Brown and Enquist, University of New Mexico biologists, began a collaboration at SFI to study these universal scaling laws. The collaborators were brought together by Mike Simmons, former SFI vice president and now an external faculty member living in Washington, D.C., who knew of their respective professional backgrounds and their shared interest in

scaling laws. The researchers met and immediately clicked, both professionally and personally.

"We came to the same problem from very different angles," West said. At Los Alamos, West was applying his high-energy physics background to the mystery of why all animals, regardless of their body size, obey the same simple scaling law for metabolic rate. The answer, he believed, was essential to understanding how evolution maximizes fitness.

"Metabolic rate—how much energy an organism consumes per second to maintain life—is proportional to body mass to the three-fourths power," West said. (To obtain three-fourths power of body mass, take the square root of the square root of an animal's weight and cube it.) The law, known as Kleiber's Law, has been around for decades, but no one understood the reason for it, West said. "A cat is roughly one hundred times larger than a mouse," West explained, "so you'd expect a cat's metabolic rate to be one hundred times larger than a mouse's, but it isn't. The metabolic rate is only about thirty times larger—a number predicted by Kleiber's Law."

For their part, Brown and Enquist were trying to solve the riddle of why the metabolic rate of plants exhibits the same quarter-power-scaling phenomenon observed in animals. As ecologists, they were interested in determining how population densities and other environmental laws relate to the biological laws that govern individuals.

Like West, they proposed that quarter-power-scaling laws arose from a common underlying mechanism: the transport of materials sustains living things through a linear network that branches to supply all parts of the organism.

"Jim and I knew that the structure and dynamics of the supply network would hold the answer but we didn't have the background in physics and math to carry out the calculations," Enquist said.

"Working with Geoff was like having a mind extension for physics. We could never have made the same kind of progress without him."

For West, the collaboration was a good way to study the bigger problem of scaling. "Jim and Brian didn't know the physical and mathematical details to build a precise model," he said, "but their intuition about living systems was tremendous. I'd crank something through the model,

obtain results, only to have them shake their heads and say, 'It really couldn't work that way in a living system.' They kept me from running up a lot of blind alleys."

The researchers built their model on three assumptions: that a space-filling fractal-like branching pattern is required to supply life-sustaining fluids to all parts of the organism; that the final branch of the network—the twigs of a tree or the capillaries of a circulatory system—are the same size regardless of a species' body mass; and that the energy used to transport resources through the network is minimized.

The first assumption came from the researchers' observation that a space-filling branching network is a natural structure for transporting nutrients to every cell in an animal's body. Thus, the billion plus cells present in the human body are fed regularly through the cardiovascular system which transports oxygenated blood through the aorta, decreasingly smaller arteries, and through about ten billion capillaries, each of which feeds a small number of cells.

The second assumption arose from the researchers' knowledge that all living cells, the building blocks of life, are the same size regardless of an organism's species or body weight.

Lastly, to minimize the energy required to transport resources through the system it is necessary to minimize the total hydrodynamic resistance. From this it emerges that the network must be a *fractal branching* one. Fractals are structures that exhibit self-similarity in the manner of Russian nested dolls or snowflakes. The smallest fraction of the system must be a miniature replica of the entire network, the only difference between the two being one of scale.

While fractal branching networks exhibit the same type of self-similarity as nested dolls and snowflakes, their organization is tree-like. Cardiovascular systems, respiratory systems, plant vascular systems, river systems, and insect tracheal tubes are all examples of fractal branching networks. "When it comes to energy-transport systems, everything is a tree," West said.

Plant vascular systems follow the "vessel-bundle" structure of multiple parallel tubes. In this easy-to-model type of network,



fluid velocity is constant throughout the network, independent of tube size. This property arises from the "area-preserving" nature of the network. Each time a tube splits into smaller tubes, the cross-sectional area of the larger tube is the sum of the smaller cross-sectional areas of the two daughter tubes. Mammalian energy transport systems are a bit trickier to model. The branching cannot be entirely area-preserving because blood must slow down in mammalian systems to allow materials, such as oxygen, to

diffuse across capillary walls.

The difficulty is solved by the heart, which pumps blood into the aorta and larger arteries in waves. The waves damp down and disperse as they leave the larger area-preserving vessels of the system and travel through the progressively smaller cardiovascular tubes. A crossover from the pulsatile waves in the aorta and larger arteries to an almost steady oscillatory flow in the smaller transport vessels ensures that energy-carrying waves are not reflected back up the tubes at branch points.

The crossover from one kind of traveling wave to another kind is achieved through *impedance*

matching, an exact analog of the impedance-matching phenomenon that

occurs at the junctions of electrical transmission lines.

By making the total opposition to current flow in an alternating-current circuit equal to the amount of current, the most efficient transfer of power is achieved.

The cardiovascular system has inherent features such as vessel-wall thickness and elasticity to pull off the same kind of impedance matching at its crossover branch points. The system loses a small amount of energy after the crossover, ensuring the blood moves slowly enough for oxygen to diffuse across capillary walls.

"Given the physical and geometric constraints implicit in these three principles," West said, "out pops quarter-power scaling." The model accurately predicts the structural and functional properties of the mammalian cardiovascular and respiratory systems. For example, given the body mass of an adult male, the model can compute the length and cross-sectional area of his aorta. The

researchers plan to extend their work to consider its broader implications, but their research so far suggests that quarter-power scaling is perhaps the single most pervasive theme underlying all biological diversity.

"Scaling laws mean that organisms of different sizes use energy and other resources at different rates," Brown said. "They also operate at different spatial and temporal scales. For example, a bacterium lives fast and short with a lifespan measured in minutes and within a space measured in millimeters. Contrast that with a whale that lives for decades and moves over space of hundreds or even thousands of kilometers."

The quarter-power-scaling laws

are obeyed with remarkable precision for body sizes over twenty orders of magnitude, ranging from single-cell organisms to blue whales. The scaling laws are also unaffected by the exact details of a system's design as long as it has a fractal nature.

Although the model addresses fractal branching networks on the macrobiological level, the researchers predict that subcellular systems will exhibit the same fractal patterns and obey the same laws of quarter-power scaling. A paper describing their research results, titled "A General Model for the Origin of Allometric Scaling Laws in Biology," appeared in the April 4 issue of *Science*.

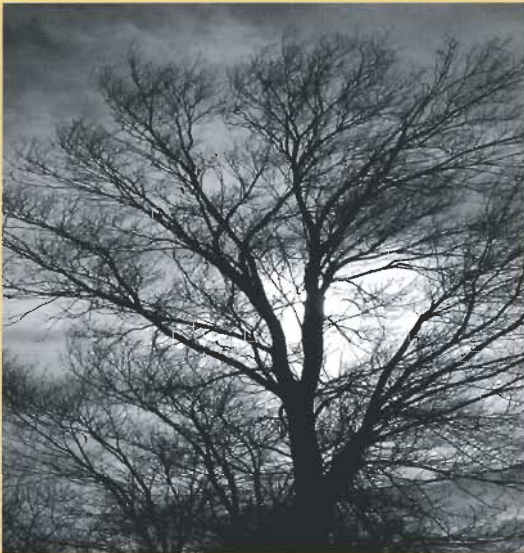
"We hope our research will stimulate more intersection between physicists and biologists," West said.

"Scientists today are incredibly partitioned into their own disciplines," Enquist noted. "Even within biology, we are partitioned into cell physiology, anatomy, ecology and so on—and each sub-partition has its own department, journal, and language."

West nodded in agreement. "We need to return to seeing things as a whole." He lamented the passing of the "natural scientists" such as Sir Isaac Newton and Charles Darwin who were versed in life and physical sciences. "They were the renaissance men of science."

Diane Banegas is a writer who lives in Santa Fe.

SCALING IN BIOLOGY—FROM ORGANISMS TO ECOSYSTEMS



Building on the discoveries of Jim Brown and Geoffrey West, the Santa Fe Institute is launching a broader-based research program on allometric scaling to explore the consequences and extensions of the first model. The program will try to develop explicit models to relate the fractal design of distribution networks and the three-fourths-power scaling of metabolic rate at the level of individual organisms to the allometric scaling of life-history features (such as life span and embryo-development time) and ecological characteristics (such as population density and species diversity).

To publicize the model, encourage evaluations and extensions, and promote creative and interdisciplinary research, Brown and West will co-chair a workshop/symposium on "Scaling in Biology" at SFI in October 1997.

The workshop will bring together about twenty-five biologists representing subdisciplines ranging from plant, animal, and microbial physiology and biomechanics to ecology and evolution. These researchers will explore relations between their specialized areas and expertise, and the broader, more general causes and consequences of allometric scaling laws.

A product of the workshop will be a book in the SFI proceedings series, which, it is hoped, will signal the promise of allometry to contribute to the conceptual unification of biology.

A second, later workshop to explore more general implications of fractal-branching networks and scaling relationships (in neural networks, river systems, and human distribution networks such as electric grids, water mains, and highways) is also planned.

THAW CHARITABLE TRUST TO SUPPORT ECOLOGY WORK AT SFI

The Eugene V. and Clare E. Thaw Charitable Trust has made an award to the Santa Fe Institute to pursue three fundamental themes of ecological research—scaling in biology from organisms to ecosystems, unifying principles of ecosystem development, and ecological discontinuities.

The project will encompass theoretical, computational, and empirical studies and provide support for collaborative research among leading members of the ecological and related scientific communities.

These new initiatives grew from a March workshop, titled “Universal Phenomena in Ecology?” held at SFI under the joint sponsorship of the National Center for Ecological Analysis and Synthesis, the Thaw Foundation, and SFI. More than a dozen leading ecologists attended the workshop, which focussed its discussions on identifying the major questions outstanding in ecology.

WORKS IN PROGRESS: BIOTICS/ABOTICS

TOWARD UNIFYING PRINCIPLES OF ECOSYSTEM DEVELOPMENT

Between Biotics and Abiotics

The context of evolution lies at the interface between the biotic and abiotic components of ecosystems. Energy from sunlight and other sources is the major driving force in the growth of organisms, subsequent accumulation of biomass, and transformation of the physical environment. However, energetic pathways are mediated by material flows and, in particular, essential nutrients for the formation and catalysis of high-energy chemical bonds.

This initiative centers on understanding the interplay between abiotic processes such as energy flow and nutrient cycling and biotic processes such as growth, accumulation of biomass, and metabolism. In the long term, the development of models that address the mechanisms by which organisms change their physical environment and how these changes in turn feedback to affect evolution may lead to novel and perhaps universal principles for understanding ecosystems.

In fact, there have been previous attempts at building comprehensive views of organisms and ecosystems. Many of these efforts have also been focussed on the role of nutrients and energy as constraints on ecological systems. This work builds on the ideas set forth by researchers such as C.S. Elton, R. Lindeman, and E.P. and H.T. Odum. Previous efforts, however,

have either tended toward precise measurement of abiotic processes in ecosystems or have resulted in more or less narrative descriptions of ecosystem phenomena. Progress has been hampered by lack of empirical studies coupled to theory and by limitations in our mechanistic understanding of how organisms and environmental constraints interact to build ecosystems.

A general theory of biotic-abiotic interactions in ecosystems is only likely to emerge over years of intensive study. However, a provocative starting point is the question of the influence of stoichiometric (nutrient ratios) and allometric (body-size scaling) constraints on the dynamics and evolutionary properties of ecosystems.

ADAPTIVE FEEDBACK IN ECOSYSTEM DEVELOPMENT

Current thinking in ecology views plants and animals as occupying more or less static niches that define the basic resource requirements for each species. In an alternate approach, organisms and ecosystems develop together over time and space; feedbacks can, over short time scales, cause functional and numerical ecological responses and, over longer time scales, affect evolutionary trajectories that, in turn, influence ecosystem properties. The prin-

cial mechanisms of feedback between organism and environment are conservation of energy and conservation of matter. Organisms require free energy to grow and reproduce. However, the ability of organisms to use available free energy depends on the availability of basic ecosystem nutrients such as carbon, nitrogen, and phosphorus. Thus, organismal processing and cycling of nutrients constrains the energy flows in ecosystems. (This is principally due to the fact that bio-chemical energy is stored in chemical bonds and because particular elements may be necessary for the formation of enzymes.)

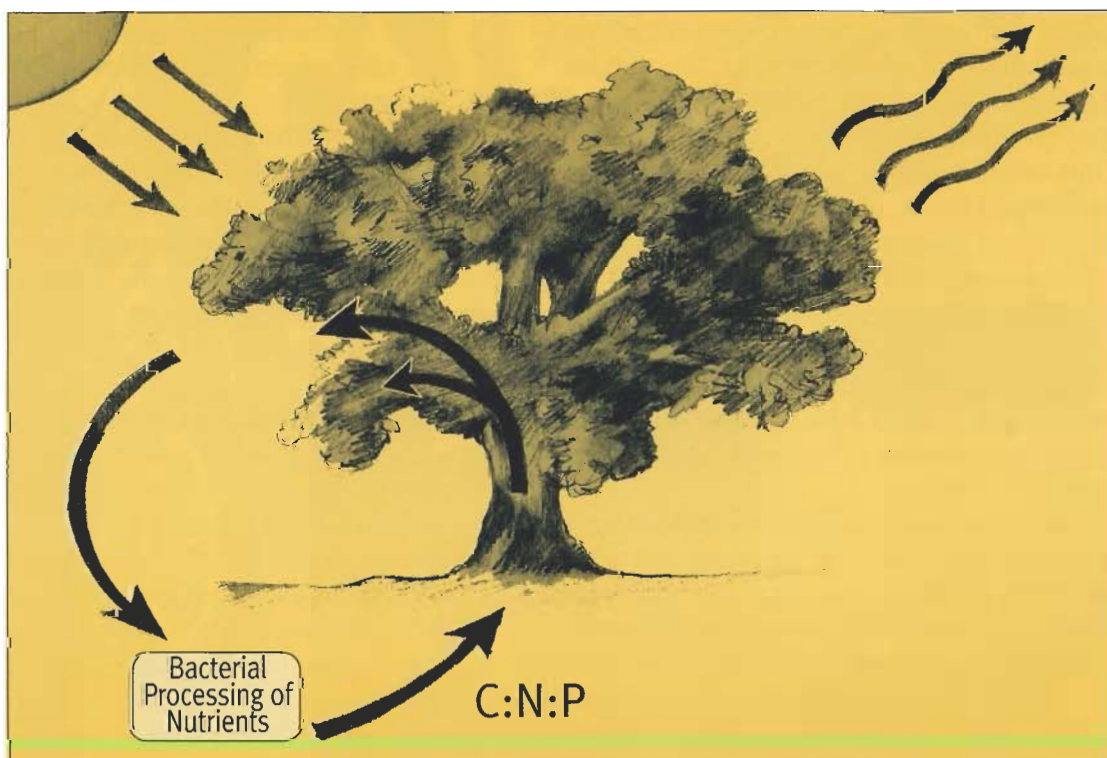
In a broad sense, this new approach cuts at the heart of complex adaptive systems. The project hopes to take a step toward understanding how a set of evolving "agents" co-evolve in response to one another within the constraints of a set of limiting resources. In this respect, the conceptual model is not unlike an economy in which agents transform raw materials into products that then become resources for the next agent and so on.

A QUESTION AND HOW TO GET FROM HERE TO THERE

In the near term, the research will focus on how stoichiometric and allometric constraints interact to control the dynamic and evolutionary properties of ecosystems. To address this question, the project will link groups of scientists who understand functional attributes of organisms (e.g., allometry, scaling rules, stoi-

chiometry); who understand ecosystem-level mechanisms and feedbacks; who understand how physical properties such as conservation of energetics and matter translate to ecological systems; and who can integrate and explore the intersection of these groups through quantitative and explicit models.

Specifically, Geoffrey West, a Los Alamos National Laboratory physicist; and Jim Brown, Charles Curtin, and Brian Enquist, University of New Mexico biologists, continue to refine a model of allometric scaling in plants and animals. This model needs to be directly connected to rates of nutrient uptake and release by organisms. To a large extent, this work is a natural extension of the previous development of the model. A second group—Mike Pace from the Institute of Ecosystem Studies at Millbrook, Lars Hedin from Cornell, and Tim Keitt, a postdoc at SFI—is reviewing existing ecosystem models with respect to nutrient cycling and the relationship between energy flow and nutrient fluxes. Once the current state of ecosystem models is fixed, they will synthesize their ideas into a model that explores ecosystem processes in relation to the adaptive response of organisms to changes in the physical environment. Such a model will allow them to explore how nutrient and energy coupling among species drives evolutionary processes.



TOWARD UNIFYING PRINCIPLES OF ECOSYSTEM DEVELOPMENT

Ecological Discontinuities

Do ecological systems organize themselves into discrete assemblages of co-adapted species, or are species distributed continuously and independently in space and time? Of course, not all species are independently distributed. Predators tend to occur where their prey is found, and one partner in an obligatory mutualism is only found in places that also harbor the other partner. But does it go beyond such obvious examples? In particular, do species that tend to occur together tend to evolve positive direct and indirect effects on one another? Do they also evolve mechanisms that increase their probability of co-occurrence in the future?

These questions were strongly debated in the early part of this century, but now most ecologists believe the debate has been resolved. They think species primarily are continuously and independently distributed rather than occurring in discrete, co-adapted species assemblages.

The debate was supposedly resolved along two distinct lines of argument. First, early proponents of the discrete assemblages viewpoint largely thought of such assemblages as "superorganisms" and based arguments about their emergence on an ill-defined process of natural selection acting among groups. Most evolutionary ecologists now reject the notion of selection among groups of organisms and therefore reject the concept of superorganisms because of the lack of any known mechanism that might result in their emergence. Second, empirical studies, particularly influential investigations by R. H. Whitaker and other plant ecologists, were widely accepted as showing that species are continuously and independently distributed along environmental gradients.

The time has come to reopen this debate. First, intergroup selection may not be required for discrete, tightly organized species assemblages to come into existence. Such discrete assemblages may emerge from the interplay of complex interactions among and between species and their physical environments and the action of natural selection at the level of the individual. Second, the statistical analysis done to date (and the data sets upon which such analysis has been done) may have been inadequate to detect patterns of nonindependent distribution of species.

Work will involve three parts: (1) a thorough review of the literature and re-analysis of existing data sets

on species distributions along environmental gradients; (2) the development of a modeling platform and a theoretical analysis of conditions that might lead to the emergence of discontinuities in species distributions; and (3) the development of an empirical research program that would collect the types of data needed to resolve the issue.

The first two prongs of this work will take place at the Santa Fe Institute and the National Science Foundation's National Center for Ecological Analysis and Synthesis (NCEAS). A large but scattered body of data exists on the distribution of organisms along environmental gradients. Bringing these data together will involve a major task of synthesis through NCEAS. The data base will provide the underlying empirical support for, and tests of, the modeling program.

A major concern is to consider the evidence on the role of positive feedbacks between organisms and between organisms and their environment as a key process determining the distributions of organisms along environmental gradients.

Parallel work at SFI will focus on the development of a modeling platform and theoretical analysis. Aside from modeling with Swarm and ECHO, work will continue on an alternative platform (METAMAP) being developed in Ron Pulliam's laboratory at the University of Georgia. METAMAP is an object-oriented program that either imports Geographic Information Systems representations of real landscapes or creates novel landscapes and allows diverse agents (individuals of given species and phenotype) to survive, move, and reproduce on those landscapes. Each patch in the landscape is characterized by environmental variables and resources that determine survival and reproductive consequences for occupants of the patch. Individuals may move from one patch to another, based on a variety of movement and stopping rules ranging from passive dispersal to active habitat selection.

The empirical research program will be a parallel effort for which funding will be sought from other sources.

See page 10 for a description of the third theme of ecological research, "Scaling in Biology—From Organisms to Ecosystems."

Turning the Corner on the Search for the Origin of Life

by Peter Wills

Ask four or five people researching the origin of life what they are doing and what they expect to find out, and you will inevitably get a different answer from each. And the answers are likely to be mutually contradictory.

However, a new consensus has emerged within a diverse group of physicists, chemists, and biologists associated with the Santa Fe Institute. These scientists believe that the most important questions on this subject can now be addressed experimentally. Theory can leave the realm of speculation to focus attention on the interpretation of real data.

That doesn't mean there is agreement about what "life" is. But the finding of what seem to be microfossils in Martian rock has jolted people into contemplating the possibility that terrestrial biology may not be as special and mysterious as had long been thought. Perhaps there have been, or still are, life forms of some sort nearby in our solar system. The question has excited National Aeronautics and Space Administration (NASA) scientists enough to stimulate a major new investment in a field they are calling *astrobiology*.

Experimentalists and theorists at SFI and elsewhere are embarking on a new, joint approach to some of the questions that have bugged research into the origin of life for decades. What were the first replicating biological molecules on earth? Nucleic acids or proteins? What are the thermodynamic conditions that have to be satisfied for systems to become progressively more ordered and specialized? What sort of chemical-reaction network is needed to produce anything as complex as cellular biochemistry? Is there some special principle underlying the coordinated processes that maintain the integrity of organisms, even the simplest cells? The SFI-based team intends to combine laboratory and theoretical work in its attempt to find fresh answers, and it is looking to NASA for support.

Central to this effort is the recent demonstration by Reza Ghadiri's group at Scripps Research Center in La Jolla, California, that showed that a small protein can self-replicate. It had long been thought that this fundamental biological property was the sole preserve of the nucleic acids that make up the genes of present-day organisms.

"We're turning a kind of corner" says Stuart Kauffman, SFI faculty member and project coordinator. "For thirty-five or forty years—in fact, since we've known about the structure of DNA—the symmetry of the DNA or the RNA double helix has been such a commanding image of how a reproducing molecule might occur, that it has dominated almost everybody's imagination. On the other hand, the idea that you might have autocatalytic sets or collectively autocatalytic sets of proteins goes back at least to the late 1960s in a book by Melvin Calvin on chemical evolution. That's the theme that several of us have picked up, more or less independently at about that time. And here Reza has made the first peptide that is able to catalyze its own formation by catalyzing a ligation reaction."

Kauffman does not dismiss the possibility of building autocatalytic sets of nucleic acids. In fact, another project participant, Andrew Ellington of Indiana University is already extending earlier work of Günther von Kiedrowski on the autocatalytic ligation of short oligonucleotide sequences.

But what now looks possible with peptides, something which has so far eluded those who have investigated self-reproducing nucleic acid systems, is the creation of collectively autocatalytic sets comprising many, perhaps hundreds or thousands of molecules. Although the theory of how these systems work has been around for a decade or more, it has not been taken seriously. "It's a very new picture of the origin of life that now looms as a real experimental possibility," says Kauffman, "and that's extremely exciting."

Ghadiri shares Kauffman's enthusiasm for what can be gained from experiments with replicating molecular systems. He has already found combinations of templates that operate a kind of error-correction mechanism and others that form a classic catalytic "hypercycle" of the sort that SFI Science Board member Manfred Eigen of the Max Planck Institute proposed in 1971 as a prerequisite for evolutionary molecular self-organization.

Ghadiri views his experimental systems as representing a model for one stage of the transition from inanimate chemistry to living organisms.



MOROWITZ FAVORS THE IDEA
THAT LIFE FIRST AROSE
AROUND "SMOKERS,"
VOLCANIC VENTS DEEP
UNDER THE OCEAN

"Understanding this transition requires moving from an emphasis on molecular properties to the properties of molecular ecosystems—how the population of molecules have organized to give lifelike signatures." In the end, the systems should show "emergent properties that are larger than the sum of the components," and in that sense, the problems of understanding the origin of life and creating artificial life coalesce.

Kauffman sees this as the key to the cooperation between experimentalists and theorists. "We're within a decade or so of making communities of self-reproducing systems," he says, "and we're going to invent a whole new body of theory of emergent biological phenomena."

If you ask Ghadiri what he is going to discover about *the* origin of life, as it actually occurred on Earth, he is bluntly dismissive. "I have no idea. There are various plausible scenarios, and there is most likely some truth in each. All we can do as experimentalists is simply to show the plausibility of some of these pathways. We cannot make any judgment about what was *the* origin of life."

However, Harold Morowitz, from George Mason University's Krasnow Institute, is not so skeptical. Even the question is, for him, an experimental problem. If the origin of life is a very, very improbable event that happens just once and only then by some extraordinary fluke, that puts it outside the domain of science. If it's something that has a fairly high probability, as he thinks, then the circumstances are such that they should be experimentally reproducible.

Morowitz favors the idea that life first arose around "smokers," volcanic vents deep under the ocean. It is now possible to mimic in the laboratory the extreme physical conditions found in violent regions of the ocean floor. At pressures of about 500 atmospheres and temperatures of 300° to 500°C, water behaves much more like a hot organic solvent. The reactions that take place between the oxides of carbon and nitrogen in a reducing aqueous environment produce the compounds that we now see linked in the citric acid cycle, biochemistry's basic thermodynamic engine.

Morowitz tells a convincing story. "Analysis of the metabolic chart makes it very likely that the first chemistry was the reductive citric acid cycle. You now have a source of those compounds to jump start the process—to get life started. The next thing you need is to be able to encapsulate this chemistry. An amphiphilic bilayer folds itself into a vesicle. No mystery at all. Once you have that, you are well on the way to being a cell, because you have an inside and an outside. You can have transmembrane potentials, transmembrane concentration differences, transmembrane pH differences. So you have the right intermediate chemistry going, and it is encapsulated in a vesicle. Now, the intermediate chemistry of the reductive citric acid cycle leads through ani-

mation reactions to amino acids, through thioacetate or some sort of sulfur-acetyl compounds to lipids, and through pyruvate to sugars. So that you have the core, based on the modern metabolic chart, of the pathways to make everything you need to be a cell. And now we can get a number of these reactions to go without enzymes."

All of this points to an analysis of evolutionary self-organization based more in the domain of physical chemistry than in some kind of esoteric complexity. Morowitz does not want to argue against the idea of self-organization in populations of macromolecules except to say that any such process is not as fundamental to the origin of life in his picture as in others. "Self-organization is driven by a kind of deterministic organic chemistry," he says. "It doesn't require very unlikely chance events or it doesn't require sorting through this enormous number of compounds for the things which then organize themselves."

Princeton University's Philip Anderson thinks now is the time for theorists to get real and to put the kind of detailed physical and organic chemistry that Morowitz is talking about into their models. It is a matter of making qualitative models quantitative. In many computational models of prebiotic chemical processes, no attention is paid to the quantitative side, to actual reaction rates and conditions such as molecular concentrations and so on.

"So what . . . seems to me would be useful now," says Anderson, "would be to begin to try and put something more realistic in place. Not, of course, to reproduce what really happened but to try to see whether there are constraints that are somehow more general. I mean, can catalysis actually happen? Can molecules A, B, and C find each other if molecule C is supposed to catalyze the reaction of A and B? Is it reasonable that molecules C, A, and B all find themselves at the same place at the same time? What has been done until now is equivalent to assuming that you have some kind of big pot, and you have stirred all the molecules up together, and every molecule finds every other molecule. Is that possible? It's an important question."

In a similar vein, Anderson is interested in the thermodynamics of forming large, complex molecules from simpler, smaller ones. We know that living systems maintain themselves by making large, complex molecules, whereas an equilibrium mixture of carbon, hydrogen, nitrogen, and oxygen at terrestrial temperatures is composed of carbon dioxide, water, and nitrogen oxides. Thus, by considering a variety of nonequilibrium conditions, can we understand how the processes where larger molecules are formed got started?

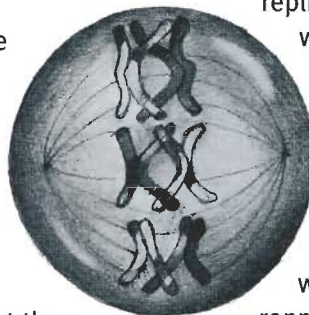
Anderson thinks the question can be approached in terms of real organic and physical chemistry, both experimentally

and theoretically. As he puts it, "If we take realistic reaction conditions and realistic concentrations and realistic rates of feeding from an external energy source, can we expect to see energy getting hooked into increasing the size of some molecules? As far as I can see, questions like that have just never been approached."

Even when we have answers to Anderson's questions, the problem of macromolecular replication will still remain. This is where Indiana's Andrew Ellington is concentrating his efforts. What sort of macromolecule may be able to replicate autonomously, using only the smaller constituents likely to arise in the vicinity of Morowitz' "smokers" or whatever the early prebiotic environment was?

Thomas Cech's discovery of catalytic RNA blurred what appeared to be a firm distinction in the biological roles of nucleic acids and proteins: information-carrying and catalytic activity. Catalytic RNA could have played the role of chicken as well as egg in the prebiotic world, and it is widely believed to be only a matter of time before someone finds a candidate for the first model RNA replicator—a ribozyme with template-dependent polymerase activity. Such a molecule should be able to replicate by sequentially copying its nucleotide residues one by one.

However, Ellington does not insist the replication take place through the sequential addition of nucleotides. He considers it more likely he will find conditions under which he can select a group of small oligonucleotides that perhaps assist their mutual replication in a hypercyclic arrangement and which can be joined together to form a ribozyme with ligase activity. Such a collection of molecules would represent an autocatalytic set with diverse functional specificities—a precise example of the sort of complex adapted chemical system that Kauffman describes as the precursors of biological systems. The attraction of a replicating ribozyme is that it could have served simultaneously as a gene.



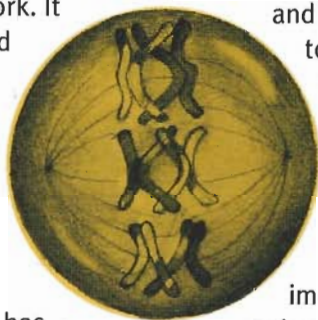
For Kauffman, it is essentially irrelevant whether the first populations of reproducing macromolecules were nucleic acids or proteins. He would like to see the projects of his co-workers extended into the realm of what he calls *autonomous molecular agents*. It is not enough to have sustained chemical reactions encapsulated in a vesicle making more and more complex molecules, some of which are able to replicate through a collectively autocatalytic network. It would not even be enough to find within such a system an inherently information-carrying molecule such as RNA able to replicate itself through a stepwise copying procedure. What Kauffman demands is that the system *do* something. If we are going to say it is "living," then it has to act as an agent of some sort.

The minimal "action" an agent can take to stay alive is to maintain its structure and displacement from equilibrium, or, as Kauffman says, to "construct itself." In thermodynamic terms, this means that some of the work done in the system must perform the task of constraining the flow of energy in the system, so everything needed for the maintenance of the structure gets done. Coupled thermodynamic work cycles are an ubiquitous feature of all living systems, from single cells up to the level of the biosphere. So Kauffman wants to incorporate an analysis of such cycles into the theory of replicating systems and then create communities of his autonomous molecular agents in the laboratory. This is an ambitious project whose overall aim is to provide a general theoretical foundation for biology that goes beyond any details of what can be observed in the one (terrestrial) example given to us. What perhaps distinguishes this initiative from others of similar ambition is that it maintains contact with concepts that can be used to construct and analyze real experimental systems. At this stage, all that can be said is that these new ideas will have to stand the test of time and scientific scrutiny, but Anderson at least is convinced that they can

be developed collaboratively by theorists in the group. He and Kauffman already have had extensive discussions on the topic.

Whether proved right or wrong, this impulse seems to have found resonance with NASA's aim of broadening its scientific mission to include an expanded program in astrobiology. As Kauffman says, "We're on the threshold of creating self-reproducing molecular systems and maybe on the threshold of a general biology. It is impossible to try to undertake the question of what a general biology would be and not realize that it would be profoundly beholden to theory. Not in the trivial sense that there aren't any exemplars yet, but in the much deeper way that if we had a dozen exemplars now of independently formed living systems, we would want to account for their general properties, and we don't know how to do that yet."

Through its coordination of theory and experiment, the SFI-based team is aiming to put its work at the front of the field.



Peter Wills, a professor of physics at the University of Auckland, will be in residence at SFI, collaborating with Stuart Kauffman and others, through December 1997.



PHOTO: © CARY HERZ

ANDERSON RECEIVES AWARD

Philip W. Anderson, a professor of physics at Princeton University and a member of the Santa Fe Institute's Science Board, was awarded in February the John Bardeen Prize for his contributions to the understanding of broken symmetry, the order parameter in the A and B phases of superfluid helium three, and the role of impurities in metallic superconductors.



THE CASE OF THE INDEPENDENT TEST

SANTIAGO

CODE McMULLIN: THE CASE OF THE INDEPENDENT TEST

BY BARRY McMULLIN

Prologue

Some might say that science in the modern sense was born when Galileo Galilei turned his first crude telescope on the night sky and discovered the moons of Jupiter. I'm of the opinion that modern science really arose not when Galileo observed the sky, not even when he published his results, but when he willingly, enthusiastically even, accepted the challenge of independent testing. This allowed others to discover for themselves that Galileo's wild claims had some basis in fact. And in time, it permitted improved design and understanding of telescopes and the critical conditions necessary to the various observational phenomena he described.

Independent, critical testing is a hallmark of what we call science. But even as this core principle has remained unchanged, scientific practice has had to continually debate and refine its interpretation in the face of new technological apparatus, new theoretical understanding, and even, occasionally, the opening of whole new domains of science. Given its focus on innovative and transdisciplinary science, this is a topic of acute interest and ongoing concern to many of the Santa Fe Institute community.

This is my installment in the debate. It's a story of scientists and real science (in all its messy human imperfection), somewhat in contrast to the sanitized and platonic science that scientists sometimes prefer to advertise.

ALife: Hype or Science?

As an emerging domain of intellectual inquiry, Artificial Life (or ALife) has strong ties to SFI through the original series of ALife Workshops organized at the Institute by resident faculty member Chris Langton. There is now a complementary European conference series, and more than one popular book has been published on the subject. At the very least, it qualifies as hype, but is there still rigorous science behind the hype?

The guiding inspiration of ALife is to realize "lifelike" phenomena in artificial media (especially computers) and thus both to improve our understanding of natural living systems and to open up entirely new technological opportunities for artificial living systems.

ALife is then a synthetic enterprise, the attempt to create artificial life. A typical report of research in the field may consist of the claim that a certain lifelike phenomenon will be exhibited by a certain configuration or arrangement of some artificial system or, more specifically, by the execution of a certain class of computer model. Because of this synthetic aspect of the work, the nature of scientific testing or criticism can acquire a somewhat different flavor from more traditional science. Sometimes ALife models are directly related to particular natural living phenomena, and claims arising from the ALife research may be testable by comparison with empirical biological data. However, there is also a class of ALife model in which the relationship to specific living systems is remote or abstract. The objective is not so much to explain in particular natural phenomenon as to claim that a certain kind of phenomenon will be robustly exhibited by a broad variety of systems having a certain abstract or formal structure. Critical testing need not involve comparing with preexisting empirical phenomena but may require independent synthesis of artificial systems, conforming to the stated abstract structure and testing whether they do, in fact, demonstrate the claimed phenomena.

A Case Study

In the early 1970s (well ahead of the modern rebirth of ALife), two Chilean biologists, Humberto Maturana and Francisco Varela, were struggling with the question of how a living system (such as a bacterium) is different from an arbitrary assemblage of its raw molecular constituents. What is so special about the organization of the molecular constituents of a living system that gives rise to the living phenomenology?

Their tentative answer was: living systems are distinguished by a special class of organization that they termed *autopoiesis*, from the Greek root for self-producing. Roughly speaking, an autopoietic system is a closed network of chemical reactions, such that every molecular constituent of the system is a product of one of the reactions in the network, and the system establishes and maintains a spatial boundary that serves to concentrate the reactants and sustain the network.

Varela and Maturana also developed a minimal model for the kinds of components and interactions that seem to be required to support autopoietic organization; that is, they described an abstract artificial chemistry in which autopoietic systems could both spontaneously emerge and sustain themselves. Finally, with another colleague, Ricardo Uribe, they developed a specific computer model or realization of this artificial chemistry and demonstrated that it did indeed support at least the basic autopoietic phenomenology.

Clearly, this work falls under the general class of synthetic ALife work described earlier. There is a claim that certain lifelike phenomena (autopoietic organization) can arise in any system that realizes a certain set of abstract "chemical" interactions. And equally, although this particular claim cannot be meaningfully tested against natural, biological data, it is still perfectly scientific in the sense that it can be tested by the synthesis of independent realizations (computer models) of the same abstract chemistry. Of course, all that remained was for someone to actually do such tests.

I became interested in the computer modeling of autopoiesis about five years ago. While the phenomenology described for the basic system was rudimentary, I hoped it might be possible to extend it to support systems that can grow and reproduce via a crude fissioning mechanism. This could allow an approach to modeling and understanding the evolutionary emergence of genetic reproductive architecture and thus the emergence of "evolvability." However, the first step would clearly have to be to reproduce (sic!) the basic phenomena of the original model.

At first sight, this should not have been difficult. The original published description of the work included both an informal description of the essential interactions in the artificial chemistry and a more detailed algorithmic description of their particular computer program that realized this chemistry. In the noble tradition of the professional academic researcher, I therefore simply presented this paper to an unfortunate graduate student, Hyder Aswad, and asked him to reimplement it.

Which he did.

Unfortunately, this reimplementation failed to exhibit the original phenomena.

I must confess, to my shame, that (remaining true to the aforementioned noble academic tradition) my immediate reaction was to blame my poor graduate student and declare he must have made some mistake (probably more than one!) in his reimplementing. He, on the other hand, rather trenchantly insisted he had checked his work with great care. So, to prove my point (and, as an added benefit, to underscore why I was the exalted professor and he the humble graduate student), I duly rolled up my virtual shirtsleeves and proceeded to take his program apart, line by line, to identify exactly where he had gone wrong.

Unfortunately, it was now my turn to fail. Miserably. I could find absolutely nothing wrong. Granted, some aspects of the original description were unclear or ambiguous; some details of the implementation might have been done more efficiently. But none of this could apparently explain the failure of the model.

At this point, I contacted Francisco Varela, whom I had met briefly. He told me that, regrettably, the original program had been lost during the political/military events in Chile in the 1970s. Varela had been a "militant supporter," as he put it, of the government of President Miguel Allende. After Allende was killed on September 11, 1973, Varela was threatened by the Chilean military. On two occasions, night patrols came looking for him at his house, but they did not find him—he no longer slept there. Then he was dismissed from his job at the University of Chile on the orders of men only identified as "superiors." He and his family decided to sell their belongings and leave the country. "The diaspora of the department's scientists ended a period of science in Chile, an important stage of my personal life, and with it the context which gave birth to the idea of autopoiesis," he said.

Nevertheless, he directed me to a student of his who had recently reimplemented the model as an exercise. The student, François Jullien, kindly forwarded his Pascal program code to me, noting, however, that it had been developed on an obsolete computer platform that he no longer had available, so he could not run the program anymore. Obviously, I did not have access to this obsolete platform either, but I hoped to glean some insight from an examination of his code. The identifiers and comments in the code were in French, with which, unfortunately, I have only modest familiarity. But Jullien generously agreed to provide an overall technical description of the program in English.

In parallel with this, I also sought other researchers who might have successfully reproduced the original phenomena. A literature search revealed only one substantive example, by Milan Zeleny. But at least the code from this example had apparently been made publicly available. Admittedly, it was written in a somewhat obscure and specialist computer language (APL), but I was prepared to deal with that if necessary. However, when I contacted Zeleny, I learned that while his code had been made freely available at the time, this was some time in the past, and all copies had now, as far as he knew, been lost.

Broadcasting a message on a related Internet mailing list elicited one additional lead: John Mingers, a researcher based in the United Kingdom, had also independently reimplemented this model as an exercise. But, as with Jullien's version, this had been developed on a computer platform that was now obsolete and no longer available. In fact, only a hard copy of the code rather than a machine-readable version had been retained. However, Mingers had recently had the program manually rekeyed from the hard copy, though he had not made any attempt to get it running. He was willing to give me a copy of this and, indeed, collaborate with efforts to get it running again.

I studied the code from these two independent reimplementations carefully but could identify no significant difference from that of my own student. Clearly, I would need to see either or both of these actually working if I was to make further progress. Over a period of time and with some assistance from both Jullien and Mingers, I ported both of their programs to a common platform that I had available and got them "working." That is, they executed, apparently as their respective authors had intended, but they did not exhibit the autopoietic phenomena described in the original publication.

At this point, frustration and other demands on my time meant I had to put this project aside. About a year and a half later, I was eventually able to return to it during my sabbatical visit to SFI. At the Institute, my first step was to build yet another model of the artificial chemistry, this time layered on top of the general-purpose simulation system Swarm. This adopted a different (agent-based) simulation architecture, ignoring the original algorithm and attempting to capture only the original qualitative chemistry. Not surprisingly, this again made no substantive difference: the original phenomena could not be reproduced with this model either.

At this point, the failures could be clearly related to a single core problem: a certain reaction of the artificial chemistry ("bonding"), when implemented according to the original specification, was taking effect "prematurely" relative to the putative autopoietic network. It seemed something must be missing from the model, something that was not included in the published description but was present in the original simulation code. But how could this possibly be identified now, a quarter of a century after the original experiments had been carried out?

Solution by Serendipity

By a wonderful and utterly unpredictable chance, just as I contacted Francisco Varela at Ecole Polytechnique in Paris again to pass on my assessment that there was some crucial lacuna in the original published descriptions of this model, he received a shipment of papers from Santiago, dating from the general period when this work was carried out. Searching through these materials, he found a single hardcopy printout of a single version of the original simulation program, together with a short textual description of the model, which he believed to be at least roughly contemporary with the program listing.

This was the intellectual equivalent of striking gold. If there was ever to be any possibility of definitively resolving what was going on in the original program, then access to that program's code was the absolute *sine qua non*, and here, against all rational expectations, was, precisely, a snapshot of that program code. Granted, a rather grainy snapshot, in black and white, but more than I had expected to have available.

Notwithstanding all that, I hesitated long and hard before committing to a detailed examination of this code. It was written in the long obsolete language, FORTRAN-IV. It was only sparsely commented, and those comments were in Spanish. Variable names were generally less than four characters long and had little obvious mnemonic content. The program was structured as a single monolithic block of code, rather than using procedural decomposition (i.e., subroutines). There was no guarantee this particular version of the program even worked. Nor could I assess its likely relation to the version used to generate the published results. Trying to get it working would require manual rekeying, which is intrinsically error prone. However, while all these were legitimate causes for concern, I had exhausted all other avenues of attack, so it seemed I had little to lose by attempting to decipher this program.

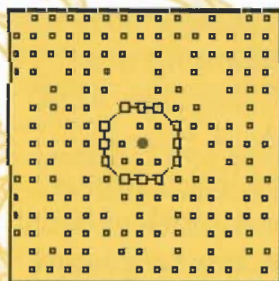
My strategy was to first rekey the program and attempt to get it “working” in some minimal sense, without making any serious effort to understand its detailed functioning. Only if I could achieve this and if the program exhibited the crucial phenomenon (i.e., that premature bonding was somehow avoided) would I invest the effort to analyze the program’s mechanisms in detail.

Somewhat to my surprise, this strategy worked amazingly well. Only a small number of keying errors occurred, most of which were identifiable from compiler diagnostics. There was only one “external” fragment of code missing, and its function could be reasonably inferred from the code at hand, so it was possible to design a functional replacement. This resulted in a program that could be executed, and against my more pessimistic expectations, it reproduced a variety of characteristic phenomena of the original published results. Most crucially, it exhibited the avoidance of premature bonding that had plagued all my attempts at reproducing the full-blown autopoietic phenomena.

Now knowing that the answer to my question was surely concealed somewhere in this program, I threw myself into a sustained attempt to understand it. It was not necessary to grasp the program in its entirety, a key factor, but only the implementation of one particular interaction. Thus, a relatively cursory examination of the program quickly allowed me to narrow my search down to one relatively small section—and to identify exactly what was built into the program that was preventing the premature bonding. This turned out to be a special inhibition effect that only came into play in certain special but critical circumstances. This effect had not been mentioned in any of the published descriptions of the model but seemed to be responsible for the key difference between the original program and my attempts at reproducing it.

So, finally, I was ready to make the critical, scientific test: I incorporated the appropriate analog of this new effect into my own, otherwise entirely independent, implementation of the model. I had to experimentally adjust other parameters of the model to take account of this, but in less than half an hour (and after only five years of trying), I finally saw the full-blown autopoietic phenomena reported in the original paper.

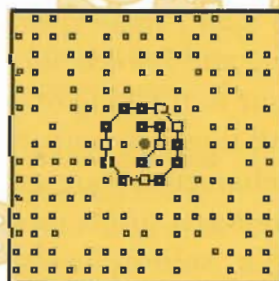
ARTIFICIAL CELL



ARTIFICIAL CELL: TIME 0

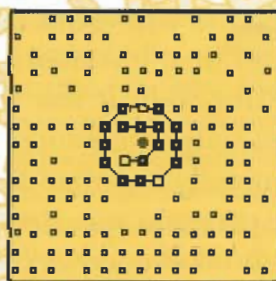
The Artificial Cell Substrate particles can diffuse through the membrane, where they react under the influence of the catalyst particle to produce more membrane particles. These are trapped by the membrane and thus build up in concentration, held ready in reserve to effect a repair to the membrane whenever it should rupture. Such repair re-establishes the conditions for the continued maintenance of the cell.

PREMATURE BONDING



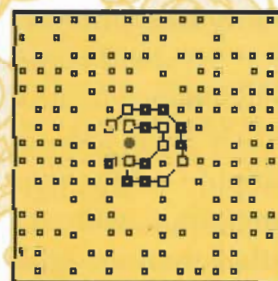
EXPERIMENT 1: TIME 30

However, with the model as originally described, the new membrane fragments spontaneously and prematurely bond together, inside the cell. This makes them immobile and thus unavailable to migrate to a rupture site and effect a repair.



EXPERIMENT 1: TIME 114

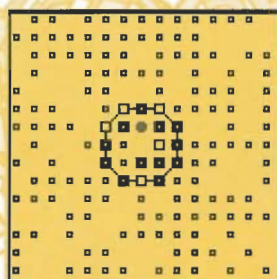
DEGENERATION



EXPERIMENT 1: TIME 238

When the cell ruptures, instead of a repair, the interior fragment gets incorporated, leaving the catalyst particle free. The conditions for continued repair no longer exist—the cell is effectively dead.

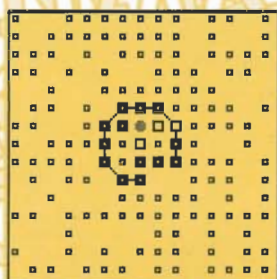
BOND INHIBITION



EXPERIMENT 2: TIME 170

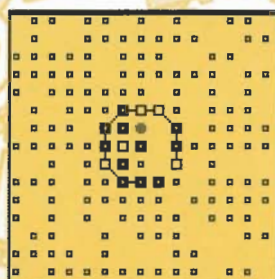
The newly rediscovered effect stipulates that free membrane particles may not bond if they are immediately adjacent to already doubly bonded membrane particles. This prevents premature bonding and permits the buildup of a reserve of free, mobile, membrane particles.

SELF-REPAIR

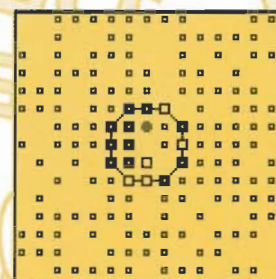


EXPERIMENT 2: TIME 180

At time 180 the membrane ruptures; by time 217 a replacement particle has drifted into position; and at time 226 the repair is successfully completed.



EXPERIMENT 2: TIME 217



EXPERIMENT 2: TIME 226

Epilogue

There are a few worthwhile points to my story, I think. First, that science, in its necessary sociality, relies on scientists being open and willing to engage in critical assessments of their own work. In this respect, I am left with an abiding admiration for Francisco Varela, who happily and enthusiastically took up the critical challenge with which I presented him and was unfailingly courteous and open in his responses. The tale could never have had a satisfying resolution otherwise.

Second, science is actually an art. By which I mean that it is not a specific method or process or algorithm by which knowledge is somehow guaranteed but is rather a challenge and an adventure, where failure is at least as common as success, and luck, intuition, and informal judgments play a pervasive role. I imagine that most scientists already know this, but we somehow feel a need to disguise or hide it. Perhaps it's time to come clean.

Finally, science is open and unlimited. Entirely new domains of inquiry, such as ALife, do emerge and can and should be subjected to the hard scientific criterion of independent, critical, testing.

BARRY McMULLIN IS A PROFESSOR IN THE SCHOOL OF ENGINEERING AT DUBLIN CITY UNIVERSITY IN IRELAND. HE IS FINISHING A SABBATICAL YEAR AT SFI, WORKING WITH CHRIS LANGTON AND THE SWARM GROUP.

SIX JOIN THE INSTITUTE

In March, five distinguished scientists: Randall Beer, David Griffeth, Charles Manski, Daniel McShea, and Jonathan Shapiro became Santa Fe Institute External Faculty members. Although their home institutions are elsewhere, these scholars play a lead role in the Institute's research programs. In addition, Burton Singer was named to SFI's Science Board.



RANDALL BEER received his Ph.D. in computer science from Case Western Reserve University and is currently on the faculty there, both in computer science and biology. His main work is on computational neurobiology, using computer simulations of the neural level to model the behavior of animals. Beer's work is a wedge into the large explanatory gap between the brain and adaptive or intelligent behavior that now exists.

He is actively collaborating with neuroscientists, ethologists, and dynamical systems theorists on research that is bridging this gap. He is also deeply interested in philosophical questions related to artificial intelligence, cognitive science, and artificial life.



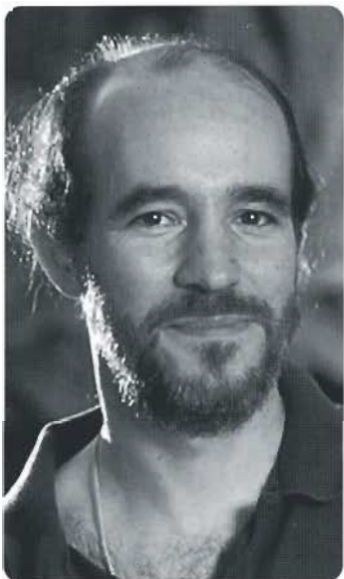
DAVID GRIFFEATH is professor of mathematics at the University of Wisconsin. His expertise is the analysis of interacting particle systems, a broad class of mathematical systems that includes classical percolation systems, "stepping-stone" systems, "voter models," and stochastic cellular automata. Each of these systems is closely related to science at SFI, with relevance ranging from physics to computer science to economics to biology. Griffeth

is a pioneer in the use of probabilistic analysis for these systems, and, together with an ever-growing family of collaborators, former students, and postdocs, he has succeeded in establishing a body of rigorous mathematical results for stochastic cellular automata.



CHARLES MANSKI is Hildale Professor in the economics department at the University of Wisconsin. Two areas of Manski's research are especially relevant to SFI's economics program. He has done significant work on the econometric identification of interactions relationships from economic data sets. Much of the theoretical work associated with SFI economics assumes the presence of various interaction structures that link different eco-

nomic actors. With the exception of Manski's research, little work has been done on how the nature of these interactions can be empirically measured. Manski has also done work on the identification of individual expectations from survey data. Given the Institute's work in bounded rationality, this is relevant in assessing the empirical relevance of a range of theoretical frameworks.



DANIEL MCSHEA is a paleontologist and evolutionary biologist on the zoology faculty at Duke University. McShea's work studies large-scale trends in evolution, from empirical, simulation, and theoretical perspectives. His dissertation, titled "Complexity in Evolution," questioned the widely accepted (but not well-defined) "truism" that morphological complexity increases in evolution. It investigated this purported increase by first

defining specific quantitative measures of morphological complexity and then empirically examining (via fossil measurements) the trends taken by these measures in the evolution of various species. More recently, he has looked at several possible mechanisms by which morphological complexity might increase in evolution and by his empirical work has been able to give some evidence for their relative plausibility.



JONATHAN SHAPIRO is senior lecturer in computer science at Manchester University, England. His work has centered on the theory of genetic algorithms (GA's). As Science Board member Richard Palmer writes, "He was the first to supply exactly what has been missing in the field for almost 20 years—a useful in-depth theoretical analysis that focuses on the important quantities. Past GA theory has generally not been very useful, often because

of the traditional computer science training of the theoreticians, where 'every bit counts.' Jonathan brought a much more useful statistical mechanics viewpoint to bear, one which tries to average over the fine details, keeping only the essentials. Others (including [Jim] Crutchfield, [Melanie] Mitchell and [Erik] van Nimwegen at SFI) have taken up this approach and developed it in new directions."



BURTON SINGER Trained in mathematics and statistics, Burton Singer continues to contribute to mathematics while applying his talents and intellect to problems in econometrics, health and disease, and sustainability. After many years at Columbia University, Singer went to Yale University, eventually becoming associate dean for public health. He joined Princeton University three years ago, where he is in the Office

of Population Research and associate director of the Princeton Environmental Institute. He also directs the environmental studies program. Singer is a member of the National Academy of Sciences, a fellow of the American Statistical Association, and a winner of the Mindel Sheps Award in Demography.

The Institute's Science Board advises on broad issues relating to SFI's scientific agenda.

Few research institutes receive as much attention or generate as much controversy as does the Santa Fe Institute. Over the past decade, the Institute has had, by most accounts, remarkable successes in stimulating new approaches to the study of complexity, in establishing a reputation as an innovative center for scientific research, and in seeding networks of researchers whose common interests transcend traditional disciplinary boundaries.

That's an impressive track record, but not exactly how success is usually measured by scientific research institutions. "Famously interdisciplinary" was the term used in a recent *Washington Post* article in referring to SFI. Others, of course, regard it as just plain "flaky."

Are we innovative or just plain flaky or both? Opinions diverge even among ourselves. And, not surprisingly, when it comes to the hard questions of mapping out the future directions and applicability of research in complex adaptive systems, the perspectives are as numerous and diverse as the definitions of complexity itself.

Differences of opinion notwithstanding, one thing that unites members of the SFI community is their conviction that the most fragile and valuable aspect of the Institute is its commitment to supporting fresh, catalytic research not likely to occur elsewhere. Which brings up the question: how good a job are we doing at keeping that commitment alive?

To address these concerns, we decided early this year to organize an internal review of the Institute's programs. As befits the Institute, the review process won't bear much resemblance to the conventional convening of representatives from various disciplines for the purpose of assessing contributions to those disciplines. In fact, the term *review* here is perhaps something of a misnomer: the goal of the process is to help structure our own internal, ongoing debate on issues relating to the scientific excellence and vitality of the Institute.

To help plan the review, a one-day retreat was held in January. Representatives of the resident faculty, staff, the Science Board, and the Board of Trustees attended the retreat.

As a result of discussions at the retreat, the charge to the review panel was structured to focus on five questions that are explicitly designed to address issues of process and thereby indirectly also to raise issues of scientific content. The five questions for the review panel are:

Given our goals at SFI, how should we define and evaluate scientific excellence? Since our goals of producing innovative science often differ in fundamental ways from traditional research, how do we evaluate the

INSIDE SFI HARD QUESTIONS BY ERICA JEN VICE PRESIDENT FOR ACADEMIC AFFAIRS

quality of our work, both as individuals and as an institute? What is the relative importance or effectiveness in generating a stream of ideas, seeding new networks of interested researchers, pushing problems from the initial stage of formulation to later ones of solution, proposing versus validating models, attaining recognition from the mainstream scientific communities, publishing in respected scientific journals? How good a job have we been doing at achieving scientific excellence by our own measures? What have been our successes and failures?

What should be the role of the SFI site in relation to the metabolism of the whole of the SFI community? The SFI site has begun to play an increasingly influential role in determining the research programs at SFI, and the effect has been in some ways a problematical one: the consensus holds that if the SFI community were to "implode" onto the site, the Institute would die. What should be the relative roles of the resident and external researchers? How can we better use the talents of Science Board and External Faculty members? How could the on-site activities such as the visitor program be better organized—through the use of theme structures, for example—to make the visits of off-site affiliates more productive? What should be the criteria for resident researchers? What should be the on-site mix of students, postdocs, junior, and senior researchers? Should there be more turnover of long-term resident researchers?

How do we encourage the processes of renewal, follow-up, evolution at SFI? How can we do a better job of identifying good, new research personnel with good, new research agendas? What mechanisms should be in place for initiating, nurturing, and phasing out programs and projects? How good a job have we been doing at identifying areas in which SFI can make a difference? How can we facilitate the processes of cross-fertilization, follow-through on research ideas, and evolution into new research directions? How can we overcome the problems imposed by our own limitations in infrastructure (small library, lack of experimental facilities), our own accidents of history? How do we work to close the gap between model and problem, catalysis and excellence, formulation and solution? How could we improve our understanding of research taking place outside the SFI community?

How do we reestablish broadly based discussions of fundamental intellectual themes? The consensus holds that in the early years at SFI, there was a greater propensity for cross-disciplinary discussion of fundamental intellectual themes. How can we reestablish this as a dominant mode of operation at SFI? Would it be useful to sponsor a series of working-group meetings similar to the "founding workshops"? Could the program structure and other administrative structures at SFI be modified to facilitate more broadly-based discussions?

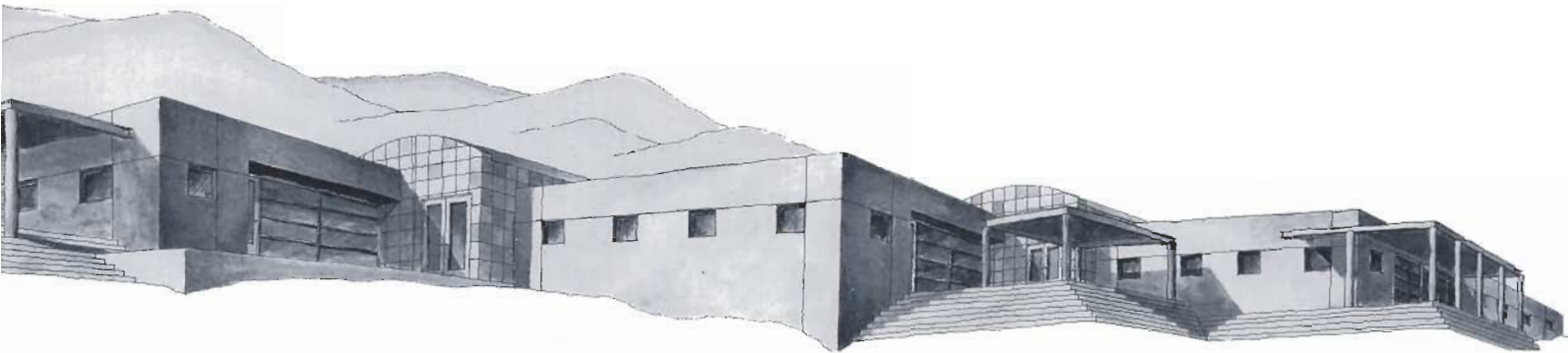
What is the appropriate funding model for SFI? Current funding sources for research at SFI include government agencies, private corporations, foundations and individual donors. Funding is provided for unrestricted core research; for research restricted to specific topics; for multidisciplinary, collaborative projects involving several individuals; and for individual research projects.

However, it has become apparent that (1) dependence on funding sources with strong programmatic missions has had in some cases a deleterious effect on the balance and priorities of Institute research; and (2) there are, for some of the individual research grants sponsored through the Institute, significant issues of appropriateness of scientific content, coordination with the whole of Institute research, and even conflict of interest on the part of the individual researchers. What funding model should be operative at the Institute to encourage creativity, maintain competitiveness and intellectual freedom, and avoid Balkanization of scientific research?

Also, on the basis of recommendations that emerged from the retreat, it was decided that the review panel should ideally consist of a subset of SFI science board members who have some familiarity with Institute programs but are not necessarily intimately involved with them.

SFI President Ellen Goldberg has now appointed the review panel, which consists of Robert McCormick Adams (University of California/San Diego and former Secretary, Smithsonian Institution), Simon Levin (ecology, Princeton University), Nancy Kopell (mathematics, Boston University), Roger Shepard (psychology, Stanford University), and Henry Wright (history, University of Michigan). All members of the review panel have thus far completed at least one visit to SFI and are currently in the process of soliciting additional input from both on- and off-site SFI researchers, as well as meeting among themselves to formulate their recommendations.





KRESGE AWARD GOES TO SFI

The Kresge Foundation of Troy, Michigan, has awarded the Santa Fe Institute a \$500,000 grant for the renovation and expansion of its campus facility. The grant has been made on a challenge basis, requiring SFI to raise \$1.2 million to complete its expansion campaign by September 1998.

Kresge notified SFI of the grant award in March.

In 1996, The Kresge Foundation reviewed 699 proposals and awarded grants totaling nearly \$88 million to 144 charitable organizations in forty states and Canada. Grants are made to institutions operating in the areas of higher education, health and long-term care, the arts and humanities, human services, science and the environment, and public affairs.

SFI used a new approach, marketing its campus campaign on its home page on the Internet this spring. People who logged onto the SFI site could receive information about the expansion as well as order SFI promotional items like hats and T-shirts, sweatshirts, mousepads, and tote bags. Proceeds from these sales went to the Campus Fund.

As fund-raising continues, permits are in hand, and ground-breaking is scheduled to begin in July.





1



7



14

IN THIS ISSUE...

FEATURE STORIES

- 1** Jonathan Haas Studies Pueblo Political Life
- 7** Tree of Life: Scientists Model Nature's System of Fractal-branching Networks
- 18** Code McMullin: The Case of the Independent Test

WORKS IN PROGRESS

- 11** Between Biotics and Abiotics
- 13** Ecological Discontinuities
- 14** Turning the Corner in the Search for the Origin of Life

NEWS

- 26** Six Join the Institute
- 28** Inside SFI: Hard Questions

**INSIDE
BACK
COVER**

Kresge Award to SFI



18

Santa Fe Institute
1399 Hyde Park Road
Santa Fe, New Mexico 87501
U.S.A.

Non-Profit
Organization
U.S. Postage
PAID
Permit No. 388
Albuquerque, NM