EXCERPTED FROM

STEPHEN WOLFRAM
A NEW KIND OF SCIENCE

NOTES FOR CHAPTER 1:

The Foundations for a New Kind of Science

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An Outline of Basic Ideas

■ **Mathematics in science.** The main event usually viewed as marking the beginning of the modern mathematical approach to science was the publication of Isaac Newton's 1687 book *Mathematical Principles of Natural Philosophy* (the *Principia*). The idea that mathematics might be relevant to science nevertheless had long precursors in both practical and philosophical traditions. Before 500 BC the Babylonians were using arithmetic to describe and predict astronomical data. And by 500 BC the Pythagoreans had come to believe that all natural phenomena should somehow be reducible to relationships between numbers. Many Greek philosophers then discussed the general concept that nature should be amenable to abstract reasoning of the kind used in mathematics. And at a more practical level, the results and methodology of Euclid's work on geometry from around 300 BC became the basis for studies in astronomy, optics and mechanics, notably by Archimedes and Ptolemy. In medieval times there were some doubts about the utility of mathematics in science, and in the late 1200s, for example, Albertus Magnus made the statement that "many of the geometer's figures are not found in natural bodies, and many natural figures, particularly those of animals and plants, are not determinable by the art of geometry". Roger Bacon nevertheless wrote in 1267 that "mathematics is the door and key to the sciences", and by the 1500s it was often believed that for science to be meaningful it must somehow follow the systematic character of mathematics. (Typical of the time was the statement of Leonardo da Vinci that "no human inquiry can be called science unless it pursues its path through mathematical exposition and demonstration".) Around the end of the 1500s Galileo began to develop more explicit connections between concepts in mathematics and in physics, and concluded that the universe could be understood only in the "language of mathematics", whose "characters are triangles, circles and other geometric figures". What Isaac Newton then did was in effect to suggest that natural systems are at some fundamental level actually governed by purely abstract laws that can be specified in terms of mathematical equations. This idea has met with its greatest success in physics, where for the past three centuries essentially every major theory has been formulated in terms of mathematical equations. Starting in the mid-1800s, it has also had increasing success in chemistry. And in the past century, it has had a few scattered successes in dealing with simpler phenomena in fields like biology and economics. But despite the vast range of phenomena in nature that have never successfully been described in mathematical terms, it has become quite universally assumed that, as David Hilbert put it in 1900, "mathematics is the foundation of all exact knowledge of natural phenomena". There continue to be theories in science that are not explicitly mathematical—examples being continental drift and evolution by natural selection—but, as for example Alfred Whitehead stated in 1911, it is generally believed that "all science as it grows toward perfection becomes mathematical in its ideas".

■ **Definition of mathematics.** When I use the term "mathematics" in this book what I mean is that field of human endeavor that has in practice traditionally been called mathematics. One could in principle imagine defining mathematics to encompass all studies of abstract systems, and indeed this was in essence the definition that I had in mind when I chose the name *Mathematica*. But in practice mathematics has defined itself to be vastly narrower, and to include, for example, nothing like the majority of the programs that I discuss in this book. Indeed, in many respects, what is called mathematics today can be seen as a direct extension of the particular notions of arithmetic and geometry that apparently arose in Babylonian times. Typical dictionary definitions reflect this by describing mathematics as the study of number and space, together with their abstractions and generalizations. And even logic—an abstract system that dates from antiquity—is not normally considered part of mainstream mathematics. Particularly over the past century the defining characteristic of research in mathematics has increasingly been the use of theorem and proof methodology. And while some generalization has occurred in the types of systems being studied, it has usually been much limited by the desire to maintain the validity of some set of theorems (see page 793). This emphasis on theorems has also led to a focus on equations that statically state facts rather than on rules that define actions, as in most of the systems in this book. But despite all these issues, many mathematicians implicitly tend to assume that somehow mathematics as it is practiced is universal, and that any possible abstract system will be covered by some area of mathematics or another. The results of this book, however, make it quite clear that this is not the case, and that in fact traditional mathematics has reached only a tiny fraction of all the kinds of abstract systems that can in principle be studied.

■ **Reasons for mathematics in science.** It is not surprising that there should be issues in science to which mathematics is relevant, since until about a century ago the whole purpose of mathematics was at some level thought of as being to provide abstract idealizations of aspects of physical reality (with the consequence that concepts like dimensions above 3 and transfinite numbers were not readily accepted as meaningful even in mathematics). But there is absolutely no reason to think that the specific concepts that have arisen so far in the history of mathematics should cover all of science, and indeed in this book I give extensive evidence that they do not. At times the role of mathematics in science has been used in philosophy as an indicator of the ultimate power of human thinking. In the mid-1900s, especially among physicists, there was occasionally some surprise expressed about the effectiveness of mathematics in the natural sciences. One explanation advanced by Albert Einstein was that the only physical laws we can recognize are ones that are easy to express in our system of mathematics.

■ **History of programs and nature.** Given the idea of using programs as a basis for describing nature, one can go back in history and find at least a few rough precursors of this idea. Around 100 AD, for example, following earlier Greek thinking, Lucretius made the somewhat vague suggestion that the universe might consist of atoms assembled according to grammatical rules like letters and words in human language. From the Pythagoreans around 500 BC through Ptolemy around 150 AD to the early work of Johannes Kepler around 1595 there was the notion that the planets might follow definite geometrical rules like the elements of a mechanical clock. But following the work of Isaac Newton in the late 1600s it increasingly came to be believed that systems could only meaningfully be described by the mathematical equations they satisfy, and not by any explicit mechanism or rules. The failure of the concept of ether and the rise of quantum mechanics in the early 1900s strengthened this view to the point where at least in physics mechanistic explanations of any kind became largely disreputable. (Starting in the 1800s systems based on very simple rules were nevertheless used in studies of genetics and heredity.) With the advent of electronics and computers in the 1940s and 1950s, models like neural networks and cellular automata began to be introduced, primarily in biology (see pages 876 and 1099). But in essentially all cases they were viewed just as approximations to models based on traditional mathematical equations. In the 1960s and 1970s there arose in the early computer hacker community the general idea that the universe might somehow operate like a program. But attempts to engineer explicit features of our universe using constructs from practical programming were unsuccessful, and the idea largely fell into disrepute (see page 1026). Nevertheless, starting in the 1970s many programs were written to simulate all sorts of scientific and technological systems, and often these programs in effect defined the models used. But in almost all cases the elements of the models were firmly based on traditional mathematical equations, and the programs themselves were highly complex, and not much like the simple programs I discuss in this book. (See also pages 363 and 992.)

■ **Extensions of mathematics.** See page 793.

■ **The role of logic.** In addition to standard mathematics, the formal system most widely discussed since antiquity is logic (see page 1099). And starting with Aristotle there was in fact a long tradition of trying to use logic as a framework for drawing conclusions about nature. In the early 1600s the experimental method was suggested as a better alternative. And after mathematics began to show extensive success in describing nature in the late 1600s no further large-scale efforts to do this on the basis of logic appear to have been made. It is conceivable that Gottfried Leibniz might have tried in the late 1600s, but when his work was followed up in the late 1800s by Gottlob Frege and others the emphasis was on building up mathematics, not natural science, from logic (see page 1149). And indeed by this point logic was viewed mostly as a possible representation of human thought—and not as a formal system relevant to nature. So when computers arose it was their numerical and mathematical rather than logical capabilities that were normally assumed relevant for natural science. But in the early 1980s the cellular automata that I studied I often characterized as being based on logical rules, rather than traditional mathematical ones. However, as we will see on page 806, traditional logic is in fact in many ways very narrow compared to the whole range of rules based on simple programs that I actually consider in this book.

■ **Complexity and theology.** Both complexity and order in the natural world have been cited as evidence for an intelligent creator (compare page 1195). Early mythologies most often assume that the universe started in chaos, with a supernatural being adding order, then creating a series of specific complex natural systems. In Greek philosophy it was commonly thought that the regularities seen in astronomy and elsewhere (such as the obvious circular shapes of the Sun and Moon) were reflections of perfect mathematical forms associated with divine beings. About complexity Aristotle did note that what nature makes is "finer than art", though this was not central to his arguments about causes of natural phenomena. By the beginning of the Christian era, however, there is evidence of a general belief that the complexity of nature must be the work of a supernatural being—and for example there are statements in the Bible that can be read in this way. Around 1270 Thomas Aquinas gave as an argument for the existence of God the fact that things in nature seem to "act for an end" (as revealed for example by always acting in the same way), and thus must have been specifically designed with that end in mind. In astronomy, as specific natural laws began to be discovered, the role of God began to recede somewhat, with Isaac Newton claiming, for example, that God must have first set the planets on their courses, but then mathematical laws took over to govern their subsequent behavior. Particularly in biology, however, the so-called "argument by design" became ever more popular. Typical was John Ray's 1691 book *The Wisdom of God Manifested in the Works of the Creation,* which gave a long series of examples from biology that it claimed were so complex that they must be the work of a supernatural being. By the early 1800s, such ideas had led to the field of natural theology, and William Paley gave the much quoted argument that if it took a sophisticated human watchmaker to construct a watch, then the only plausible explanation for the vastly greater complexity of biological systems was that they must have been created by a supernatural being. Following the publication of Charles Darwin's *Origin of Species* in 1859 many scientists began to argue that natural selection could explain all the basic phenomena of biology, and although some religious groups maintained strong resistance, it was widely assumed by the mid-1900s that no other explanation was needed. In fact, however, just how complexity arises was never really resolved, and in the end I believe that it is only with the ideas of this book that this can successfully be done.

■ **Artifacts and natural systems.** See page 828.

■ **Complexity and science.** Ever since antiquity science has tended to see its main purpose as being the study of regularities—and this has meant that insofar as complexity is viewed as an absence of regularities, it has tended to be ignored or avoided. There have however been occasional discussions of various general aspects of complexity and what can account for them. Thus, for example, by 200 BC the Epicureans were discussing the idea that varied and complex forms in nature could be made up from arrangements of small numbers of types of elementary atoms in much the same way as varied and complex written texts are made up from small numbers of types of letters. And although its consequences were remarkably confused, the notion of a single underlying substance that could be transmuted into anything—living or not—was also a centerpiece of alchemy. Starting in the 1600s successes in physics and discoveries like the circulation of blood led to the idea that it should be possible to explain the operation of almost any natural system in essentially mechanical terms—leading for example René Descartes to claim in 1637 that we should one day be able to explain the operation of a tree just like we do a clock. But as mathematical methods developed, they seemed to apply mainly to physical systems, and not for example to biological ones. And indeed Immanuel Kant wrote in 1790 that "it is absurd to hope that another Newton will arise in the future who will make comprehensible to us the production of a blade of grass according to natural laws". In the late 1700s and early 1800s mathematical methods began to be used in economics and later in studying populations. And partly influenced by results from this, Charles Darwin in 1859 suggested natural selection as the basis for many phenomena in biology, including complexity. By the late 1800s advances in chemistry had established that biological systems were made of the same basic components as physical ones. But biology still continued to concentrate on very specific observations—with no serious theoretical discussion of anything as general as the phenomenon of complexity. In the 1800s statistics was increasingly viewed as providing a scientific approach to complex processes in practical social systems. And in the late 1800s statistical mechanics was then used as a basis for analyzing complex microscopic processes in physics. Most of the advances in physics in the late 1800s and early 1900s in effect avoided complexity by concentrating on properties and systems simple enough to be described by explicit mathematical formulas. And when other fields tried in the early and mid-1900s to imitate successes in physics, they too generally tended to concentrate on issues that seemed amenable to explicit mathematical formulas. Within mathematics itself—especially in number theory and the three-body problem—there were calculations that yielded results that seemed complex. But normally this complexity was viewed just as something to be overcome—either by looking at things in a different way, or by proving more powerful theorems—and not as something to be studied or even much commented on in its own right.

In the 1940s, however, successes in the analysis of logistical and electronic systems led to discussion of the idea that it might be possible to set up some sort of general approach to complex systems—especially biological and social ones. And by the late 1940s the cybernetics movement was becoming increasingly popular—with Norbert Wiener emphasizing feedback control and stochastic differential equations, and John von Neumann and others emphasizing systems based on networks of elements often modelled after neurons. There were spinoffs such as control theory and game theory, but little progress was made on core issues of complexity, and already by the mid-1950s what began to dominate were vague discussions involving fashionable issues in areas such as psychiatry and anthropology. There also emerged a tradition of robotics and artificial intelligence, and a few of the systems that were built or simulated did show some complexity of behavior (see page 879). But in most cases this was viewed just as something to be overcome in order to achieve the engineering objectives sought. Particularly in the 1960s there was discussion of complexity in large human organizations—especially in connection with the development of management science and the features of various forms of hierarchy—and there emerged what was called systems theory, which in practice typically involved simulating networks of differential equations, often representing relationships in flowcharts. Attempts were for example made at worldwide models, but by the 1970s their results—especially in economics—were being discredited. (Similar methods are nevertheless used today, especially in environmental modelling.)

With its strong emphasis on simple laws and measurements of numbers, physics has normally tended to define itself to avoid complexity. But from at least the 1940s, issues of complexity were nevertheless occasionally mentioned by physicists as important, most often in connection with fluid turbulence or features of nonlinear differential equations. Questions about pattern formation, particularly in biology and in relation to thermodynamics, led to a sequence of studies of reaction-diffusion equations, which by the 1970s were being presented as relevant to general issues of complexity, under names like self-organization, synergetics and dissipative structures. By the late 1970s the work of Benoit Mandelbrot on fractals provided an important example of a general approach to addressing a certain kind of complexity. And chaos theory—with its basis in the mathematics of dynamical systems theory—also began to become popular in the late 1970s, being discussed particularly in connection with fluid turbulence. In essentially all cases, however, the emphasis remained on trying to find some aspect of complex behavior that could be summarized by a single number or a traditional mathematical equation.

As discussed on pages 44–50, there were by the beginning of the 1980s various kinds of abstract systems whose rules were simple but which had nevertheless shown complex behavior, particularly in computer simulations. But usually this was considered largely a curiosity, and there was no particular sense that there might be a general phenomenon of complexity that could be of central interest, say in natural science. And indeed there remained an almost universal belief that to capture any complexity of real scientific relevance one must have a complex underlying model. My work on cellular automata in the early 1980s provided strong evidence, however, that complex behavior very much like what was seen in nature could in fact arise in a very general way from remarkably simple underlying rules. And starting around the mid-1980s it began to be not uncommon to hear the statement that complex behavior can arise from simple rules—though often there was great confusion about just what this was actually saying, and what, for example, should be considered complex behavior, or a simple rule.

That complexity could be identified as a coherent phenomenon that could be studied scientifically in its own right was something I began to emphasize around 1984. And having created the beginnings of what I considered to be the necessary intellectual structure, I started to try to develop an organizational structure to allow what I called complex systems research to spread. Some of what I did had fairly immediate effects, but much did not, and by late 1986 I had started building *Mathematica* and decided to pursue my own scientific interests in a more independent way (see page 20). By the late 1980s, however, there was widespread discussion of what was by then being called complexity theory. (I had avoided this name to prevent confusion with the largely unrelated field of computational complexity theory). And indeed many of the points I had made about the promise of the field were being enthusiastically repeated in popular accounts—and there were starting to be quite a number of new institutions devoted to the field. (A notable example was the Santa Fe Institute, whose orientation towards complexity seems to have been a quite direct consequence of my efforts.)

But despite all this, no major new scientific developments were forthcoming—not least because there was a tremendous tendency to ignore the idea of simple underlying rules and of what I had discovered in cellular automata, and instead to set up computer simulations with rules far too complicated to allow them to be used in studying fundamental questions. And combined with a predilection for considering issues in the social and biological sciences that seem hard to pin down, this led to considerable skepticism among many scientists—with the result that by the mid-1990s the field was to some extent in retreat—though the statement that complexity is somehow an important and fundamental issue has continued to be emphasized especially in studies of ecological and business systems.

Watching the history of the field of complexity theory has made it particularly clear to me that without a major new intellectual structure complexity cannot realistically be studied in a meaningful scientific way. But it is now just such a structure that I believe I have finally been able to set up in this book.

Relations to Other Areas

■ **Page 7 · Mathematics.** I discuss the implications of this book for the foundations of mathematics mainly on pages 772–821 and in the rather extensive corresponding notes. With a sufficiently general definition of mathematics, however, the whole core of the book can in fact be viewed as a work of experimental mathematics. And even with a more traditional definition, this is at least true of much of my discussion of systems based on numbers in Chapter 4. The notes to almost all chapters of the book contain a great many new mathematical results, mostly emerging from my analysis of some of the simpler behavior considered in the book. Pages 606–620 and 737–750 discuss in general the capabilities of mathematical analysis, while pages 588–597 address the foundations of statistics. Note that some ideas and results highly relevant to current frontiers in mathematics appear in some rather unexpected places in the book. Specific examples include the parameter space sets that I discuss in connection with shapes of plant leaves on page 407, and the minimal axioms for logic that I discuss on page 810. A more general example is the issue of smooth objects arising from combinatorial data that I discuss in Chapter 9 in connection with the nature of space in fundamental physics.

■ **Page 8 · Physics.** I discuss general mechanisms and models relevant for physical systems in Chapter 7, specific types of everyday physical systems in Chapter 8, and applications to basic foundational problems in physics in Chapter 9. I mention some further fundamental issues in physics around page 730 and in chemistry on page 1193.

■ **Page 8 · Biology.** The main place I discuss applications to biology is on pages 383–429 of Chapter 8, where I consider first general questions about biology and evolution, and then more specific issues about growth and pattern in biological organisms. I consider visual and auditory perception on pages 577–588, and the operation of brains on pages 620–631. I also discuss the definition of life on pages 823 and 1178, as well as mentioning protein folding and structure on pages 1003 and 1184.

■ **Page 9 · Social and related sciences.** I discuss the particular example of financial systems on pages 429–432, and make some general comments on page 1014. The end of Chapter 10, as well as some parts of Chapter 12, also discuss various issues that can be viewed as foundational questions.

■ **Page 10 · Computer science.** Chapter 11 as well as parts of Chapter 12 (especially pages 753–771) address foundational issues in computer science. Chapter 3 uses standard computer science models such as Turing machines and register machines as examples of simple programs. In many places in the book—especially these notes—I discuss all sorts of specific problems and issues of direct relevance to current computer science. Examples include cryptography (pages 598–606), Boolean functions (pages 616–619 and 806–814), user interfaces (page 1102) and quantum computing (page 1147).

■ **Page 10 · Philosophy.** Chapter 12 is the main place I address traditional philosophical issues. On pages 363–369 of Chapter 8, however, I discuss some general issues of modelling, and in Chapter 10 I consider at length not only practical but also foundational questions about perception and to some extent general thinking and consciousness. (See page 1196.)

■ **Page 11 · Technology.** The notes to this book mention many specific technological connections, and I expect that many of the models and methods of analysis that I use in the book can be applied quite directly for technological purposes. I discuss foundational questions about technology mainly on pages 840–843.

■ **Scope of existing sciences.** One might imagine that physics would for example concern itself with all aspects of physical systems, biology with all aspects of biological systems, and so on. But in fact as they are actually practiced most of the traditional sciences are much narrower in scope. Historically what has typically happened is that in each science a certain way of thinking has emerged as the most successful. And then over the course of time, the scope of the science itself has come to be defined to encompass just those issues that this way of thinking is able to address. So when a new phenomenon is observed, a particular science will typically tend to focus on just those aspects of the phenomenon that can be studied by whatever way of thinking has been adopted in that science. And when the phenomenon involves substantial complexity, what has in the past usually happened is that simpler and simpler aspects are investigated until one is found that is simple enough to analyze using the chosen way of thinking.

The Personal Story of the Science in This Book

▪ **Page 17 · Statistical physics cover.** The pictures show disks representing idealized molecules bouncing around in a box, and the book claims that as time goes on there is almost inevitably increasing randomization. The pictures were made in about 1964 by Berni Alder and Frederick Reif from oscilloscope output from the LARC computer at what was then Lawrence Radiation Laboratory. A total of 40 disks were started with positions and velocities determined by a middle-square random number generator (see page 975), and their motion was followed for about 10 collision times—after which roundoff errors in the 64-bit numbers used had grown too big. From the point of view of this book the randomization seen in these pictures is in large part just a reflection of the fact that a random sequence of digits were used in the initial conditions. But what the discoveries in this book show is that such randomness can also be generated without any such random input—finally clarifying some very basic issues in statistical physics. (See page 441.)

▪ **Page 17 · My 1973 computer experiments.** I used a British Elliott 903 computer with 8 kilowords of 18-bit ferrite core memory. The assembly language program that I wrote filled up a fair fraction of the memory. The system that I looked at was a 2D cellular automaton with discrete particles colliding on a square grid. Had I not been concerned with physics-like conservation laws, or had I used something other than a square grid, the teleprinter output that I generated would have shown randomization. (See page 999.)

▪ **Page 19 · Computer printouts.** The printouts show a series of elementary cellular automata started from random initial conditions (see page 232). I generated them in 1981 using a C program running on a VAX 11/780 computer with an early version of the Unix operating system. (See also page 880.)

▪ **Timeline.** Major periods in my work have been:

- 1974–1980: particle physics and cosmology
- 1979–1981: developing SMP computer algebra system
- 1981–1986: cellular automata etc.
- 1986–1991: intensive *Mathematica* development
- 1991–2001: writing this book

(Wolfram Research, Inc. was founded in 1987; *Mathematica* 1.0 was released June 23, 1988; the company and successive versions of *Mathematica* continue to be major parts of my life.)

▪ **Detailed history.** See pages 880–882.