A Reclusive Kind of Science

By David H. Bailey

A New Kind of Science by Stephen Wolfram,
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IF SOMEONE HAD ANNOUNCED THAT A BRILLIANT, INDEPENDENT-MINDED SCIENTIST HAD SEQUESTERED HIMSELF FOR MORE THAN A DECADE, SELDOM VISITING WITH COLLEAGUES OR ATTENDING PROFESSIONAL MEETINGS, ALL THE TIME WORKING alone on a big project, most of us could sketch out the probable outcome. Indeed, Stephen Wolfram's long-awaited tome on cellular automata exhibits characteristics you might expect: encyclopedic in detail and exhaustive in treatment of its topic, but uneven in writing style, and a bit brash in tone and sketchy when acknowledging others' work.

Before anyone dismisses my review as another instance of Wolfram-bashing, let me first say that I am awed by Wolfram's book. For those who have read various titles in the quasi-popular scientific book genre, desperately trying to gain substantive insight into recent developments outside our own specialty, Wolfram's book is a refreshing break from tradition. Just a brief scan through this 1,200-plus-page book, which contains 973 beautiful high-resolution graphics, makes it clear that this is not a typical scientific book. It is sobering to realize that Wolfram generated many, if not most, of these graphics with his own Mathematica programs.

Wolfram bows to nonexpert readers by keeping the body of the text mostly free from potentially intimidating material, but he does include substantial technical detail in the endnotes. Indeed, readers can think of *A New Kind of Science* as two books bound together as one. The first book, which is approximately 850 pages, attempts to convey the field's outline and flavor to nonscientists (although you could argue that toward the end of this section, such an audience cannot read the book). The second book, which is approximately 350 pages, fills in many of the first section's gaps, including historical background, Mathematica scripts, mathematical arguments, and other, more advanced technical material.

A new kind of science

Wolfram discusses cellular automata—namely, simple discrete processes defined on finite-state systems. He focuses on a class of system that begins with a linear configuration of black-and-white squares. The system then sequentially evolves by changing the color of each existing square based on its previous color and that of its immediate neighbors. There are 256 systems of this particular class. Many of these examples quickly converge to constant or repetitive patterns; others generate interesting but largely repetitive nested structures. However, a few examples (Wolfram's favorites are Rules 30 and 110) generate endlessly novel patterns.

Wolfram describes other classes of cellular automata (including those with more than two colors) and 2D or 3D examples, but he concludes that none exhibits behavior significantly more complicated than the simplest 1D class exhibits. He concludes that once we pass beyond a certain class of automata that generate repetitive and nested patterns, all such automata are equivalent. In later chapters, he extends this notion to the Principle of Computational Equivalence. He demonstrates to the reader via graphics arguments that his Rule 110 is, in the computational sense, universal, like systems such as Turing machines. Wolfram's presentation does not constitute a rigorous proof that Rule 110 is universal, but in the endnotes he mentions that his associate, Matthew Cook, has achieved a rigorous proof of this fact.

Wolfram spends considerable time discussing connections to numerous scientific fields, including artificial intelligence, biology, chaos theory, computer science, consciousness, economics, extraterrestrial intelligence, fluid dynamics, logic, mathematics, and, of course, physics. The common theme of these excursions is that simple underlying mechanisms can explain complicated phenomena. He shows, for example, that simple models based on 2D cellular automata yield computer-generated "snowflakes" that are remarkably similar to natural snowflakes. In another example, he argues that many crustacean shell types are merely simple variations of a single unsophisticated "program." A third speculation of this sort is that the universe might be a

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cellular automaton (which Edward Fredkin originally suggested in the 1980s).

Writing style

Wolfram's high opinion of himself and his work is immediately evident. What can I say about an author who declares that his discovery regarding simple mechanisms for complicated phenomena ranks among "the more important single discoveries in the whole history of theoretical science"? In the endnotes, Wolfram defends his brashness, explaining that he personally dislikes the self-imposed modesty of most scientific writing. He finds such writing confusing because determining what the author really wants to say is often difficult. I respectfully disagree—a certain degree of modesty is essential to maintain an atmosphere of objectivity in scientific communication.

There are also some style problems. The book's main text has choppy one- and two-sentence paragraphs, with an annoyingly large fraction of these sentences starting with "And," "So," or "But." As before, Wolfram defends this style in the endnotes, explaining that this style is essential for splitting otherwise lengthy sentences and paragraphs. Additionally, readers will likely find this section to be repetitive. For example, Wolfram repeats dozens of times the mantra that simple systems can generate complicated phenomena.

Curiously, the endnotes are much better written than the main text. Paragraphs are coherent, and the exposition is significantly tighter than in the first part. It is as if a different author wrote it.

Attribution

Wolfram does not include references or a bibliography in the book. This is already a topic of controversy in the scientific community, as some scientists feel that Wolfram has appropriated other scientists' work. Other scientists have complained that he has not fully acknowledged their contributions, or that he mentions only one researcher involved in a discovery when several should be mentioned. In Wolfram's defense, it seems that many of his critics have not read the endnotes, where he mentions many key contributors. However, the lack of a bibliography remains a significant and puzzling deficiency.

Wolfram's central point

Wolfram's defenders have responded to these criticisms by arguing that we should focus on Wolfram's scientific contributions in the book, not on relatively superficial issues. Accordingly, I will now turn to such matters.

Wolfram's central point is that complex behavior does not require complex systems; instead, simple systems can generate substantial complexity. He states this point repeatedly, as if attempting to convince recalcitrant scientists who otherwise would never accept such a controversial proposition. Who are these scientists? I certainly do not find Wolfram's point controversial. I can cite numerous examples of this principle, some of which have been known for many years.

An example is the notion of normal numbers in mathematics (which Wolfram briefly mentions in the endnotes). A real number is said to be normal base b if its base-b digit expansion has the property that every m-long string of digits appears with precisely the limiting frequency, namely b^{-m}, which we would expect of a "random" expansion. It is a well-known, if counterintuitive, fact of measure theory, first proven in the early 1900s, that almost all real numbers are normal base b for all bases b. Furthermore, it is widely believed, although not proven, that all the classical constants of mathematics, including π, e, $\sqrt{2}$, and log2, are in this category. We can even conjecture that every irrational algebraic number is normal, because there are no known or suspected counterexamples. In short, from measure theory and the theory of normal numbers, random-looking, complex behavior clearly is the norm—orderly behavior is exceptional.

As another example, consider the recently discovered iterations that generate digits of certain mathematical constants.[1] For example, the simple iteration $x_0 = 0$, and

$$x_n = \{2x_{n-1} + 1/n\},$$

(where {} denotes fractional part) generates the binary digits of log2 with progressively greater fidelity, by noting which subinterval, [0, 1/2) or [1/2, 1), each successive iterate lies in. A similar sequence that generates hexadecimal digits of π is given by $x_0 = 0$ and

$$x_n = \left\{16x_{n-1} + \frac{120n^2 - 89n + 16}{512n^4 - 1024n^3 + 712n^2 - 206n + 21}\right\}$$

In both instances the starting point is 0, so the iterates' random behavior is certainly not rooted in the initial conditions; instead, it is inherent in the iteration definition.

A third example is the well-known logistic map of chaos theory—namely, the simple iteration

$$x_n = rx_n(1 - x_n)\ .$$

(He briefly mentions this in the endnotes). For values of r in the range $1 < r < 3$, iterates converge to a single limit point. For $3 < r < 3.44$, iterates alternately visit the neighborhood of two distinct limit points. For slightly larger r, there are four limit points, then eight, and so on. Finally, for r in the range $3.57 < r < 4$, all periodicity disappears—the iteration is completely chaotic. Furthermore, this chaotic nature is a fundamental characteristic of the iteration itself, not of initial conditions. As before, this is not a new observation—scientists have known this for 40 years (as Wolfram acknowledges)—but it is a good example of his principle.

Wolfram might counter that these examples are based on the mathematics of the continuous real line, and that in a computer implementation, finite-precision arithmetic results in distinct behavior. However, we can make this same comment about cellular automata. They also require finite registers for real-world implementation (as he acknowledges), and these finite-register automata are distinct from the idealized examples. In this sense, we can regard Wolfram's chaotic cellular automata, as implemented on finite-register computers, as merely a new class of pseudo-random number generators. One of these schemes is in fact used as a pseudo-random number generator in Mathematica.

Not rigorous enough

A second fundamental criticism of this book is that all too frequently readers find phrases such as, "It seems clear to me," and "I suspect that…" instead of rigorous scientific arguments. Many of Wolfram's arguments in the natural sciences are not impressive.

For starters, I am disappointed in his Principle of Computational Equivalence. After several chapters of relentless buildup, readers expect at least a rigorous statement of this principle, yet he declines to provide such a statement, preferring instead to rely on intuition.

In the Evolutionary Biology section, Wolfram asserts that simple programs underlie at least part of the complexity that we see in the biological world. He gives several examples, such as seashells that appear to be minor variations on a simple program, but biologists have already presented similar examples, for example, in Richard Dawkins' books.[2,3]

Wolfram mentions applying cellular automata to fluid dynamics, giving some graphical examples that he generated, but this too is not new. I saw Wolfram give such a demonstration on an early Connection Machine in 1986. He claims that these cellular automata-based simulations are now gaining greater acceptance, but I am not aware of any fluid dynamicists who concur. To the contrary, Navier-Stokes-based schemes are the methods of choice in almost all practical fluid dynamics simulations today.

As I mentioned earlier, Wolfram argues that the universe might be a cellular automaton at some level, a notion first suggested in the 1980s. After all this time, you might be inclined to think that Wolfram surely has some significant, new results. He does mention some interesting ideas, but he does not provide a clearly falsifiable test—much less any proof—of this hypothesis.

I began my review by stating that *A New Kind of Science* exhibits several weaknesses that you might expect from a reclusive, single-person effort. Perhaps there is a lesson here. The traditional collaborative method of doing science, which Wolfram evidently dismisses, has distinct advantages.

The publishing system results in peer-reviewed journals and conference proceedings—it has the important advantage of establishing a clear "paper trail" of idea ownership. Because of this system, squabbles over priority are almost always nipped in the bud, and cooperation, collaboration, and goodwill between scientists are greatly facilitated. Additionally, it significantly improves the quality of published work.

The established system of scientific conferences and journals also helps to separate research that is worth doing from research that is not. If a paper is not accepted at a conference, even if it is technically sound, maybe that should be a cue to researchers that the ideas (or even the overall topic) they presented are not very interesting to the community, so that perhaps they should redirect research efforts to other questions.

With regards to *A New Kind of Science*, I would argue that to the extent that cellular automata in general, and Wolfram's study of it in particular, constitute fruitful, promising avenues of research, they deserve to be studied by more than a solitary scientist, however great his talents and resources might be. A larger community in the field could bring to bear many different types of expertise that Wolfram lacks, especially in allied fields such as biology and human consciousness.

Why hold your research so tightly to the chest? If you have great ideas, let others share in the joy of discovery and the triumph of success.

References

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A NEW KIND OF TELEOLOGY

By George Cybenko

ACCORDING TO THE BOOK JACKET, *A NEW KIND OF SCIENCE*'S "UNEXPECTED RESULTS FORCE A WHOLE NEW WAY OF LOOKING AT THE OPERATION OF OUR UNIVERSE."

To be sure, Stephen Wolfram has written a valuable digest of several areas in computational science and presented it in a masterfully produced package. These aspects alone make *A New Kind of Science* worth reading. However, the synthesis he presents, when taken in the informed context of modern science, computing, and mathematics, falls short of launching the kind of scientific revolution that he so often leads the reader to expect.

Wolfram's conclusions essentially recapitulate what many scientists have been implementing in their work for some time. This opinion is not meant to denigrate the work but to put it into a larger perspective. By weaving together several threads of ongoing scientific investigation, Wolfram articulates a context for modern scientific computing understandable to a limited extent by a general reader. As a result, we can view it as a manifesto for moving computational science ahead in the 21st century.

A New Kind of Science has four dimensions to it: the aesthetic presentation, which sets a new standard for the quality of technical publications; the 350 pages of endnotes surveying modern science, computing, and mathematics; the first 11 chapters, which summarize Wolfram's experiments with and observations about simple programs (which, in this book, are synonymous with cellular automata); and Chapter 12, in which Wolfram synthesizes the Principle of Computational Equivalence from the results of his experiments.

Presentation

The physical presentation is nothing short of spectacular and worthy of its price as a work of art. Additionally, Wolfram has developed a novel typesetting process that has essentially eliminated word hyphenation.

Endnotes

The endnotes section is essentially a replacement for citations to a traditional bibliography. Wolfram chose to write the main text in a conversational voice, delegating the context and historical development of ideas to the endnotes. There is, in fact, no bibliography, which will no doubt irritate many readers. Because Wolfram has clearly devoted much time, thought, and effort to his work, his choice to leave it out seems questionable and unnecessarily detracts from his overall effort. Nonetheless, readers can peruse the endnotes separately from the main text; they offer a comprehensive, but, in places, highly idiosyncratic and personalized perspective on many scientific developments.

Wolfram's 11

The first 11 chapters are a sort of scientific *roman a clef*. They recount Wolfram's extensive experimentation with several classes of cellular automata and present those results in the guise of startling new general scientific discoveries.

Although some of the discoveries might be new for the special classes of cellular automata considered, and are certainly worthy of publication as such, the individual implications Wolfram extracts from those exercises are hardly novel. This will surely frustrate some readers.

Wolfram's observations about the behaviors of simple programs in the first 11 chapters are manifold. They include

- Simple programs can exhibit a wide variety of output patterns, such as trivial degenerate patterns; regular, highly complex, nonperiodic patterns; complex, random-like patterns; and hybrid combinations of the preceding three pattern types.
- Simple programs can produce patterns reminiscent of structures occurring in nature and human-engineered systems.
- Simple programs can be universal—they can simulate more sophisticated programs if the right encoding is found.
- Simple questions about simple programs can be undecidable. (An example is the "Halting Problem," which asks whether a given program will halt after executing a finite number of steps on some input data or run forever.)

Using examples, Wolfram argues that the threshold for universality is low and that such universality is the norm rather than the exception. This is an interesting claim, but he offers no framework for making this conjecture concrete, let alone settling it in any rigorous manner.

The breadth of Wolfram's discussion in the first part of the book, which moves from snowflakes to networks to human intelligence, is impressive and thought provoking. However, many of the observations appear to restate previously known results, even in the context of cellular automata. For example, observing complex behavior in simple programs operating on simple initial conditions has been recognized and widely used for some time in the context of linear feedback shift registers, which are arguably simpler than the 1D cellular automata considered in *A*

New Kind of Science. Scientists familiar with such devices would not be surprised by cellular automata's complex behavior. The same has been widely known for 2D cellular automata in the specific form of John Conway's Game of Life and its variants.

Similar observations could be made about much of the first 11 chapters, but such deconstruction would miss the point; the goal is to lay the groundwork for Chapter 12. Without doubt, Wolfram could have shortened much of the first 11 chapters by summarizing the research of others more aggressively rather than developing ab initio arguments on the basis of his own experiments with cellular automata.

Several of Wolfram's positions strike me as curious. For example, he often points to the complex, random-like patterns that cellular automata can produce but acknowledges that these patterns are not truly complex (according to Gregory Chaitin's and Andrey Kolmogorov's highly developed and rigorous theories). He essentially dismisses this inconsistency, stating that those theories do not apply to the present case, offering no further discussion.

Additionally, Wolfram occasionally bemoans traditional mathematics and theoretical science as being incapable of producing formulae that can describe the overall behavior of many common, simple systems, in particular the 1D cellular automata he frequently uses in the book. However, this is not a failing of mathematics or science, but rather of the process by which the languages of mathematics and science evolve—the languages being largely human-crafted formulae that allow parsimonious representation and discussion. (I look forward to Alan Sokal's review in this respect.) The key is not the vocabulary and consequent formulae but the manner in which the formulae are evaluated—specifically, the computational complexity of evaluating those formulae.

Principle of Computational Equivalence

This takes us to Chapter 12 and the heart of Wolfram's Principle of Computational Equivalence. The principle, the work's intellectual capstone, states, "Almost all processes that are not obviously simple can be viewed as computations of equivalent sophistication." Wolfram states that our universe is one such process because it computes its future state according to the laws of nature that govern it. This restatement of the Newtonian view of nature seems oddly retro in the context of this work because it is entirely consistent with how scientists have interpreted natural and engineered systems for several centuries.

Wolfram believes that accepting our universe as a large computer of equivalent theoretical power to our general-purpose computers and to our own human intellect leads to a scientific standoff of sorts. Owing to the inherent undecidability of many possible questions relating to such processes and the impossibility of any such process to leapfrog any other in terms of raw computing power and expressiveness, the best we can do is hope to simulate one kind of process by using another.

What Wolfram has tried to achieve with the Principle of Computational Equivalence is nothing more than a justification for the growing trend within science and engineering to rely more on computer simulation and less on the "shortcuts" that traditional mathematics and theoretical science sometimes offer. This trend is emerging because the latter are fundamentally constrained, such as in their ability to deal with issues arising from undecidability. This line of thinking has been in development for over 300 years—starting with Newton's laws and the concepts of "state" within physical systems, through the development of universal computing devices such as Turing machines, to results on the incompleteness and undecidability of simple but general arithmetic-logic systems.

If every nontrivial device implemented in our universe has equivalent computational power, then the expressive powers of the virtual machines with which we work and the efficiency of implementing them on an underlying physical computing device become the main differentiators. That takes us to software, an area to which Wolfram has made outstanding contributions over the past 14 years in the form of his Mathematica system.

Unconvincing as new science

In the end, perhaps we can read the 1,280 pages of *A New Kind of Science* as justification for how Wolfram has spent much of his time and intellectual capital recently. In that light, we now have two things for which we can thank him—an outstanding software system and an attempt to develop an intellectual context in which to understand such software systems as we move into the 21st century.

As to this work's self-professed revolutionary impact, I remain unconvinced. One yardstick for revolutionary impact would certainly have to be empowerment—namely, does this knowledge let readers do something they couldn't do before? Even with respect to complex systems, an area in which the author promises us new insights, the bottom line is disappointing because we are told to keep doing what we have already been doing for the past few decades—namely, model and simulate.

Perhaps it would have been more accurate to title the book *A New Kind of Teleology.* The scientific heavy lifting underlying this book's conclusions has been in development for many years by many scientists, while the conclusions are popular restatements of beliefs about natural design that many practicing scientists already hold. Wolfram offers the reader a specific worldview and justification in which to practice existing science, not new science, as claimed.