luck? It is a strange coincidence in science that luck always favours the prepared mind. The process of scientific discovery produces many answers but raises even more questions. Albert Einstein once noted that the larger a circle of light, the greater is the perimeter of darkness around it.

The account of Blumberg's life, his professional career and the story of HBV should also be read by policy makers and administrators who are responsible for formulating and implementing national education and research policies. Blumberg's early education was at a New York high school that has the distinction of producing three Nobel Laureates, including the legendary Richard Feynman. This was not a coincidence, but the result of a very high intellectual atmosphere at school. It is often asked why India, with a vast army of trained scientists and engineers, has not been able to produce a Nobel Laureate in its 50-plus years of independence? Do we inculcate scientific temper in our schools or simply carry out rote learning of science? Do we provide an intellectual atmosphere to our young people? Some of these questions do come to mind on reading about Blumberg's life.

On page 207, one paragraph is devoted to the period in 1986 Blumberg spent at the Indian Institute of Science as a Raman Professor. He offered sane advice to Prime Minister Rajiv Gandhi about hepatitis B vaccination, improvement of water supply and sanitation, and the manufacture of disposable needles and syringes. Our scorecard after 15 years is a mixed one. We seem to have finally found the resources and the political will to begin the process of including hepatitis B vaccination in the national programme of universal immunization. The country has, to a large extent, shifted to using disposable needles and syringes but their safe disposal and alleged recycling remain difficult issues. We have failed to improve sanitation and our drinking water supplies have deteriorated further in the face of greater urbanization and increasing population. On the whole, there seems to be little improvement on the public health front.

One glaring omission from Blumberg's interaction with India is his research on the plant *Phyllanthus amarus* and its use as a therapy for hepatitis B. Between 1988 and 1990, Blumberg published three research papers on this subject with colleagues from Chennai. Since Blumberg has a patent on this application (US Patent 4,673,575) and considering *Phyllanthus* extracts have been used in traditional Indian medicine for ages and continue to be used today, it is surprising not to find any mention of it in this autobiography.

Blumberg's research started out as an esoteric investigation of human diversity and resulted in a discovery whose applications contributed directly to preserving lives, preventing illness and saving millions on health-care costs. This is therefore a good example why basic research should be supported. Good basic research with an open (and prepared) mind is the way to good applications.

The book makes interesting reading and would be of interest to scientists of all hues, students aspiring to learn about the process of scientific investigation and anyone who is interested in the mysteries of viruses. It should be made compulsory reading for the many scientist-bureaucrats at granting agencies who often question the value of basic research and its relevance to the country's problems.

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A New Kind of Science. Stephen Wolfram. Wolfram Media, Champaign, IL 61820, USA. 1200 pp. 2002. Price: US$ 44.95.

Whatever else this book may or may not be, it represents a most unusual effort. Its British-born author has always been known to be brilliant, indeed a prodigy. He got his Ph D from Caltech when he was 20, was a MacArthur Fellow at 22, became well known for his early research, and went on to set up a company of his own to produce and market the very innovative and powerful software known as *Mathematica*. He has now written a book which, with 'the science it describes[,] has been a vast personal undertaking, spanning the better part of half my life so far'. He would never have been able to do it, he says, if he had remained part of the traditional academic community as he was at one time, 'even in the highly favourable academic circumstances in which I found myself'. In the event we have a work here of which Wolfram is author, publisher, patron, client and perhaps many other things as well, such as reviewer and editor – one man who has concentrated within and into himself all the functions that the vast scientific enterprise of the world has found necessary to distribute to different people and agencies. Wolfram can clearly afford to do this, and we are seeing here a new breed of professional that may be called the entrepreneur-scientist.

The book is 1200 pages long, not including a preface and an extensive index; of this length almost 30% is Notes at the end of the book. Although some of these end-notes are historical, the book in fact contains no specific references at all to any earlier work (you can anyway find them on the internet, says the author). And there are no equations either in the main text (a few appear in the end notes). (This absence of equations is, by the way ironic, for what the author's own *Mathematica* did was to provide a powerful tool for manipulating symbols, algebra and equations.) The book has in fact been written to communicate directly with the non-scientist reader as well as the scientist. It does this effectively because of the graphics, which (as might be expected from a software maestro) are both professional and beautiful, even without the aid of colour: the work is in many ways a computer-age book. It has already created big waves in the lay press (*New York Times*, *Time*, *Newsweek*, etc.).

The author's scientific ambition, and his self-confidence, are almost boundless; and the language he uses to announce his thoughts is appropriately decisive and sweeping. Thus, he has found 'a crack in the very foundations of existing science'; his work 'touches almost every existing area of science, and quite a bit besides' (this includes the social sciences, the question of free will, etc.), and uses 'new ideas and new methods that ultimately depend very little on what has gone before'; and he provides an opportunity for the reader to 'glimpse for the first time some new and basic truth'. The book starts with the statement, 'Three centuries ago science was transformed by the dramatic new idea that rules based on equations could be used to describe the natural world. My purpose in this book is to initiate another such transformation, and to introduce a new kind of science that is based on the much more general types of rules that can be embodied in simple computer programs'. The author clearly ranges himself alongside such figures as Newton and Darwin.

So what is the main thesis of the book? It starts with 'cellular automata', a concept introduced by von Neumann and Ulam in the 1950s (and independently discovered by himself, according to the author). Here both time and space are discrete, and an 'automaton' in each spatial cell updates itself from its state at any given time to another state after a specified time step, usually following very simple rules. (For an accessible introduction see Raj Lakshmi, M., *Sâdhanâ*, 1989, **14**, 133–172.) For example, each cell may be in just one of two states – say white or black (or, if you want to be digital, 0 or 1). The state-changing (or transition) rule specifies what the colour of the cell is going to be at the next instant; for example, the rule may depend only on the colour of the nearest neighbours of the cell – one on each side in the simple one-dimensional case. The author goes through all possible types of such rules, and picks some for extensive study. The algorithms for running such cellular automata are so absurdly simple that, with the power of present-day machines, taking millions or billions of time steps is no bother. The evolution of the system can be plotted in a diagram, with cell location along one axis and time on the other. Let me call such a diagram a 'quilt'.

The fascinating thing is how complex the resulting patterns of these quilts can be. Wolfram's Rule 250, for example, gives a checkerboard quilt ('crystal'?); Rule 90 gives an intricate nested pattern; Rule 30 (which is asymmetric between left and right) produces order at the left edge, irregular structures towards the right edge (reminding one of transition from a laminar to a turbulent state as a fluid flows from left to right); and Rule 110, perhaps the most interesting of all, produces another strange mixture of order and apparent randomness. The wealth and diversity of complex behaviour that results from these simple algorithms is breathtaking; and 'quilts' from these and from two-dimensional cellular automata can be sometimes ornamental, intriguing at others.

Two sections of the book particularly attracted my attention. One is on growth in plants and animals, which the author seeks to reproduce, once again with very simple algorithms that are basically neighbour-independent substitution systems, i.e. simple branching rules repeated very many times. These and other similar algorithms mimic the kind of geometry we associate with trees and flowers and artichokes and strawberries and bullhorns and zebra stripes – and much else besides: certainly very different from Euclid's geometry of triangles and parallelograms.

The other is fluid flow (of personal interest to me). It has been known for some time that cellular automata with simple, artificial collision rules among molecules having a very small number of possible velocities (e.g. fixed magnitude and only one of six possible directions), can mimic rather complex fluid flows, such as, for example, the well-known Karman vortex street in the wake of a circular cylinder.

There is no question that these and many other examples that the book is filled with are both impressive and beautiful. The point that complex behaviour can result from some very simple algorithms is effectively made (although no formal definition of complexity is offered beyond the certificate of visual perception). But does it follow that all complexity in nature necessarily follows from simple algorithms? Are we being urged to abandon Newton, Einstein, Schrödinger and Navier–Stokes? Or is the universe just a simple program (as the author claims), from which all the known laws will somehow *emerge*?

The author stakes his claims on the basis of what he calls the Principle of Computational Equivalence: 'All processes, whether they are produced by human effort or occur spontaneously in nature, can be viewed as computations'. Plausible, certainly to Indians (I will come back to this point), but then look at this additional statement: 'almost all processes that are not obviously simple can be viewed as computations of equivalent sophistication'. Is that obvious? What the author implies is that 'there is essentially just one highest level of sophistication, and this is achieved by almost all processes that do not seem obviously simple'. In other words, 'all is computation'; but what is more, the world is not a jungle of computations of a very wide variety of degrees of sophistication; there are only the simple and the (highly) sophisticated. Furthermore, there is great universality in the overall *types* of behaviour among cellular automata.

So the book may be said to advocate what I like to call 'computational positivism' (loosely speaking, the view that computation and empirical experience are the two most important sources of knowledge). Classical Indian mathematical astronomy also did something similar (as I have argued elsewhere), but with one difference: it prized *drg -ganit ′-aikya*, literally *identity* of the seen and the calculated. Wolfram, on the other hand, makes no *quantitative* comparison between his results and any experimental or observational data whatever – in any field. So the thought arises in one's mind: certainly those simple algorithms are wonderful mimics; but are they more – can they give us hard numbers that are of real use to the more worldly among us?

If Wolfram's 'bold hypothesis', namely that all the known laws emerge from the simple program that the universe is, is true, we can only say that that simple program has not yet been found. It is indeed difficult to imagine that all the predictive power that is compressed into the scientific web of the well-known laws of mechanics (classical and quantum), thermodynamics, relativity, etc. will emerge from some simple, single program: there is an existential question here. *If* they do, one would have to set up a system by which a new simple algorithm would have to be found whenever the next experimental surprise turns up, e.g. that the speed of light has varied in time. As the author himself admits, there may be no incremental procedure in going from one algorithm to the next; it is also not clear if there will be reducibility in limiting cases, as there has often been when new theories are made. The system that the author suggests for finding such algorithms appears to be some kind of an automated computer search among algorithms of different classes. But the search-space may be huge: if one uses three colours in the simple linear cellular automaton mentioned above, the number of possible rules is close to 8×10^{12}! His present approach is that of a naturalist, for there is no 'theory' – at least, not yet – for selecting potential candidates for consideration.

The basic question is whether 'useful lies', in an artificial Boolean micro-world, can lead to useful truths in the real, physical macroworld. There can be no valid philosophical objection to this possibility, but there may be serious technical problems. Let us briefly examine the specific experience we have in the small but interesting world of fluid dynamics. The really simple cellular automata that were initially devised for solving fluid-flow problems – by Wolfram himself, and by Frisch and his colleagues [*Phys. Rev. Lett.*, 1986, **56**, 1505], going back to even earlier work in the 70s – were quickly found to have numerous difficulties [e.g. Orszag and Yakhot, *Phys. Rev. Lett.*, 1986, **56**, 1691]. These included lack of Galilean invariance, viscosities that were both vague and high, noisy solutions, velocity-dependent equations of state, unphysical and false conservation laws, etc. In the years following, several ingenious fixes to some of these problems were in fact devised, but in the end these proved inadequate. One may mimic the Karman vortex street, but can one get the right frequencies, not to mention the correct value of cylinder drag? The all-important high Reynolds-number regime proved immune to any such fix. But more importantly, I think, these attempts revealed a fundamental problem with the new approach: as Frisch (in *Whither Turbulence*? Springer, 1990) pointed out, if one wanted to go beyond simple incompressible flows, each additional effect (gravity, rotation, surface tension, chemical reaction, shock waves, magnetic fields, etc.) required another ingenious and imaginative leap in devising the appropriate automaton. I believe the approach therefore lost in the competition for intellectual minimalism or parsimony if one wanted a capability, or framework, to handle fluid flows in general – a framework that current theory does in fact provide. In the 1990s, even as computers kept tracking Moore's Law and became ever more powerful, interest in the use of cellular automata for solving fluid-dynamical problems declined. The first announcements of such cellular automata had been accompanied by much hype: better aircraft and automobiles were promised. But in the big design offices as in the research laboratories of the world, Messrs Navier and Stokes quickly recovered to continue their reign, as they proved in all ways to be more flexible, reliable, accurate and cost-effective, and in a fundamental philosophical sense, intellectually more economical as well.

Is there any evidence in the book that, in fluid flows or any other area of physics, its methods are going to abolish Newton *et al.*? Unfortunately, I can see none. The Simple Rule has certainly not been able to overtake the Simple Equation – at least not yet.

But the author is unworried. In one extraordinary section titled 'Ultimate Models for the Universe' he asks, 'Could it be that underneath all the complex phenomena we see in physics there lies some simple program which, if run for long enough, would reproduce our universe in every detail?' *In every detail*? And, in answer he presents a long chain of speculation, full of phrases like 'I expect', 'I strongly believe', 'I strongly suspect...', '... my guess is ...', etc. The reader would have been happier if he had encountered a *simple* program that (whether it was beautiful or not) would satisfactorily reproduce *in detail* some small part of physics – planetary orbits, fluid flows, the hydrogen atom or whatever; he would then have been readier to indulge the author in the extensive speculation that punctuates his book. Mimicry is wonderful, and indeed can be very insightful; but how far can cellular automata take us beyond mimicry and impressionism into reality?

The answer appears to be that there may be areas where they can provide special numbers, especially when it can be shown that parameters in the real solution do not depend on the detailed microstructure of the system. Critical exponents in certain kinds of phase transition (as N. Kumar pointed out to me) are one example.

Furthermore, there are many lines of work that may (with further development) prove very *illuminating*. For example, there is the potential power that cellular automata – but also other kinds of computer simulation – may have in bringing some order to understanding the diversity of shape and form in complex natural systems. To this non-biologist, it has always seemed attractive to examine the idea that there may be a coarser level (beyond molecules) at which it should be possible to explore why and how natural systems – trees and flowers, and even hills, clouds and so on – organize themselves the way they do. (Fractals are only part of the story.) There may be a whole field of what one may call 'algorithmic geometry' that is waiting to be opened up. This geometry is non-Euclidean, but not in the sense of Lobachewski or Riemann: it looks at the forms and shapes one sees in the natural world around us, not the simpler, artefactual world of squares and triangles that Euclid examined and proved theorems about. This objective will undoubtedly appear too modest to the author, but a book filled with more, pictures studying in greater detail what would be a minimal set of algorithms that could reproduce the bulk of the morphological diversity we observe in nature, could be very enlightening for science – old or new style. An algorithmic vocabulary would incidentally also be a very interesting complement to the work of Turing on morphogenesis.

And there are other similar programs that are suggested by the work reported in the book (e.g. evolutionary dynamics), but this review is not the place to discuss them. Furthermore, it is entirely conceivable that some scientific cultures take more easily to rules and algorithms than to axioms and proofs.

So at the present time all one can say is: Wolfram's case is not made, but do not for that reason ignore the book.

For the book carries a hidden but larger message, concerning the changing role of the computer. In the 1950s and 60s, the computer was a powerful slave: an input/output machine, and 'garbage in, garbage out', we said. In the 1970s and 80s it graduated to a powerful tool, even ally, in a whole wide variety of scientific investigations. Since 1990 it is becoming a powerful teacher: we now seek to learn from it, as Gregory Chaitin has shown how to do, even in such areas as the philosophy of mathematics. The fact is that the computer offers an instrument of unprecedented power in the hands of man, and surely represents the greatest revolution in mankind's ability to calculate since the invention of the Indian numeral system. And if history is any guide, it is bound, like the telescope and the microscope and the steam engine, to alter the way we look at the world, the way we ask questions, and the way we find answers. From this point of view, Wolfram may have only fired the first shot, even if he has missed his target.

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