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A NEW KIND OF SCIENCE

HEARING

BEFORE THE

SUBCOMMITTEE ON SCIENCE, TECHNOLOGY, AND SPACE

OF THE

COMMITTEE ON COMMERCE, SCIENCE, AND TRANSPORTATION UNITED STATES SENATE

ONE HUNDRED EIGHTH CONGRESS

FIRST SESSION

SEPTEMBER 4, 2003

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SENATE COMMITTEE ON COMMERCE, SCIENCE, AND TRANSPORTATION

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THURSDAY, SEPTEMBER 4, 2003

U.S. SENATE,
SUBCOMMITTEE ON SCIENCE, TECHNOLOGY, AND SPACE,
COMMITTEE ON COMMERCE, SCIENCE, AND TRANSPORTATION,
Washington, DC.

The Subcommittee met, pursuant to notice, at 3:36 p.m. in room SR–253, Russell Senate Office Building, Hon. Sam Brownback, Chairman of the Subcommittee, presiding.

OPENING STATEMENT OF HON. SAM BROWNBACK, U.S. SENATOR FROM KANSAS

Senator BROWNBACK. I call the hearing to order. Good afternoon to everybody.

It's my pleasure to welcome one of the world's most respected scientists to testify today, Dr. Stephen Wolfram. He's the author of the best-selling book, "A New Kind of Science."

Dr. Wolfram studied at Oxford and, at the age of 20, received his Ph.D. from Caltech. In the early 1980s, he made a series of discoveries about systems known as cellular automata, which have yielded many new insights in physics, mathematics, computer science, biology, and other fields. In 1986, he founded Wolfram Research, Incorporated, and began the creation of Mathematica, now the world's leading software system for scientific and technical computing. In addition to leading this company and creating innovative technology, Dr. Wolfram is now developing a series of research and educational initiatives in the science he has created.

Dr. Wolfram, I asked you to come before this Committee today, because I'm intrigued by the work you have done and documented in your book. The theories you propose are exciting, and I'm very interested in how your work might, if possible, be used by the National Science Foundation, the National Institute of Health, NASA, and other Federal agencies to study the universe and how it operates, or perhaps answer some of the deepest questions about nature, space, and science. I'm anxious to hear from you on these thoughts.

I had a chance to visit with Dr. Wolfram before the hearing today, and I found it very educational and interesting. I'm seeing here a basic science that has the possibility of being used in a number of places throughout the world. Since this country invests so heavily in forward-thinking science, research and development, I think Dr. Wolfram's ideas are worthy of a good hard look and we'll see if there are things that we should be doing additionally.

Dr. Wolfram, I'm delighted to have you here. Thank you for giving us your time, your talent, and your information. I look forward to your testimony and to the answers to some of the questions I may have afterwards.

Welcome.

STATEMENT OF DR. STEPHEN WOLFRAM, FOUNDER AND CEO, WOLFRAM RESEARCH, INC., AND AUTHOR OF *A NEW KIND OF SCIENCE*

Dr. WOLFRAM. Thank you. And thanks very much for inviting me here today.

Nearly four centuries ago, Galileo turned a telescope to the sky for the first time. And what he saw changed forever our view of the universe and ultimately launched much of modern science. I've had the privilege to begin to explore another new world made visible not by telescopes but by computers. And in that world, I've made some most surprising discoveries that have led me to a new kind of science.

Well, the computer revolution has been fueled by our ability to have computers run specific programs built for specific tasks. But what if we were to explore the world of all possible programs? What would we find out there? Well, here's a representation of a very simple program for coloring squares down a page. So this is what happens if we run the program. Simple program. Makes a simple pattern.

Here's a program, though, that I call "rule 30." And, again, it's a very simple program. But now look at what it does. So that little program makes all of this. It's amazing. And I think it's also profoundly important. Because I think it finally shows us the essential secret that nature uses to make so much complexity.

For about 300 years, the exact sciences have been built on mathematical equations, and they've made and continue to make great progress on many fronts. But in the face of significant complexity, they've consistently gotten stuck. And I believe the reason is a fundamental one, and that to go further one needs a new kind of science whose foundation is programs, not just mathematics.

At the core of this new kind of science is an exploration of the abstract world of simple programs. But from this, come applications, both immediate and profoundly far-reaching. If in the past we'd been faced with something like this, we would never expect to understand it. But now we've discovered that this can come from that little very simple program.

In nature we find many elaborate patterns like the one on this mollusk shell, for example, which we can now see can be explained by very simple programs. And, for example, throughout biology, complexity can come from simple programs, which then finally begins to give us the possibility of, sort of, a true theoretical biology.

Today, for example, we know the genome. But now we must work out how it operates. And I think simple programs are key. Fifty years ago, we found the basic mechanism for heredity. Perhaps now simple programs can show us the basic mechanisms for processes like aging.

Traditional mathematical science has had its greatest success in physics. But still we do not have an ultimate theory of physics.

And, indeed, our theories always just seem to get more complicated. But one of the suggestions of my work is that at the very lowest level, below even space and time, there may just be a simple program, a program which, if run for long enough, would actually reproduce in precise detail everything in our universe.

Finding that program will be a dramatic moment for science. But from the progress I've made, I'm actually hopeful that it may not be too far away.

There are also many more everyday issues to which simple programs bring new models and perspectives, not only in physics and biology, but also, for example, in earth and social sciences. Simple programs also seem uniquely suited to analyzing many critical systems that involve large numbers of interconnected parts.

Having captured phenomena scientifically, one can start to harness them for technology. So now we can begin to create technology based not on concepts like wheels or waves, but on processes like this one. And as we explore the vast world of simple programs, we can, sort of, systematically mine it for technology, finding new and unexpected programs that can be used for encryption or pattern recognition or decentralized control, perhaps even finding programs that manipulate information more like humans, and, indeed, creating a whole new generation of technology from which new industries can grow.

One of my surprising discoveries embodied in what I call the Principle of Computational Equivalence is that powerful computation is fundamentally common. It doesn't take a sophisticated CPU chip to be able to do computation. Simple programs do it, like this one or like many others in nature.

This has many deep implications for what can and cannot be done in science, but it also immediately suggests that we can use much simpler elements to make computers, which, for example, points to a new approach to nanotechnology.

Well, over the past year, my book has stimulated great activity in many scientific and technical communities, as well as, of course, as some of the turbulence one should expect in any potential paradigm shift.

In moving forward, education is key, and there's certainly no lack of enthusiastic students. Institutional structures will take time to develop, but it's been exciting to see how quickly teaching of some of the core ideas has begun, even at a high school level.

One day the study of the computational world will no doubt be an established science, like a physics or chemistry or mathematics. But today the exploration of the computational world still stands before us as a great frontier with the potential not only to unlock some of the deepest questions in science, but also to define a whole new direction for technology.

Thanks. Well, I just tried to cover 25 years of work in 5 minutes, but I'd be happy to expand on anything.

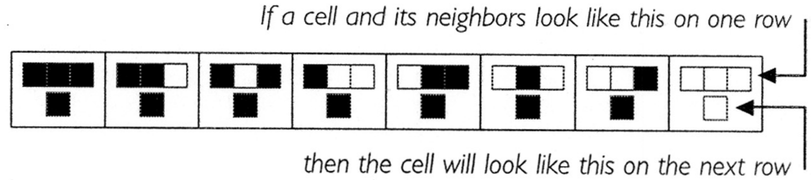
[The prepared statement of Dr. Wolfram follows:]

PREPARED STATEMENT OF DR. STEPHEN WOLFRAM, FOUNDER AND CEO, WOLFRAM RESEARCH, INC., AND AUTHOR OF *A NEW KIND OF SCIENCE*

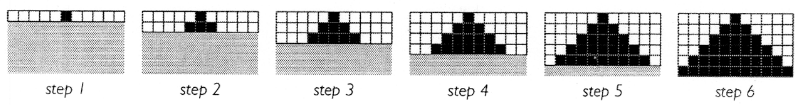
Thank you for inviting me here today.

Nearly four centuries ago, Galileo turned a telescope to the sky for the first time. What he saw changed forever our view of the universe—and ultimately launched much of modern science. I have had the privilege of beginning to explore another new world, made visible not by telescopes but by computers. And in that world I have made some most surprising discoveries—that have led me to a new kind of science.

The computer revolution has been fueled by our ability to have computers run specific programs built for particular tasks. But what if we were to explore the world of all possible programs? What would we find out there? Here is a representation of a very simple program for coloring squares down a page.

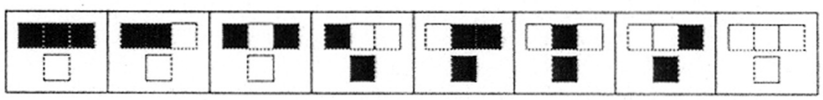


This is what happens if we run the program.

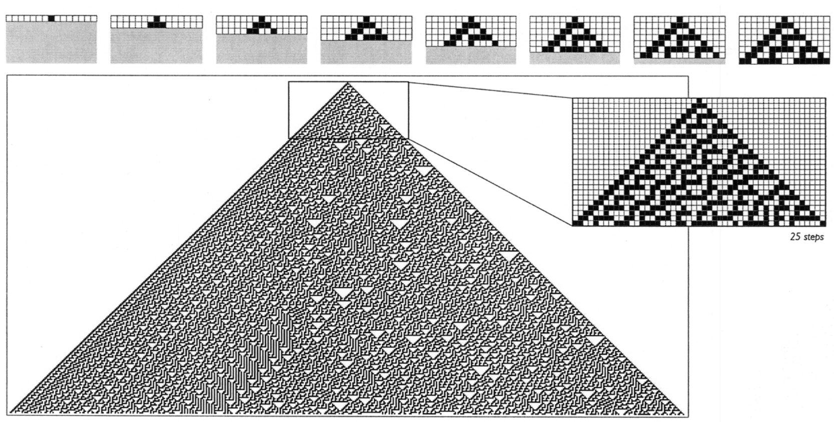


The simple program makes a simple pattern.

But here is a program I call rule 30.



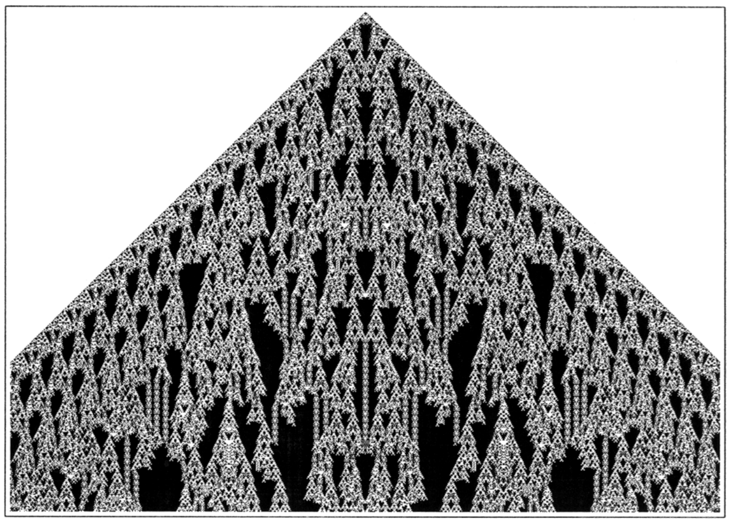
Again it's a very simple program. But now look at what it does . That little program makes all of this. It's amazing. And I think it's also profoundly important. Because I think it finally shows us the essential secret that nature uses to make so much complexity.



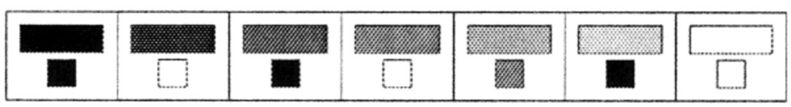
For three hundred years the exact sciences have been built on mathematical equations. And they have made—and continue to make—great progress on many fronts.

But in the face of significant complexity, they have consistently gotten stuck. And I believe the reason is a fundamental one. And that to go further one needs a new kind of science, whose foundation is programs, not just mathematics.

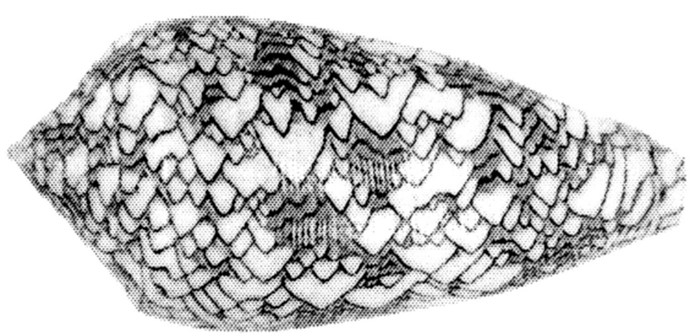
At the core of this new kind of science is an exploration of the abstract world of simple programs. But from this, there come applications, both immediate and profoundly far-reaching. If in the past we had been faced with something like this we would never expect to understand it.



But now we have discovered that it can just come from this very simple program.



In nature, we find many elaborate patterns—like the ones on this mollusk shell. Which we now see can be explained by very simple programs. And for example throughout biology, complexity can come from simple programs which then finally begins to give us the possibility of a true theoretical biology.



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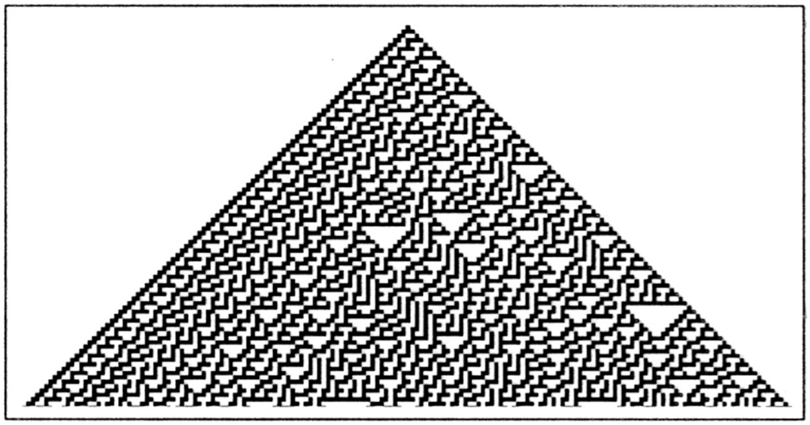
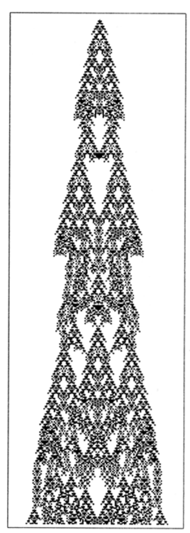


Traditional mathematical science has had its greatest success in physics. But still we do not have an ultimate theory of physics. And indeed our theories always just seem to get more complicated. But one of the suggestions of my work is that at the very lowest level—below even space and time—there may just be a simple program. A program, which, if run long enough, would actually reproduce in precise detail everything in our universe.

Finding that program would be a dramatic moment for science. But from the progress I have made, I am hopeful that it may not be too far away.

There are also many more everyday issues to which simple programs bring new models and perspectives—not only in physics and biology, but also for example in earth and social sciences. Simple programs also seem uniquely suited to analyzing many critical systems that involve large numbers of interconnected parts.

Having captured phenomena scientifically, one can start to harness them for technology. So now we can begin to create technology built not on concepts like wheels or waves, but on processes like this. And as we explore the vast world of simple programs, we can systematically mine it for technology. Finding new and unexpected programs that can be used for encryption, or pattern recognition, or decentralized control. Perhaps even finding programs that manipulate information more like humans. And indeed creating a whole new generation of technology—from which new industries can grow.



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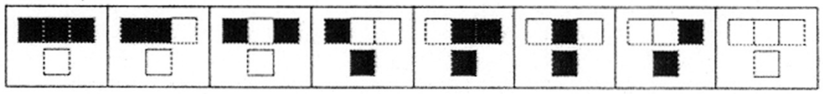
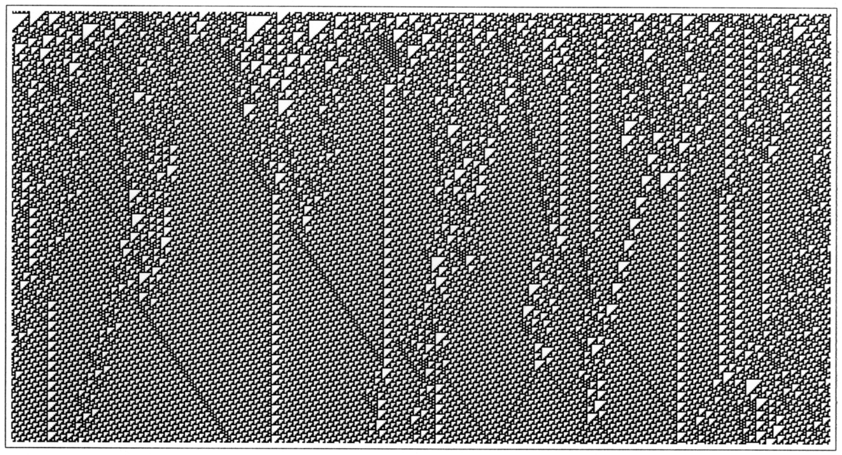


Or like many of the ones in nature.



This has many deep implications for what can and cannot be done in science. But it also immediately suggests that we can use much simpler elements to make computers. Which for example points to a new approach to nanotechnology.

Over the past year, my book has stimulated great activity in many scientific and technical communities. As well, of course, as some of the turbulence one should expect in any potential paradigm shift.

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One day the study of the computational world will no doubt be an established science, like physics, or chemistry, or mathematics. But today the exploration of the computational world still stands before us as a great frontier. With the potential not only to unlock some of the deepest questions in science, but also to define a whole new direction for technology.

Thank you. I just tried to cover twenty-five years of work in five minutes. I'd be happy to expand on anything.

Further Information

Book: Stephen Wolfram, *A New Kind of Science* (Wolfram Media, 2002)
Website: *www.wolframscience.com*

About Stephen Wolfram

Stephen Wolfram was born in London and educated at Eton, Oxford, and Caltech. He received his Ph.D. in theoretical physics in 1979 at the age of 20, and in 1981 was recognized with a MacArthur award.

In the early 1980s he made a series of discoveries about systems known as cellular automata, which have yielded many new insights in physics, mathematics, computer science, biology and other fields.

In 1986 he founded Wolfram Research, Inc. and began the creation of Mathematica, now the world's leading software system for scientific and technical computing.

With Mathematica as his tool, Wolfram spent the 1990s pursuing an ambitious program of basic science, culminating in the 2002 release of his 1200-page book *A*

New Kind of Science. An immediate bestseller, the book has been widely hailed as initiating a paradigm shift of historic importance in science.

In addition to leading his company and creating innovative technology, Wolfram is now developing a series of research and educational initiatives in the science he has created.

Senator BROWNBACK. Well, please pardon me at the outset if I ask some dumb questions, OK? Because what you've put forward is very profound, it's going to take me some time to really absorb it.

As I understand, the thesis in your book is that all systems of nature are basically a set of simple programs. Is that a working thesis for what you work under? I mean, as you demonstrated in the shell diagram, I think I've seen where you have butterfly patterns at some points. Virtually everything in nature is a simple program?

Dr. WOLFRAM. Yes, I believe that simple programs are a good way to describe many kinds of systems in nature. There are other ways to describe systems in nature—for example, traditional mathematical equations—that have their domains of applicability. But I think, for many of the kinds of situations where particularly we see complex behavior in nature, simple programs are the right form of description.

Also, it's my guess that if we look at the most, sort of, fundamental ultimate level underneath physics, that ultimately there should be a simple program that describes everything in our physical universe as something that, sort of, is the ultimate law for physics.

Senator BROWNBACK. Have you moved further on that theory, then, as well, on the ultimate law of physics into a simple program?

Dr. WOLFRAM. Yes, I've made a—I think, a certain amount of progress. The thing that one sees, if one looks at, sort of, the history of physics is that there tends to be—as soon as one looks a greater level of smallness, from atoms to particles to quarks to strings and so on, it seems like the theories that one's using are getting ever more complicated. But what's happened, from the work that I've done on studying, sort of, the computational world, is that I've, sort of, developed the intuition that there might be, ultimately, a simple program that actually produces the kinds of things we see in physics.

Just like, for example, in this example here, you can see these little structures running around that have, in many ways, characteristics a bit like the particles we see in elementary particle physics. This just a, sort of—this is just a, sort of, simple idealization of that, but it gets, kind of, the point across, that from, for example, the very simple rule that's defined here, you can see there are several different kinds of structures that arise that are at least a caricature of the kinds of things that we see or the different kinds of particles—electrons and quarks and those sorts of things.

I've been interested in trying to understand what type of rule might actually be "the" underlying rule for the universe. One of the things one realizes is that if there is a simple underlying rule for the universe, it's almost inevitable that the things that we're familiar with, features of space and time and so on, will not be immediately visible in that underlying rule, that, sort of, there isn't room to fit in all those details that we know about the universe into some tiny rule. So that means that, as we try to study that rule, we're, sort of, confronted with kinds of things that, to us, must seem very abstract, because they're not familiar from our everyday experience.

While I have, sort of, a definite kind of approach to what that underlying rule might be like, and it has to do with various kinds of discreet networks of points and so on, but one of the key ideas is that, for example, space, which we usually think of as just being, sort of, a background on top of which everything in the universe exists, that space actually has a definite structure, and that, sort of, underneath space, there is a kind of a discreet network that is what everything we know in the universe is built up from.

It's kind of like when you look at a fluid, like water, for example, we perceive it as kind of a continuous material, but we know, from what's been discovered about atoms and so on, that underneath this apparently continuous material there are a bunch of discreet molecules bouncing around. And I'm guessing that the same is true of space.

So there's a fair bit to say, and I've made a fair amount of progress, and I've been very encouraged. As one tries to assess a scientific theory, one of the ways that one does that is to say, sort of, How much does one get out for what one put in? And what I've been very encouraged by is that by putting in only very small amounts, it's been possible to get out a lot of things that one can, sort of, explain, in terms of what one knows about the way that gravity works and the way that various other features of the physical world work.

Senator BROWNBACK. Well, talk to me about gravity. Have you found a simple program associated with gravity, thus far?

Dr. WOLFRAM. A slightly complicated answer, but I can—let me give it a try here.

So the—one question is, sort of, we have to start talking about what the—let me see if I can get this to work—we have to start talking about what the structure of space might actually be. I'm trying to show an example here.

My concept of the structure of space has to do with making, kind of, a network where—in space, there are just these discreet points, and every point is connected to other points. And one might think, How could anything like space, as we know it, arise from some structure like that? The answer is, if you have enough points—just like if you have enough molecules in water, so to speak, the, sort of, aggregate behavior of all of these is like a continuous fluid, and the same kind of thing happens in space.

And when it comes to thinking about gravity, one of the, kind of, key ideas, due to Einstein originally, in thinking about the way that gravity works, is this notion that one can think of gravity as related to curvature in space. And it turns out that, in this, kind of, model of what's underneath space, of these kinds of discreet networks that lie underneath space, there's an analog of that kind of curvature that Einstein studied in the General Theory of Relativity and so on. And it turns out that the features of that curvature that seem to arise from properties of these networks are exactly the features—seem to be exactly the features that Einstein showed were there in his General Theory of Relativity.

So what happens, to give some indication of how—well, let's see—this is just an example of, kind of, how the notion of curvature in space could arise, what one can have if one has—I think I'm—I'm happy to talk about this, but I think it may get—may veer. This is—one of the challenges in my book is, I wanted to write the things I wrote in a way that would be as accessible as possible, without, sort of, the need to know all of the technical details of the development of physics for the last long period of time. And the question of studying gravity is one that, to really explain it well, involves quite a few steps of explaining what's been done in physics over the past hundred or so years.

Senator BROWNBACK. But, if I'm understanding what you're saying here is, this is a pattern you would suggest might show a simple program that creates a gravity network through the curvature. Am I gathering what you're saying?

Dr. WOLFRAM. Roughly so, yes.

I mean, to give some idea of how this might work, if one of these networks represented what space looks like, on an incredibly small scale, one question one can ask is, How does space change over time? And to give, sort of, an indication of how that might work, I have, sort of, an example of, kind of, how one of these networks—I think I have an example; yes—of how one of these networks, sort of, rewrites itself according to a very simple rule. And the rule that's being followed is one where every time there's a—as a piece of network that has a particular structure, it gets transformed into a piece of network with a different structure. And just applying that same rule over and over again, one builds up a sequence of different networks. And the point is that when one looks at a large enough version of that network, the, kind of, large-scale properties of the network seem to correspond to what we know about the way that curvature in space works and the way that gravity works.

Senator BROWNBACK. Take me to your mollusk example. And what's the program of the pattern of the mollusk on the shell?

Dr. WOLFRAM. So here's a, sort of, an idealized version of the mollusk doing its thing. And the way it works is—I think I have a—one of these right here—the way it works is, there's a line of cells on the growing edge of the—there's a creature that lives inside this shell, and it lays down the shell in lines, and there's a row of cells on the growing edge. What happens is that the question—these cells either secrete pigment, or they don't. And it seems that the—one can describe the rules by which they decide whether to secret pigment by something like this that says: if a cell to the left is secreting pigment at this step and the cell to the right is not, for example, then at the next step the cell in the middle will secrete pigment. So it's, kind of, a simple rule that describes whether pigment will be secreted by a particular cell as the mollusk grows.

And the point is that, from that very simple underlying rule, it happens, purely as a, kind of—as a matter of, kind of, abstract fact, that that simple rule—from that simple underlying rule, there emerges this complicated pattern that one sees on an actual mollusk shell.

One of the things that's interesting, perhaps, is if one looks, for example, at different mollusks. You ask, what kinds of patterns can different mollusks produce? One of the things that I find very encouraging, from the point of view of, sort of, building the theoretical biology, is the fact that it seems that if you look at this, sort of, diversity of patterns that you see on different mollusks that exists, that the collection of patterns that you see corresponds well with the selection of patterns that you would see from, sort of, all possible simple programs of a certain kind. So it's as if these different mollusks were just, sort of, sampling simple programs at random, and then we get to see the results of those programs displayed on the mollusks' shells.

Senator BROWNBACK. So that each of these mollusks have some simple pattern. It's a slight derivation of each other, it looks like. I mean, they all have a pattern, and you're just saying that each of these are going to have some sort of fairly simple discreet patterns, slightly different, that produce these different coloring patterns on the mollusk.

Dr. WOLFRAM. Right. So the question is, What are the underlying rules? So sometimes those underlying rules lead to very simple patterns of stripes, let's say. Sometimes the underlying rules lead to much more complicated patterns that are perhaps hard to describe verbally.

What is interesting, I think, is that if one looks at the different possible rules that could be what was being implemented by these mollusks, then one can look at, sort of, the selection of possible rules of a certain simple kind. This is all possible rules of a certain simple kind. And what we see is that the types of rules that are represented here, the types of patterns that are produced correspond well with the patterns that we actually see in the biological organisms that exist.

So if we were—normally, in biology, it tends to be the case that one imagines that the structure of organisms today, for example, is something that reflects some long evolutionary history, and that the details of organisms today can only be, sort of, explained on the basis of knowing the, sort of, series of historical accidents that took place in the course of their evolution. One of the things that's sort of interesting about this is that there's a suggestion that one could actually have an actual predictive theory of how these organisms might work, because it seems to be the case that's what going on is in—at some level, that the organisms are just sampling different possible programs at random. So, just by knowing abstractly about what features, sort of, the space of all possible programs has, we can say things about what features these biological organisms are likely to have.

Senator BROWNBACK. So, have you come up with some computational pattern for some of these mollusk shells, where you've said, "OK, this would appear to be the pattern for this shell"?

Dr. WOLFRAM. Yes. Yes. I mean, the—it's a——

Senator BROWNBACK. Well, run one of those out, or enlarge one—can you enlarge one shell—showing me the simple pattern, and then let it run its course.

Dr. WOLFRAM. The particular technology I have here would—it would take me some futzing around to actually——

Senator BROWNBACK. OK, then don't.

Dr. WOLFRAM. But coming back to something like this, what—let me—well, what we see—one thing to understand is, whenever you make a model of something, there's a question of, sort of, What's the essential feature that you want to capture, and what are you going to idealize away? So when we make—when we try to reproduce these patterns in a model based on simple rules, what will happen is that we will have been successful if we manage to reproduce the, sort of, essential features of this pattern. There will be little bumps and perturbations that, you know, might have corresponded to, you know, what the mollusk ate on some particular day, so to speak, that we will not be able to reproduce. But the point is that we will reproduce the fact that we get, sort of, an overall pattern of stripes, or that we get some elaborate pattern which contains lots of little triangles, and so on. That's what we would expect, and that's what we do succeed in reproducing from this kind of simple model.

Senator BROWNBACK. Take me back to your evolutionary point that—we, in biology, have looked at this for some period of time as, sort of, a series of historical anomalies that then got built into the pattern. But what you're saying is then you can predict somehow, in the future, what that pattern may look like? I'm not sure I caught that point of what you were saying, that we've been focused on mostly observation, but you think there are predictable sets of computation—or a predictable set of programming options that may be presented?

Dr. WOLFRAM. Right. So one of the questions is—in biology, what seems to be going on is that there's some underlying program that's represented in the genome, and, in the actual development of an individual organism, what's happening is that that program is being run to produce whatever structure exists in the organism. The question is, for example, How did that program get picked? Which program is picked? How is it chosen, and so on?

Well, one of the things that will be, sort of, the simplest hypothesis is, let's say that some of the—that programs are just picked at random by, sort of, random mutations that take place in the course of biological evolution. What one might have thought is that no process like that—that one wouldn't expect that one would ever get anything complicated happening from a process like that. Kind of, the traditional intuition has tended to be: in order to get something complicated to happen, you have to, sort of, go to a lot of effort and put a lot of things in.

What I've discovered from looking at, sort of, what's out there in the computational world is that that's not the case. In the world of programs, you can have a very simple program where, in a sense, you put very little in, yet you get great complexity of behavior out. And so what, sort of, in a sense, the simplest hypothesis is—let's say that some particular feature of some particular class of organisms were—just came—let's say that the programs that generated that feature were picked at random. What would that feature then look like?

Well, what seems to be the case—and I've, sort of, opened up the study of this question and certainly haven't filled in all the details of it—is that, in a variety of situations that I've looked at, it is—

seems to be so that among the different organisms that exist on the Earth, so to speak, that they have, kind of, sampled a large fraction of the possible—of the space of possible simple programs. So that a good hypothesis for figuring out what one will see in these different organisms—what kinds of mollusk-shell patterns one will see, what kind of shapes one will see in leaves, things like that—that a good first hypothesis is that among all the leaves that exist and all the different species of plants and so on, they will be distributed roughly in the way that one would expect if one just picked simple programs for making leaves at random. And that's interesting, because that then gives one an actual, sort of, abstract prediction that says, just given the study of the properties of simple programs, we can then make a prediction about what we would expect to see among the different kinds of leaves that exist on actual plants.

Senator BROWNBACK. Give me some other examples that you have of what you've observed in nature. I mean——

Dr. WOLFRAM. Well, so within biology there are—let's see—somewhere here I should have some—ah, within biology, I mentioned shapes of leaves. They're kind of interesting because there's such a diversity of different kinds of shapes, from smooth, very simple shapes to very complicated shapes and so on, and it's not obvious that there should be some underlying—some simple underlying process that produces these. Well, it turns out that it seems like there is, and this is an example of, kind of, the—applying a set of rules that produce a pattern that corresponds to a particular kind of leaf. If you look at, sort of, all possible rules of this kind, it seems to cover well the considerable diversity of different shapes of leaves that——

Senator BROWNBACK. Wow.

Dr. WOLFRAM.—we actually observe.

Senator BROWNBACK. So that's a series of simple programs, to the lefthand column, that produce that type of complexity of leaves, to the right?

Dr. WOLFRAM. Yes.

So the—I mean, this is—within the biology, here are some examples. I think the—sort of, the—one of the, I think, fundamental questions in biology is—when we see things that go on in biological organisms, what is the underlying mechanism that produces the behavior we see? Is it something that's a very complicated thing that we can never really expect to understand in any fundamental way, or is there ultimately some quite simple rule, some quite simple mechanism which, when, kind of, played out, produces some very complicated behavior or structure in biological organism? And I think what one's seeing, in the examples that I've looked at, is, sort of, an encouragement that there are much simpler, much more understandable kinds of mechanisms taking place in biological systems.

It's, sort of—in a sense, when one tries to, for example, make models of biological systems, there are certain kinds of raw material that one can use for those models. For example, one could use, oh, something from traditional mathematics, where one's saying, you know, there's a particular equation that's satisfied by this chemical concentration, let's say. Or one can use some very mechanical kind of explanation that says, you know, when you push this end of a lever, so to speak, the other end will go up. This is, kind of, a different form of mechanism, where one's saying—where one has a simple program of such and such a kind, then when one, sort of, plays out the consequences of that, one will see this particular form be produced that may be a complicated form, as in the case of, for example, these leaves or the mollusk patterns.

Outside of biology, there are all sorts of other examples. Here's an example, for instance, in physics, sort of, a typical kind of elaborate pattern that we see often depicted as snowflake shapes. There's a question of, "Why do snowflakes end up having these complicated shapes?" And it turns out that, again, there's a—if one—in this case, one can, sort of, trace down the physics of how snowflakes are formed. And as one tries to, sort of, capture the essential mechanism that's going on, it seems that when one does capture, sort of, what seems to be the essential mechanism and, kind of, plays out what those rules imply, this is what happens.

Senator BROWNBACK. That was a simple program that you just put forward?

Dr. WOLFRAM. Yes.

And so if you look, for example, at all the stages that you produce, they correspond well to the actual shapes we see in snowflakes. It seems like from just having a simple rule that's saying something about how you have a hexagonal grid of cells, where every cell either has solid in it corresponding to ice or doesn't have solid in it, there's a simple rule that says you add a cell of solid if—in this particular case, if there's exactly one cell of solid on the previous step. That's the whole rule. And that captures various physical processes that go on in the actual formation of a snowflake. And as you, kind of, see what the consequences of that rule are, this is what their consequences are.

Senator BROWNBACK. Now, I've heard it said that no two snowflakes are alike. I don't know if that's accurate or not. Is that true?

Dr. WOLFRAM. Not entirely. What tends to happen is that two snowflakes that you collect nearby, they often come from far away in the cloud, and so they've come through different, kind of, life histories, and so they tend to look rather different. But the——

Senator BROWNBACK. But then there are obviously a lot of different types and structures of snowflakes.

Dr. WOLFRAM. Yes.

Senator BROWNBACK. Millions.

Dr. WOLFRAM. Well, I think that—actually, that snowflakes go through, kind of, definite stages, and those stages correspond well to what you see in this kind of model. And what is surprising to people, I think—and it's an example of a, sort of, general surprise that one has about the way that complicated behavior arises—is one sees these very diverse kinds of shapes—because, I mean, some of these shapes just look like, kind of, simple hexagons; some of them seem to have lots of treelike arms and so on—and one might imagine, if one just saw one of these shapes, one might say there couldn't be a simple way that this was produced. Because our intuition tends to be that—when we see something complicated, that it must have had a complicated cause. The surprising thing, and the, sort of, thing that, sort of, I have gradually come to understand from, sort of, exploring the computational world, is that actually there can be simple rules that underlie even these sorts of complicated patterns.

Senator BROWNBACK. And even something that seems so random to us, as a series of different shaped snowflakes, could actually be all in the same computational—simple computational model.

Dr. WOLFRAM. Yes.

One of the things that's often interesting, there are many phenomena that we just say—we might just say, "Oh, that seems random." And, in a sense, when we say that, that's really just saying, "Well, we don't have a theory, a method for predicting how this particular phenomenon is going to work." So we just say, "Let's just say it's random. Let's just say it's something that we can't make predictions about."

Senator BROWNBACK. Different leaf shapes. You know, the different ones. It just seems like it's fairly random, what tree ended up with what shape of leaves.

Dr. WOLFRAM. Right.

Well, so, for example, another case that I've studied, to some extent, is the case of turbulence in fluids. It's a case where it's a very fundamental physical phenomenon that has great engineering importance, that if a fluid, like water or air, flows rapidly past an obstacle, it kind of curls up behind the obstacle making a very random-looking pattern. The question is, Where does that randomness come from? What's the, sort of, fundamental origin of that randomness? Is the randomness, for example, some reflection of, sort of, underlying, sort of, randomness in the atoms in the air or water? Is it something that comes from some, sort of, detail about the way that the system was started off? I don't think it's either of those. Those are the, sort of, traditional theories for where it might come from.

I think, instead, it's much more like the phenomenon that one sees in these simple program systems. To give an example, well, something like this one, where what you see here looks quite random, in many ways; yet the way it was produced is by a very definite rule, just following that same rule over and over again. And what you see in this case—in fact, for a number of technological purposes, it's important to be able to make good randomness quickly, and, in fact, this rule is a good way of doing that. Even though it's a very simple system, when you run it, its behavior seems, for all practical purposes, random, if you look, for example, at the column of cells just down the center of the pattern.

Senator BROWNBACK. Now, you've made a comment, "good randomness quickly." So what do you mean, that that pattern can be produced with that program quite quickly?

Dr. WOLFRAM. Yes. Yes.

Senator BROWNBACK. So that a fluid, when it flows past something that could trigger that—I mean, or that type of programming moves into place automatic—or very quickly.

Dr. WOLFRAM. Yes. Yes.

I mean, one of the things that's remarkable, when you look at fluids, for instance, is how quickly they do complicated things. When you look at a splash, for example, splashes have very complicated structure, yet they're made, kind of, instantaneously, so to speak, in a fluid. And there's a question of, sort of, how that works.

One of the things that's true about randomness, complicated behavior made in this way, is that every time you run this particular program you'll get the exact same result. Even though it's very complicated and even though if you were presented with its output, if you tried to apply statistical methods or other kinds of things, they would just say, "No, there's no pattern to this. It just looks random." Even though that's the case, every time you run this particular program you'll get the same result. So that means that—that has an important implication when one looks at phenomena like fluid turbulence, because it says that one might expect that these apparently random patterns are actually repeatable from one run of this experiment to another. That's—and that's important, because if you want to, for example, do engineering that somehow makes use of some feature of that turbulent flow, then to know that it's repeatable is extremely important, because then you can actually engineer with that in mind.

And so knowing the basic science, the answer to the basic science question of where did this, the randomness, come from in something like a turbulent fluid flow, has considerable importance. And it's something which there haven't really been tools or methods that have allowed one to really get at that question of, sort of, where does the randomness come from. And that's something which the study of simple programs let's one do.

Senator BROWNBACK. You mentioned, early on, that this has important implications for, say, something like nanotechnology, which this Committee has looked at previously. What are the implications there?

I mean, it seems to me the implication is that, if you can discover the simple program that produces a complex pattern, that we would be able to use that technologically in very small structures.

Dr. WOLFRAM. Yes, that's right. And then—so, for example, let's say that you wanted to make a system out of atoms, let's say, that could act as a computer. So what one might think at first is, OK, let's take, you know, the structure of a Pentium chip or something and let's make it really, really small and have that be the way that we make up our computer. One of the things that one discovers from what I've done is that actually you don't need all of the elaborate structure that exists in, let's say, a Pentium chip to be able to achieve the objective of being able to do computation.

And, for instance—well, this example that I showed here—this is an example of a rule that I know is capable of doing any computation that any computer can do. Essentially, you set up a—the top row in the right way—you're, kind of, programming it by the way that you set up the top row. And then as it evolves down the screen, the behavior that it produces can correspond to any computation that any computer can do. Yet the rules, the underlying rules that this thing operates according to, are just those rules at the bottom there. And those are very simple rules, which, because they're so simple, one can much more readily imagine being able to set them up as rules that could be applied and that could correspond to the behavior of some particular molecule or some such other thing.

So, in a sense, there have been a couple of traditions in nanotechnology. One is, take, sort of, the devices that we know from, kind of, large-scale engineering and shrink them down to atomic scales. The other tradition is, kind of, take what we see in biology and try and, sort of, piggyback on what we see in biology, because biology is the one, kind of, clear example of, sort of, nanotechnology that works, so to speak.

In a sense, what we're seeing here is something which is kind of a merger of those approaches, where one's saying this—these kinds of simple rules seem to be the essence of what's going on in, for example, some molecular biology situations, and they also seem to be things that we can, sort of, understand as achieving technological purposes, and this, sort of, provides us a different approach to doing nanotechnology.

Senator BROWNBACK. So that if you could discover the pattern that creates the various parts of an ant and how it operates, then you could use that into nanotechnology development on our part? Is that the sort of thing? Or are you talking more of a virus that——

Dr. WOLFRAM. I think the—so the question of what one is trying to achieve in the technology—let's say that what one's trying to achieve is to make a computer. Then this is the type of rule that one can use.

Senator BROWNBACK. Well, let's say what you're wanting to achieve is something that you could inject into me or you to go to the damaged area of the heart and fix it.

Dr. WOLFRAM. Yes. Then, sort of, the first step there is to understand, for example, the morphology of heart tissue. What is it like? It has certain structure. There are attempts to do tissue engineering, where one's interested in making something that, kind of, fits in with the tissue that's already there. So one of the first things is to try and understand—in the case of the heart, there's some rather complicated morphology that exists, and there's a question of how does that morphology come to be? What are the rules that make it? If we know those rules, then we can start creating artificial things that——

Senator BROWNBACK. OK. All right.

Dr. WOLFRAM.—will be able to, sort of, fit in with it.

Senator BROWNBACK. Well, how do you discover that pattern, that simple computation pattern, then, of heart tissue?

Dr. WOLFRAM. Well, so that's a—if one looks at, sort of, the development of science, typically, sort of, taking rules and figuring out what the consequences are is a lot easier than taking a phenomenon and figuring out what its underlying rules are. I mean, in, for example, the development of traditional mathematical science, there was, sort of, at first, the development of, you know, calculus and so on, where one could compute—given Newton's laws and so on, one could compute things about the motion of planets. Much later came along the field of statistics, where one could go and, sort of, take features of the natural world and, kind of, infer from those features some aspects of the rules.

In this case, the, sort of, general problem of, given a phenomenon, from what rules did it come, is extremely hard to solve. It's analogous to the, sort of, most general problem of doing cryptanalysis. If you're, sort of, shown the output from some process of coding, can you deduce the key that it came from? That's, in general, a very hard problem. And, in fact, one of the things that's come out of the work that I've done is, sort of, a proof that there is some fundamental difficulty in solving that problem.

Now, having said that, things are not as bad as that seems. Because if the programs for things that one's interested in are sufficiently simple, then essentially by searching or by building up a big library of those programs, there is a good chance that the things one's actually interested in may actually be accessible to a search or exists in the library that one builds up.

So, for example, one project that we've just been starting is to try and, sort of, buildup a giant atlas of simple programs and what they do. And, sort of, the concept of that is—because one's discovered that the programs for many very interesting things can be extremely simple, it is quite plausible that in the first, let's say, billion-billion programs, which is quite easily accessible to, sort of, frontier computing right now, in those first billion-billion programs could be programs that are relevant for lots of kinds of practical applications, whether they're for mimicking biology, whether they're for creating computational algorithms that are important, or whatever else. And, sort of, as we—if we can, kind of, do well at exploring and documenting the, sort of, computational universe, then we can expect to go and effectively mine it for the things that are useful for our particular modeling purposes or technological purposes.

And I think that's, kind of, one of the things that's, sort of, been opened up by what I've done, is the idea that it really is worth exploring this computational universe. Because one might have expected that the kinds of things that one could find by, sort of, just going and looking at all the possibilities, that one would never get to anything terribly interesting by doing that, that even if one looked at a billion-billion possibilities, that all of those would be—would somehow be too simple to actually do anything interesting and relevant to what we're interested in for modeling natural systems or for doing technology.

So one of the things that's, sort of, I think, a great opportunity that's suggested by what I've done is, if we can go and explore this computational universe and really have—and have a good map of what's out there in the computational universe. I think, and have good evidence, that we're going to find that there are lots of very, very useful things out there for modeling natural systems, for creating technology, and so on.

Senator BROWNBACK. How has your work been received by the scientific community to date?

Dr. WOLFRAM. We've had about 30,000 e-mails from people saying, "We want to follow up on this or that aspect of what you've done." I think the world would not be as it is if one didn't see a, sort of, spectrum of response to almost any new thing. So it's a spectrum of response, from tremendous enthusiasm to tremendous skepticism.

But I think the thing that I've been most encouraged by is in universities, government labs, companies, and so on. There are an increasing number of people who have obviously read the book in great detail and started to really do significant work that's based on what I tried to set down in the book. And, sort of, the challenge, in a sense, now is there are these many, kind of, different threads of development that seem to be starting out, and there's, sort of, a question of whether one can coordinate these in the best possible way. I think one of the things that I've seen from—I'm, sort of, something of a student of the history of science, and so—and I, sort of, believe that one might be able to learn something about the way things unfold now from studying what's happened in the past as developments have occurred in science.

I think one of the challenges is, there are many potential applications of what I've done, and those applications should and will come to live in the different areas to which they apply, like physics or biology or mathematics or computer science. But there's also, kind of, a separate area of scientific endeavor, which is the, sort of, basic science of understanding, kind of, what's out there in the computational universe. And I think one of the challenges is to see that actually, sort of, come into existence and prosper as an independent science, like a physics or a chemistry or a mathematics.

But I think the—I would say that, right now, the—it's been—in the last year and a half since the book came out, I have—we have had a hard time, kind of, keeping up with all the different things that people are starting to do, based on the book, which I suppose is an encouraging sign.

Senator BROWNBACK. If the Federal Government wanted to pursue this more—we're best at putting in resources and trying to focus attention and finding on particular lines—what are the things that we could do to be most effective to further try to understand this? I gather, from one that you're talking about, it's just gathering up a series of these simple patterns in large, large numbers.

Dr. WOLFRAM. Right. I think there are really two key directions. One is, sort of, education. This is a new methodology, and there's a question of, kind of, how can the people who could use this methodology really get good access to it? And that's, sort of, an issue.

And I know, in the general use of computers, actually, let alone the kinds of things that I've tried to do, one of the challenges is, among, for example, technical R&D folk and so on, how does one get to the point where people are really able to use computers and use computational ideas and methods, sort of, starting from the highest possible platform? Because, a lot of times, people have, sort of, first learned about computers 25 years earlier, and—but they're physicists, for example, and they don't see—you know, they don't know that they should go back and, kind of, learn more. And so I think one place where there's clearly value to be got is by, as much as one can, seeing ways to, sort of—channels for educating people about what is actually possible, what the methodologies are, what the tools are, and so on. That's one thing.

Another thing is really being able to map this computational universe. I think it would be extremely fruitful to have, kind of, a clear, sort of, coherent map of that universe; just as, for example, you know, we have a clear coherent map of the human genome, or we've had some coherent maps of the astronomical universe, so to speak. That to be able to have something where one has really said, "OK, we're going to look at, sort of, a billion-billion or more of these simple programs. We're going to catalog them." It's a little bit analogous to what's happened, for example, in chemistry, where there are these giant data bases of organic chemicals that have been filled in over the course of a century or more, where one's—if one wants to find a chemical that's relevant for some industrial process or for some biomedical application, one of the places one first looks is in one of these big data bases of organic chemistry. And, sort of, one of the things that one would like to have, I think, is a coherent big database of what's out there in the computational world.

And of the things that will be there, as I say, there will be both things that are relevant for, kind of, modeling questions in natural science, and there will be things that are quite directly, in some cases, relevant for technology. I mean, for example, in these—some of these pictures I showed, like this rule-30 picture, is directly relevant if you want to make randomness in some technological system. So, similarly, there will be other cases where one has something that's directly relevant to, for example, doing data compression or doing some form of pattern recognition. And I think that's the—to have a, sort of, coherent, widely accessible database of that kind would be something of great interest.

Senator BROWNBACK. I mean, you talk about it as a map of the computational universe. How would you go about discovering that? I mean——

Dr. WOLFRAM. It's—the good news is——

Senator BROWNBACK.—you sound like you should just start putting together computer programs, or simple ones, as many as you could think of, see them run, and then categorize them?

Dr. WOLFRAM. That's the first level of it, yes. There's a certain amount that can be done in that way, where it's essentially using lots of computer time, but the process is fundamentally very well defined. One knows what to do. It gets a little bit more complicated when one really wants to get, sort of, the best-developed, kind of, map, because a lot of these—let me show you an example of what you see in, kind of, the most basic kind of map.

This will be just a collection of the first—I think that's the first 128 programs of a particular kind. These are these cellular automaton programs. And what you see is, some of them do very simple things. Some of them make these elaborate, kind of, nested patterns, which, by the way, turn out to be—I mean, these nested patterns have been seen elsewhere, but, by the way, turn out to be relevant for some recent technological applications. And in other cases you see more complicated patterns going on.

So, sort of, the first level of this is just—you generate a very large number of these patterns. But the place where it becomes, kind of, nontrivial is how do you figure out which of these are interesting, which ones are going to be relevant for particular technological applications, and so on? And that's where considerable, at this point, human effort has to be spent to do the analysis to figure out methodologies to working out how do you sample the most interesting programs, those sorts of things.

I mean, another very straightforward—conceptually, at least—kind of thing is each of these programs can be thought of—it generates—for example, you can represent some aspects of the patterns by, let's say, sequences of ones and zeroes. And one thing one can ask to be able to do is, given that one has got a sequence of ones and zeroes, find the simplest program that reproduces that sequence of ones and zeroes. That's relevant if one has found that sequence of ones and zeroes in some actual experiment or some actual observation and one wants to try and work out where did this come from? What's the underlying model? What's the underlying process that produced this? And that's, kind of, an example where one has to have this, sort of, large library of what these simple programs do, because then one can go back from that and figure out, given something that one actually observed, where does it potentially come from?

Senator BROWNBACK. So that you would go throughout nature and you would observe the swirls that happen after a particular fluid flow, leaves, mollusks, and butterflies. You would see these patterns and then draw back from that what simple program can produce this pattern?

Dr. WOLFRAM. That would be a hope. All the steps to be able to do that, I don't know how to do yet. I know, in some cases, how—I mean, the process of going from a phenomenon to an underlying rule is one that, as I say, is, sort of, a fundamentally difficult thing. But what one can do—by having a very large library of simple rules and what behavior they produce, one has the chance to be able to say, "Oh, yes. This behavior that I'm now seeing in this fluid-flow example looks like this behavior that one sees in this particular type of simple program." And then one can go and start to do science based on that to make predictions about what one would see in the fluid flow and those sorts of things.

It is not the case—and, in fact, I think I can even, sort of, at a theoretical level, prove that it's not going to be possible to just say, given any phenomenon that you see, to systematically go backward and say, What did this come from? But what will be the case is—the encouraging thing is that, for many phenomena, the underlying rules may be—one can expect will be simple enough that one will be able to deduce what they are by essentially looking them up in the library and seeing what they came from.

Senator BROWNBACK. How did you get started thinking about this this way—the universe this way?

Dr. WOLFRAM. Well——

Senator BROWNBACK. What was the apple that fell off of the tree?

Dr. WOLFRAM. The main—I think the very original—I was interested in some questions about cosmology and questions about—this was about 24 years ago, or something, now—questions about how organized structures arise in the universe. And I, kind of, realized that the basic questions being asked about how organized structures arise weren't things that needed—that could only be asked in the context of this, sort of, complicated cosmology situation; they were questions that also arose in basic areas of physics and biology and so on. And so I started looking, what are the simplest possible models that I can make that could reproduce this basic phenomenon? And I ended up with these cellular automaton systems.

And, actually, what happened, as is so often the case in things that get discovered in science, when I first did the experiments I was so sure that I would not see the phenomenon that I eventually ended up seeing that I basically managed to ignore it for a couple of years.

But finally the thing that—I finally essentially generated this picture, and I finally actually realized I should—I had not believed that something like this could be possible. That is, I had believed that when the rules are simple, the behavior must somehow be correspondingly simple. And so I had actually produced a picture like this 2 years before I realized that this really was something real and something important. And it then took me another 10 years after I had, kind of, absorbed this picture to realize just, kind of, what the significance of it was, in a broader context, and its applications in different areas, and so on.

Senator BROWNBACK. Was there a moment—was there one event that you went, "Aha, this is it"?

Dr. WOLFRAM. No. It was, unfortunately, slower than that. I wish it had all been compressed into——

[Laughter.]

Dr. WOLFRAM.—"Now I understand how this all works." I think it is common—I mean, in a sense, it's, sort of, been a gradual process of realizing that this paradigm of thinking in terms of simple programs and so on, that it really is a powerful paradigm that one can apply in a lot of different areas. I had—as I did the work for my book, and so on, I had at first thought that this kind of paradigm might apply in some kinds of questions in science, but that other kinds of questions in science would necessarily require a very different paradigm. And I was—but I, in many cases, kind of, started looking, "Well, maybe I can see—is this paradigm applicable?" And I discovered that it was, and often in very interesting ways—for instance, to something like the foundations of mathematics was one that I had not really expected that this paradigm would have things to say about, and it turned out it had a lot to say about them.

Senator BROWNBACK. It sounds like you almost describe a universe where there's no such thing as randomness. This is all—everything has some simple pattern to it, or a multiple set of simple patterns that, layered on each other, produce everything we see.

Dr. WOLFRAM. Yes. If I'm right, the universe is the result of running a definite rule from a definite starting condition. And, in that sense, there is nothing about the universe—if I'm right in my ideas about fundamental physics, then everything, every detail of everything that happens in the universe is something that follows from those underlying rules, and follows in a definite way.

Now, you might think, if that was the case, then surely we can predict everything about what will happen in the universe. Things—it doesn't work that way. Most importantly, there's a phenomenon that I call computational irreducibility. And the point of that is the following. If you have a sufficiently simple pattern—let's say something like this—you can readily say what the color of a particular square will be any distance down this pattern, because there's a very simple—there's a very simple form—there's essentially a formula that tells you, after a million steps it'll be black if it's an even-numbered cell or something.

But the point is that when you look at something like, let's say—I don't know—when you look at something like this, you can no longer easily make a prediction about what will happen in this pattern after a certain number of steps.

See, one of the features of, sort of, the mathematical approach to science has been, sort of, the emphasis on essentially computationally reducing phenomena in nature. So, for example, you know, in a certain approximation, the Earth goes around the sun in a roughly elliptical orbit. And then there are equations that describe the position of the Earth. And if you want to know where the Earth will be a million years from now, you don't have to explicitly follow a million orbits; you must plug a million into some formula, and you can immediately deduce where the Earth will be a million years from now.

But in a case like this, there's a question of whether you can, kind of, reduce the computational effort of finding out what will happen in that kind of way. Can you jump forward and kind of not have to go through all the steps that this system itself has to go through to work out what it will do?

Well, the thing that I argue and have shown in at least certain cases is that there is a phenomenon that I call computational irreducibility, which says that there's really no way to predict what the system will do by any sort of procedure that is computationally more efficient than just, sort of, following each step and seeing what will happen. And that's—that idea has a bunch of consequences. It, for example, explains why, when we think about doing computer simulations of things, it's not only convenient to do those computer simulations, but, in some fundamental sense, necessary. There isn't going to be a way to just write down a formula for what happens. We're going to actually have to simulate each step to see what comes out.

And so, for example, when we think about doing things with the universe, the question of, sort of, what eventually happens in the universe—and even though we may know the underlying rule, even though we know exactly—we may know exactly how this network that underlies space and time works, and so on—to actually deduce the consequences of that for the whole behavior of our universe is, in a sense, I think, irreducibly computationally difficult. So, in other words, the universe has taken its 12 billion years, or whatever, to, sort of, get to the state that it's at right now, and it's, in effect, done some huge number of computational operations to get to that state.

The point of this phenomenon of computational irreducibility is that we can't expect to, kind of, crush, down to a very small number of computational operations, the process of working out what will happen. The reason for this is, it's—well, it's kind of a—it's related to this thing I call the principle of computational equivalence, and it has to do with the following. When—typically, in science, we make a certain idealization. Often, science has progressed by realizing that idealizations that had been made weren't actually correct.

One particular idealization that we make is that we, as observes of the natural world, are, in a sense, computationally infinitely—we're infinitely more computationally sophisticated than the things that we observe in nature. So—and that's why, for example, we expect that we can—that even though in nature some process may take a huge number of steps to occur, that we, as, sort of, infinitely computationally more sophisticated entities, can work out what will happen in a much reduced number of steps.

Well, one of the consequences of this principle of computational equivalence and this computational irreducibility phenomenon is that that isn't the case. In the, sort of, competition between us, as an observer of a system, and the system itself just doing its thing, we can't expect that we are, sort of, computationally more sophisticated than the system. And that's, in a sense, why this phenomenon of computational irreducibility exists.

Senator BROWNBACK. I'm not sure I gather that point, that we——

Dr. WOLFRAM. It's a hard point.

Senator BROWNBACK.—well, that we can't think computationally as sophisticated as the system thinks?

Dr. WOLFRAM. Right. So here's the, kind of—the issue. So as we look at different kinds of systems, they have different kinds of underlying rules, and they are capable of doing different levels of computation. One of the things that one might have thought, long ago, is that as one looks at different computational tasks—you know, if you want to do addition, for example, you might think, OK, I'll go and buy an adding machine to do that; if you want to do multiplication, I'll go and buy a different machine, a multiplying machine to do that. But, sort of, the big discovery of the 1930s that, kind of, launched what became the computer revolution was the fact that one could have a single universal computer, a single, kind of, universal machine, which, if fed the right program, would, on the one hand, be able to do addition, on the other hand, be able to do multiplication, and be able to do all these different kinds of computations that we associate with computers.

So one question is, one might have thought that it would be the case that—as we look at systems with different underlying rules, that they'd all be able to do different levels of computation; that as the rules get more complicated, they'd be able to do more sophisticated computations, and so on.

One of the surprising things that I've discovered is that as you increase the sophistication of the rules for a system beyond some very low threshold, all systems seem to be able to do the same set of computations. So that's why, for example, in that case that I showed that I think is relevant as an example in nanotechnology, for instance, they're very simply rules, yet that system is capable of computation as sophisticated as any system.

So what comes out of this principle of computational equivalence is the idea that most sets of rules that one might use in systems end up having—giving—allowing the systems to be equivalently sophisticated in the computations they can do. That is, it isn't the case that, as we look at a succession of different rules for different kinds of systems, that we'll see different levels of computational sophistication.

So when that comes to—when it comes to looking at systems in nature, we had, in the past, kind of assumed that typical systems that we see in nature were computationally much less sophisticated than our computers, our mathematics, our brains, and so on. One of the consequences of this principle of computational equivalence is that that isn't the case, that all these different systems are equivalent in the level of sophistication of computations they can do. And that's why we, with our mathematics computation, whatever, can't, kind of, jump ahead of these systems in nature in working out what they're going to do, that we're, kind of—that we're just equivalent, in our computational ability, to those systems.

Senator BROWNBACK. Let me back up to one other point. The theory that you're working under is, there is no such thing as randomness in the universe. It's all—there is a computational pattern to everything in the universe.

Dr. WOLFRAM. That's ultimately correct. Now, having said that, if we could trace everything from, sort of, the lowest level of these little, sort of, networks that underlie space and time, if we could trace all of that, then that would be the conclusion.

Now, as a practical matter, when we study, kind of, everyday questions in, let's say, physics or some other area, we don't want to have to go all the way from the networks underneath space and time up to the system we're studying. We want to be able to think about that system and model it more at the level of the kind of components that we can immediately see in that system. And at that level, it may be that we have to describe something that goes on in that system as being, sort of, externally random, because we're not describing things right from the lowest-level kind of things underneath space and time right up to the system we're looking at. If we did that, then I think there would be no, quotes, "randomness" there.

But if we're describing it only at the level of description of components that we can readily see, for example, there maybe some sort of input from the outside that we're not capturing in the model of the components that we're actually looking at.

Senator BROWNBACK. But those inputs, themselves, would have a pattern to them, the inputs from the outside that you're talking about that might——

Dr. WOLFRAM. Well, yes, ultimately, if you, sort of, trace it all the way back, you get back, to the—sort of, the underlying rules for the universe, and then I believe it's—you know, this is the kind of thing where—you know, I spend some part of my life creating technology, and when one creates technology, one starts from nothing, and one builds something, and one, kind of, knows what one has. In doing science, one, kind of, has to say, well, this is how I think it's going to work. But until you've, kind of—you're kind of guessing against the universe, so to speak. And until you can, kind of, see that everything absolutely matches up, you can't say for certain that that's really the way things work. But it's my guess, which I find—which I'm encouraged in by, sort of, increasing evidence that I seem to find, that that's the way things work and that there really is such a definite rule.

Senator BROWNBACK. What are some of the best questions you've been asked about this as you've made these presentations at various places? I want to make sure to give you a chance to address any points that I have not asked you about that we really should hear about here in the record and the Senate Commerce Committee.

Dr. WOLFRAM. I think one of the things that—well, we've covered quite a bit of stuff here.

[Laughter.]

Dr. WOLFRAM. I'll think of it just after we end.

Senator BROWNBACK. Well, I wanted to make sure to give you a chance to address anything that we should hear about, because I find this fascinating. I found it fascinating when I—your information in the book broke into the popular press, you know, what they were describing of the universe of patterns. And I found that just fascinating conceptually at that time when I first heard about it. And I know that, since then, you would have had a lot of interaction with a number of different people and minds that have considered, critiqued, thought about what you've put forward that's challenged, probably, your thinking when you came out with the idea and the notion at that period of time. That's what I wanted to give you a chance to address, anything that's the most challenging or that we should know about here.

Dr. WOLFRAM. Well, I think the thing that I perhaps should, kind of, come back to is, sort of, the importance of what seems to be, kind of, an abstract piece of basic science, of studying, kind of, what's out there in the computational world. This is something that, if you look at, kind of, the history of things, it's something that, in a sense, could have been done a very long time ago. These kind of figuring out, kind of, what the consequences of simple rules are could have been done, but there, kind of, wasn't the right, kind of, conceptual framework to try to do it.

I think that the main thing that I think is really exciting is that now one's, kind of, seeing there are exciting things, kind of, out there in the computational world, and we're beginning to have a conceptual framework to think about these things. General principles, like this principle of computational equivalence, which, at first, seemed to be, in a sense, very abstract kinds of principles, that then quickly end up having very definite consequences about the ways that we can make computers and so on.

Senator BROWNBACK. Well, and for years we've done things where we've observed nature and then mimicked it in some form to be able to use for our own technology, our own use. It's been—my field, background, is in agriculture. We've spent a long time observing nature, whether it's just to see initially what plant produced a seed and now what does this seed do, to today where we mimic so much of how nature used to operate to try to maximize our agricultural production, various patterns.

It seemed like what you're doing here is, you're taking that just back another step. Instead of observing the growth consequence in nature, you're saying, "Here's the program that produced that, and let's discover the program so that we can take that on forward," which is fascinating conceptually and something that could be incredibly useful technically and for us, as mankind.

It's also, in a very theological basis, of where did the program come from—the very simple program that produces that incredible pattern, where does that come from? And it's very interesting.

Thank you very much for coming here today, sharing your wisdom, your insights, your thoughts on this. It's been fascinating for me. I'm hopeful we'll be able to work with you on looking at some of these. I hope that our National Science Foundation, our people are looking and observing this process.

As I mentioned to you privately, the sort of thing that we do best, I think, at the Federal level, is to fund basic research, really trying to find those underpinnings technologically that private groups can't fund because they just don't have the—frequently, the income coming in to be able to do that. But that's what we do do best, and let people build on top of that, so that this may be one that would be very useful for us. It's also how we grow our economy and grow our contribution to mankind, is by discovering fundamental things that then others can build on top of. And here's—this may be an absolutely incredible opportunity for us to be able to do just that.

Thank you very much for coming here today. The hearing is adjourned.

[Whereupon at 4:50 p.m., the hearing was adjourned.]

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