Episode 101 – A Journey of Computational Complexity
An Interview with Stephen Wolfram

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[Intro Music]

[00:34] Haley: We are here at the Ninth International Conference on Complex Systems and I have the honor to sit at a table with Stephen Wolfram. Stephen Wolfram is the creator of Mathematica, Wolfram Alpha, and the Wolfram Language, the author of A New Kind of Science and the founder and CEO of Wolfram Research. Over the course of nearly four decades, he has been a pioneer in the development and application of computational thinking and has been responsible for many discoveries, inventions and innovations in science, technology, and business. He has received his Ph.D. in Theoretical Physics from Caltech by the age of eighteen and he is the true founder of the complexity science field. Hi Stephen.


[01:18] Haley: It's an honor. Honestly, to sit down here and kind of unreal. But we're really glad that you took the time to sit here and chat with us for a few minutes.


[01:27] Haley: So, would you just start by introducing yourself a little bit and telling us a little more about your work. In case our listeners are unfamiliar.

[01:33] Stephen: Gosh, well in terms of complexity. I got interested in that when I was a kid. I guess that my first exposure to what would become my interest in complexity was this particular book cover that I got when I was twelve years old, that was a book about statistical physics. I was interested in physics, I ended up being a little bit of a physics prodigy type but this book had an illustration of gas molecules bouncing around and making a complex pattern of behavior and so I was really interested in how this happened. Actually, I got involved in learning how to program computers. Computers in those days were with the size of desks and so on, that was 1972, to reproduce what I saw on that book cover.

Actually, I didn't succeed in doing it with a computer at that time and I ended up studying particle physics and getting interested in sort of fundamental physics and so on. But then a few years later after I'd had the experience of building my first big computer system which was aimed at doing kind of, mathematical computations of the type two and physics. After that, I kind of got back to thinking about okay so how does this complexity stuff work? My first assumption was I
know all this fancy physics, surely, I can just use this to understand what happens in fluid flow or snowflake growth or whatever else but didn’t work. So, I then was in the okay, so what else can I do? Well, fortunately I knew a bunch of stuff about computers and that kind of gave me the idea of saying, well let's try to generalize this way of making models of nature from the kind of mathematical equations approach that people have used for like three hundred years and let's try to see what's a more general version of that.

That ended up with me thinking about using programs as a way to make models for systems in the world and then that got me into the question of okay so programs that were used to are ones that we wrote for a particular purpose, that long complicated things but what's out there in the computational universe of all the possible programs? If we just have a tiny little program that just has a few rules about how black and white squares work or something like this, what can that program do? And so, my first assumption was if the program is simple, the behavior will be somehow correspondingly simple but sometime in about 1981, I actually did the experiment of you know to run all the possible programs, the particular programs that I was interested in. The things called cellular automata which just have a line of black and white cells and each cell at each step is updated according to some simple rule and I just said, okay let me just do the computer experiment.

Let me just try all the possible rules of a particular type and many of them behaved in very simple ways as I might have expected from the fact that they were simple rules but just a few of them behaved incredibly complicated ways. My all-time favorite thing called rule thirty started off from one black cell and it makes this really complicated pattern the behavior. I first kind of understood that, that was a thing in about 1983 and 1984 and that kind of launched me on this whole direction and eventually I ended up writing a big book called A New Kind of Science, which suggests that I think of it as kind of a new kind of science to explore these kinds of things but what do I do for a living? Well, I was a physicist for a while then I ended up basically building the tools that I wanted to have and so I built my first big computer system. I built in 1981 was a thing called SMP that caused me to start my first company. Then in 1986, I started building what became Mathematica, which is the thing that I've been developing for the last thirty-two years now and that's turned into a thing called, Wolfram Language, which is sort of an effort to encapsulate as much computational intelligence as possible in something which can become a ubiquitous layer for computation and it's been really nice to see.

We just celebrated the 30th anniversary of Mathematica a month ago now and it's been really nice to see over the past 30 years just an incredible number of things that have been invented and discovered in the world with our technology in lots of different areas. It's very widely used in the research and development sector of the world and also, it's increasingly widely used in a lot of enterprise production settings. Where somewhere inside that big production website you see or that big company there's a Wolfram Language system that's running. Although one doesn't see that on the surface and also built something called Wolfram Alpha which is rather widely used. Well, by lots of kinds of people but particularly a large fraction of students know about it because they use it to solve their math, chemistry homework problems and such like.

That's been as a piece of technology it's interesting because it's something where you're going from human natural language and you're making use of sort of a very large collection of curated knowledge about the world to be able to compute answers to questions. But there are many relations between these different things that you ask me to sort of describe my story basically
it's an alternation of working on basic science and working on technology and what's been really
cool not originally intentional in my life is that I've ended up doing basic science that has led me
to realize that I can build certain kinds of technology. I built the technology, then that's ended up
giving me tools that allow me to do more basic science and I've kind of iterative that about four
times so far in my life and it's been; each time it is a little bit surprising how it works out, but it's
been very satisfying and productive.

[07:10] Haley: Sounds like a lot of feedback loops and some emergence that's come out in your
work as well and we've been following your work. We have I think four copies of A New Kind of
Science in our office.


[07:20] Haley: And it was a privilege to sit and listen to you today, giving your talk. Talked for
about an hour and a half and you opened your presentation wanting to share this clip from the
Santa Fe Institute. Would you talk a little bit about what that conversation looked like?

[07:36] Stephen: That was interesting. So, back in about 1983 or so I discovered a bunch of
things in the context of solar automata and I kind of, discovered what I guess now gets called
emergence, you know I had kind of discovered the surprising complexity of simple programs
and the computational universe and so on. I thought this is a pretty neat area and there's a lot
that can be done with it and there's kind of a whole science that can be built out of it. My first
sort of personal instinct was, well let me get a whole army of people to work on the science.
There's me doing stuff and then there's the army I can recruit so to speak.

So, it's like how do I get the most people I can to get behind this idea of building a science of
complexity. So, I started exploring how to do that and I developed this idea that I would start
some research center. I started a journal that's still running today called, Complex Systems that
was the first journal about complex systems. So, in the middle of all of this, there was a group of
people that had come out mostly out of Los Alamos and so on that was starting this thing which
at the time was called the Rio Grande Institute though it soon turned its name to Santa Fe
Institute.

I've been a consultant at Los Alamos, so I knew a bunch of these people and they had then said
will I come to this conference sort of, discussing the creation of the Santa Fe Institute thing. I
was kind of, interested because I was twenty-four or something and most of the people in the
room was like sixty plus which I is why get now to write some commentary on the re-published
version of that proceedings because I'm told essentially, everybody else who is in the room is
now over the age of ninety.

So, it was interesting because sort of the original idea was there were universities and they get
sort of, siloed into particular areas. How can one create sort of a grand inter-disciplinary
university and I guess the most bizarre point for me was all these people sitting around mostly
old. I am afraid they were all men, which is kind of a sign of the times, discussing if we raise two
billion dollars how will we build a great inter-disciplinary university. I felt kind of bad but by that
time I'd started a company. I knew a certain amount about the world. It was kind of like excuse
me, but if you guys only raise a few million dollars what will you actually do? Well, I have a
suggestion, let me tell you about complex systems theory. It's really this good area the ultimate
sort of, interdisciplinary area. So, I literally just this afternoon there’s a tape of what I said which I tried to play a little piece of but it was way too noisy and there’s a transcript of it which I just got and some of it is shockingly similar to what I would say today, which is both good and bad after thirty-five years but the part that I was trying to play here was I kind of said I wonder what will happen to this field.

There are ways that it could develop well. There are ways that could develop not so well. So, it’s interesting to see what’s happened and I would say that the thing that’s good is there are conferences like this. The field exists there are energetic people working on all kinds of interesting things. For me, if there’s a disappointment it’s that the basic science of complexity has not been more explored. I’ve spent a bunch of effort just studying what happens with this or that simple computational system. What does it do and there’s just amazing richness to be understood from that and there are pretty direct applications to modeling of various kinds of things to various kinds of technology, but people have mostly really concentrated on the application areas not so much on the basic science. It’s kind of like mathematics has a good deal because turns out people learn math because they think math is useful in the world and they learn pure math because it’s somehow something foundational to what might be useful in the world. It’s sort of, interesting in the Middle Ages logic and math was kind of neck and neck but logic lost out, math advanced.

What we haven’t yet had is a real commitment to things like complexity, the study of the computational universe as a basic science and there are just an awful lot of wonderful things people can do. Eventually, they probably will but it’s been a much smaller set of people that have been studying that very basic science, that have been studying all the applications. The great thing about the basic science is it will survive forever. The things that I did on cellular automata, these other simple systems that I did thirty-five, nearly forty years ago they are as sort of crisp today as they ever were but the things that I did that I like, modeling different kinds of things. Gradually as times have changed and people’s interest in modeling has changed those things they become less relevant, perhaps sometimes they become more relevant actually. There are some things which I thought was kind of a stupid model and then now it turns out to be this standard model for one or other kind of thing.

[12:31] **Haley:** I think that it’s interesting that you said you know when you were talking about that clip that you shared the Santa Fe Institute talking about the future of complexity and saying that you would actually say some of those things today. You also said that complexity science has been this concrete thing that we should study because it’s going to live through time basically like, Old Faithful. Would you call it an Old Faithful kind of science?

[12:54] **Stephen:** Well it’s, look it is like mathematics. It’s the basic science of complexity or as I more tend to think of it, the exploration of the computational universe that’s as basic as it gets and the mathematics that people studied in the time of Euclid is still completely valid mathematics today. The same is true with the study of the computational universe, it’s simply abstract and true so to speak. It’s not the question of whether this or that model for the economy is right. That’s a whole different question and that’s a question that is very ephemeral. These more basic questions are just the program runs and it does what it does, and the identification of which program is worth running and the understanding of what kinds of things it does that’s pretty interesting and pretty immortal so to speak.
Haley: I wanted to ask you this question because it seems like based on what we’ve talked about even in it being the thirty-year anniversary for you know and going back in time and seeing where you were at the start of the Santa Fe Institute. If you could go back and talk to your younger self like whoever was on that recording that young Stephen Wolfram what would you say to him?

Stephen: So, I made a couple of mistakes, one mistake was I definitely recognized even at that time the value of the sort of the basic science of complexity. But I would have said that’s what you really need to concentrate on. That’s what you really need to push people to do and in fact all of these applications are in some sense the enemy of that because the more the applications kind of crowd in, the more people say, oh we don’t need the basic science thing.

What happened with complexity was I pushed pretty hard in the mid-eighties to kind of get complexity as a field up and running and it didn’t work. Basically, there were efforts you know I started a complexity institute, the Santa Fe Institute got started. The Santa Fe Institute wanted me to come run their research program. I didn’t do it because I actually didn’t really quite believe in it as an entity because I thought there were too many very old physicists who bought retirement homes in Santa Fe. Then the question was what would happen to complexity as a field? Well, in 1986 basically I pretty much started my center for Complex Systems Research at University of Illinois and I had realized I made kind of a mistake which was that my goal was to sort of push the science of complexity but I ended up mostly being the one who was thinking about raising money for research center etcetera, etcetera, etcetera, and it was not a particularly efficient use of my time.

I thought look I’m somewhat competent at organizing things and this is not the environment in which my organizational skills are best used, and I would say that the University of Illinois I give it credit for having lived up to the promises that it made which is quite unusual for organizations. So, lots of kudos to them but I kind of quickly realized that this wasn’t the most efficient way to pursue what I wanted to pursue. So, I went to plan A was raise an army, get complexity study by lots of people. Plan B, late 1986 was, build the best tools in the best environment I can for myself and do as much as I can myself.

So that’s why I built up Wolfram Research my company and Mathematica, Wolfram Language and then in 1991 I kind of, dived into okay so the point of this was to build sort of a set up where I could progress as quickly as possible with my research myself. So, I thought okay I’ll just go figure out the things that I meant to figure out before but now I have better tools will be really much faster to figure them out. The bug was turned out as I started digging there was a very long way to dig and there were just a lot of things to discover and I ended up spending in all ten and a half years exploring what ended up becoming my big book New Kind of Science.

Haley: Your exploration, what you do and what you’ve practiced and applied is inspiring to people here and I mean just trying to catch you after your session here we’ve got a crowd of people around you and they were asking you tons of questions.

Stephen: Yes.

Haley: If you had any advice for a young, a complexity thinker who wants to just dive into this field. What would be your advice for them?
Stephen: Well one piece of advice that might sound self-serving, but it isn't in a sense is that I spent a lot of time building tools that I think are really well optimized to actually explore many questions. But among them, questions and complexity the whole Wolfram Language stack is really well optimized for these kinds of explorations. Learn it as well as possible. It's always been surprising to me in my life because I make a point of learning tools, building tools if I don't think they exist, and you know I see people who are like oh well you know it's good enough for me to just do this and this and this. It's like no you're almost certainly wrong you know for example when I worked on New Kind of Science which was as I say took me ten and a half years to work on this book at a very slow rate because I was figuring out everything that was in the book. But you know I try to figure out. If I hadn't built Wolfram Language or Mathematica first would I have finished the whole project sooner or later? My very clear conclusion was I wouldn't have finished that project in my lifetime. In other words, even though it took me five years roughly to build the first version of those tools. Building those tools accelerated getting the project done by at least thirty years and so it's kind of the main advice which people are surprisingly bad at taking is learn the best tools you can. You know I've spent a lot of effort building these tools very well optimized for this particular purpose. So, it's kind of, silly not to use those tools and as a practical matter, you can even get them free everywhere pretty much at all the universities and even on the web and etcetera, etcetera, etcetera.

So, it's kind of, like the only excuse is, well I can't be bothered to learn it. One of the things for me that's interesting is I've been teaching Wolfram language a bunch to kids. Turns out by the time the middle school kids can get proficient with this so can a complexity researcher, one would hope.

Haley: Yeah, we do hope.

Stephen: That's one thing I would say. Another thing I would say about research, in general, is people go through school, they learn about a bunch of methods, how to do things the question of whether you are really successful in research is vastly more about did you ask the right question then were you good at the mechanics of answering the question. The strategy is a critical thing in research, what is the point? What are you actually trying to figure out Recently, Smith and Jones did this and now we're going to do that is you are not going to get to a big point by doing that. In my life because of spent a lot of time running organizations and so on that's all about strategy, make decisions, figure out what you're going to do. That's really important for research as well.

I would say another thing that I would comment on is, I have sort of developed a very simple strategy for making good research progress, which is in today's world it's kind of like if there's any field called X, there either is now a computational X or there will be. You know the people who kind of, learn the computational thinking to be able to participate in that will typically be the leaders in these fields but given a field X and you're trying to apply computational thinking, you're trying to apply ideas of complexity to that field. How should you do it? Well, my strategy is to ask the question what is the foundational question of that field? And what's interesting is fields that are fairly young fields the people who invented the field are still around and worst
case you can just ask them and they will pretty much tell you but by the time a field is older like third or fourth generation, nobody knows what the foundational questions are anymore, well they kind of know but they say, oh you can't work on the foundational questions they're way too hard and by the way all the answers to those already known because my strategy tends to be what is the foundational question of that field and people may have ignored the foundational question. They may have been pursuing a frontal attack on that question for thirty, fifty years whatever. More likely they're ignoring it and what I found is that often with new methods that one has computational methods, complexity science methods, one has a chance to attack that question perhaps to attack it from the side rather than make a frontal attack on the question and actually make surprising progress. I think that's a really good strategy for people doing research. It kind of anchors you to do something important. What is the foundational question? Can you actually make progress with that foundational question? Rather than saying, well I'm only going to work on these very incremental kinds of things.

Another criterion and specifically in complexity is and it sounds paradoxical given the field name but you know work on simple questions so to speak, in particular what I've seen for example with simple cellular automata I used to when I still have physical files, I used to have a file folder for every one of the two hundred and fifty-six elementary cellular automata rules. It's like okay, every time a paper would come in about something related to rule eighteen or rule ninety or rule one eighty-four goes in that file folder. Okay, so I thought you know many of these file folders I'm never going to use them, turns out over the course of years almost everyone got some papers in it about all kinds of different things.

So, what that tells one is when the model is simple enough it's worth studying it and you study it as basic science. You study its implications it's pretty much inevitable that that will be a surviving thing. If you study this very, very elaborate complicated thing then who knows what will happen it may all turn out to be irrelevant but if you're studying something where the structure of the model is simple enough it's kind of, inevitable that it will end up being relevant. It is the same thing as what's been seen in areas of mathematics for example where things eventually become relevant that was just once it's a clean and simple enough thing that you're studying it's going to end up becoming relevant. Even cellular automata, where I thought these cellular automata are just so dull, nobody will ever find a use for it. Rule 184 actually was one of the ones which I was just like well kind of, interesting for phase transitions, but I don't think it's really quite simple and its behavior nobody is ever going to care about it turns out to be the now standard model for road traffic flow. It is based on basically, Rule 184 and some generalizations of it. So, it just goes to show that when the model is simple enough it will end up turning out to be relevant and for people who want to do surviving work, working on the sort of basic science of the computational universe that's a really good bet. I mean the challenge just to say, there is sort of different niches you can get into. One is the let's work on something popular, the other is let's work on something unpopular. If you're working on something popular, if you figure out something then everybody will know why it's important because it's been a popular field, but you have a zillion competitors and if you don't do it somebody else is likely to do it.

If you work on something unpopular you work on something that people thought was a backwater or something that seemed to be completely moribund. Then often there's quite low hanging fruit to be picked and second of all you know when you figure something out there are no competitors. There's nobody paying any attention to that stuff. Of course, then you have the problem of telling the world why they should care about this, which is a different kind of problem.
It depends on one’s personality which is better. I mean for myself I’m much more interested in the second case, so I like studying these things. I suppose it’s a piece of egotistical personality trait or something that I like to think that I’m doing stuff that if I didn't do it nobody was going to do it. I don't like thinking, oh if I'm doing this I don't need to do it because somebody else is going to come to do it. I suppose my favorite activity is making what I like to call alien artifacts, which means things that nobody sort of thought would exist, but I can make them exist. In a sense, some of what I’ve done in the computational universe kind of has that character that it's building out sort of directions in science that nobody really thought were there. You know then one has to tell people why they should care about it. The good news coming to this conference is at some level we succeeded. There are people who care about the science of complexity.

I mean just to make one last comment if you asked me what advice I would give my younger self what happened was in 1986 I decided I'm going to go in the tool building business. I'm going to sort of do the stuff for myself and I didn’t, I completely disengaged. There wasn't really yet a complexity community, but I completely disengaged from the sort of social development of the field of complexity and arguably that was a mistake. Kind of why I did it was that it was just a lot of trouble to go deal with following all these threads. About a few years ago probably the tenth anniversary of my New Kind of Science book we did a survey of all the complexity institutes in the world and there were like three hundred of them at that time, very cool. It's like one question that I have for myself is, how much of a difference did I make to the fact that there are now three hundred complexity institutes in the world?

So, I'm interested in the history of science and so I wanted to figure out if I had never done any of the things that I did in complexity, how would that have changed the outcome? The disappointing thing was it was really hard to figure that out because we surveyed these complex institutes and we said what was your origin story so to speak? A surprising number of them didn't even know. That is, we say, where did this come from? Oh, it was a grant that was written by these people and so it turned into this and that and the other. What actually happened in the field of complexity research I think in the mid-90s when I was off being a hermit working on my science project. It started to take off, it came through some really unexpected areas like funding for climate science fed into a bunch of complexity kinds of things, it was sort of a very strange development there.

It's been an interesting journey and it's cool to see that even though I think there needs to be more basic science that's done in the area. It's nice to see that there's a community of people that identify themselves around a bunch of ideas and methods and so on. It's always nice one of these people is very curious how things work out whether they are how things work out with people, how things work out with ideas, how things work out with technologies and I'm now ancient enough that I've been able to see and I started my career young enough that I've been able to see a reasonable span. This is kind of complexity plus thirty-seven years and this is what happens.

[27:31] Haley: I wonder if you had a little bit stronger of a community when you were first starting out if that would have made a difference and really glued you to continue down that path or not you know if the community makes a difference in people's focus in that area.

[27:44] Stephen: Oh sure, these things the development of communities it's complicated you know I've been lucky enough to initiate several kinds of things in my life. Typically, when you
introduce some new methodology, start some new field, there will be people who kind of rush into that field. One of the things that surprised me was they're not always young, that is I remember when we first had Mathematical Wolfram Language, I remember standing around at our first user conference and this was before the web, so you couldn't go look everybody up and so on and we're like who's going to show up to this thing.

The big surprise was it was a more or less uniform distribution of ages. It wasn't you think oh new things are always adopted by the young, it's not true. I think that for all of us there are things which are well optimized for our particular way of thinking or whatever else and if you're lucky you live in a time in history when that way of thinking lets you do something worthwhile. I feel I'm pretty lucky that I've lived in a time in history when I've gone from computers kind of exist but are a little bit funky to computers are everywhere and computation happens to be a thing that fits very well with my particular way of thinking and so on.

But you know I think what happens when new fields arise is that there are people who are attracted to those fields. What I've noticed as you go many, many years later about half the people who came in at the beginning are still there and half have gone on to perhaps some new field or whatever else. I think with complexity at the beginning there was a lot of trouble because people would say, well I'm a biologist but I'm going to do complexity and really, they ended up being mostly biologists by methodology and what we need is sort of a methodology of complexity and that's part of you know comes back to this basic science thing. Once you have a uniform set of methodological ideas you have much more of the development of a community and so on. Back when I was starting some of the stuff out, it was very confusing who one should recruit for this community because there were people who were well pedigreed in existing fields who would bring the methodology of those fields with them. There were people who were just completely wild who might have had very interesting ideas, but they were very un-academically appropriate, so to speak. There were amateur science people for example, who were doing really interesting things but the idea of, oh you write an academic paper it's like I've never written an academic paper, I don't know what's in an academic paper even though the results they're getting very interesting. But it's like how do you then build that? How do you glue that together into something which can be you know in the world as it is with the whole institutional structure of things? And I think there's a certain degree of complexity the field could study itself. When I came out of my hermit mode of working on my book, kind of, went into the hermit mode for a very simple reason which was that I would start telling people about what I was doing, and people would say that's really interesting you should study this and that and the other thing.

No, I can't do that. I have this table of contents and if I am going to get it done in this lifetime I gotta stay focused on the topic. I don't want to deal with any of these external inputs. I didn't think it was going to take ten and a half years. I thought it was going to be much less time than that and it was a very difficult project actually because by the time you spend a decade now you know I was running a company as well.


[31:17] Stephen: It is also twelve hundred pages. I finally stopped the book kind of when the binding technology was of such but it was interesting to me to see what happened when that book came out because one of the most ironic things was I consider myself a fairly serious
student of the history of science and so I actually I'm very well aware of the phenomenon that if you're in the paradigm shift business the best indicator of good final outcome is that people are really upset when the thing first arrives. So, one thing I did not expect was that people who had been sort of pulled into the complexity area in some cases even by me fifteen years earlier or something were often quite hostile to like hello, I'm back and I've got this thing and it's like well, we were happily doing our thing you should just leave us alone and that was in terms of community development I think it was a little disappointing, from my point of view when we were sort of studying the what happened to the complexity field it really is to me still an open question. What the actual chain of causality about the development of complexity has developed but as I say my big regret I suppose about the history is I just should have arranged for a more basic science of complexity to be possible.

I mean the thing to understand is mathematics managed to get itself a really good deal. Because pure mathematics and things like that are thought of as being an underpinning to a lot of things that people think they should learn. What we haven't yet done with basic science of complexity is really make it extremely clear that is an underpinning of a lot of what people should learn. So, for example, one thing is people are learning elementary math, they could be learning how cellular automata work, it's perfectly elementary and I view sort of following cellular automata rules and things like that and understanding how it works a little bit like pre-computer science but it's also something that is kind of a foundational thing and I think one of the things that one should try to make happen is to have those kinds of foundational ideas really integrate into the educational arc that people follow. I just recently noticed from my favorite field of physics, particle physics the just crippling mistake made by particle physics which probably killed it pretty much in many ways for decades to come is that particle physics failed to inject itself into the high school physics curriculum.

There's actually nothing difficult about knowing a proton has three main corks in it. You don't need to know quantum field theory to understand that but yet that isn't part of high school physics. People learn that atoms have nuclei, they don't learn that protons have corks inside and they could. The fact that that wasn't injected into standard K through twelve education meant that people don't have an understanding of that. So I think that some of these basic ideas about the computational universe and about complexity is something that's quite important for the future that that gets to be part of this canon of knowledge that the typical person has.

That's something that we should hope for and I think it's something that certainly if I had my way, the way that we teach computational thinking and so on one of the pieces of that will be and then you should have intuition about how the computational universe works and that includes lots of stuff about complexity and that will become just sort of one of the standard things that people learn along with learning lots of other facts about the world.

[34:48] Haley: It would be really interesting I think too because a lot of what you learn in school is so disconnected and separate then bringing them together and seeing those connections there's a lot of fun and inspiration that comes out of that. So, I think it would be more engaging for students as well.

[35:01] Stephen: Yeah, right well one of the things that happens in this arc of research and so on is that when fields get older they get very solidified. So, what is physics? Well, it's this area where there's a methodology that's based on mathematics that does this and that and the other.
What's happened in education is another kind of solidification, I mean most of what is taught today is along lines that were set down a hundred years ago or more and breaking out of that is an interesting challenge. The computational paradigm which kind of which is the paradigm for the twenty-first century, the idea that, that if one can think about things and computational terms and complexity is one of the phenomena that is important, this is the dominant intellectual paradigm of our century basically. It will be interesting to see the dynamics of how that really impinges on what people learn and spend some effort trying to inject some of these kinds of things into the educational process.

It is interesting we talk about what happens many years later. I kind of know that these kids who come to the summer camps and things that we put on. Who learn about computational this that and the other, it is my sort of private entertainment that thirty years from now you know a bunch of these kids will be the leaders and computational XYZ and it's fun to sow these seeds, it was fun to sow the seeds of complexity science. Although as I say the frustration is it's actually kind of hard to tell exactly how those plants grew but it's very cool to see that something grew but there's a nice collection of people, very energetic and intellectually curious people who are at this conference.

**[36:44] Haley:** Yeah, we are very fortunate to be a part of that community and to be sharing interviews like this and being a part of that conversation. So I want to be mindful of our time. I know we have taken up quite a bit of your time. So, just in wrapping up could you share where listeners can find you and get ahold of you?

**[37:00] Stephen:** Yeah, there is a website stephenwolfram.com that has my various productions. I seem to end up writing blog posts, I really should just call them essays about all kinds of different things they have a habit of sometimes running to like forty pages and things. So, they often get cross-posted to a bunch of publications but yes so stephenwolfram.com is where to find me. If you really, really want to find me in person go to the contact section there then you can figure out how to find me in person.

**[37:28] Haley:** Well, thank you so much for taking the time to sit with us and tell us about your history. It has been a real privilege.

**[37:34] Stephen:** Thanks.

**[37:35] Haley:** Thank you.

[Outro Music]

**[38:31] End**

*DISCLAIMER: Humans transcribed this content. Please keep in mind, there could be some human error.*