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Complexity Economics:
Why Does Economics Need This Different Approach?

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W. BRIAN ARTHUR Good evening. What I want to talk about tonight is how complexity economics came to be, at the Santa Fe Institute here, what it's about, and above all what difference this new approach makes—if any. People often say to me, you have this complexity economics or nonequilibrium economics, call it what you might, but isn't it something of an add-on to economics? It gives agent-based models, and emergent phenomena, and self-organization, and all that, but isn't it really a bolt-on to standard economics? So, what does complexity economics provide that's different, that would change the way we see the economy or understand the economy, or change policy? Where does it fit in? And how should be think of it?

Let me begin from some basics. The economy, whether in Europe or the US, is an enormous collection of arrangements and institutions and technologies and human actions, buying and settling and investing and exploring and strategizing. It's a huge hive of activity, where the individual behavior of agents—banks, consumers, producers, government departments—leads to aggregate outcomes. A couple of hundred years ago, economists such as Adam Smith noticed in this that there was a loop. Individual behavior leads to aggregate outcomes, and aggregate outcomes—the aggregate patterns in the economy—cause this individual behavior to adapt and change. The individual elements of the economy in other words react to the patterns they cause.

It is this loop that tells us the economy is a complex system. In this sense complexity is not new in economics. As in all complexity, individual elements (human agents in this case) react to the pattern they create. Complexity, in a phrase, is about systems responding to the context they create.

For the first hundred or so years of the field, many economists thought in these terms, but this view wasn't easy to formally analyze. So economics came up with a simpler way to look at things. Starting about 1870 it asked what individual behavior would lead to a collective outcome that would validate or be *consistent with* that behavior—what outcome would give behavior no incentive to change? The situation would then be in stasis or *equilibrium* and we could much more easily look at it via equations and mathematize economics. We could keep the object still while we were examining it This was a kind of finesse; it was clever because you're saying if we can simplify the economy into an equilibrium system, we can reduce economics to algebraic logic. If you are willing to assume, in the part of the economy you are looking at, that individual behavior produces an outcome that doesn't give that behavior any incentive to change, you have the idea of a solution. This worked. It's a brilliant strategy: it gave insight, you can teach it, and you can use elegant mathematics. I was trained as a mathematician and this attracted me into economics. I thought I could make a killing because I knew quite sophisticated mathematics.

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It's worth looking more formally at how this equilibrium finesse works in modern economics. You take some situation in economics, whatever it might be; it could be the theory of asset pricing, or of insurance markets, or international trade, and you define this as a logical problem. You assume each category of agents—they might be consumers, or investors, or firms—has identical agents. They are all the same, and they know that everyone else is identical to them, and that they all face the same problem. Further you assume the problem is well-defined, and agents are infinitely rational, so they can optimize given the constraints of the problem and arrive at the best outcome for themselves, knowing others are behaving the same way. What overall outcome validates and gives no incentive to change their individual behavior? This is the basic recipe for doing theoretical economics, and its modern version is largely due to Paul Samuelson and a few economists who preceded him. The recipe is quite sensible in a way. It gives us a logical approach that can be made mathematical, and therefore scientific.

THE STANDARD APPROACH

1. Well-define a situation as a logical problem.

2. Assume identical agents who behave identically, rationally, and who know that other agents are identical and behave identically and rationally.

3. Assume agents' behavior produces an aggregate outcome that validates their individual behavior.

4. The problem becomes mathematical.

5. The solution is an equilibrium.

Slide 1

I've put this in a slide (slide 1) that's a bit complicated. The payoff is that if you can understand this slide it's worth five years of graduate school in a good economics department, because this is the standard method they teach you.

And so everything in economics is brought into this framework and put through this form of analysis if it's to be seen as theory. If agents were identical drivers of cars in crowded traffic, this is like asking what speed would produce a traffic flow where no car has any incentive to change. It has a rather lovely property of everyone achieving what is best reachable for them in an outcome where everyone else is attempting to achieve the best reachable to them. Any time I look at this, I find it beautiful. It's elegant. It's pure and it's perfect. You can prove theorems showing that if people behave this way, it'll be in some way optimal or efficient and it's highly mathematical. I want to say that this particular way of looking at the economy—it may be mechanistic; a lot of it was borrowed in the late 1800s from physics—has been useful. It has allowed economists to solve problems of finance, of central banking, or industrial production. We understand markets, we understand international trade, we understand currency regulation, and many other issues. We really understand large parts of the economy and we're not having major depressions like we had in the 1930s. This way of looking at the economy has achieved a lot.

But of course there's a "but." A very large "but." And that is if we assume equilibrium so that we can do mathematics, it puts a very, very strong filter on what we can see. If we can only look at equilibrium situations there's no scope for adjustment or for exploration or for creating startups or for any phenomenon in the economy like bubbles and crashes that appear and disappear like clouds in the sky. There's no structural change; there's no unexpected innovation—they're not equilibrium things. And, if everything is the same over time,

time disappears and there's no history, so there's no path dependence. We accepted equilibrium because it is so analytically useful, but it gives us a Platonic universe. It's beautiful, and ideal, and pristine, and lovely, but it's not really real. So economists in the last fifty years have begun to question equilibrium. They're questioning the standard theory's authenticity. I asked the computer scientist John Holland once in the early days at Santa Fe what he thought of equilibrium. Anything at equilibrium, said John, is dead.

So the question is, is the economy more alive, more vital, than equilibrium would show us? This was the question that landed in the late 1980s in Santa Fe.

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In 1987, the Santa Fe Institute was in the old convent on Canyon Road and it was very much a startup. My colleague Kenneth Arrow at Stanford brought me there, and he brought several other economists there. He and the physicists David Pines and Phil Anderson had brought some scientists to meet their counterparts in economics. One of the people on the scientific side was John Holland. Another was Stu Kauffman. There were quite a few people: David Ruelle, mathematician, and physicists Richard Palmer and Doyne Farmer. On the economic side we had Larry Summers, Tom Sargent, Buz Brock, and other eminent economists.

The two groups met and paraded ideas and this went on for ten days. It was awkward at first but in the end extraordinarily successful. We got a kind of sugar high on the ideas, and the Institute, after this now-famous conference, decided it would start its first program that was going to be funded by Citibank, by John Reed. Arrow and Anderson and Pines would be the godfathers of this, and they asked John Holland and myself to run the program. John couldn't get away from Ann Arbor, so I was brought in on a sabbatical from Stanford to lead this program. That was the good news. You know, "Brian, you're running a program on 'The Economy as a Complex System.' You're going to have the equivalent of a couple of million dollars, which allows you to buy out quite a few people on sabbaticals. You can have Arrow and Anderson (both Nobel Prize winners) do the inviting so nobody is going to say no. You can bring them all to a convent in the Rockies. They can all think what thoughts they want and nobody's going to object. It'll be isolated and nobody's going to know what you're doing."

The bad news was that when we got together in the fall of 1988 we weren't sure what to do. I remember Ken Arrow saying, "Well, we could do something in chaos theory. That's interesting." I was thinking we could do something on network effects or increasing returns, but I'd already spent many years doing that and was tired of it. We thought maybe we could take spin glasses or something from physics and translate that into economic terms. Nothing was quite settled, and this went on for about a month. We'd meet in the kitchen of the old convent. We'd have coffee, and we'd say, "Okay, what's the theme going to be? What's the direction going to be?" Finally I called Ken in Stanford to ask him what direction we should move in, and he called Phil Anderson, who called our funder, John Reed, the chairman of Citibank. The word came back, and it was unexpected. "Do whatever you want, providing it is at the foundation of economics and is not conventional." I was stunned. I couldn't believe it.

So we had permission to do what we wanted but still didn't know what that might be. Strangely, it was someone who didn't know anything about economics, Stuart Kauffman, who broke the logjam. One morning Stu said, "You know, I'm listening to all of this," he said, "and you keep talking about equilibrium. It's like a spider's web where everything's in equilibrium, the economy, but what would it be like to do economics *not* in equilibrium?" I tried to quiet Stuart. I thought this wasn't a good idea. This was freaky. We weren't going to mess with that. It was a kind of third rail in economics and I didn't see how we could do it. But Stu had planted a seed. It started to grow in my brain and in other people's brains and we started thinking seriously about it.

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Let me show you for a moment what it's like to think outside equilibrium. Here's a realistic situation. I have in mind typical problems these days in Silicon Valley where I work. Maybe you're starting up a company or several companies are starting up in the autonomous trucking industry, where there might be fleets or convoys of driverless trucks rolling across the country. Each company is trying to figure out how to strategize, how much to invest, what the technology should be. In a case like that, it's not at equilibrium. The companies don't know what they're going to face. They don't know the technology or how it will work out. They don't know who the other players are going to be in this business. It could be Google—likely Google. It could be Amazon even. They don't know what the reception is going to be for this new technology. They don't know what the regulations are going to be, the legalities, the insurance arrangements. And yet they're in a position where they have to ante up maybe two, three, four billion dollars just to sit at this casino table and play. Now, an economist would say that's not a question of probabilities. It's a situation where the firms face what economists call *fundamental uncertainty*. As Keynes put this in 1937[1] "the prospect of a European war … the price of copper in three years' time …. About these matters there is no scientific basis on which to form any calculable probability whatever. We simply *do not know*." Of course our firms know plenty, but if they want to define a real optimization problem or calculate probabilities, they simply do not know.

FUNDAMENTAL UNCERTAINTY

Realistically, agents are not identical and they don't know other agents' circumstances or likely behavior; or the situation is complicated and not fully known.

There is fundamental uncertainty:

Therefore "the problem" is not well-defined;

Therefore "rationality" is not well-defined;

Therefore "optimal" behavior is not well-defined;

Behavior and outcome unlikely to be in equilibrium.

Slide 2

There's a syllogism here. If a situation is subject to some high degree of fundamental uncertainty, the problem it poses is not well defined—you can't express it in clear logical terms. If the problem is not well defined, rationality is not well defined—there can't be a logical solution to a problem that is not logically defined. There is then no optimal solution, no optimal set of strategies, for agents and players—optimality is not well-defined. So if you simply don't know what you're facing, rationality isn't well defined and optimal behavior isn't well defined. So where an economic situation contains significant fundamental uncertainty, if we are rigorous it can't be credibly reduced to a rational, deductive economic model. And where rational, deductive behavior is not well defined, any outcome is unlikely to be temporary and not in equilibrium. I remember sitting in Santa Fe in the early days and saying to Ken Arrow, "We should do something out of equilibrium, Ken. We should do something that has fundamental uncertainty. Don't you think that's important?" And Arrow, who was our godfather and arguably the top theorist in the world, looked at me and he said, "I realize these are huge problems," he said, "but what can you do about them? What can you do?"

In the face of real, fundamental uncertainty, the standard approach gets stalled.

[1] Keynes, J.M. The General Theory of Employment. *Quarterly Jnl Econ*, **51**, 209-33 (1937).

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This is where we were in the fall of 1988. And we talked a lot about it.

But something had resonated from the previous year, and it came as an inspiration from a modest, diminutive man. In 1987, I had a house Santa Fe Institute had rented for me with a housemate who turned out to be John Holland. I had no idea of what he did; I had no idea that he was a large figure in computer science. I'd had a lot of discussions with him late at night over beer, and we talked a lot about John's passion of teaching computers to play chess or checkers. A month later when we held this big symposium, John gave a talk the first afternoon. I listened and he was on about genetic algorithms and classifier systems and how to teach machines to get smart playing board games—how to write algorithms that could change their code and get smarter as they develop. We now call this evolutionary programming. John had all this down and was describing it and I'm sitting there thinking, there's something here we need to pay attention to. And the nearest I got to figuring out what it might be was just a hunch. What John was saying seemed to me immensely important. I sat there thinking, if John Holland is the solution, what is the problem?

[Laughter]

John—and it took me quite some months to realize this—was pointing out two things: that people, or, if you like, automata, algorithms—*can* and do act in situations that are not well defined. This was mind blowing for us in the social sciences. Of course we do that. We enter contracts like marriage—not well defined. We decide we're going to get into Buddhism, but we don't know what that is. We do these things all the time and John was pointing this out, but I'd been overeducated. Second, in not-well-defined situations, John said, in various ways people try to make sense: they experiment, they explore, they adjust, they readjust, but not just in terms of having some wondrous mathematical model of the situation and updating a parameter. They form a hypothesis, maybe they have multiple hypotheses or ideas about the situation they are in, and they put more belief in the ones that work over time and throw out hypotheses that don't.

This resonated with me, at least a bit later as our program progressed. I was working for a large bank in Hong Kong, in their Analytical Division. I had a team and we were trying to crack the foreign-exchange market at three in the morning when all the play was happening. We were using what now would be called machine learning. I noticed that the actual traders had hypotheses: "Oh, such and such has just happened, the central bank of New Zealand has just come into the market. But hang on, I think the Chinese are going to do that. And if the Chinese central bank does this, then that's going to happen." But then something else would happen. They'd throw out the hypothesis they were using and take in others. John had been explaining all this to us and the breakthrough was I realized we could do this in economics. We could look at problems that were not well defined, whether there's fundamental uncertainty, problems where there is no equilibrium, and we could unleash agents that could explore, take up hypotheses, throw them out, and generate new ones and get smart.

We would have to use computers of course to track all this, and they would have to be based on the behavior of multiple "agents"—artificially intelligent computer programs that could have multiple hypotheses and use or change these. There was no name for this in 1990. And people were already writing programs that had multiple elements following rules and getting smarter. Not long after that the approach came to be called *agent-based modeling*. Agent-based modeling *emerges* from this approach as a natural technique.

Let me come back to our idea of an economic approach. We could model agents who had disparate ideas or hypotheses about the situation they were in, they could base their actions on these, and learn which hypotheses worked and which didn't, over time getting smarter.

Notice one thing here: these very actions of agents' exploring, changing, adapting, and experimenting further change the outcome, and they'd have to then re-adapt and re-adjust. So, they are always re-adapting and re-adjusting to the situation they create. This behavior is at the heart of complexity. I remember being asked in the middle of a talk like this, "Professor Arthur, how would you define 'complexity'?" I could have talked about elements reacting to the situations those elements create. But I had a better definition, "Have you ever had teenage children?"

[Laughter]

Here's what I'm saying. In general in a difficult or novel economic situation, there's no optimal solution. Agents are coping, exploring, adjusting, experimenting, whatever. But that very behavior changes the outcome and then they have to change again. It's a bit like surfing, where you don't know where the wave will go next and you're adjusting and readjusting to stay in the green water.

This lands us in a world where forecasts and strategies and actions, whichever you're using for that sort of situation, are getting tested within a situation—I would like to call it an *ecology*—those forecasts and strategies and actions create. And, if you look at it that way, that's the essence of agent-based models. Complexity economics is basically seeing the economy not as a machine, balanced and perfect and humming along, but as an ecology. New strategies, new things are coming and going, and striving to survive and do well in a situation they mutually create. We can describe this algorithmically, but not easily by equations, not just because the situation is complicated to track but because new behaviors and categories of behavior are not easily captured by equations.

This approach began to clarify itself in Santa Fe in the early 1990s, and there were other pods of researchers doing parallel work elsewhere. By the end of the decade the approach needed a name. In 1999 I was publishing a paper in *Science* on complexity and the economy. The editor called me from London and said, "You need a name for this approach." I said, "No, I don't." "Yes, you do." "No, I don't." He prevailed, and standing in a corridor on a landline I called it "complexity economics." I thought it should have been called "nonequilibrium economics" afterwards, but it was too late. It's been locked in.

By the way, I'm not saying here that human agents in a particular situation are algorithmic, that they're walking around like robots. It's more subtle than that. You can model people learning in new situations where they don't know. The outcomes likely will not be in equilibrium. So people adjust again. The outcome changes again. More readjustment. The situation may eventually settle or it may show perpetually novel behavior.

And one other thing. With algorithms or computation, you can model agents *acting* as well. This is because if you describe something algorithmically, you can allow verbs. Equations only allow quantitative amounts of things—nouns. With algorithms you can directly use verbs: agents can buy, sell, change their minds, throw things out, create new things. These are actions. Verbs. So this new economics doesn't have to be about the levels of prices, or interest rates, or the amount of production, or levels of consumer trust. In complexity economics you get real verbs—real red-blooded actions—happening, and the state of affairs they produce could be complicated and real.

What Difference does Complexity Economics Make?

I've given a picture of how complexity economics came to be. Now I want to go back to our original question and ask what difference does it make? Complexity economics I believe makes practical differences to economics, and also a difference to how we understand the economy. I'll point quickly to four specific areas.

1. The *persistence of financial crises*. Standard economics has become good at dealing with recessions and preventing full-scale depressions, but if you look at financial crises in developed countries, these haven't abated in the last fifty years. If anything they have got worse. And when they hit, they cause serious hardship and disruption. Contrast this with the number of fatalities to passengers on jet aircraft. Around 1950, this were about forty-five fatalities per million passenger miles. Now it's down to four or five, close to zero. Passenger flying, seismic codes, cardiac procedure deaths have all improved greatly in the last fifty years. Your overall health has improved, your pet's health has improved.

What hasn't improved is the persistence of financial crises. In Russia's big bang around the early 1990s, Russia decided to go capitalist, but the thinking was equilibrium based. Old socialist planning equilibrium. New capitalist equilibrium. What might happen in between was fuzzy, not understood. In reality a small group of private players took control of the state's newly freed assets for their own benefit and industrial production plummeted. You can see the results of this in the vicious mortality statistics Russia suffered for five or ten years after, because they got it wrong. Similarly in 2002 California freed up its electricity market, and a small number of outside suppliers manipulated the market to their own profit. The state's finances were put in jeopardy. And we all remember the US subprime mortgage collapse of 2008, where exotic derivative products and negligent oversight caused an unstable structure to spectacularly collapse. Each of these systems was manipulated or "gamed," and all broke down. Nearly always when you see a financial crash, you'll find a small group exploiting part of the economy; this causes wider fissures and failures and may eventually lead to a collapse.

After the financial crisis of 2008, in England the Queen famously asked, "Why didn't economists anticipate this disaster?" It's a good question. Some did of course. A few said, "We have this weird mortgage-backed securities market in New York and I don't feel good about it." But overall the profession does not excel at seeing trouble ahead and unlike engineering disciplines it doesn't have a branch of forward-looking failure-mode analysis or of post-crash forensic analysis. Why is this? The reason I think is subtle. If you *assume* equilibrium, which economists do, then by *definition* cascades of collapse cannot happen. It's like *assuming* an engineering structure is in stasis, therefore it can't collapse. Similarly at equilibrium there's no incentive for any agents to diverge from their present behavior—that's the definition of an equilibrium. Therefore, exploitive behavior can't happen. A subtle, muted, unconscious bias precludes ideas of exploitation or collapse.

Complexity economics, by contrast, sees the economy as a web of incentives for novel behavior. It sees the economy as open to new behavior, and that might turn out to be exploitive behavior. And it starts to ask, we are in this situation now, or we have this policy system in place now, what's the likely response going to be? Can someone exploit the system? Can some group game it? These are healthy questions to ask. The economy is not a closed static equilibrium system; it is a system perpetually open to novel behavior, and complexity economics forces us to keep this in mind.

2. *Awareness of the propagation of change.* The second thing complexity economics can help a great deal with is an awareness of the propagation of change. If you're only looking at equilibrium, there isn't any change, and so there isn't any propagation of change. But, as we heard in a previous talk from Matthew Jackson, banks—or people, or economic agents in general—can and do affect each other, and they do this through networks of connection: trading networks, information networks, lending and borrowing networks, networks of disease transmission. In these networks, events can trigger events and failures can cascade and cause disaster. Banks in distress can infect other banks. If I'm a bank and I'm in trouble and fail, then I pass on distress to my creditor banks who may pass on distress to their creditor banks. Stress can cascade through a system like this.

Equilibrium economics traditionally didn't cope with this sort of thing very well. It traditionally assumed that firms were independent, and so changes would be independent, and so their sizes and aggregate effects would be distributed normally. One of the great things about complexity is we now understand this sort of thing—interconnections between firms and agents and how these work—much, much better. We understand that individual banks and firms are not independent. Events with one bank can trigger events in other banks in the network, and so systemic risk—overall risk to the system as a whole—is not the summation of independent events, but it is reflected in domino-style avalanches of various sizes and duration. This gives higher probability to large disturbances than normal distributions would predict. And it yields power laws of various kinds. Propagation lengths, sizes of firms, and magnitudes of cumulated events are distributed not normally but according to some law where lengths or sizes or magnitudes fall off logarithmically, much like earthquakes do on the Richter scale. This sort of thinking is now crucial in modern finance and derivatives trading, and understanding gained from complexity in general and network behavior in particular excels in this area.

3. *A Strong Link with Political Economy.* Complexity economics also connects with the insights of earlier political economists and this gives both a depth and a validation to its insights. And it opens up a venerable literature to modern perusal. This isn't obvious, so let me explain.

I realized quite a while ago that in economics there are two large groups of problems. See slide 3.

TWO GREAT PROBLEMS IN ECONOMICS

Allocation in the Economy

Quantities, preferences in balance: General equilibrium, international trade, game-theory outcomes . . .

Formation in the Economy

Processes: Of econ development, discovering novel technologies, structural change, arrival of new institutions, temporary phenomena like bubbles, crashes …

The former is mathematizable; the latter, not.

Slide 3

You could call one group *allocation problems*. That's international trade theory, strategic outcome theory or game theory, general equilibrium theory, etc. Those are all well understood. And they have been mathematized, heavily mathematized. They are about quantities and preferences and prices in balance, and this gives itself to the equating of things—to balance and to equilibrium.

The other group of questions are ones of *formation*. How do economies come into being? How do they develop? How does innovation work? Where do institutions come from? How do institutions change things? What really is structural change, and how does it happen? These latter questions of formation are all ones that complexity economics can look at. Formation precludes equilibrium, and it's about how structures or patterns form from simpler or earlier elements. That's what complexity is about, par excellence, and complexity opens the door to rigorous study of all these questions of formation.

Formation is not new just because complexity has discovered it. It was studied in earlier economics, particularly within the older schools of classical economics, political economy, and Austrian economics. These differ somewhat, but together they provide a view of the economy as not necessarily being in equilibrium, always being in process, always subject to historical contingency, always creating and responding to a rich context it creates. None of this can be mathematized easily into balance equations; but it can be studied via the methods of pattern formation, network analysis, and algorithmic models heavily used in complexity economics. And so not only can we can meet up with these older and wiser approaches to economics and learn a lot from them, we can help make rigorous their insights, and thereby see the economy not as a static system, but as one in process, always exploring, and always forming itself from itself.

4. *We can model reality more rigorously.* Complexity economics sometimes uses mathematical equations, but more often its models are complicated, so to track things properly we have to resort to computation. This gives us if used properly an important advantage. Mathematical models in economics are forced to be kept simple, largely to allow pencil-and-paper analysis. They typically track rates of how aggregate variables influence aggregate variables—how average wages, say, are related to unemployment rates. And they need to be based on simple assumptions: identical agents that are rational and behaving in an equilibrium setting. With algorithmic models we can include as much detail as we want, to an arbitrary depth. And so we can free models from the tightness and inaccuracy of unrealistic assumptions. We can have agents who are realistically diverse;

we can include details of how they interact, of the institutions that mediate these interactions, details of networks and interconnections. Of realistic behavior. We have to be careful here, as with all modeling it's easy to lapse into throwing in useless or inaccurate detail. But done properly such detail adds realism—and thereby rigor.

Not surprisingly this new realism is giving us much better understanding of the processes of the economy, how, say, the 2008 crash happened, how diseases actually transmit, how economic development actually takes place. More precise detail allows give sharper resolution to the instrument of economic analysis and we see things that would not be visible otherwise. I don't think there is an upper limit to what we can learn here.

Closing Thoughts

Let me close with some thoughts here.

Complexity economics brings a different view of the economy—a different understanding to the economy. In standard economics, problems are well-defined, solutions are perfectly rational, outcomes are pure, possibly artificial in a way, but above they are elegant. And by assumption outcomes are in equilibrium. Loosely the economy *is* in equilibrium. Standard economics in a word is orderly. To borrow architect Robert Venturi's phrase, it has "prim dreams of pure order."

In complexity economics, agents differ and in general lack full knowledge of each other and of the situation they are in. Fundamental uncertainty is therefore the norm; ill-defined problems are the norm; and rationality is not necessarily well defined. Agents explore and learn and adapt and open to novel behavior. Outcomes may not be in equilibrium. Complexity economics deals with historical contingency, path dependence, and to quite a degree indeterminacy. It is organic, with one thing building on top of another. It is a cascade of events triggering other events, and so it is algorithmic. To borrow another phrase of Venturi, it's full of messy vitality. In philosophical terms, standard economics I would say is Apollonian, pure and ordered; complexity economics is Dionysian—it is structured and generative, with just a dose of wildness.

To go back to our original question, is this economics just a bolt-on to standard economics? Absolutely not. Nonequilibrium includes equilibrium, so it's a more general theory. It's relaxing the restrictions of equilibrium economics—well-definedness, rationality, identical agents—and thus generalizing standard economics.

Does this go with the *Zeitgeist*? I'd say yes. The sciences from roughly 1900 to about now have gone through a shift, I believe, from order, determinacy, deductive logic, and formalism to formation, indeterminacy, inductive reasoning, and organicism. There are many reasons for this. Part of it is the rise of biology and molecular biology as serious sciences and computer science and new types of mathematics. And, by the way, one of the symptoms of this is complexity itself. Complexity is not the cause of this viewpoint; it is more the outcome.

Let me finish with a rather beautiful allegory that the economist David Colander has given. Colander says, imagine that around 1850 all economists are gathered together at the foot of two huge mountains and they decide they want to climb the higher mountain. But both mountains are in the clouds and they can't see which one's higher and which one is not. So they decide to climb the one that's more accessible, the one that they can get their equipment through to, and the one where the foothills are better known. This is the mountain of well-definedness and order. They get to the top of that and once they're above the clouds, they declare, "Oh my God. This other mountain, the one of ill-definedness and organicism, is higher." That's the mountain we are starting seriously to climb, and much of the journey started at Santa Fe.

[Applause]

Questions & Answers

ALEX HESS I'm with Third Point. About financial crises, you said there's always a small group of actors that create the crisis. In the case of the 2008 financial crisis, who do you think that group constitutes?

W. B. ARTHUR I don't know this history very exactly, but a number of people in the audience do know it well and could answer this better than I could. What really caused this crisis, I think, were people who allowed good credit ratings to be given to what turned out to be worthless products and derivatives.

What I would point out is this: I'm trained as an engineer and after the crash I read a lot about failure mode analysis in aircraft. What you learn is that there's always some tiny crack that appears, some tiny event that triggers another event that might trigger another event. Nearly all the time nothing disastrous happens. But sometimes these events triggering events lead on and eventually disaster happens.

Why should complexity economics be any better looking at things like that than static equilibrium economics? Complexity economics is a way of looking at the economy based on events triggering events. Static economics equilibrium, looks at, well, equilibrium. It doesn't give you any feel for this triggers that, that triggers this. An economics that's built on stasis isn't biased toward seeing cascades of collapse.

VITALIY KATSENELSON: I'm a value investor. My question to you is, there is an underlying assumption in modern portfolio theory that people are rational. That seems to be the same assumption in traditional economics. Are there similarities between modern portfolio theory and traditional economics?

W. B. ARTHUR: I spent time with Blake LeBaron, who's in the room, and John Holland and Richard Palmer, looking at what's called asset pricing under conditions where people aren't necessarily rational. They're exploring and trying to find out how the market works as they invest. Sometimes standard economic theory works very well. Sometimes you can think in terms of rational players, think of probabilities of what earnings might be, and you could think of lowering risks by diversifying among stocks that are not that correlated. That's portfolio theory and it's fine. When our team in Santa Fe looked at asset pricing we used a computer model with "artificial" investors; these weren't "rational," and didn't know anything at the start (say, as in Alpha_Zero) but could learn from scratch what behavior was appropriate in what market situation. We found that realistic market phenomena emerged from our model: technical trading, bubbles and crashes, autocorrelated prices, random periods of high and low volatility. These were emergent phenomena and they occur in real markets.

I was talking about this Santa Fe artificial stock market one time in Singapore and a hand went up in the audience. "You've said in this talk that ninety-eight percent of pricing can be established by standard economic models and about two percent of pricing can be explained by these events triggering other events, bubbles, crashes, and so on. So we're only off by two percent. What do you think of that?" I said, "Well, that two percent is where the money gets made."

[Laughter]

You could equally argue that from outer space the oceans and the world are perfectly at equilibrium spherically, within maybe fifty or a hundred feet. But the non-equilibrium part is where the ships are. And that's what counts. It's the startups that make it. It's people who are lucky, the ones who understand how the nonequilibrium patters work. You can't model that with equilibrium.

AUDIENCE MEMBER Could one argue that complex systems like emergent phenomena are a form of equilibrium? Maybe they don't last for a long time, maybe they change slowly, but there is still some kind of equilibrium.

W. B. ARTHUR: My answer to that is no. Not really. You can do that I suppose, but it's a case of paradigm stretching. You can think, as I said, of cars with an equilibrium flow, of cars going down a freeway. And you could say that, in a situation like that, if the cars get quite dense, then an emergent phenomenon occurs, there might be a traffic jam. A car slows up for some reason. Maybe some animal runs on the road, cars behind it slow up, and quite soon there's a traffic jam, an emergent phenomenon. Now, and this is subtle, you could define a stochastic process that was in equilibrium, a stationary stochastic process of car flow, and you could set that stochastic process up so that every so often it fetches up occurrences like traffic jams. You've preserved "equilibrium." But I'll give you my reaction to that—Yeecchh!

[Laughter]

Why not just relax and say, "Equilibrium's wonderful. I'm not against it. It's given us a huge amount of economics. It's given us wonderful insights, but there are situations that are not in equilibrium," rather than try and say it's all equilibrium.

NANCY HAYDEN I'm from Sandia National Laboratory. How do you build a common knowledge with the new kinds of analytic frameworks that we have for nonequilibrium so that people easily can understand what they're seeing when they're looking at analyzing these kinds of verb type of measures and ways of framing the problem rather than the nouns? We haven't figured out how to do that easily for just kind of the general common knowledge other than specialists.

W. B. ARTHUR Earlier today Eric Beinhocker gave a brilliant talk. He said we need to look at things—in the economy, maybe in politics, or situations in general around the world—using different vocabulary, different types of instruments. We need to admit different types of phenomena besides just equilibrium ones. We need to allow that the world is messy, always unfolding, always giving us new things.

I think the entire *Zeitgeist* is changing, and we're in the last stages of Enlightenment thinking. In the 1730s, people thought everything was mechanism and everything was right and everything was properly ordered, and "All nature is but Art, unknown to thee; all Chance, Direction, which thou canst not see; […] all Partial Evil, universal Good." That's from Alexander Pope's 1733 *Essay on Man*, basically saying everything is ordered, everything is understandable if only we could look into it and understand its mechanisms or "art." And that's changed. I don't know whether it's world wars, or quantum physics, or biology. We're looking now much more at the world as swirling and changing and contingent and not fully knowable and organic, with one thing building on top of another. Santa Fe Institute would be in a very good position to say, if there is a new way of looking at the world, what exactly is it? Where is it coming from? What evidence do we have? Why do we think this way now, when we didn't do this forty or fifty years ago?

Our outlook has changed and I believe we will be talking a slightly different language. If you think everything should be perfect, and ordered, life gets brittle. "Oh, my life isn't perfect. Things are out of order. I haven't done this properly. I haven't done that." We get nervous. If we say, "It's pretty good but could be better," then it's one damn thing after another, which is a definition of life, and we're okay. We will adapt. Then people relax, the polity relaxes, social ethics relax.

I think we're missing a way of looking at the world where we accept life. I was influenced by Robert Venturi's 1965 book *Complexity in Architecture*. He meant complication. But he made a huge distinction between what he called the "prim dreams of pure order "of the Bauhaus, all these geometric perfect things, versus the ambiguous, the contingent, and then a whole list of things. "What I'm for is messy vitality," he said. I think if there's a message in complexity and in its *Zeitgeist*, it is let's cool it on the order. Let's just relax into a world of some degree of messy vitality. For my money, complexity shows us what that world looks like. It's not optimal, but it's pretty damn good. And it's alive.