*Quantitative Finance*, 2018
https://doi.org/10.1080/14697688.2018.1533738



Source: Santa Fe Institute

# Kenneth Arrow and nonequilibrium economics†

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On 6 November 2014, Kenneth Arrow gave an evening keynote talk at the Santa Fe Institute's Annual Trustees Symposium. The meeting's topic that year was Complexity Economics and Ken spoke about general equilibrium as a backdrop for the talks that would follow. His lecture was detailed, precise, and lengthy, and he spent much of it showing point by point how general equilibrium did not match with real economies. Ken was shining a keen light on the creation he was best known for: his work in the 1950s that established equilibrium as the basis both of economic theory and of our view of the economy. And he was showing a mismatch between theory and reality. I was struck by the sheer honesty of this. But it was more than just honesty. It was as if Ken, now 93, was looking back on his earlier ideas and was coming from a different way of thinking about them. He still regarded general equilibrium as important and elegant, but he also saw it as an ideal that might live in some Platonic world but not perfectly apply to this one. Kenneth Arrow was rigorous not just with mathematics but with his own thinking, and he was willing to reassess his own ideas if that furthered the truth.

I first met Ken when I came to Stanford in 1982. I was working on increasing returns. With increasing returns problems typically there is more than one equilibrium, and so the solution is indeterminate. My idea was to look at such problems dynamically, showing how small random events could steer the system sometimes into one equilibrium, other times into another. I had talked with several of my colleagues about this and was vaguely aware that Ken Arrow took an interest. But he and I had not directly discussed this. Then in 1985 I was giving a talk at Stanford on increasing returns—on agglomeration economies to geographical location in this case. For the first five minutes there was the usual banter and intellectual jostling. Then unexpectedly Kenneth Arrow walked in. The room hushed. People straightened up. I straightened up. My language became suddenly formal. I had listed several mathematical results as 'facts' to avoid being seen as showing off. Why hadn't I called these 'theorems?' asked Ken. I couldn't quite answer. But Arrow was interested in the ideas and he got the approach. In fact, Ken had been teaching the history of economic thought at Stanford for a long time, and knew all about increasing returns from Adam Smith to Alfred Marshall to Nicolas Kaldor. Ken's knowledge of economics was deep.

It was from Ken that I first heard about the Santa Fe Institute. In April 1987 Ken stopped me on the way to my office. There was going to be a meeting between economists and physicists in September at a small institute in Santa Fe just starting up. Would I like to come? I said yes but wasn't sure what I was committing myself to. I had never heard of the Santa Fe Institute.

Ken's relationship with Santa Fe went back to a year or so before this. John Reed, CEO of Citicorp, had been urging the new Institute to do something about economics, and physicist Philip Anderson had contacted Ken about putting together a group of economic theorists in September 1987

†This article makes use of material from my preface in Arthur (2014). See also Waldrop's historical account of SFI (Waldrop 1992).

to exchange ideas with a group of scientists. The conference when it happened turned out to be a serious affair. Among the 10 economists Arrow chose were Larry Summers, Tom Sargent, Jose Sheinkman, and William (Buz) Brock. Among Anderson's 10 or so scientists were John Holland, David Ruelle, Doyne Farmer, Stuart Kauffman, and David Pines. Ken and Phil presided. One participant would talk in the morning and another in the afternoon. We were learning not just solutions to problems in the others' disciplines, but about how each discipline formulated their problems, and how it thought about these, and what mindset it brought to bear on these problems. Questions not normally raised within economics were directed at Ken—why do you guys cling onto perfect rationality? Why do you assume so much linearity? And Ken questioned the physicists back. Why is a problem 'solved', say in spin glasses, when it has not settled to a steady state? We discussed chaos theory and nonlinear dynamics in both economics and physics. We discussed the modeling of positive feedbacks and of interactions, again in both disciplines. People would meet in the evenings to talk over ideas and problems.

No big issues had been resolved by the end of the 10 days, yet Anderson, Pines, and the scientists were left with a respect for the sheer complicatedness of the economy—the elements in the economy (people), unlike the ions in a lattice, could decide what to do next not just based on the current situation of themselves and other elements, but on what they thought those other elements might do given what they might do. And Arrow and his group were left with a feeling for modern physics, for its interactions and nonlinearities, its multiple possible end states, its lack of predictability—indeed, for its complicatedness. The meeting resulted in a book of papers edited by Arrow *et al*. (1988).

Word began to leak out after the conference that something interesting had happened at Santa Fe and the new institute's Science Board decided to follow the conference up by initiating a long-term research program on the Economy as an Evolving Complex System. Arrow, Anderson and David Pines would oversee the program and John Holland and I were asked to come to Santa Fe the following year to lead it. John couldn't get away from Michigan and declined. So I found myself heading up the Santa Fe Institute's first research program; it would start in August the following year, 1988.

Our immediate problem of course, working from Stanford, was to put together a team of first-rate people for the new program and decide its direction. We included some people from the conference. John Holland promised to come for a couple of months, and the physicist Richard Palmer for much longer than that. Stuart Kauffman would be in residence. From my own network I was able to bring in David Lane and Yuri Ermoliev, both excellent probability theorists. And Ken brought in for shorter visits Tom Sargent and Frank Hahn. Where I found it hard to cajole people to join in, Arrow or Anderson, both Nobel Prize winners, could simply lift the phone and quickly get people to join us.

Even with a really good team, we were not sure which direction the new program should go in. Ken thought that chaos theory might be interesting, but to me the idea somehow didn't appeal. I thought we should look at increasing returns problems, which I was more than familiar with, at how some of the physics methods could be transferred into economics, and at nonlinear dynamics in the economy. Also we might be able to do something interesting with computation in economics. When the program opened finally in 1988 I was now in Santa Fe and still groping for a way forward. I phoned Ken at Stanford and asked for his advice and Phil Anderson's. They got in touch with John Reed who was funding the program, and the word came back: Do what you want, providing it deals with the foundations of economics and is not conventional. For me and the others on the team, this directive seemed as astonishing as if the Vatican in 1520 had asked the Augustinians in Wittenberg to rethink theology. We had carte blanche to do what we wanted, Kenneth Arrow would support it, and at Santa Fe we wouldn't have colleagues from the discipline asking why we were doing things differently.

In the end, our group decided to focus on the idea of nonequilibrium, and our overseeing committee of Ken, Phil, and David Pines backed this.

Thus began a long course of research that advanced over several years.† We saw the economy not as a system in an equilibrium steady state, with identical agents facing well-defined problems and using perfect deductive reasoning; but as a system always in process, always changing, with diverse agents trying to make sense of the situations they face using whatever reasoning they have at hand, and together creating outcomes they must react to anew. The resulting economy would not be a well-ordered machine, but an evolving ecology that was imperfect and perpetually constructing itself anew. This view gave us a world closer to that of political economy than to neoclassical theory, a world that is organic, evolutionary and historically-contingent. In a 1999 paper in *Science* I gave the new approach a name—complexity economics

Ken did not contribute directly himself, but crucially he oversaw the work and backed it and encouraged it. I remember being glum at the end of the first year in Santa Fe; we had no papers to show as yet. But Ken told me we had got further than the Cowles Foundation project had in its first year. A year or two later we had developed an artificial stock market on my NeXT machine, one of the very early agent-based models. It had an interface similar to that of real markets that showed an automatically updating price chart. Ken said little about it, but stared fascinated at the screen for a long time.

For me, the remarkable thing was that Kenneth Arrow was willing to back a project whose purpose was diametrically opposed to his own equilibrium thinking—his very creation. Ken had a curiosity that overcame any defensiveness. I've heard it suggested that Ken was just backing another horse in the race, albeit a long-odds one. Perhaps. But I believe his honesty and integrity trumped lesser motives. Ken was intellectually generous to a remarkable degree.

Ken could be impatient with people he thought were uninformed. But if you went to lunch with him in Santa Fe or Stanford you could question him, and you got wise answers back. At Stanford he had a continual stream of visitors, so many that wags in the department had placed a line of large arrows on the floor pointing to his door. I went upstairs in the early 1990s to see him one day. The door opened and

† For an account of the Santa Fe program's history see Fontana (2010).

standing next to Ken was a thoughtful-looking man. 'Brian', says Ken, 'this is Rudy Kalman'. I had worked on Kalman filters and was astonished to meet the man himself. 'Oh', said I, 'Kalman the engineer?' 'No', says Kalman, 'Kalman the mathematician'. Ken, ever kind, betrayed no smile.

## References

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