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An Anthropological Problem, A Complex Solution

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This article means to introduce complexity science, more specifically agent-based models, to an anthropological audience. It does so by laying out a research problem that has concerned the author for some time—how can illicit drug epidemics be explained? Traditional social research is simply not adequate to the task. After introducing the newer framework of agent-based modeling, the author argues that both complexity and anthropology focus on "a narrative of connections and contingencies." With an example of an agent-based model from his own work, the author shows the promising synergies between complexity and anthropological theory and practice.

Key words: complexity, agent-based models, substance use, epidemiology

Let me begin—as I will later end—by posing a problem that Heather Reisinger and I have been working on for the last several years. It is the kind of problem in explanation that will be familiar to any anthropologist. Illicit drug epidemics happen all the time. They are "epidemics" because a fairly constant and low baseline rate of dependence increases dramatically over a brief period of time. Since World War II, serious *illegal* drug epidemics usually involve heroin, cocaine, or methamphetamine, "serious" in the sense that a close look at high-dependency sites will overwhelm you with visions of personal and community damage that no degree of cultural relativity can block.

Why do these epidemics happen? Sit with a roomful of drug experts and listen to the candidate reasons. Poverty, oppression, self-medication, psychopathology, dysfunctional family, anomie, receptor sites, availability, marketing, or—God forbid—fun, at least in the beginning. The problem is that none of these *variables*, as the experts call them, *causes* a particular epidemic in any straightforward way, not singly, nor in combination. Any or all of them may or may not play a role, a role that changes over time as an epidemic begins and ends, and they will connect with each other in webs of interaction that also change over time.

Each particular drug epidemic happens in a particular way. In each case that we have looked at so far, Reisinger and I wound up telling a story, showing how things fell into place, sometimes by chance, sometimes planned, sometimes things whose role only became clear with hindsight (see, for example, Agar 2003a; Agar and Reisinger 2002). We constructed a *narrative of contingencies and connections*. The phrase will be familiar to anthropological readers, if not always celebrated, because it also describes a style of ethnographic writing in the ascendancy since *Writing Culture* (Clifford and Marcus 1986).

We eventually turned more and more to historical sources, whether those histories were professional, popular, or embedded in archives or interviews. Historians seemed to be the only ones who described epidemics like we did. In fact, we began to think of ourselves as historians of a peculiar sort, a notion that Mintz (1985) used to describe his own work on sugar some time ago.

But we didn't want to end up with a disjointed collection of stories, each dealing only with the local and the particular. We also wanted to find a trail from epidemic narratives to a theory of epidemics in general. Couldn't we do that?

Not the way mainstream social science works, we couldn't. To briefly reinvent Newton versus Hegel, the linear causal models celebrated in traditional science don't work with history. They don't help much with the dimension of time, either, unless you're particularly fond of entropy. The historian Gaddis (2002) also worried about this problem. He wondered why he couldn't get much help from sociology. It was because of their endless quest for an "independent variable," he said. The problem with history was, there weren't any.

This problem—the conflict between abstract formal clarity and a specific local story—is an old one. I think it is fair to say that mainstream American anthropology has always favored the "specific local story" end of the spectrum, and for good reason. A representation that illuminates how the world works must tend in that direction. Reisinger and I certainly built such local-story representations in our work. But as we worked on those cases, we saw that some kinds of general patterns were replicating. Maybe they didn't look like Euclidean

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axioms or crisp hypotheses of quantifiable covariation, but *something* was recurring. Wasn't there a way to talk about the specific stories of contingency and connection and still come up with some general ideas of how epidemics worked?

This question landed us squarely in recent developments in a "different" kind of science, one that appeared under various monikers like "chaos" and "complexity." This science is different from old science in many of the same ways that our epidemic case studies were different from the usual epidemiologic report. Like our cases, this different science was about how a particular story of contingencies and connections could conclude with results that might on the one hand be surprising and on the other hand vary in development from one time to another.

"Chaos" and "complexity" are often confused with their natural language counterparts. "Chaos" is taken to mean complete disorder, when in fact it centers on how deterministic equations can produce results that appear random. "Complexity" is taken to mean complicated when actually it means that simple interactions among many different agents can produce emergent order.

I do not intend to review the entire field, impossible in an article-length treatment at any rate, nor do I hope to describe antecedents of this field in the older systems theory and cybernetics, from which it differs in substantial ways. What I do hope to do is introduce a specific version of "chaos/complexity" that we found useful in our research on drug epidemics. That version is called "agent-based modeling." When I need to refer to the entire field, I'll use the term "complexity." When I'm talking about agent-based modeling, I'll use the common abbreviation "ABM."

What is an Agent-Based Model?

Let's jump right in with some examples. An early and simple model was designed to explain how birds flock (Reynolds 1987). You can see a version of the model for yourself by downloading the accessible programming language Netlogo (Wilensky 1999) and selecting his "flocking" model under File > Models Library > Biology.

Why do birds flock? Usually people answer with the concept of a "leader"—Big Bird, so to speak. Others might come up with reasons like the earth's magnetic field, or meteorological patterns, or terrain contours.

The flocking model shows that you don't need any of those to get flocks. What you need are birds that follow a few simple rules—don't get too close to the nearest birds, aim more or less at the center of the flock, and move with the same direction and speed. Take a bunch of birds with those rules, throw them into the air, and eventually they form flocks.

Notice several things here that sound familiar to anthropological readers. First, structure *emerges* from agency over time. Second, it emerges in different ways at different times, depending on *contingencies* that start with that first toss of the birds into the air. Third, webs of connections form such that several birds are mutually influencing each other. And finally, if you run the model several times, flocks will take different shapes in the end; indeed, flock shapes will change even during a particular run. But...you usually get flocks.

For a more socially familiar example, consider the work of Thomas Schelling (1971) on segregated housing patterns. Schelling worked before the boom of the 1980s and is now considered a neglected pioneer. This model, called "segregation," is also available from the Netlogo download under File > Models Library > Social Science.

In the Schelling model, instead of birds, you toss a bunch of red and green critters into a checkerboard-like space, one critter to a square. Each critter has eight neighboring squares—above, upper right corner, right side, lower right corner, below, and so on. Each critter wants a certain percentage of its own color on surrounding squares. It looks around, and if it has that certain percentage or more, it stays put. If it doesn't have that percentage, it is "unhappy" and moves to a different empty square. The critters jump around until they are all happy.

What percentage of same-colored critters does each have to want before you get segregated clusters in the end? Most people think you need racist critters, ones who want most, maybe all, neighboring squares of the same color. If you play with the model later, you'll see that at high percentages, critters actually keep moving around forever. It's hard to get happy if you're a racist in a diverse world.

But it will surprise you—it did me—to see that an initial value of 30 percent *still* generates segregated clusters of critters. Even critters with substantial openness to diverse neighborhoods will wind up living with mostly like-colored neighbors.

Again notice the characteristics of the model. Just like the flocks of birds—structure emerges from agency. Contingencies shape what happens over time. Connections produce mutual influence as they develop but also change. And the final shape of the neighborhoods looks different from time to time. But segregation results, even for pretty open-minded critters.

This ABM business is a kind of science whose models have a familiar anthropological ring. They certainly rang familiar when I discovered the complexity literature with the problem of explaining illicit drug epidemics in mind. Lots of connections and interactions—*nonlinearity*. Local events that can turn out to have massive consequences—*contingency*. Changes through time—*dynamic*. Some of the things we wanted to explain in our work with drug epidemics had just these characteristics.

As I learned more over the last several years, I have come to think that anthropology and complexity can converse in ways beyond the problem of figuring out illicit drug epidemics. A small number of colleagues agree (see, for example, Fischer 1994; Lansing 1991; White and Houseman 2002). In fact, there is compatibility between this "different" style of science and anthropological research and application that, in my mind, foreshadows a powerful synergy that has only just begun.

Of course no ABM is as rich as the original problem. There is more to flocks than birds keeping their distance, and there is more to race than a percentage of how many like-colored agents another wants around it. The point isn't to claim agent-based modeling is an adequate anthropological representation. That would be ridiculous. The point is to show that key arguments, distilled out of the results of anthropological research, might gain power through modeling with an ABM.

As I worked on this article I came up with the phrase I used earlier—"a narrative of contingencies and connections"—to highlight the relationship between complexity and anthropology. Whatever the mix of humanities and sciences that go into a reader's version of anthropology, that phrase captures a foundation stone of the elusive anthropological perspective that we all talk about. The phrase also captures a focus of this different kind of science—multiple interactions, nonlinearity, contingency, dynamics. I believe that these two—anthropology and complexity—have much to offer each other, from epistemology down to methodological detail. Anthropologists so far have been slow to notice. My hope is that this article gets a few more interested.

Prediction

Any anthropologist of a certain age will be skeptical of yet another call for an imported formalism as a solution to our age-old problems. Applied anthropologists are likely to be even more skeptical, since not only have they watched the formalism come and go; they have also seen that it didn't work. Is there any evidence that claims for complexity aren't just another in the eternal cycle of broken promises?

I think there are several kinds of evidence. Given limits on space here, let me offer one example that tangles with a core concept of research *and* application—prediction.

Agent-based models complicate the age-old research goal of prediction in ways that reflect anthropological discomfort with the notion in the first place. In traditional science, prediction is the pot of gold at the end of the rainbow. All that work to carve out independent and dependent variables was a means to the end of predicting the latter from the former. The holy grail of the Newtonian tradition, the dream of Laplace—tell me the measurements of all the independent variables and I'll tell you how the universe will run forever.

Attempts to predict the economy or the weather, to take two popular examples, litter the landscape with a number of classic models that have never worked reliably. In fact, Sherden wrote a book with the provocative title *The Fortune Sellers* (1998). He describes how self-proclaimed LaPlaces sell their ability to forecast the future, in the markets and in many other domains. They continue to earn good money despite of their record of failures. The public needs its illusion of certainty about the future and they need to pay the rent. Malinowski's theory of magic lives and generates a profit.

Hobsbawm (1997), a historian long admired by anthropologists, wonders in a recent book why most of the world-shaking events of the 20th century were *unexpected*. The same is true of the illicit drug epidemics Reisinger and I have been looking at. Once the use of heroin or crack or methamphetamine reaches a tipping point that propels it into public discourse, one of the first questions the public asks is: "Where did *that* come from?" No one answers that question in any satisfactory way. There are no simple independent variables.

Prediction isn't dead. But when it comes to complexity, it's been reduced to a supporting role instead of star billing. Prediction in an ABM now means several different things, most of which are more modest, but more interesting, than the traditional notion implied.

For instance, massive changes in the global or local environment still work as potential linear causes. Consider famine as a powerful macro- and microcause. If blight wipes out a country's main crop, a lot of people will starve and a lot of others will emigrate. And if people are starving, they aren't going to feel much moral revulsion at the thought of stealing food. Those are two of the more tragically safe linear causal predictions available, the kind old-fashioned science celebrates.

Massive changes still predict. And short-range prediction has a reasonable chance of succeeding. Weather is a good example: generally weather forecasters get the next day right. Not always, but usually. After that, good luck, with more luck needed the farther you go into the future. In fact, statistical sources I can no longer remember argue that there are two safe short-term predictions to make about the weather or most anything else, "safe" in the sense that odds are you'll be right and win the bet. The first one is what happens tomorrow will be pretty much like what happened today. The second is if something out of the ordinary happened today it will probably be more like the ordinary tomorrow. In other words, in the very short range things probably won't change too much. As we'll see in a moment, you won't always win the bet, and sometimes you'll lose in spectacular fashion. But whether you win or lose, extend the time range out and you'll know less and less about how things will go. This is like old-fashioned prediction, kind of, but it won't work very far into the future.

We can talk about predicting in terms of massive environmental effects and short-range trends. But neither satisfies the usual call for prediction, the LaPlacian dream, to predict the future indefinitely based on measurements of the right independent variables. Neither tells us anything about how some corner of the world actually works. And neither solves the problem of that grand old man Hobsbawm, that most everything of global consequence in 20th century history was unexpected.

Complex systems can produce unpredictable results. This is old anthropological news. As I say to my complexity colleagues, anthropology talked about "emergence" before "emergence" was cool. Predict all you like if it will give you some Malinowskian magical reassurance. But as the old joke goes, you want to make God laugh? Tell him *your* plans.

Maybe "prediction" would be more interesting if we expanded what it meant in a complexity/ABM sort of way. Even the most rabid social constructionist will admit that if you understand how a complex system works, you can rule out at least *some* future possibilities. The systems don't just move randomly through time. They change by *self-(re)organizing*. Their shape at Time 2 is limited—not determined, limited—by the possibilities in their shape at Time 1. We know that complexity means change, but not in the same way as old-style longitudinal research to find out what X predicts for Y in the future. Complexity means change, but it also means *surprise within a range*.

Change Within a Range

This more refined sense of prediction makes anthropological sense and opens up an interesting way to think about change. Here, I'd like to mention some research by people outside of anthropology. I do so in part because they are closest to the approach to change that Reisinger and I developed in our work with illicit drug epidemics. But I also want to show anthropological readers how the complexity-ethnography link has been noticed outside our own field.

Consider Elisabeth Wood (2003), a political scientist who did ethnographic research in El Salvador. She asked how it happened that ordinary Salvadorans went from decades of passive acceptance to active insurgency in a short time. Or consider Margherita Russo (2000), an economist who did ethnography in Italy. She wondered why an innovative technical process for manufacturing tiles rose to prominence but then faded so quickly from sight. And consider how well those questions fit Reisinger's and my research project. Why do incidence curves in illicit drug epidemics turn skyward and accelerate rapidly, then flatten out?

Such questions have to do with rapid and surprising change, not with a system in equilibrium. The questions are inspired by the observation of an end result that wasn't expected—a revolution happened, a product disappeared, a lot of people used a new drug. The research question that followed from the observation then asks about the processes that produced those results.

Notice that the question—what processes produced this outcome?—could in principle be asked about anything. But the studies mentioned here, including my own, take us back to the introduction of this article, to the examples of flocking birds and house-seeking critters. Just as with those earlier examples, the outcome is systemic, but the process is made up of local-agent interactions—contingencies and connections that change over time. And notice that the focus is on an outcome that was abrupt and dramatic, the opposite end of the scale from the traditional social research bias for the stable, the fixed, the static. This shift to rapid and surprising change responds to Hobsbawm's question cited earlier: how is it that most of the significant events of the 20th century were a surprise?

The next step, then, in this style of complexity/social research: Wood, Russo, Reisinger and I all looked for the beginning of the story. And where is the beginning? The beginning begins when the process that will lead to the outcome first appears as a change—when Salvadoran workers started to organize, when an inventor created a new way to make tiles, when people first used a new drug. In the jargon of complexity, we look for the beginning of a *phase transition*, the time when something began to change into something else. The term "phase transition" has its origins in clear-cut physical processes, like when ice changes to water changes to steam.

And how did the process bring about the phase transition? The answer will be a *narrative of connections and contingencies through time*, that phrase which I think links anthropology and complexity in such a compelling fashion. In fact, during a phase transition, a system is at a place where a high degree of disorder allows connections and contingencies to play their most powerful and formative role. Dramatic changes become both possible and likely.

How do researchers analyze what happened? They go after a story—a narrative, complete with complication, development, and resolution. "Phase transition" and "narrative" stand in a metaphorical relationship. Researchers build narratives out of data of different types. They don't always know what will be useful until they find it, and what they find will change what the story becomes and what they look for next. That description will sound familiar to anthropologists and historians, among others. The style of research into dramatic change described here lands you in ethnography, ethnography as a kind of research logic.

A second way that ABM leads you down the ethnographic path: since the story involves human consciousness and action, material has to get the researcher "inside the event," to use Collingwood's (1946) famous phrase. Gaddis fits the bill as well when he talks about the importance of empathy in historical research. Any resemblance to our useful concept of emic is purely intentional. The story of a phase transition, like any story, represents several different points of view.

So one builds a story of contingencies and connections, from the beginning of a phase transition through to the observed system outcome that motivated the research in the first place. The story lays out a "path" that traverses time. Echoes of Frost's poem, "The Road Not Taken," not to mention recent films like *Run Lola Run*. Why did the story unfold in a certain way? It depended on a web of connections changing over time in surprising and unexpected ways.

Agent-Based Models Again

But does this mean that a story could have unfolded in an infinite number of ways? Was it all completely unpredictable? No. As we saw in the earlier discussion of prediction, no single outcome is predictable, but not all outcomes are possible, either. Here is where I think things get really interesting in the realm of agent-based models. Before we can explore this space between certainty and uncertainty, though, we must ask two questions of any example of this kind of social research.

The first question involves *simplification*. In the story we have constructed, there must be just a few key contingencies. Can you briefly summarize what you found out for a colleague in conversation, or could you write a brief abstract that summarizes the work? If you can boil it down to—in this kind of a world, with these critical connections and contingencies, the following result emerged—then you can move to the next question.

The second question is about *numbers*. Since the next step will be computational, propositions must be converted into numerical form. Note that this is not a question of measurement in the traditional sense of the term. Rather, it is more a question of translation. In my own work, numerical translations were sometimes easy to find. In other cases the translation was arbitrary (see Agar 2003b for more discussion).

If a project can answer these two questions—not all projects can or should—then it can experiment with an agent-based model. At the beginning of this article, two such models were presented: flocking birds and segregated critters. Those models put several agents, who have some characteristics and who do a few things, into a world. The agents and the world interact with each other over time and both change as a result. Note that these are *not* models that try to *resemble* a world, like a flight simulator. Instead, they are thought experiments, laboratories for testing an argument that a few key connections and contingencies, left to work themselves out over time, produce an emergent result like the one you wanted to explain. You build it, turn it on, let it run, and see what it does.

What do you get besides a video game?

First, you test the story you already built. Does your model of contingencies and connections produce the same emergent property you went after in your research? It probably will, but it will yield more than that. Because of the connections and contingencies, the model will not produce the same story every time you run it. This range of stories will have its limits. More than one story will be possible, but not all possible stories will occur. In the jargon, this is called the "phase space" of the model, the picture of the possible outcomes given the initial settings. Another term for this picture of possibilities is the "attractor."

Next you can change the initial connections and contingencies and run the thing again to see how the new "initial conditions" affect the stories that result. Will the stories change? Probably. The entire phase space will shift. There will be a different range of possible results that emerge.

One possible shape a phase space might take can be pictured as a curve that represents an "inverse power law" (Barabasi 2002; Buchanan 2000). What this means, in English, is that small outcomes will occur a lot and major outcomes will only occur once in awhile: a few large cities and a lot of small towns, to take one example; a few monster companies and a lot of small ones, to offer another. And inverse power laws also describe many nonhuman phenomena as well: a few big earthquakes and a lot of small ones, for instance.

The fact that inverse power laws show up all the time is one of the discoveries of complexity. Traditional social science features the normal curve as the way to describe distributions of most things in the world. It turns out that an inverse power law better describes variations in what a complex system produces. In fact, the inverse power law calls attention to missing pieces in Reisinger's and my work on illicit drug trends. Our cases are all major "earthquakes." Where are all the little ones?

This use of complexity and ABMs is, I think, an extremely powerful postethnographic exercise. One limit of ethnography has always been the single case study. Now, with a model that is compatible with our epistemology, we can generate additional examples for our key conclusions. The examples will show: 1) that our single case explanation was a plausible one; 2) that the same explanation might produce other outcomes, but not all imaginable outcomes; and 3) that in a different kind of world a different range of stories will occur.

This is prediction of a different type, a type that makes sense given how we work. We cannot predict that X causes Y. But we can predict that in a particular kind of world, a particular range of outcomes will occur.

Let me give an example of an agent-based model that I've written about elsewhere (Agar and Wilson 2002). In our work with heroin-using youth in Baltimore County, Maryland, Reisinger and I concluded that narratives of experience were the most important source of information about a new drug. So perhaps the dynamic that drove local diffusion was mainly the "news" that one heard by word-of-mouth. Dozens of studies have been done over the years that show how first use of an illicit drug usually happens among friends.

We wondered if the flow of these narratives could produce an epidemic incidence curve. Narratives that users told us were both good and bad, the balance shifting from the former to the latter as time went on. Couldn't the appearance of a new and interesting drug, like heroin, spark interest that drives the curve up and then, with time, generate some bad news that would break the curve and flatten it out? Couldn't narrative flow among agents give us the incidence curve?

Recall the birds and critters. Like the birds, our agents attend to the "buzz" right around them, the stories about a drug, but they also communicate with the members of their social network, who will probably be somewhere else in the model, having different kinds of experiences. What are they all telling each other?

They are telling each other about what has happened to them and what they've heard about the new drug. The game is loaded, based on what the youth taught us, toward good news about a drug at the beginning with bad news becoming more likely as time goes on and "addicts" appear around them. The way the model works can be changed for different drugs, and recall that we're dealing with drugs that have shown this dynamic historically, like heroin and crack. There are a lot of other details required for a full description of the program, and these are available elsewhere (Agar n.d.). For present purposes, it is enough to have the general idea and to know that this model is in the family of models shown in the introduction—the birds and the critters.

If you turn the model on and let it run, with a pretty good drug and with the agents' attitudes set in the beginning so that the world isn't full of antidrug crusaders, you get the classic S-curve characteristic of an epidemic. Often, but far from always, the curve takes off, shoots up, and then flattens out. But the curves for that same model, if you run it many times, show variation. Now and again nothing dramatic happens. Now and again you get a monster.

Notice several earlier themes appearing here. In ABM terms, the agents self-organize into an epidemic. To use the term beloved by both complexity and anthropology, the epidemic is an *emergent* property of the system. Many agents do different things locally. Some of these local things could in fact be predicted on a step-by-step basis. If an agent has a strong antidrug attitude, and if it isn't likely to take any risks, then it won't try a drug right now. But as the agents wander around their world, their attitudes change.

The bigger picture for that world—the incidence curve for the population as a whole—changes as well. A world without the drug becomes a world with dependent users in it. The dependent agents aren't necessarily the ones you would have predicted in the beginning, and some agents you would have bet would be dependent turn up clean in the end. The different paths they traveled through connections and contingencies varied from agent to agent and from time to time.

I hope this sketch of an agent-based model shows that it can test an anthropological explanation and show how agency and structure mutually produce each other. The aim of the model blends in, at least at a general level, with the goals of theorists like Bourdieu and Giddens. In the words of two modeling pioneers, if you think you understand how a system-level characteristic came about through agent interactions, you ought to be able to make an agent-based model that "grows" that characteristic. Their models, they say, are neither deductive nor inductive, they are generative (Epstein and Axell 1996).

And I hope this sketch suggests practical applications of such a model. With repeated runs, one sees the variety of ways an epidemic develops. Increases and decreases in "at-risk" agents, increases in agents who have "ever-used," and increases in "dependent" agents—the graphs change their trajectories over time from one run to another. With good information about ongoing use patterns, a community could more effectively shift resources among prevention, early intervention, and treatment to suit particular epidemics as they developed, each in their own way, but all within the same range of possibilities. Samples of runs of this model and demonstrations of how differences suggest varying intervention strategies are available elsewhere (Agar n.d.).

Conclusion

Applied anthropologists work in worlds where clients want plans that will unfold as expected. Never mind that anyone with more than an hour's experience knows things never work out that way. Instead of adapting to pathological needs for certainty, why don't we play to our strengths and offer the concept of *plans based on connections and contingencies over time*? The time might be right, in part due to complexity's diffusing influence in the organizational literature (see, for example, Stacey 2001; Weick and Sutcliffe 2001). Such an approach would identify *likely* contingencies and connections and keep watch for unexpected ones. Rather than some program evaluation and review technique (PERT) chart, such plans focus on knowing the complex system before, during, and after implementation, the competence that applied anthropology claims as its expertise.

A second major implication: I've started giving talks about ethnographic research itself as a complex adaptive system. Contingencies and connections, changing over time, lie at the heart of the story of how any study is done. Here is one way to translate this simple observation into the language of this alternative science. One can describe a complex system by its "algorithmic complexity." Informally, all that means is that a system is as complex as the program you have to write to produce its results. For ethnography, the story of the method is pretty much also the story of the study, because so much of any given study depends on the contingencies and connections among so many different things as it develops. Algorithmic complexity is high. This is a major reason why anthropologists have trouble with the concept of a separate methods section in proposals before or articles after their work, and justifiably so. Study and method are inseparable and unknown until after they are done.

For applied work, then, this "alternative science" is helpful, because it describes what we can do and how we can do it more effectively than any previous epistemological framework. It helps—me, at least—better define our niche in a complicated ecology of organizations and ideologies, something I've been motivated to do since I became a "non-academic" several years ago. What we get from complexity, I think, is an ability to better articulate just how rich our approach is and what its uses are. And we acquire a powerful transdisciplinary framework to back up our practices.

Coda

In the end, I wouldn't have written this article if I didn't want to encourage readers to explore complexity in general and agent-based modeling in particular. This paradigm—I think it might actually be a new one for Western science in the Kuhnian sense—is compatible with and supportive of our tradition, transdisciplinary in orientation, and stimulating for both theory and application. It deserves a look. Mitchell Waldrop (1992), a science writer, tells the story of the development of the Santa Fe Institute in his book *Complexity*. Though a bit dated now, it is commonly cited by social researchers, and many others, as the source of their interest in the field. And in the introduction to this article I cited the

Netlogo programming language (Wilensky 1999). It is free for the asking, contains good tutorials and sample models, and was designed to be accessible. Some readers may have learned geometry in grammar school with the logo "turtle." Netlogo is what happened when the turtle grew up, went to college, and turned into an agent.

Two next steps for an interested reader might be recent review articles in the *Annual Review—of Anthropology* and *of Sociology*. Lansing (2003), cited earlier for his work on Balinese irrigation systems, writes an overview for anthropology grounded in his perspective as an ecological anthropologist. He emphasizes the link between emergence and adaptation, offering more technical—but clearly presented—background on the development of models in the complexity field. Part of his review, given the framework I've used here, could be titled "connections and contingencies: step 2," so in that sense he develops some of the perspective presented here. He also provides additional examples of complexity from economics, game theory, and anthropology.

The overview from the *Annual Review of Sociology* reveals a different angle in its title, "From Factors to Actors" (Macy and Willer 2002). Like this article, they begin with flocks of birds. And like this article, they emphasize the value of models that show how system characteristics emerge from agent interactions. As sociologists often do, they generate a typology of research, lay the types out in a chart, and then review several studies. The topics should sound familiar, though—social influence, diffusion of innovation, homophily, bandwagons, and the like. Their review, like Lansing's overview, is worth reading.

From there interested readers of this article can set out on their own. My own recommendation, given our ethnographic penchant to dive into a world in its own language, is to advocate Netlogo with its clear tutorials and abundant models. Recently I've introduced the agent-based modeling concept, using Netlogo, to a variety of audiences—graduate students in architecture, qualitative health researchers, and educational ethnographers. Audience response and subsequent emails indicate that Netlogo is a powerful hands-on introduction that gives a feel for what can and can't be done. After a little experience, readings become more coherent and capable of evaluation. So for my money, play with Netlogo first.

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