We Have Met the Other and We're All Nonlinear: Ethnography as a Nonlinear Dynamic System

Ethnography as a Nonlinear Dynamic System

MICHAEL AGAR

Michael Agar is with the Friends Social Research Center and Ethknoworks, 1040 Park Avenue, Suite 103, Baltimore, MD 21201. For additional writing on ethnography and complexity, see forthcoming articles in Human Organization *and* Journal of Artificial Societies and Social Stimulation.

"Ethnography" and "Social Science" have always had an awkward relationship. As far as research practice goes, ethnographers often feel that they have more in common with investigative reporters, historians, and intelligence analysts than they do with colleagues in sociology and economics. The usual social research chant of "theory/hypothesis/measurement/sampling design/significance level" does not describe much of what ethnographers do.

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For an ethnographer, what's interesting is the discovery of connections. A holistic perspective is foundational for anthropology, sort of a proto-systems theory, which helps explain why two anthropologists, Gregory Bateson and Margaret Mead, participated in the post-World War II conference that founded cybernetics.

It's not that ordinary social science isn't allowed to look for connections. It's that ordinary social science begins with variables already given by some theory, and then tries to figure out how to locate, decontextualize, and measure those variables. A card-carrying holist notices a "variable" in a situation, maybe one that he/she had never thought about before, but then he/she wonders what other things it might be connected with, in that situation and outside of it. The goal is to build patterns of many interacting things that *include* what was noticed, not to *isolate* what one was *supposed* to notice and *measure* it.

It's a problem that many students from traditional social science have discovered for themselves. I wish I had a nickel for every time this happened in my teaching days: A student from outside of anthropology walks in, looks around nervously, and says in a

Correspondence to: Michael Agar, 1040 Park Ave. #103, Baltimore, MD 21201, E-mail: magar@anth.umd.edu

quiet voice. "I'm working on my dissertation and I've got a problem. I'm supposed to do a linear regression to explain this dependent variable. But…"

And with that the student pulls out a piece of paper with stains made of equal parts of tears and pizza grease. A diagram shows a large number of tiny boxes. The boxes are filled with all manner of things that the student knows are relevant to what he or she is trying to understand. Arrows fill the spaces between the boxes, It looks like spiders of different sizes pressed flat on the paper. "Can you help me with this?" asks the student.

"No," I say, or learned to say with experience. I tell them to go back to their department and never talk to anyone like me again until their dissertation is signed. They will just make a mess of their exams and annoy their faculty, and someone like me as one outsider among five examiners won't be able to prevent the slaughter. Writing a dissertation is as much, if not more, a political act as it is a scholarly act. Ask Foucault. But their goal—the complicated picture of pattern, the many links among many different things—is exactly what an ethnographer aspires to.

Understanding how the social world works is poorly served by traditional social research approaches. Truth-in-packaging compels me to say that counterexamples to this argument exist and should be consulted, like Russ Bernard's overview of anthropological research with just this mainstream social science framework in the foreground [1]. And toward the end of my own methodology book I show the relevance of traditional methods when built on ethnographic findings in a "grounded theory" sort of way [2].

But still, theory-driven linear causal models that equate prediction and explanation just aren't the flags most of us fly under. That leaves the problem of designing some new flags. Along with many other colleagues, I've tried to do this over the years. My flags went by names such as rich points and abductive logic and

massive overdetermination of pattern [3], concepts that I'll draw from in mercifully brief fashion later in this article. For now, I'd like to see if I can carry the banner a little further by looking at a system of ideas that ethnographers are now beginning to consider.

During the 1980s and 1990s a new research framework grew at an increasing rate under the popular—and often misinterpreted—monikers of "chaos" [4] and "complexity" [5]. The growth was driven by a problem shared by researchers of all kinds—how are we to understand systems with so many interacting elements that it's hard to say what they will look like in the future? This growth was enabled by advances in computer technology, because simulation is the only way to model and explore such systems. We rapidly approach the time when you can pick up enough computer power to run such models at the convenience store.

This comparatively new framework— new for science, not for Buddhists—intrigues people like me and, presumably, readers of this journal. Gaddis [6] recently explored the framework for history, noting that social science has always troubled him because he can't find any of the "independent" variables they all seem to insist on. If the venerable *Annual Reviews* are any indicator, then the framework has arrived with recent review articles for both sociology [7] and anthropology [8]. And from within the framework itself, studies of "artificial societies" [9] and reports on them in the *Journal of Artificial Societies and Social Simulation* already have a pedigree of several years' duration.

In a landmark event that placed this framework squarely in the middle of public discourse, Wolfram published a book called *A New Kind of Science* [10]. This event raised the stakes, because now we (the public) were talking about a paradigm shift in the classical Kuhnian sense—not just a little conceptual tinkering. I say "public" as reflected in such places as mainstream book reviews, congressional testimony, and

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featured NSF and NIH presentations. No, this was claimed to be the real thing, a shift in the epistemological tectonic plates by which the powers that be define what "science" in fact is.

What's especially interesting about all this, for an ethnographer, is how familiar this new epistemology sounds. Connections, the missing piece from traditional science, in fact turn into the point of it all. "Emergence," to take another example, is a core concept, the observation that a lot of interacting elements can self-organize into surprising system results. As I enjoy saying to my colleagues in this new field, "anthropology said emergence before emergence was cool." The notion that most things one is trying to figure out aren't just a matter of a few variables causing another to act in a certain way— it's not a surprise if you do ethnography.

Let me begin referring to this "new kind of science" with the standard phrase "complex adaptive systems," abbreviated "CAS" for ease of writing. I like this phrase rather than the many other descriptive terms for the field because it summarizes the basics, with one caveat. In social research, "adaptive" has problematic connotations. A blind commitment to adaptive may well miss the point of what is going on, and many of the social systems I deal with are better described as "maladaptive." Such are the dangers of cross-disciplinary metaphors. In more precise terms, adaptive just means that both sides of the structure/agency opposition must be taken into account in analysis and explanation. On this both ethnographers and CAS can agree.

Many are the ways to think about the relationship between ethnography and CAS. For awhile now I've been arguing that ethnography, in this day and age, actually labels a kind of logic rather

than a unit of study. Waldrop [5], in his overview of complexity, offers a tantalizing description of CAS as a kind of "qualitative holistic math." There are interesting *epistemological* parallels to explore here. In fact, if you take CAS seriously and want to do *social* research, ethnographic logic is where you have to go. One sketch of that logic will be included later in this article.

A second way to explore the relationships between ethnography and CAS lies in the question of just what it is we produce at the end of a study. The old "domain plus detail" model of traditional ethnography was shredded long ago. No more "Chapter III: Kinship: Part I: Mother's Brother." Modern ethnography and CAS agree that what they're after are ways to describe systems that mix order and disorder, systems that move and change, sometimes in small ways that react to circumstance, sometimes in major ways that change the nature of what it means to be a participant. There are interesting *representational* issues here, and a few will be described at the end of this article.

A third way to explore the relationship—the way I will emphasize here—involves how the *research process* itself mirrors the epistemology and the representation. In other words, ethnographic research is, in and of itself, a complex adaptive system. The process involves an ethnographer, at least one, and different people that he/she spends time with, and in this day and age lots of information from other sources as well. The process begins in comparative disorder, shifts and changes through time, and typically winds up with conclusions that were not expected at the beginning.

Is this an interesting way to think about ethnography? I'd like to explore the question here by looking at a few key concepts from CAS to see if they clarify issues for ethnography and suggest new ways of thinking about those issues. Let's begin with the question of whether or not ethnography is an CAS at all.

ETHNOGRAPHIC COMPLEXITY

On the face of it ethnography is obviously a CAS. Several measures have been proposed to diagnose how "com-plex" a system is. The one that makes most sense here is "algorithmic complexity." An algorithm is just a set of procedures for doing something. So, one measures algorithmic complexity by answering a question: "Is there an algorithm to produce the expression of interest that is simpler than the expression itself? How much simpler is it?"

One informal example is the contrast between paint-by-numbers and a Jackson Pollock painting. Paint-by-numbers is simple by comparison. Pollock in an interview was asked what he did about "mistakes" and he just laughed. You can't have mistakes if you're not following an algorithm.

At the ridiculously simple end of the ethnographic scale, suppose you developed an algorithm that went like this: Say "hello how are you" to everyone within a one-mile radius of where you stand when the algorithm begins. Record the answers. When no one is left you haven't said hello to, stop. Publish a list of the answers in the *American Anthropologist*. This looks like a Monty Python version of Newton and Locke. Algorithmic complexity is extremely low. But what the algorithm produces is certainly not any kind of ethnography.

Now consider something from the ridiculously complex end of the scale. Well, I can't write it, because the complex end means that the algorithm would be as long as the study. In other words, every moment of the study would call for procedures unlike anything that had been done before. It would be a social constructionist fantasy, every moment built up in a unique way. No pattern, no repetition, no algorithm. The description of a study would be as long as the study itself. Algorithmic complexity would be extremely high. This is not any kind of ethnography, either. Maybe Hunter Thompson aims in this direction, at least in the old days as he swallowed a handful of pills before describing a scene, but not ethnography.

The algorithmic complexity of an ethnographic study will be between these two extremes. But the point here isn't to measure ethnographic complexity in a precise way. The point is to show

how different it is when compared to traditional social research. An ethnography will *always* be higher in algorithmic complexity. When you study methodology in traditional social science, what do you learn? You learn some algorithms. The idea is, you can implement those algorithms to do research in any number of areas.

So you learn how to design and validate survey instruments, for instance. You take statistics courses that nowadays are concepts plus computer packages. You take courses that teach you the difference between a true random and a stratified sampling design. You learn these algorithms so that you can execute them in sequence to do a full study. It's not very nonlinear or dynamic, but then it's not supposed to be. It's supposed to be linear and predictable and, above all, *controlled*. Take a research problem, plug in the algorithmic modules in the right sequence, and bingo, it's a study. Doesn't matter who you are or when or where you do it, as long as you follow the algorithms.

Traditional social research is *lower* on the algorithmic complexity scale compared with ethnography. An ethnographer of course also learns some algorithms in graduate school. And some, like the algorithms learned by more traditionally trained social scientists, will have to do with things like asking questions, and figuring out whom to talk with next, and comparing different kinds of data by way of analysis. The ethnographic versions will be much more varied and loose, though.

And our ethnographer-to-be will also learn a meta-lesson. ("Anything you can do I can do meta," as a colleague is fond of saying). The meta-lesson says, learn as many algorithms as you can, but understand that you won't know which ones will apply, at what point in the study, in what kind of combination, until you're actually doing the study itself.

As if that weren't bad enough, the student will absorb a second meta-lesson. That lesson teaches that issues considered "methodological" will come up *during* the study. They will lead you to ways of learning and documenting that you had no idea existed when you first *started* the study. You will learn how to ask the right question of the right people in the right way using knowledge you didn't know existed. You will see that certain kinds of data belong together in ways that you would never have imagined until you'd worked on the study for awhile.

No ethnography will lie at the *maximum* algorithmic complexity end of the scale. It would actually be funny to write a parody that represented such a study, something like an extreme *Being There*, the novel by Jerzy Koszinski. But any ethnography will be *more* complex, algorithmically speaking, than any study done with traditional social science methods.

There are a couple of conclusions to foreground here. The first is, algorithmic complexity helps understand why the concept of "methodology" was and is so slow to develop in anthropology. "Methodology," as understood in social science in particular, science in general, is all about neatly compressed algorithms that work to generate data and analysis, whatever the problem, whatever the context. But the idea of a "methodology" section—algorithms that generate a study—is anathema to ethnographers. They know that a study always develops, methodologically speaking, in ways unforeseen at the beginning. In fact, "methodology," many would argue, is something best told in the course of telling the study, not as a separate section. The story of methodology *is* the story of the study. This is a reasonable argument given a relatively high degree of algorithmic complexity.

A second conclusion: Traditional social science methodology lies *low* on the algorithmic complexity end of the scale. Things like nonlinearity and researcher influence and path-dependence are problems to be eliminated, not features to be celebrated. The problem here is that we've designed a research animal that doesn't have much evolutionary potential. One of the strengths of complex adaptive systems is that new circumstances can result in reorganization to better respond to those changes. Methods "evolve" as local information about how to do a study accumulates. Ethnography does this. Traditional research prohibits it.

So if we think of ethnography as a CAS, we get some understanding of why we look so different from other social sciences. And it turns out the differences are an advantage, assuming that a researcher wants to improve methodology with experience in the course of a study. With ethnography, he/she can maintain flexibility and creativity to adapt method to unforeseen circumstances, and he/she can reorganize methods to adapt to research problems the likes of which were unknown when the proposal was written or, for that matter, until well after the study was already underway.

Did we know all that already? Sure. Did we know how to say that already? Not very well. Did we have a transdisciplinary framework as a foundation for the knowing and saying? No.

SENSITIVITY TO INITIAL CONDITIONS

So say ethnography is a CAS, as indicated by its algorithmic complexity. Let me now move to one consequence, the famous "sensitivity to initial conditions." In popular discourse this is called the "butterfly effect," meaning that a butterfly flaps its wings in one part of the world and eventually the disturbance becomes a major storm in some distant location. The phrase "iron butterfly" comes to mind. The example is a little melodramatic, but it just means to show that a little difference in initial conditions, when you turn on a CAS, can make a big difference after it runs for awhile.

We need other dramatic phrases, like a "bureaucrat effect," meaning no matter how much energy you put into changing initial conditions you get the same result. And we need a "goes around, comes around" effect, meaning that you change initial conditions and get a result that is pretty much what you deserve. All these are possible CAS pro-cesses, the problem being you never know at the beginning which way it will turn out in the long run.

Sensitivity to initial conditions is a "signature," as they say, of a CAS. What does this have to do with ethnography?

A lot. Consider the problem of ethnography by different people on the "same" community. The problem is part of the early history of American anthropology. The famous Redfield-Lewis debate, for example, centered on whether villagers in a Mexican pueblo lived in a fundamentally harmonious or a conflict-ridden world. In fact, many of these debates—Benedict-Bennett and Mead-Freeman are other examples—pivot in large part on just this difference (For a review of the classic restudies issues, see Ref. 11). Was the village a place of self-interest and competition or a place of altruism and cooperation? Any "village" is of course both. And it is interesting that just this issue—competition vs. cooperation—is a centerpiece of recent CAS research as well, but that's another story [12].

The anthropological debate took more political shape after the 1960s. In this still ongoing dispute, the question takes a couple of different forms. One is, can an ethnographer who is *not* a Y ever do an adequate job of studying the Y, or can only a Y do such research? A second form is, given ethnographies of the A's that have always and only been done by B's, what might the B's have missed or misunderstood that would show up if ethnographies of the A's were done by C's?

There are enough writings and debates to fill an encyclopedia. One concern centers on identity issues: feminist, minority, gay/lesbian. Another centers on ethnographers from locations that have been traditional destinations for European and American ethnographers, the so-called "indigeneous ethnography." The question here is, "how have we been (mis)represented by ethnographers *not* of our identity, and how might we see things in a particular ethnography, of them or anyone else, that people *not* of our identity would miss?"

However the problem is phrased—science and/or politics—ethnographers have a long tradition of wondering *why*

two different studies should come up with different results. If we take seriously the idea of ethnography as a CAS, this "problem" should not be a surprise. In fact, it's not a "problem" in the sense that something has gone wrong that can be repaired. Instead, it's a normal part of a CAS—*sensitivity to initial conditions*. The idea that a study is a process that can have only one outcome is old science. The idea that a study can take different shapes depending on initial conditions is CAS. Two ethnographies, considered as CASs, may well produce different results, even if applied to the "same" pheonomenon.

What rescues us from epistemological anarchy is this: The fact that more than one ethnography of the X is possible doesn't mean that *any* imaginable ethnography of the X is acceptable. In fact, before I began my nonlinear readings, it was clear that a methodological problem for ethnography was figuring out the edge between credible and incredible, between studies (note the plural) that made a convincing case and other studies that shouldn't be believed.

To put it in CAS terms: For any given community, network, location, event, or whatever focus an ethnography might take, what does the "attractor space" of *possible* ethnographic results look like? If we could know that space, we could say in the end that given the constraints of an ethnographic research process, and given the different points of view that can be represented at that historical moment on the part of an ethnographer, here's the space that defines the acceptable ethnographies that might emerge. Not much of a clear solution, I'll admit, but an interesting way to better understand the problem, given that we think of ethnography as an CAS.

THE ETHNOGRAPHIC AGENT

Another way that CAS and ethnography link: Consider the thought experiment laboratory that CAS has spawned, *agent-based modeling* [13]. Such models put a number of autonomous agents into a simple world. The agents know some things, know how to do some things, and they and the world change as a result. When you take a look at the system that results from all those interactions over a period of time, the system will produce some characteristics that were not foreseen when the model started to run—the unexpected *emergent* properties.

Once again, this CAS framework will sound familiar to ethnographers. You, the ethnographer, aren't just *watching* the ethnography run. You are *in* the ethnography as well, part of the machinery that *makes* it run. Hence the oxymoron "participant observation." You are one, but only one, of the agents. As just described in the previous section, you are *part* of the initial conditions to which the CAS will be sensitive. But whatever the story of the ethnography will turn out to be, you are also an active agent in its construction.

For years social science has obsessed about the "Hawthorne effect," named after the research project conducted for Westinghouse by the Harvard Business School. When the researchers made changes in a wiring room, productivity went up. Funny thing was, it didn't matter what changes were made, even changes that undid a previous change. The reason productivity went up was because morale went up due to the attention given to the workers, mostly immigrant women. The method *produced* the results. Never mind that Heisenberg was figuring this out for physics at roughly the same time. The Hawthorne effect was an evil to be avoided in social research.

Ethnographers think—at least this one does—that if you believe you've eliminated the Hawthorne effect, you have probably smoked too much for breakfast. An ethnographer *has* to accept that he or she is *part of* the data, or, to return to the CAS version, that he or she is an agent in the simulation. Telling a story that you were part of makes more sense than telling a story and pretending you weren't there.

This can go to extremes. In one no doubt apocryphal account, a former positivist on a panel at an anthropology meeting said he became a born-again poststructuralist. He decided that he'd lean into the study and make his presence known. After about an hour of interviewing in this new mode, his interviewee interrupted him. The interviewee said, "You know, this is all very interesting, but can we talk about *me* for awhile?"

CAS offers an ironic combination of the poststructural and the scientific, a framework that accepts the heresy of researcher influence but then deals with it in a systematic fashion. Some of the consequences of this "distributed authority," as the computational types call it, haven't been well worked out yet for debates about "authority" in anthropology. But placing the ethnographic "agent" in the story does recognize that authority. On the other hand, because the ethnographer is only one among many, he/she is less significant overall than he/she probably thinks. In fact, during most of the ethnographic story, he/she probably has little if any authority at all. He/she certainly isn't responsible, alone, or even mostly, for emergent properties that result.

CAS brings agency and distributed authority and reflexivity into the equation, three issues that have driven ethnographic debate since the 1980s. An ethnographer is part of the study, and he/she and "the others" collectively create the story that will later be told as an ethnography. Different ethnographers will produce ethnographies that will differ from each other. That is to be celebrated because they can be compared to obtain a larger perspective than just one alone can provide. The fact that a Czech feminist critical theorist and a Nigerian masculinist methodological individualist differ in their ethnography of the White House does not mean that any ethnography of the White House is credible. But more than one is possible, and it is likely that those two ethnographers would prove the point in extremely interesting ways.

By the rules of old science, the Hawthorne effect is an embarrassing problem. By the rules of CAS, it is an aspect of an ethnography to be expected. It is a feature of a research process to be celebrated for its realism, flexibility, and adaptability, not something to try and conceal.

FRACTALS

Let's take a look at another age-old ethnographic process. I've used a frame-

work since the 1990s [2], one that describes a core process of ethnographic research. The process cycles as long as the study continues. It goes, in outline, like this: Ethnography can be described as translation writ large. It bridges the gap in understanding between the world of an audience that the ethnographer means to address, including him/herself, and the world that is being researched. Ethnography, in other words, is a construction that shows how social action in the context of one world can be understood as coherent from the point of view of another.

Much of what an ethnographer does is focus on the *differences* that he/she notices, what I have called "rich points." Rich points are moments when something doesn't make sense, ranging from a complete surprise to a departure from the expectations that the ethnographer brought with him/her. Such rich points signal differences between the two worlds and, as such, constitute the basic problem of an ethnographic study.

In pursuit of this problem in understanding, an ethnographer engages in a cyclical process, modifying and trying out frameworks that initially didn't work until they work so well across so many kinds of data that they become candidates for an ethnographic conclusion. The processes of modification and validation are too complicated to detail here, but the critical point for now is that they typically require several cycles. They are *iterative*.

And while in the middle of working on one rich point, another often comes up. What is an ethnographer to do? He/she now applies the same process in which he/she is currently engaged, only this time to a rich point that appeared as the process was ongoing. In other words, the cycle isn't only repeated over and over again. It is also applied within itself. The process is also *recursive*.

This description bears an astonishing resemblance to the idea of *fractals* in CAS (see Ref. 4. for the classic introduction). In its pure mathematical form, a fractal is a simple algorithm applied over and over again at different levels of scale. Fractals have been used to describe natural formations, such as

the development of the circulatory system or the growth of trees or the formation of mountains and coastlines. They have been applied in economics, as in the way stock prices squiggle in similar ways at different time scales. They have been applied to produce those interesting visual images that now adorn the sides of coffee mugs. And they form the basis of Wolfram's book, cited earlier, where simple algorithms, iteratively and recursively applied, generate patterns of amazing complexity, the basis of his "new kind of science."

Hold in your mind, for a moment, the idea that ethnographic research rests on a fractal-generating process. One rich point leads to another, more detailed, rich point. Things are not so neat as in the natural science versions, but I want to describe a second resemblance to fractals before dealing with that issue.

The second resemblance is actually a core concept of American cultural anthropology. One clear example of this core concept lies in the title of Ruth Benedict's classic book, *Patterns of Culture* [14]. Another example, one that I used long ago, is Morris Opler's idea of "themes" [15]. Numerous others exist.

What is this idea about? This is about differences between worlds that are so fundamental that they appear at many different levels of scale and across many different domains of experience. An example that I've used elsewhere [16]: Living and working in Austria over the years, I became fascinated with the concept of Schmaeh. I heard it and read it all the time and native speakers gave a bewildering variety of definitions, some saying it couldn't be defined at all.

Here is a gloss on what I eventually came up with: The term encodes a general principle of irony plus humor. Nothing is what it seems, what it actually is, is worse, and you might as well joke about it rather than getting upset. There's a joke I heard all the time, in various forms, with a particular punchline: The German says, "The situation is serious but not hopeless." The Austrian replies, "No, the situation is hopeless, but not serious."

What I'll call a "Schmaeh attitude" appears across many domains and at many

levels of scale. It can appear in a conversational moment, a family dispute, the media, public policy, or even be used as a label to characterize the entire city of Vienna. Once I worked out an outsider's understanding of "Schmaeh," many pieces of the Austrian puzzle fell into place.

Call it a "theme," call it a "cultural pattern," call it what you will. It acts like a fractal, in the sense of being an algorithm that applies iteratively and recursively to create patterns at different levels.

But are these fractals—methodological as discussed earlier and thematic as just described—are they really like the neat math that explains trees and blood veins and the stripes on Waldo the cat? They don't just reproduce identical patterns. But then that is common in nature as well. As the old saying goes, no two snowflakes are alike, yet the algorithms that produce them are the same, the variations being a product of contingencies and environment. "Context" we would call it.

But is the rich point cycle or the Schmaeh-like attitude really an algorithm? Neither is a "finite procedure, written in a fixed symbolic vocabulary, governed by precise instructions, moving in discrete steps 1, 2, 3,…, whose execution requires no insight, cleverness, intuitions, intelligence, or perspicuity, and that sooner or later comes to an end" [17]. Clearly they don't fit this definition, though there is a family resemblance here. A better social interpretation of algorithm lies in Bourdieu's "dispositions," or perhaps some schema-theoretic constructions like the default value, but I think those attempts just hide the problem under a new set of ambiguities.

How are rich points and Schmaeh like algorithms then? They are certainly finite and symbolic, but they also require rather than exclude "insight, cleverness, intuitions, intelligence and perspicuity." They can be learned by modeling them as, "instructions," but those instructions will seldom be precise or move in discrete steps. And in what I jokingly call "the ethnography halting problem," it's not clear that they ever end.

Perhaps it would help to think of an *area* rather than a *path*. The computational notion of algorithm, at least the one cited above, is like a path with a clear beginning and end, a narrow strip cut through thick woods. The ethnographic notion of algorithm is more like a cleared ski area, with a large mountain face bounded by limits, but with plenty of room for a skier to carve an infinite number of paths from peak to base. My readings on fuzzy logic [18] suggest that the notion of a "fuzzy algorithm" might help.

I can't resolve the issue here. But however it is eventually resolved, the method fractals and the results fractals don't stand in a one-to-one relationship. The method fractal produces data in a fractal-like way, but it doesn't necessarily—or even usually—produce fractal-like themes at the same time. As an ethnographer works on a rich point and then another rich point surfaces, the second rich point may well belong to a different thematic pattern compared with the original rich point. Noticing a problem, then working on the problem, and noticing a new problem embedded within it—this does not guarantee that the two problems noticed will also express the same theme. The rich point cycle, in other words, doesn't also do the thematic analysis. It produces what it is that needs to be analyzed by other means.

LANDSCAPES

What of all the talk about *ethnographic representation* over the last couple of decades? What, if anything, might CAS help with here?

The representation debate has opened up a confusing space of possibilities. In the old days, an "ethnography" was a piece of writing, usually a book-length thing. It had certain characteristic sections, like the arrival scene to establish that the ethnographer had come a long way and actually was "there." It had certain topic requirements—social organization, religion, etc. It reflected certain writing conventions—minimization of the ethnographer's presence through passive voice, for example.

Now the space is wide open. Ethnographic results are presented in ways other than texts—museums, films, plays, computer programs, executive summaries, program designs—virtually any way that study results can be "represented" in another format. And consider the ways that a representation might be constructed just in the textual realm—anything from a formal mathematical analysis to a book of poetry.

As far as how to *build* a specific representation, then, I don't think CAS can be of much help, because we live in a historical moment when the possibilities are overwhelming. When so many experiments are underway, it is not at all clear which of the new ethnographic "species" will survive into the new era. But framing things in terms of "survival" does lead to an CAS type of question. We know that many new representation "species" are out there now. Maybe this suggests thinking about representations in another way. What kind of environment are they competing in? To what conditions must these competing representations adapt if they are to survive and flourish?

Here we enter the realm of another CAS concept, that of a *fitness landscape*. A fitness landscape is a picture of the world within which agents are dying, surviving, or flourishing, as the case may be. It shows what agent characteristics will have a higher survival chance than others. Often the landscape is visualized as hills and valleys, with the higher altitudes representing better adaptation. Fitness landscapes can vary—there might be one high mountain, or they might be a lot of low ones scattered about. And of course the landscape, like everything else in CAS, can change with time.

For present purposes, we just need to understand this: "Fitness landscape" suggests we think about representations in terms of what they accomplish in the world. In other words, can we imagine why some representations would do better than others, no matter how they were built by an ethnographer? Any answer to this question is of course open to debate. But it occurs to me that a few characteristics are at least worth considering.

A representation must certainly be *intersubjective*. It is intersubjective between ethnographer and "others," the people whose point of view an ethnographer is exploring. It is also intersubjective between an ethnographer and an audience of the representation.

Whatever it looks like internally, the representation must link, at some point, with *available* representations for the ethnographer, for the people studied, and for the consumers of the representation. It has to make sense of things for all those points of view individually, and it has to enable common action for any two of them talking or working together as well. Let's call this the *triad constraint*, triad as in, the representation has to make sense of the C from three different points of view A, B, and C itself.

This constraint limits the possible shapes a representation might take. In fact, much ethnography fails the test, because many ethnographers just write for a few others like themselves. People about whom a book is written find it boring and irrelevant, and the public can't make much sense out of it either.

Here's another constraint a representation has to live with: It has to have some kind of a "zoom" function, like Mapquest or a video camera. The idea is that you can focus on a particular rich point, and then either zoom in for more detail or zoom out for broader context around it. The question here is, will the representation look similar at different levels as we zoom in and out? Will those levels *scale*? Recall the earlier discussion of fractals, both the substantive kind called "themes" and the methodological kind produced by rich points. Given these two built-in scaling processes, we can say that ethnography "wants" to find scaled patterns. It's a unique strength, but also a strong bias that must be watched carefully. In the end, though, scaled representations are probably one of the most important results we offer. Let's call this the *scaling constraint*.

Here's another constraint: Many have written about narrative as a natural representation for nonlinear dynamics given that ethnographers, and historians, tell a story of contingencies and connections and how they interact over

time. But a representation will have to contain at least *two* narratives. One tells the "backstory" of what is being shown in the representation itself, the story of where the main features of a representation came from. A second tells the story of how the representation itself was constructed, the story of the study, telling and showing the results at the same time. This double narrative will be a part of a representation, but not necessarily—not usually—all of it. Let's call this the *double narrative constraint.*

Another constraint: A representation, by its nature, will be fixed and static, because it is an end product presented in a book or film or museum exhibit. But even though it is fixed, it will contain attractors and mixes of order and chaos, so in this sense it will also be dynamic. It will display the dynamics learned during the period of research. So the representation will have the odd feature of being a butterfly on the wall with its wings still flapping, together with the story of how it landed there and what it might do to get un-stuck and where it then might go. We can call this the *dynamic constraint.*

The representation must also link with actual practice, the things that people say and do, because understanding that flow was the point of building it in the first place. The methodological test for an ethnographer is, can he/she use the representation as a vehicle to get from conscious change to habitual new pattern? Can he/she now explain exactly what initially was the signature problem of ethnography—two people communicating with each other with the result that what they said and did didn't make sense to an outsider? And then can the ethnographer convey that transformation from rich point to coherence to an audience, so that an audience can take a rich point example, learn the representation, and then see the rich point as coherent where they couldn't before? And can someone from the researched group look at the representation and feel a shock or recognition, perhaps an "aha," because it makes the familiar explicit? Let's call this one a *pragmatic constraint.*

So far we have several constraints on the representation:

1. The triad constraint: A representation's boundary must connect with boundaries in at least three different points of view—subject, researcher, and audience.
2. The scaling constraint: The representation will allow for granularity and scale in a fractal-like way, at least in key areas.
3. The double narrative constraint: It will contain a double narrative with itself as the end point: one narrative telling the history of the differences that the representation focuses on, and the other telling the history of the construction of the representation itself.
4. The dynamic constraint: It will contain dynamic features, such as oscillators, attractors, and "edge of chaos" descriptions.
5. The pragmatic constraint: The representation will submit to test in practice, in the sense that it will enable comprehension and action across triad differences.

CAS leads us to think in a different way about a representation. Rather than foreground how to *build* it, CAS emphasizes conditions it has to meet in order for it to *serve its purpose.* Put in terms of the fitness landscape metaphor, a representation has to evolve and survive in a world that sets constraints on what will work. A good representation will *co-evolve*—that is, a good representation will in turn modify the very landscape within which it took shape. But in this article the focus is on building, and you can't play in the co-evolution game until you've got something to play with.

This is an interesting twist on methodology in general, and on my favorite question from the uninitiated in particular—"How do you know if an ethnography is any good?" Traditional social science requires an answer in terms of sample size, instrument, statistical analysis, the usual benchmarks of the traditional process. The CAS emphasis on fitness landscapes argues otherwise. In fact at its most extreme, the argument would be that it makes no difference what processes were followed to build the representation, nor does it matter what it was made out of. What makes a difference is

how well the representation flourishes in the fitness landscape.

The constraints set out a sort of "management by objective" research goal. Ordinary social science is more the old hierarchical "command and control" model.

It's not hard to imagine that a book or a movie or a museum exhibit could be built from the same research project and that any of the three—in spite of their differences—could fit the bill. And it's clear that a movie, to take one example, could be structured around plot-linked close shots or it could be a postmodern collage loaded with tracking shots and jump cuts.

But notice that the constraints do limit the possible shapes the representation can take, whatever it is one decides to build. Imagine the number of possible representations before the fact, and the number is for all practical purposes infinite. Add a few constraints of the type outlined here and the number drops dramatically. Add a few more and you're down to a few choices. Perhaps fitness constraints are a more powerful way to tell "when an ethnography is done right" than any description of a methodological recipe. This is an odd conclusion for an author who has written dozens of methodology pieces over the decades.

There are many more issues around the problem of an ethnographic representation and what if anything CAS might help us think through. For now, it is enough to leave with the already odd idea that a representation is best evaluated in terms of the fitness landscape to which it must adapt and within which it must survive. And that a few major landscape constraints specify limits on what we produce. And then to conclude that the major issue for ethnographic research might be the outcomes a representation must achieve rather than any cookbook for its construction. Clearly there are *methodological* issues here, including the possible use of traditional social research methodology. But the fitness landscape notion subordinates the methodological issues to larger goals of ethnographic research, raising the possibility that traditional

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social research will play only a partial role, or perhaps no role at all.

CONCLUSION

This article asks if any value comes from looking at ethnography through a nonlinear dynamic lens. I think it is fair to conclude that recent work in this "new kind of science" offers marginal ethnography, the step-child of U.S. social research, substantial intellectual backing. Ethnography, on the other hand, offers CAS, a form of social research compatible with its assumptions and objectives. If CAS means to investigate and theorize the social world in any serious way, ethnography is the kind of research it will have to do.

Looking at ethnography through a CAS lens also casts some age-old problems in a new light, adds clarity, and suggests solutions that are beyond the bounds of what traditional social research frameworks can provide.

First of all, we saw that algorithmic complexity is higher for ethnography by comparison with most social science methodologies, the first indication of its fit with CAS. This fit allows us to see the "unscientific" nature of ethnography as in fact the presence of characteristics that are the normal focus of CAS. The CAS nature of ethnography also foregrounds its adaptive strengths, because it can change based on what has been learned as research progresses.

Next, we explored "sensitivity to initial conditions." This robust concept of CAS helps understand decades of controversy over the influence of an ethnographer's identity on study results. Such influence is to be expected, given that ethnography is a CAS. Rather than aspiring to a delusional "objectivity," a better goal is the development of an attractor space of acceptable study outcomes. The variations in that space, due to ethnographer differences, will in fact strengthen overall study results.

Continuing this theme of ethnographer presence, CAS helped see how the ethnographer is one among many agents with an active role in the research process. This acceptance of researcher influence, compatible with CAS but dissonant with traditional social research methodology, allows poststructural issues of agency, distributed authority and reflexivity to be systematically included.

The next section of the article looked at how algorithms produce patterns at different levels of scale. This "fractal" concept applied twice over. On the one hand, methodological fractals result from the "rich point" cycle that describe a core of ethnographic methodology. On the other hand, age-old concepts in cultural anthropology, like "theme," also label a fractal-like concept, *substantive* rather than methodological. Important here was the fact that the two kinds of fractals are *not* produced by the same algorithms. Fractal-finding is a great strength, and strong bias, of ethnographic research.

Finally, the CAS concept of "landscape" added a new twist to issues of representation. At a time when experiments and innovations are encouraged, are there any constraints on ethnographic representation? "Landscape" called attention to "selective pressures" from the world in which a representation "struggles for survival." The discussion led to the strange idea that a good representation was one that got the job done, rather than one that was constructed in a specific way out of specific materials. The idea hardly fits traditional social research concepts of standardization of procedure as the ultimate benchmark of a good study.

In the end, I think CAS promises an interesting conversation with ethnography to the benefit of both. This article is only a first step; it just begins to touch on similarities among contemporary ethnographic issues and CAS frameworks. What I hope is clear, though, is that CAS is a transdisciplinary research framework where we ethno-types can find some interesting company, more interesting than our traditional "social science" classification has allowed in the past. Unfortunately, discussions about problems in ethnographic research within mainstream social science have been, for the most part, like discussing computer capacity in cubic feet. You can do it, but does it make any sense?

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