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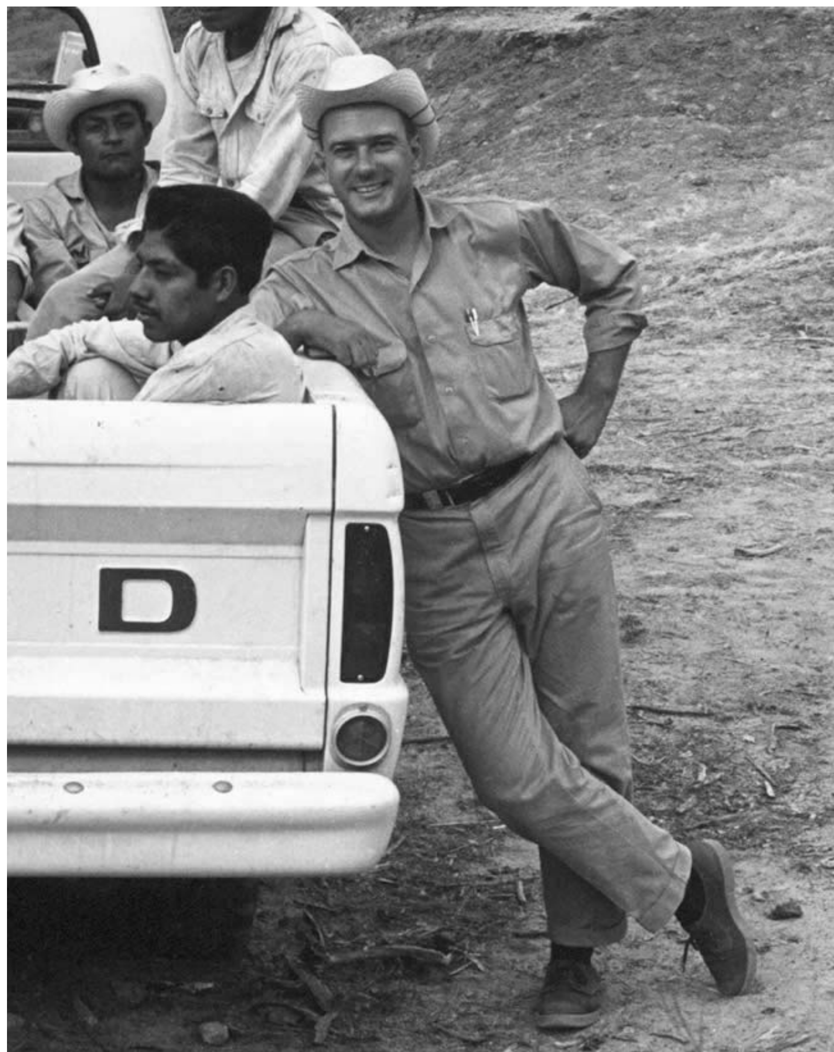

Kent V. Flannery

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On the Resilience of Anthropological Archaeology

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Key Words

New Archaeology, scientific archaeology, postmodern archaeology, fruity humanistic drivel

Abstract

I have now lived through eras when anthropological archaeology was (*a*) mainly culture history; (*b*) part of four-field anthropology; (*c*) hypothetico-deductive science; (*d*) under attack from postmodernism, postcolonialism, and feminism; and (*e*) saved from extinction by its own resilience. In this never-before-published interview, I reveal its likely future direction. (Drum roll, please.)

When the *Annual Review* asked me to contribute this retrospective, I knew it would be hard to come up with something new. So I've submitted an abridged version of the interview I once gave a famous journalist, who I'll simply call "Barbara W." The interview occurred as a result of our both having been stranded for hours in the departure lounge at Chicago's O'Hare airport. (Initially flattered by her having chosen me The Most Intriguing Person in the Lounge, I later realized that we were the only two people there.) Because I kept no notes, my version of the interview will have to be considered "novelized."

Barbara and I talked about archaeology's place in traditional four-field anthropology; about the era, prior to 1960, when archaeology was mostly culture history; about the tumultuous rise of the New Archaeology; about the efforts to derail hypothetico-deductive archaeology during the 1980s; and finally, about the rejuvenation of scientific archaeology and the decline of postmodernism.

Aside from these five main discussion topics, we touched on some of my personal anecdotes as well.

BW: Let's begin with your choice of career. I notice that when the Walter Jeffords estate was auctioned at Sotheby's recently, several of your father's paintings were included. Did you ever consider becoming an artist?

KVF: Not after one of his clients bent down, shook my tiny hand, and whispered, "Don't go into art, my boy, because you'll always be compared to your father."

BW: How did you become interested in archaeology?

KVF: Through an accident, one whose lasting effect I realized only years later. Whenever my mother left our farm to buy groceries, she dumped me in my father's studio. He was a patient man, but having a six-year-old poking into his Burnt Sienna and Raw Umber had to be annoying. One day he hit upon a solution. He asked me, "Would you like to be a cave man for a while?" and I said, "Sure."

Halfway up the wall of his studio was a small loft where he stored drying canvases and gesso panels. He lifted me up to the loft with a sketch pad, thumbtacks, and a set of pastels. Recalling his courses at the Chicago Art Institute, he explained, "Stone Age men covered the walls of their caves with paintings of woolly mammoths, red deer, and hunters with bows and arrows." Doing Lascaux-style drawings for the wall of the loft became my quality time with him.

It was only decades later, as a graduate student working in Iran, that I realized the full impact of that time. Frank Hole and I were testing a small cave in the Sar-i-Pul Valley. The deposits were shallow and the flint tools were Zagros Mousterian. We had backfilled, but for some reason I felt reluctant to leave the site. Suddenly, in a moment of epiphany, I realized that the cave was similar in size to the loft in my father's studio and that I was subconsciously waiting for him to lift me back down to the floor. My career choice made sense once I understood that my father had made cave men the most interesting friends a boy could have.

BW: Was that his main influence on your career?

KVF: Not at all. I also watched him do empirical research before every painting. I saw him walk around the subject, take notes and photographs, and make preliminary sketches. I watched him begin the picture with an egg-tempera layout in white, black, and gray, adding the oil colors only after the tempera had dried. "If you don't start with a good design," he said, "all the color in the world won't save it." It's just as true in archaeological research.

And there is one more thing: He used to sneak into exhibits of his own paintings without wearing a name tag, eavesdropping on the crowd. He was greatly amused by how inaccurate some critics' versions could be of "what the artist was trying to communicate." "If they can't even understand an artist from their own country," my father said, "what hope do they have of understanding artists from other

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countries?" Believe me, I remember that when I read what some of my colleagues write about Precolumbian art.

BW: How did you come to choose the University of Chicago?

KVF: I did not. I had no say in the matter. One day in the spring of my sophomore year of high school, my parents announced that I would soon be taking the entrance exam for Chicago. I knew that my father was a fan of their former chancellor, Robert Maynard Hutchins. What I didn't know was that Hutchins had established an early-entrant program for high school sophomores and juniors.

Seeing how much it meant to my parents, I took the exam, passed, and later that year found myself in Burton-Judson Courts, the Gothic fortress of a dormitory on 60th Street. There, we were all given placement tests. I tested out of four courses, enabling me to graduate in three years. It all happened much too fast.

BW: Did you feel that your parents were pushing you too hard?

KVF: It was not until years after my father's death that I found out why. My mother finally explained that Chicago had been his first choice for college, but they had turned him down. He vowed at that time that if he ever had a child, he would do whatever it took to get him or her into Chicago.

BW: It must have been a difficult adjustment for a teenager.

KVF: The culture change was greater than the academic stress. I had gone from a farm on the Susquehanna River to densely urban Chicago. I had gone from a private boarding school, filled with the sons of Republican businessmen, to a dorm filled with descendants of the International Workers of the World. I had grown up on country ham cured in my family's own smokehouse; my roommate arrived from the Bronx with a jar of his Mom's homemade gefilte fish.

One night I watched a candlelight procession make its way across the darkened Midway Plaisance. As the robed marchers neared us, I asked an older student the reason for the procession. His look told me that I still had lots to learn. "It's Trotsky's birthday, dickhead," he explained.

BW: What sustained you through those years?

KVF: My classmates. Hundreds of early entrants were in the same boat. Three of them went on to be archaeologists: Les Freeman at Chicago, Jim Brown at Northwestern, and Jim Schoenwetter at Arizona State. And there was this kid from Rahway, New Jersey, named Carl Sagan, who wanted to be an astronomer (**Figure 1**).

BW: I understand that you actually had food fights with Sagan in the Burton-Judson cafeteria.

KVF: Already at 16, Carl had mastered the skills necessary to land an object on the moon. Using a soup spoon and an overturned salt shaker as a catapult, he could deliver a soybean veggie cutlet into anyone's lap at any table.

BW: What did you do after graduation?

KVF: Thanks to my inspiring high-school biology teacher, I was more interested in evolution than anything else. I went into zoology, which was one of Chicago's strongest graduate programs. They had ecologists such as W.C. Allee, population biologists such as Thomas Park, systematists such as Hewson Swift, and geneticists such as Sewall Wright. I was taken on fabulous field trips by my advisor, Alfred E. Emerson, the expert on social insects before there was an E.O. Wilson.

BW: How did you wind up in anthropology?

KVF: I went to Mexico to collect Salticid spiders for a Master's thesis in zoology. While there, I got a chance to excavate the ruins of Yagul, Oaxaca, with the University of the Americas. John Paddock, the professor leading the dig, thought there might be a "big unfilled niche" for an archaeologist with a background in zoology: I could identify animal bones and reconstruct paleoenvironments.

On my return to Chicago, I started taking human paleontology courses with F. Clark Howell, and he recommended me to

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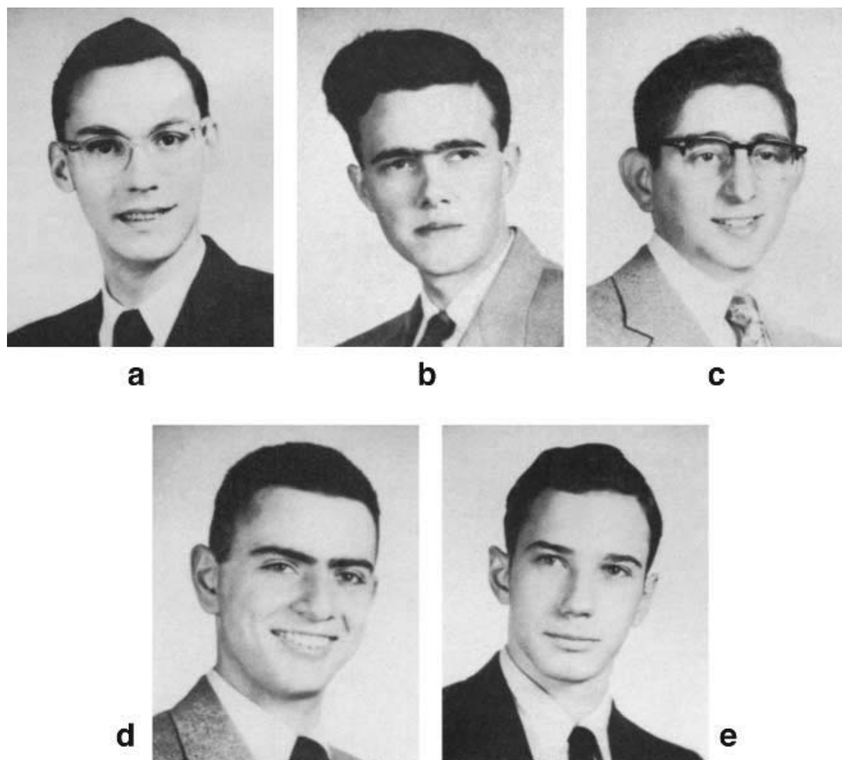

Figure 1

These five teenagers survived the leap from high-school sophomore to early entrant at the University of Chicago. For extra credit, identify the one who did not become an archaeologist. *a*, Leslie G. Freeman. *b*, James A. Brown. *c*, James Schoenwetter. *d*, Carl Sagan. *e*, Kent V. Flannery.

Robert Braidwood, who was working on early plant and animal domestication. I wound up doing an anthropological Master's thesis on how to distinguish wild and domestic pigs.

BW: What was Chicago's anthropology department like in those days?

KVF: Every bit as elite as the zoology program. In addition to Braidwood and Howell, there were Robert McC. Adams, Fred Eggan, McKim Marriott, Milton Singer, Sol Tax, Manning Nash, Lloyd Fallers, David Schneider, Norman McQuown, and a host of other luminaries.

BW: What was the atmosphere like for students?

KVF: There was no grade inflation, no coddling, and no nurturing. They threw you into the deep end of the pool without a life jacket, and you either learned to swim or drowned. I mentioned this to one of my former professors years later, and he smiled in agreement. "Students," he said, "were the flesh-colored stuff between the cleats of our hobnailed boots." It was supposed to make you tough and self-reliant.

BW: Did you adopt that approach for your students at Michigan?

KVF: No. Three archaeologists on the Michigan faculty—Bob Whallon, Henry Wright, and I—went through the Chicago system, and we all agreed to do the opposite at Michigan. We put our students in the shallow end of the pool and give them immediate CPR if they stumble.

BW: The so-called New Archaeology arose at Chicago. Tell me a little about that.

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KVF: In the spring of 1961, Lew Binford interviewed for a position at Chicago. His job talk was a mesmerizing account of prehistoric strategies in piedmont and tidewater Virginia. He was given a three-year contract, at the end of which he was "terminated with prejudice."

BW: Were they turbulent years?

KVF: Binford was a charismatic southerner who had mastered the fire-and-brimstone style of a revival meeting. He opened his first class by announcing, "My name is Lewis R. Binford, and the name of this course is Revelations!" By the end of that class, half the students were speaking in tongues.

Then Chicago made the mistake of teaming Binford with Braidwood in a course on world prehistory. It was Counter Culture vs. the Establishment. At one point Braidwood, introducing the Ubaid period in Mesopotamia, opined that "this was the first moment in the Near East when Established Village Farming so freed man from the eternal food quest that he had leisure time to elaborate his culture." Binford leapt to his feet, his voice an octave higher in protest, and replied, "Dr. Braidwood, studies show that no one on Earth had more leisure time than precontact hunters and gatherers. And most of them just spent it subincising themselves, whirling churingas, and engaging in bizarre sex practices." Had there been a mosh pit available, Binford would have been carried around on the adoring students' shoulders.

The Revolution was underway. The Establishment would be overthrown. Old-fashioned culture history would be replaced by hypothetico-deductive archaeology, with rigorous testing of hypotheses, sampling techniques, measures of significance, and other approaches seductive to people under the age of 30.

Every revolution has its nerve center, its Left Bank café where the conspirators meet. The nerve center for the fledgling New Archaeology was Stuart Struever's kitchen in the Beechwood Apartments, 1223 E. 57th, between Woodlawn and Kimbark (**Figure 2**). There, a lively roundtable was hosted by Struever, Binford's sometime teaching fellow, and his wife Alice, whose mugs of "deadly Java" could generate hypotheses all by themselves. Binford dropped by after dinner to find students like Howard Winters, Bill Longacre, Jim Hill, Jim Brown, Les Freeman, and myself already overcaffeinated.

My presence at this ongoing seminar was serendipitous: I lived in a one-room apartment on the third floor of the Beechwood, a floor to which the building's heat did not ascend. The warmest place in my room was in the center of my fist as I broke the ice on my soup. Coming down to Struever's kitchen thawed me out, and the arguments were so lively that I could not tear myself away. It's a shame the Beechwood went condo before we could put a bronze plaque on the kitchen wall.

In those days, Chicago advocated a four-field anthropology, with ethnology, linguistics, archaeology, and biological anthropology as equal partners. We were never convinced, however, that the ethnologists felt they needed us the way we needed them. Most ethnology students believed E. Adamson Hoebel's famous observation that "archaeology is forever doomed to be the lesser part of anthropology." We, on the other hand, felt that the only conceivable purpose for ethnology was to provide archaeologists with descriptions of living cultures, helping them to interpret the evidence of the past.

I was friends with most of the ethnology students—even the teaching fellow for Sol Tax's History of Anthropology course, who wrote on my term paper, "You may want to consider an alternative career." I did not know at the time, of course, that some of the ethnology students were predestined to contribute to the postmodernist, postcolonialist, and feminist critiques of the 1980s. A few of the brightest stand out in my memory. There was Herman Newtick, with whom I spent many evenings at Jimmy's Tavern on 55th Street. And of course, Eileen Farr. Since her marriage to a fellow Hyde Park radical, her colleagues know her better as Eileen Farr-Tudaleft.

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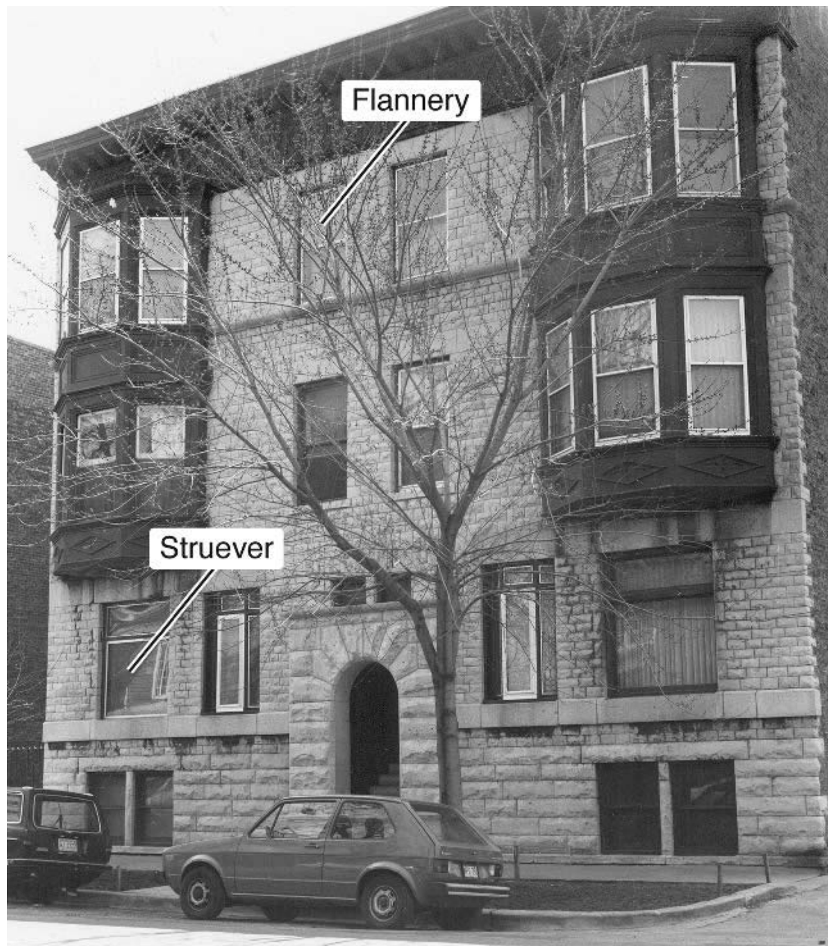

Figure 2

The Beechwood Apartments on 57th Street in Hyde Park, Chicago. In a first-floor kitchen of this building, Lewis R. Binford and his students met from 1961 to 1964 to foment the New Archaeology (photo courtesy of James Phillips).

Herman, like many of my classmates, was a Marxist. His deepest conviction was that Western Civilization was the world's most loathsome evil, and he had decided to fight it by refusing to bathe. Eileen was a Maoist and regarded Marxists like Herman to be pathetic revisionists. Every civil rights movement received her passionate support. Eileen strode proudly through Hyde Park with her T-shirt emblazoned in capital letters: "THERE'S NO EXCUSE FOR VIOLENCE AGAINST WOMEN" (and below it, in fine print, "Against men, that's a different story").

BW: How did archaeology students in those days prepare themselves to run their own big interdisciplinary projects?

KVF: By doing fieldwork on imaginative projects run by senior role models. To this day, I marvel at the opportunities I was given between 1960 and 1964. I was continuously in the field, usually on projects related to the origins of agriculture and village life, and I worked with some real heavyweights.

It began with six months in Iran under Braidwood, digging sites called Sarab and Asiab. That summer I dug in South Dakota with Warren Caldwell and Charlie McNutt. The next summer found me digging Maya sites in Chiapas with Bob Adams. He recommended me to Michael Coe, with whom I dug Salinas La Blanca on the Guatemalan coast. Coe, in turn, recommended me to Scotty MacNeish, who was working on the origins of agriculture in Mexico's Tehuacán Valley. From there I went back to Iran to dig Ali Kosh with Frank Hole and Jim Neely. The reason most of these people hired me was because of my background in zoology and ecology. They needed someone to create comparative skeletal collections of modern animals, use them to identify ancient fauna, and reconstruct past environments.

BW: What should one do when he has such great opportunities?

KVF: Observe one or two qualities of each project director and emulate them. From Braidwood I learned that you should assemble the best interdisciplinary staff you can. From MacNeish I learned how to set goals and pursue them relentlessly. From Hole I learned efficiency: how to set deadlines and meet them. From Coe I learned how to define pottery types and use them to establish a regional chronology. From Caldwell and McNutt I learned how to find postmolds and ephemeral earthen floors. From Adams I learned to think big and to never, ever sweat the small stuff.

I wrote a thesis and started looking for a job. Caldwell recommended me for a post with the Nebraska State Historical Society, but its director, Marvin F. "Gus" Kivett, decided that I had "a limited future in archaeology." Several universities felt that I was "just a faunal analyst," not the "generalist" they were looking for.

Then came a ray of sunshine. A curatorship in Mesoamerican archaeology opened up at the Smithsonian, and Coe's and MacNeish's letters convinced Clifford Evans to give me a try. Evans made it clear what was expected of me: "Start a research project in Mexico, boy, and the sooner the better."

Building on what I already knew, I headed off to Oaxaca, looking for a dry cave and a couple of early villages. I knew I would be working with plants and animals, pollen samples, nets, baskets, and chipped stone tools. The last thing I expected was that my horizon would one day be expanded to include religion, iconography, Precolumbian writing, and the rise of the state. Nor could I have anticipated that within 20 years, the whole notion of scientific archaeology would be attacked as "decadent colonialism."

I loved the Smithsonian, but in 1967 James B. Griffin lured me to Michigan with the promise of letting me train the next generation of archaeologists. For the third time in my life I was surrounded by stimulating professors, this time with names like Leslie White, Marshall Sahlins, Eric Wolf, Elman Service, Mervyn Meggitt, Robbins Burling, and new arrivals like Roy Rappaport, Ray Kelly, Conrad Kottak, and Aram Yengoyan.

BW: Exactly when did you team up with Joyce Marcus?

KVF: By the 1970s, my Michigan excavation team was creeping up on the transition from chiefdom to state in Oaxaca. Meanwhile, a survey team, made up of colleagues from Purdue, Georgia, Wisconsin, and McMaster, was collecting complementary data on the early Zapotec state. A third body of evidence, however, lay fallow: hundreds of stone monuments and texts in Zapotec hieroglyphs, whose study had not progressed much since the time of Alfonso Caso. Maya inscriptions had begun to yield their secrets by 1972; surely the Zapotec texts could too.

Michael Coe told me of a young epigrapher at Harvard who was "doing unprecedented things" with Maya glyphs; she was "the first student Tatiana Proskouriakoff had ever volunteered to work with." Could she be persuaded to look at Zapotec texts? I got my answer when Stephen Williams invited me to speak to his class at Harvard.

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The students from Williams' class filed past me before dinner at the Harvard Faculty Club. As we shook hands, one young woman after another described her thesis topic and joked about her past as a field hockey player for some girls' prep school in New England. Suddenly I came face to face with my epigrapher.

"I'm guessing that you *didn't* play field hockey in New England," I observed.

"Right," she said. "I'm Joyce Marcus, and I played beach volleyball in California. We didn't play in those little plaid skirts, either. We played in bikinis."

Suddenly everyone else in the Harvard Faculty Club had become invisible.

Joyce came to Oaxaca in 1972, and by 1973 she had already assigned the Zapotec texts to three eras: a period of militarism and conquest, bracketing state formation; a period of diplomacy, as the Zapotec achieved détente with Teotihuacan; and a phase of preoccupation with noble genealogies, as the early state broke up into balkanized principalities. Soon Joyce had teased out word order, grammar, verbs, ordinal numbers, puns, two calendars, and a series of place glyphs for territories claimed. She did it during her time off between finding postmolds (as she had done in Nevada for Robert Heizer) and excavating masonry buildings (as she had done in the Maya lowlands).

There are times in your life when you realize that you are a jigsaw puzzle with one big piece missing, and someone else is the missing piece. Joyce came from a very different academic tradition, bringing with her a background in cosmology, ideology, religion, iconography, and political anthropology, which complemented my training in biology, ecology, and evolution. When you combine them all, you get a more holistic anthropological archaeology. And so in the spring of 1973, with catering provided by Tippy's Taco House near Dumbarton Oaks, we combined them permanently. There is, after all, no better advice than that given to Nausicaa by Odysseus centuries ago, namely that

> there is nothing mightier or nobler than when two people who see eye to eye keep house as man and wife, confounding their enemies and delighting their friends. (The Odyssey, Book VI, lines 182–85 in the original Greek; see Tebben 1994)

BW: I'm guessing that this was a career turning point for both of you.

KVF: Yes. It allowed us to generate a more holistic model of the past, one previewed in our coauthored essay "Formative Oaxaca and the Zapotec cosmos" (Flannery & Marcus 1976). By then it was clear to us that even as basic a subsistence activity as agricultural water use could not be explained with the usual ecological models. The Zapotec understanding of where water came from did not match that of agronomists. With ancestors acting as intermediaries, an offering of one's own blood could induce Lightning to split the clouds and bring rain; only after the water reached Earth could the indigenous engineering of wells and canals take place.

Ecological archaeologists had always considered hydroagriculture to be the infrastructure that supported the cognitive superstructure of the Zapotec. They cared about the canals and the carbonized corncobs, but not about the bloodletting tools, the sacrificed quail, and the temples oriented to the sun's path at equinox. That was for humanists to speculate on. But when we combined native cosmology and ritual with a Western understanding of soil and water, it became possible to provide a single model that explained both. It even raised the possibility that the real infrastructure might lie in the Zapotec mind.

We met real resistance from most cultural ecologists on this idea. "Fruity humanistic drivel" was a typical comment. But we persisted, and eventually other archaeologists joined us. (Unfortunately, some archaeologists began to ignore environment and subsistence altogether, and as a result, a few really did produce fruity humanistic drivel. But that's another story.)

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BW: You were trying to do holistic anthropological archaeology, basing your interpretations on what was known of living societies and establishing a kind of dialogue between the archaeological data and the ethnographic record. It must have struck you as strange when ethnologists started saying, "Oh, stop reading those classic ethnographies. They were written by people who were tools of a colonial power."

KVF: Yes, especially since our archaeological data so often reinforce the models advanced by the best ethnologists. For example, our discovery that the first segmentary societies in Oaxaca built defensive palisades confirms Kelly's (2000) model for the origins of war. Our data on the subsequent emergence of hereditary rank in those same societies resonate with Leach's (1954) and Friedman's (1979) descriptions of the same phenomenon in Southeast Asia. Still later in the Oaxaca sequence, we have evidence for warring chiefdoms whose behavior matches ethnohistorically documented societies in Colombia (Carneiro 1991) and New Zealand (Buck 1949). Additionally, the Oaxaca data show that the Zapotec state formed in the context of rival chiefdoms, when one of those societies gained advantage over its rivals and reduced them to provinces in a larger polity. This is very much the way Cohen (1978) sees it happening, and it is clearly similar to the way historically documented states formed among the Zulu, Ashanti, Hunza, and Hawaiians (Flannery 1999).

In other words, we felt we were using anthropological archaeology to create a dialogue among ethnologists, ethnohistorians, and archaeologists. We did not anticipate that it would be the ethnologists who dropped out first.

BW: Did you think that postmodernism was a new direction or only a phase through which anthropology was passing?

KVF: Definitely the latter. Anthropology tends to pass through phases that last about 20 years (Ortner 1984). And as for "postprocessualism"—the archaeological version of postmodernism—I expected it to be even less enduring. After all, the difference between anthropological archaeology and postprocessual archaeology is like the difference between reality and "reality TV."

BW: When did the pendulum begin to swing away from postmodernism?

KVF: For social anthropology, there were plenty of indications during the 1990s: Windschuttle's (1996) *The Killing of History*, Kuznar's (1997) *Reclaiming a Scientific Anthropology*, and Lewis's (1998) "The misrepresentation of anthropology and its consequences."

For archaeology, one of the most thoughtful critiques was Bintliff's (1993) paper, "Why Indiana Jones is Smarter than the Postprocessualists." His title refers to a moment in the movie *Indiana Jones and the Last Crusade* when Harrison Ford says, "Archaeology is about facts; if you want Truth, go next door to the philosophy department." Bintliff feels that "processual versus postprocessual" is just one more stage in the centuries-old debate between positivism and idealism. He reveals that postprocessualism's influence is already

> waning in Britain, linked to the decline of its parent Post-Modernism. Both lost credibility through attempting to dominate discourse, and their negative implications for human rights. (Bintliff 1993, p. 91)

In the case of postprocessualism, of course, there was another reason it drew fire: its naïve attempt to adopt postmodern buzzwords like "identity," "memory," and "legitimacy" and transfer them wholesale to dirt archaeology. It is one thing to present evidence for those phenomena when you have living informants or really good written documents. It is quite another thing to claim that you've recovered evidence for them while digging a Natufian cave terrace or an Early Woodland midden. You can only proclaim such "insights" a couple of times before your colleagues start asking what mesh size of screen you need to recover "memory."

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BW: Wasn't there also an anti-Western, antiscience undercurrent to some critiques of anthropology and scientific archaeology?

KV: There was indeed. Windschuttle (1996) attributes at least some of it to the fall of the Berlin Wall, which necessitated a change in anti-Western rhetoric.

BW: Was that the case with your friend Herman Newtick?

KVF: Actually, Herman went into paradigm shock. As Windschuttle (1996, p. 181) puts it, "not only had communism been consigned to the dustbin of history but with it had gone the prospect of replacing capitalism with any kind of revolutionary regime based on socialism." But then Herman discovered Foucault and Bourdieu and Giddens and realized that he still had a reason to hate the West. Suddenly all Western science, including anthropology, could be seen as a plot to create an "asymmetry of power" vis-à-vis native peoples.

As a student, Herman had embraced social evolutionism because it reminded him of the Marxist stages of Primitive Herd-Matriarchy-Patriarchy, and so on. Now, alas, to be an evolutionist meant that you might be someone who considered inequality and exploitation to be the natural state of affairs in human society. And even if that turned out to be part of our natural state, it would not be politically correct to say so. People could be subjected to witch hunts by the American Anthropological Association (AAA) for expressing ideas like that.

Herman burned every ethnography in his library that had been written in the era of colonialism. "We shouldn't do any new ethnographies," he told me. "Our duty is to deconstruct the old." Herman also felt that no term should ever be applied to a society if it implied a status below that of a United Nations–recognized nation. Hunting-gathering bands were to be called "agriculturally challenged societies." Chiefdoms were simply "bureaucratically challenged states." Herman referred frequently to "the Cro-Magnon Nation," not wanting to hurt their feelings.

"I cannot believe," he told me, "that you continue to use Western, hypothetico-deductive, logical-positivist science to study Neolithic villages. You need polyvocality. You need to hear the voices of the Neolithic villagers, instead of simply pigeonholing them."

"I think I hear their voices," I replied. "They're telling me that you're about two fries short of a Happy Meal."

BW: But wasn't Herman's rejection of Western science typical of the "social science wars" described by Hochschild (2004)?

KVF: Yes. And it continued until he discovered one day that he had cancer.

Did he go to a non-Western healer? A Huichol shaman, a New Age priestess, or an expert in healing with crystals? No, he headed right for Cedars-Sinai and had radiation treatments followed by chemotherapy.

"I thought that you felt Western science was a fraud, an 'asymmetry of power' to be soundly rejected," I reminded him.

"Hey," he said. "This is my health we're talking about."

"So Western science is okay for your health, but not for your profession?"

"That's right," said Herman. "For my health I want something whose reliability I can trust, because it's been subjected to years of objective research, tested by multiple investigators, and based on an underlying universal theory that has survived repeated attempts to falsify it."

"And for your profession?"

"For my profession," he explained, "I want something politically correct, that admits to no universal regularities in human culture, and is so personal, intuitive, interpretive, and humanistic that it cannot be tested, weighed, counted, measured, or compared with anything else."

"Like the idea that illness exists only in the mind of the patient, and that no one from another ethnic group could even imagine, let alone cure, that patient's malady?"

"Exactly."

"Were you aware," I asked him, "that your radiologist was Vietnamese?"

That was the last time I saw Herman. He's cured, but now he's suing Cedars-Sinai for testing a new type of chemotherapy on him, in violation of the AAA's Human Subjects Guidelines.

I still send Herman books from time to time, such as Harris's (1999) *Theories of Culture in Postmodern Times*. But that is just to tease him. Herman's basically a good guy; he just got deconstructed by some French philosophers.

BW: Surely not all your ethnographic colleagues were as deeply affected as Herman by the collapse of world socialism.

KVF: One who remained singularly unaffected was my friend Eileen Farr-Tudaleft. It did not matter that Western capitalism had survived; there were fellow anthropologists who needed punishing even more. Her list of unacceptable research was very long. Anyone who considered the smallest percentage of human behavior to have a genetic basis was "racist." Anyone who drew blood samples from subjects was "engaged in genocide." Anyone who asked informants a question, however innocent, that they didn't want to answer "should be censured by the Ethics Committee."

Eventually, Eileen concluded that she had been put on this earth to free Third World peoples from undemocratic governments. It was not enough to study a village in Brunei; you had to bring down the Sultan. It was not enough to study poverty in Somalia; you had to depose at least one warlord.

Joyce and I bought Eileen dinner and drove her to the airport on the night she left for the Sudan. She was as happy and excited as I had ever seen her. After months of negotiation, she had received permission to study a community filled with the most downtrodden ethnic minorities in the region. "I'm going in barefoot," she said, "I'm going to be an 'engaged anthropologist.'"

I never found out how much actual ethnographic data Eileen was able to collect. Three months later, however, I did learn from CNN that she had succeeded in inciting a peasant uprising that claimed 30,000 lives.

BW: I'm guessing that some of her favorite causes lost support during her absence.

KVF: Yes. And by the year 2000, a lot of serious, empirically grounded archeologists were getting tired of seeing fairly limited, sometimes even mediocre, field data "enhanced" by the addition of postmodern phrases. We had seen half a dozen spindle whorls used as evidence that Prehispanic women were "resisting male domination." We had seen a mute, 600-year-old skeleton described as "biologically a robust male, but gender female." We had even heard archaeologists claim to have tapped into the "memory" of villagers who had been dead for 8000 years. Forgive my skepticism.

"I don't know where all this jargon has gotten us," said one of my most recalcitrant colleagues, a "hardwired behaviorist" named Dieter Ministic. "For the postmodernists, nobody 'achieves' or 'inherits' status any more; they just 'negotiate' it. ('What'll you take to let me be king next year?' 'Make me an offer.') And another of their favorite terms is 'power relations.' Speaking of which, didn't 'power' used to be the ability to force other people to do something they did not want to do? Yesterday I heard one colleague say that she had been 'empowered' by taking a course in Pilates. I told her, 'Good, then you'll have no trouble forcing North Korea to give up its nuclear weapons.'"

"Finish your beer, Dieter," I told him. "The world has passed you by."

"Just once," he said, "I'd like to see somebody say, right in the *American Anthropologist*, 'Enough already. We're tired of just spinning our wheels. We want to get back to empirical research.'"

"It'll never happen," I assured him.

But I was wrong. One day in 2004 I opened the *Anthropologist*, and a group of five authors had said just that. After acknowledging the problems identified by postmodernist, postcolonialist, and feminist critiques over the

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previous 20 years, Bashkow et al. (2004) decided that "work in the wake of these critiques has had no more success in 'solving' such problems than the theoretical traditions it seeks to supplant." They went on to say that

> [m]any, we think, have been too quick to reject, in wholesale fashion, the anthropological past—too indiscriminate in their characterization of all anthropological epistemologies as positivistic, all anthropological politics as complicit in imperialism. (p. 433)

For these statements to appear in the flagship journal of what had been, for at least 20 years, the most politicized and antiscientific organization in the social sciences meant that another sea change could be in the works. Perhaps somewhere—in a 2004 version of Struever's kitchen—a new group of anthropological archaeologists was telling each other that generalizing archaeology, committed to empirical data and aimed at discovering regularities in prehistoric behavior, was resilient enough to survive any critique. It even seemed likely that the archaeology of the future would involve science more deeply, reaching down to the molecular level through phytoliths, bone chemistry, isotopic analysis, and DNA (Jones 2001).

BW: Have you told this to your students?

KVF: I long ago advised them not to jump on the postmodern bandwagon. Science, Barbara, is an unstoppable express train. Postmodernism was just an idealistic siding that led nowhere. Most archaeologists believe that the world's fascinating past will only surrender its secrets to research that is as objective as we can make it. You certainly can't get at them through political correctness. We are tired of hearing—to borrow a phrase from Tooby & Cosmides (1992)—that we are all "racist, sexist, or crazy" unless we distort the data of prehistory to fit someone's political agenda.

BW: Since I think I just heard my flight announced at last, I'll ask you my standard final question: If you could be a tree, what kind would you be?

KVF: A bristlecone pine.

BW: What a strange choice.

KVF: Not at all. Bristlecones are not the handsomest pines—they're actually kind of gnarly—but some live more than 4000 years, which means that they witness more sociocultural change than any other tree. And even after they die, they are useful: Their thousands of rings can be used to dendrocalibrate radiocarbon dates.

BW: And what would you like on your tombstone?

KVF: That's easy: "He hated to leave while the party was still going on."

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