

Benefit of foresight: An Archaeologist in Process

Anna Roosevelt

GROWING UP TO BE AN ARCHAEOLOGIST

I decided to be an archaeologist when I was nine. My mother, a painter who grew up in the American west, loved archaeology. We would spend vacations looking at sites and mounds. After a trip to Mesa Verde I announced my vocation, and my mother said, 'OK'. Many kids want to be archaeologists, but their parents may not be as complaisant as my mother was.

My two sisters and I grew up with her (my father died young in a plane crash) and our grandmother, her mother-in-law, in Long Island, New York. I learned a lot of useful things then: how to draw (more or less), how to drive a tractor (the grass had to be mown), how to take pictures and do textiles (my grandmother was a photographer and textile artist), how to stand up for myself (the sisters had more than a few knock-down-drag-outs), etc.

The war and intelligence service by the men in the family led to highish mortality rates, so our domestic group was essentially matriarchal. I mention this facet of my upbringing because it probably helped me make my way in academe, which, especially when I started, was quite male dominated. In the world I was born in, however, women were the leaders. I assumed that was the norm. I was fortunate, too, in what male relatives I had, such as the uncles. Especially favored was Unc, one of my father's brothers, a great friend of my mother's and a great indulger of us children. He was a mining engineer and inventer and an aficionado of archaeology and art. My relatives have been a great support, even the younger ones, to whom I am 'Auntie-Anna Jones' (Indiana Jones, get it?). My sisters pitch in with the digging when the going gets tough, as in Central African Republic in 1997.

Although I did well in school (we got a quarter for every A), I didn't like school much until college and grad school. In the lower schools, the only subject faintly related to archaeology was Classics, so I took Latin and



Greek, and later Hebrew and Akkadian. I've forgotten most of the latter two, but the others have helped me with vocabulary, especially the technical words.

Modus operandi for an archaeologist

I've found it's good to be optimistic and not act from fear. In my experience, fear is often exaggerated. If you look ahead to really examine the feared outcome, it usually does not seem as bad as you thought. And preparedness can buffer any bad effects, should the worst happen.

But since outcomes are unpredictable, it's handy to be flexible. If one way to a goal is barred, you can just shift and push on. Learning and changing are enjoyable and allow self-correction and intellectual development. The new direction often is more interesting than the original one. When research turns up unexpected patterns, one can follow up on them rather than stay stuck in the original concept. It makes the research dynamic. None of the really interesting findings of my tropical lowland research – the Pleistocene foragers, pre-agricultural complex societies, archaic pottery-making fisherpeoples, whatever – would have been possible had I not bent with the findings. They opened up theoretical possibilities with more scope than the ideas I began with.

Another useful trait is independence. To get anywhere in research, one needs to think for oneself. Students can be encouraged to develop their own

ideas and independent projects. In general, I'd say to those starting out that you should be true to yourself. Don't worry too much about the downsides of doing that. You'd probably offend somebody no matter what you did, so you might as well do and say what you think right. If you try to agree with everybody you are doomed to failure because everybody doesn't agree, and, anyway, in that case you'd end up a nonentity with no ideas of your own. If everyone already agreed with your ideas, then there would be nothing for you to discover. For example, I enraged North American Paleoindian dating experts by pointing out the big problems in Clovis dating: huge errors, non cultural dates, old carbon, etc. (Roosevelt 2002). But the revised dating needed to be brought out, for it completely changed the picture of the Paleoindian radiation. In any case, most people prefer to know where they stand rather than get palliatives.

Don't be afraid to take charge and lead if you feel like it. Somebody has to, and if others can, why not you? Sydel Silverman is great to talk to about such things. Whenever I've been wondering if I should take a leadership position or urge others to do such and such, she'll say, 'of course. Why not'? For me, intellectual momentum, enthusiasm, and organization have been key elements in facilitating my projects, which sometimes have as many as 30 people in the field.

Obviously, it's good to be friendly to colleagues and students and acknowledge their work. Quite a few big-wigs in our field only want to talk about their work; they glaze when others start to tell about theirs. If you listen, you might learn something, and they might learn from you. My model for openness and generosity to others is Jeff Quilter. Envy is not useful. This is not a zero sum game. Let others have their day. But watch your flank. The wolves may be out there.

I'd also encourage people to try, try again. Each transcendent result I've had from research has come after a lot of slogging. When one technique won't work, often another will, and when one type of evidence isn't there, another often is. I also feel you should not take a reversal or problem as a categorization of failure. There's a great maxim in the novel *Brat Farrar* by Josephine Tey. It is that the difference between successes and failures is not that failures have more failures. In fact, most successes have

more failures, but they move right on from them to successes because they try harder and longer to reach the beloved goal. I can tell I'm getting somewhere with a research problem when I feel really gloomy and anguished about it. My experience is that that state of mind comes when you finally reach the core of the problem and begin to solve it. If you enjoy the research process, you've got plenty to keep you going even in the dry spells, and, anyway, the people aspect of field research is truly rewarding. I remember many instances when I and my team fell into gales of laughter just because an appalling problem had arisen. If all problems are solvable eventually, they need not be daunting.

An epistemology in the making

I was very lucky in my own mentors, who were kind, enthusiastic, knowledgeable, exigent, hands-on teachers, and full of character. They included family, teachers, fellow students, colleagues and collaborators, and people in the field.

For college, Stanford University was the right choice for me. Radcliffe, when I visited, seemed downright depressing. It was a dark, wet day; the guide was a droopy, uninspiring student. But Stanford! The very name conjured up fun, sunshine, and palm trees in my mind. Aside from partying, there were many good things for students there. The libraries were great and user friendly. Even better, the Stanford professors treated students so well. They appeared to exist to fulfill our every need and interest. I especially remember Charlie Bird and Edwin Good. When graduation came, professors then made every effort to us into grad school.

The first two archaeologists who had influence on me were Junius Bird and Gary Vescelius, whom I met as an intern at the American Museum of Natural History between junior and senior years in college in 1967. I worked with them in the lab or field off and on for about seven years, and they often served as references for me. Both were consummate excavators and meticulous lab researchers. Gary was excavating by natural microstratigraphy and fine-screening at a time when many North American excavators were using shovels and deep artificial levels and often no screens. I think he learned it from Junius. Junius, who dug some significant early sites, such as Fell Cave and Huaca Prieta, was a wonderful detail person and had tremendous

patience. From him I learned to be observant. Had I not done so, I would not have seen that the hundreds of tiny black and white bits in the tropical forest sites' soils were actually identifiable prehistoric plant and animal remains. He also was very generous to students and included them in everything he did.

In grad school at Columbia in the 1970s I would expound on the detail methods to fellow archy students but was just laughed down. I didn't mind. I realized that if they didn't use exhaustive methods, *I*, not they, would be the one to find the 'archaeological' treasures. I had already made my first unexpected finds in Stanford field school at an Archaic shell-mound, Patterson Mound, where I found an unprecedented carbonized wild leek and an unfired clay figurine simply by searching carefully with both my eyes and fingers.

Through these early mentors, I got various jobs as a lab and field supervisor while I took my courses and little by little gained the training and work experience I needed.

After Stanford, I got a Ford Fellowship to the University of Pennsylvania in 1968. But things weren't right for me there. There was quite a hierarchy in the Ancient History and Neareastern Archaeology programs, even among students, such that first-year students were not even supposed to speak up in class. I felt a chill from my department advisors, who were very different from my kindly west-coast mentors. Could there have been culture shock between pompous, authoritarian East Coast academicians and an irrepressible Sunbelt coed determined to be an archaeologist? Who's to say? The end result was that I got discouraged, and, with the generous help of Rodney Young, my epigraphy professor, transferred to Columbia in New York.

At Columbia, I studied up with Ed Lanning, until he left for Stonybrook. Inadvertently, Ed taught me an important lesson for teaching. Ed was a genial fellow who greatly privileged students. After my first class with him (Environmental Archaeology), he told me I was brilliant. What a difference from Penn! I rode out the ups and downs of the years at Columbia, buoyed by that praise. Later, when I found out that he gave that praise to *all* his students, regardless of quality, I had to laugh. But, remembering how his praise boosted my performance and originality, I try to be positive and encouraging to students while at the same time expecting a lot of them. I figure that my support will help them find whatever genius they have in them

and lift them to the best performance they are capable of. Many outstanding archaeologists have confided in me that at some point early on someone told them, falsely, that they were unsuited to go on in their discipline. Since suitability is difficult to determine early on, those discouragers were misguided. Fortunately, the archys paid no mind and went onwards and upwards. One of the best things about Columbia was that students were respected and treated as professionals.

Just as important as the methods Junius and Gary taught me was the problem orientation I learned from Marvin Harris at Columbia. He kept his eye on the big picture, always asking, what's the question? What do the data mean? What is its significance to anthropology? As chair of my committee, he took great interest in my dissertation on the prehistory of settlement and agriculture in the Orinoco. Richard Keatinge, a student of Gordon Willey's, was my advisor. Together, he and Harris got me an award of distinction for my dissertation. Now, Harris would get impatient when I talked about sherds, so it's lucky that I pursued an acquaintance with Ben Rouse at Yale during this time. He came onto my committee and was a great resource, never tiring of discussing and going over pottery.

At that time also, I got to know Howard Winters at NYU. I have a fondness for NYU, where Winters later got me an adjunct professorship while I was working at AMNH in 1985. Through him I met Stuart Struever at Northwestern. Struever made me a trustee for the Center for American Archaeology, and that led to an acquaintance with Jim Brown at Northwestern, who's been debating partner and friend ever since. They introduced me to the 'New Archaeology', whose principal contribution, I think, was to ask, 'How do you know'? They added to my education a quiver of techniques, including sampling and statistics and Jane Buikstra-type bioarchaeology. I applied all of these in the South American lowlands, where most had not yet been used. Now in Africa, too, the approach has helped me. We have found there many remains whose existence others had missed. Winters and Struever, by the way, got my dissertation published. Winters had a lunch date with Struever just after my defense and told him about the thesis. Struever, then serving as an acquiring editor for Academic press, had it reviewed and then published it (Roosevelt 1980).

The Amazon Wars

Some features of my career were shaped by the situation in South American archaeology when I came in the 1970s, and this situation had both detriments and benefits. One detriment was conflict: what Charles Mann calls 'the Amazon wars'. Betty Meggers and Clifford Evans, starting 20th midcentury, had rather a monopoly on archaeology there until Lathrap came into the picture in the late fifties with a squad of graduate students. Battles were in full swing when I began my dissertation work, adding tinder to a fire already hot enough to singe the unwary. The wars involved angry, emotional interchanges, captious criticisms, and attempts to prevent others from getting permits, grants, and publishing contracts. The records of it are in the combatants' institutions' archives, some of which I've published (Roosevelt 1991, 1995). However, serendipity and momentum won me digging permits first in the Venezuelan Orinoco through the support of Rouse and Jose Crucent and then in the Brazilian Amazon through the help of Wesley Hurt and José Seixas Lourenco.

The wars tended to polarize debates and encourage *ad hominem* attacks, which can obscure the important epistemological issues. The wars also discouraged people from coming to work in the region, leaving the culture history poorly known. But the vacuum left me a wide opening to make a mark. Almost any new knowledge could make an impact. I had the fun of documenting the first developmental sequence in the Lower Amazon. After my example showed that one could work there and survive, others began independent projects in Amazonia. Their entry has benefited me, for they have made discoveries parallel to mine, bolstering my findings. Another maxim: The More the Merrier.

Starting out in an area fraught with academic conflict, I saw that people sometimes articulated theoretical or methodological oppositions for political purposes or emotional reasons, rather than to define true epistemological alternatives. This observation was a very important one, for it meant that I knew not to cut myself off from understanding by rejecting supposedly opposing viewpoints. For example, I reject neither processualism nor post-processualism but find insights in both. Nor, as a human ecologist, do I reject the principles of sociobiology but only its empirically unsupported hypotheses (Roosevelt 2002a). It is in the refinement and integration of different bodies of theory, not in their opposition, that important steps for-

ward in knowledge come. Unlike some anthropologists, I think DNA research has the potential to revolutionize evolutionary studies of both humans and non-human primates, and I expect that the current notions of sociobiology soon will be replaced. The stakes are high. What we are learning from prehistory about the environmental contexts of early human evolution has potential for explaining and preventing the main psychological, social and degenerative diseases of the industrial world. Out of Discord, Understanding (another maxim).

CAREER FORGING

Serendipity in jobs

I got my first job because I had to find work when I left my Ford Fellowship behind at Penn and came to Columbia. After sending 50 job applications, I got 10 answers: five turndowns, four invitations to meet and be encouraged, and one job offer. At Stanford, I had made friends with Ann Hitchcock, now chief curator of the US Park Service. The Girl Network kicked in for me when she recommended me to the Museum of the American Indian. She'd been offered a job there but had to take a Smithsonian Fellowship instead. What with her and Junius' help, I got the job on the spot. Thus began a wonderful 15 years during which I immersed myself in the material culture of the indigenous New World. Piled in abandon on the shelves of that institution, I saw the evidence of human genius and practicality at work throughout the hemisphere.

At MAI I got many opportunities that the women in larger organizations did not. From the start I got to be the principal investigator on government grants, so I could design research to suit my own research questions. I also had the chance to work with knowledgeable professionals who encouraged me and taught me about museum curation, conservation, and exhibition. I worked my way up the ranks while in grad school, and by the award of my PhD, I had become the Curator for the Latin American collections. Subsequently, at AMNH, the same kind of openness prevailed. Craig Morris Chair of Anthropology and George Gardner, Head of Exhibits, made it possible for me to do field research, analyze my collections, do exhibits, and write books.

Other opportunities came up to explore new horizons and exchange ideas: a visiting professorship at the University of Florida, where I got to know Nigel Smith, the Amazon geographer, a visiting lectureship

at Harvard, where I debated with Irve DeVore, and a Research Fellow position at the Center for Remote Sensing at BU, where I met Farouk El- Baz and learned new forms of geoarchaeology.

These various job experiences together then helped me get a job combining curation and teaching in 1991. I became Curator of Archaeology at the Field Museum and then Professor of Anthropology at University of Illinois at Chicago, where I now work. UIC is remarkable for its vibrant, diverse, and hard-working student body.

For my next job, I'm hoping for a university with a good museum for my collections and a strong international focus, so I can build studies-abroad research programs and recruit students from the countries of my field areas.

Serendipity in research

An early example of serendipity and flexibility in my career was the choice of a dissertation topic. Inspired by G Reichel-Dolmatoff's theories about a shift from manioc to maize cultivation in Caribbean Columbia, I felt that archaeobotanical techniques would allow me to prove or disprove them. But when I wrote him and applied to work there, he said he did not want anyone coming into his area, even someone who fully expected to confirm his theories. Sadly, I went back to Rouse with the news. He said, 'Well, would you like to come with me and Fred Olsen to the Orinoco? We have to check on the dating of some sites. Maybe you could find a site to work at, yourself.' I happily went and decided that Parmana would be a great place to test GR-D's theories. In fact, it was much better because the Orinoco is on the Amazon frontier, the lodestone of nearly all theories about ecology and human evolution in the tropics. My rejection in Colombia did me a favor, for my future findings were to resonate further, because of the archetypal significance of the Amazon.

Another example of luck was my first research permit in Brazil. There had been a tradition of archaeologists maintaining regional fiefdoms and not allowing others in to work by denying permits. (I've published on the practice in my Marajo book, 1991a.) After my dissertation, I wanted to pursue its implications in the Amazon. Assuming that a permit to dig at Santarem would not be possible, I got a National Endowment for the Arts (NEA) grant to review museum archaeological collections from Amazonia worldwide. While in Belem, I met Jose Seixas Lourenco,

the director of the Goeldi Museum, one the biggest repositories. We gave each other publications, exchanged routine pleasantries about each other's work, and, lo and behold, the next day, he came and said, 'is there any chance, any chance at all, you'd be willing to come to dig in Brazil?' Well, it was my heart's desire. Pursuing what *could* be done, in lieu of what I originally wanted to do, in the end got me the original goal and a great deal more.

More bad into good

I'm the sort of person who gets enlisted to work on committees and boards: the Council for Museum Anthropology, American Anthropological Association, American Association of Museums, Center for Remote Sensing, Rainforest Alliance, National Science Foundation (NSF), etc. After getting my PhD, I assumed I'd be drafted into committee work at the Society for American Archaeology (SAA), but no such call came. What happened instead, however, was better for my career and intellectual development. The American Association for the Advancement of Science (AAAS) needed an Anthro Section Secretary, a sort of operations and liaison officer. I was recruited to run and was elected easily. So began more than two decades of experience in that organization, moving from anthropology section, to three-term AAAS member of the Board of Directors, and, recently, nominations committee member. From that service I learned about science policy and the inner workings of the US scientific community and also how to write for *Science*, a journal of much greater scope and exigence than SAA's *American Antiquity*. AAAS was interested in archaeologists, despite our measly membership, and so I got to disseminate my and others' research far and wide not only to the huge constituency of the magazine but also to the thousands at the annual meeting. This helped both me and archaeology in general. But most important, I learned more about other disciplines and could link up with collaborators for my projects.

Special events

My career in archaeology has been consistently absorbing and delightful, but there have been some special moments that stand out.

One was a day at Corozal, Parmana, where I was trying to trace the prehistory of cultivation (Roosevelt 1980, 1997a). I had assumed that there would be no problem recovering plant remains, but expectations

can always be wrong. In this case, though, they were right on. As I dug down with my local team, large clumps of crusty black, shiny objects began to appear in the dirt. There were not just a few corn kernels, but many handfuls of them, still attached to the cobs. The Parmanans indeed had cultivated corn. Also at the site were human skeletons, and work I did on them with Nick van der Merwe revealed a bone chemistry typical of staple maize consumption (van der Merwe et al 1981).

Another great time was when we were putting in excavations to test features in the geophysical maps Bruce Bevan had made with us at Teso dos Bichos, Marajo Island, Brazil (Roosevelt 1991a, Bevan & Roosevelt 2003). The site had an oval cluster of magnetic lows around an empty central area. Our expectation was that they represented hearths and that therefore we could find residences around them. Sure enough, each excavation we placed on an anomaly revealed a fired adobe hearth cluster on floors, with one exception, a burned floor. We concluded, therefore, that our magnetometer maps could recover site plans at sites too large to adequately sample by digging. The site layouts showed that the mounds were habitations, not just empty ceremonial centers, as had been assumed. This implied a considerable prehistoric population, contrary to existing theories.

Another special event was the award of my five-year \$250,000 MacArthur fellowship. It got favorable publicity for my work and not a few job offers and publication opportunities. Usable for whatever I wanted, the MacArthur made a big difference in the research. It supplemented my National Science Foundation (NSF), National Endowment for the Humanities (NEH), and Fulbright grants, allowing me more scope for innovation than conventional funding allowed. Government programs tend to channel research into disciplinary categories and conventional lines of thinking. The problem with this practice is that scientific phenomena just don't obligingly shape themselves within disciplinary bounds, and consensus is always out-of-date, so adherence to it prevents new understandings and research directions. When new research areas are opened up, few reviewers know much about them. When I applied to NSF for funds to research the Amazon developmental sequence, reviewers stated that you couldn't do that kind of research in tropical rainforests because biological materials were not preserved.

To persuade NSF such research indeed was viable, I followed the maxim, Seeing is Believing. For example, I borrowed a large male human femur from the Marajo skeletal collections at AMNH, took it down to NSF and waved it at the program officer's head. I got the grants. NSF, however, never funded my research sites that were to produce the most important findings: Parmana, Monte Alegre, Santarem, and Taperinha. It was left to NEH, Fulbright, Wenner-Gren, and MacArthur to do that. For the problem NSF has in being sufficiently transdisciplinary and forward-looking, I've thought of ways that it could better reorganize its programs and reviewing, some of them already in use in European Commission funding programs. Maybe someday they can be adopted by the NSF.

Taperinha was a revelation (Roosevelt et al 1991; Roosevelt 1998b; Roosevelt 1995b). The 19th century sources and my date of about 6000 BP on a shell from an old Taperinha collection at Harvard suggested that the site might be an early pottery site, and got me a grant to dig there. But we did not know how early until one day in 1988. We were waiting dolefully and expensively in a hotel in Belem with our geophysical equipment stuck in customs because of a strike, when I got a long fax from Oxford University Accelerator laboratory. The thirteen dates on the charcoal, pottery, and shells from our excavations fell at about 7000 BP, the earliest dates from such a site in the Americas. Later, the initial pottery layer at Pedra Pintada would yield even earlier beginning dates, c 7500 BP.

Pedra Pintada offered many Great Moments (Roosevelt et al 1996, Roosevelt et al 2002). This site turned out to have a thick preceramic occupation layer dating between about 11,000 and 10,000 years. It lay under a deep sterile layer. When I reached that one below the ceramic stage layers, and found nothing, I thought, well, this is the end of the site. I kept on digging, however, to reach bedrock, and suddenly something sharp and small snapped up and hit my face. It was a lithic thinning flake, and from then on the soil was literally black with carbonized plant remains, paint, and burnt bone amid chipped stone points and their debitage. We had hit pay dirt!

My team of diggers has begun to approach each season with excitement and expectation, speculating on what amazing new information will come out. I warn them that that couldn't happen every year, that the actual process of discovery is slow and tedious.

They just roll their eyes and say, 'yeah, right'. They don't believe me any more because we keep encountering significant sites and materials – prehistoric wooden harpoon points in the Xingu, handaxe sites in the Congo basin. What will be next?

THE PROCESS OF DISCOVERY

Boiled down, my academic interests have come to center on long-term human-environment interaction and its evolutionary and practical implications. With time, my focus has both sharpened and broadened.

Early farming

I got into the issue of subsistence and settlement in the humid tropical environment for a couple of reasons. One reason was my experience in Scotty MacNeish's Ayacucho Project. Gary had taken me and other students to Peru to work on a ceramic component of that project. There, I got hands-on exposure to archaeobotanical techniques. The other reason was a seminar I had with two fellow graduate students, John Hyslop and Mike Snarskis. We persuaded the department to let us conduct one to look at the Western Hemisphere 'intermediate area'. We were curious about its theoretical role.

The idea at the time was that agriculture has been developed first in the nuclear areas, Mesoamerica and the Central Andes. It happened there, supposedly, because they were ecologically diverse and had good soils. Agriculture, then, supported the rise of states and civilization and all its arts. In the intermediate area, however, according to this view, soils were too poor for agriculture, and food resources few. For the seminar, I chose to research Greater Amazonia. Because of the theory, the first thing I looked at was its agronomy. To my surprise, I found that the soil classes identified for the area were the same as those in our US corn belt! It was clear, therefore, that they could not have been a barrier to agriculture. This was the beginning of my serious doubts about the then-accepted culture history of the Americas. I resolved to get myself down there and see what had happened.

By then, I was equipped with nearly all that I needed to embark on my own research career. My and others' beliefs about causality at the time were partly wrong, as such beliefs always are. But this did not matter, because the combination of theory and method allowed me to refine or refute them eventual-

ly and gain new understandings for myself.

The ethnographic present

Through working in Amazonia, where there are still indigenous peoples and rural peasants on the land, I came to see that the best paradigmatic theory was not an either, or proposition but was integrative. Harris, as many archaeologists do still today, insisted on the primacy of material factors in human causality. But by working among local populations, I could not but observe that non-material factors were often a strong motivation for their behavior and that different spheres of their lives might have different causalities. Although cultural materialist tended to reject Claude Levi-Strauss structuralism, I saw that in their thinking indigenous Amazonians indeed exemplified the concepts he outlined.

A related inference was that the ethnographic present was not necessarily a good guide to what went before. In the Orinoco, for example, native cultivation focused on manioc, not maize, contrary to some other parts of the hemisphere. However, my excavations showed that at one time, people had intensively cultivated maize there, too. Thus began my use of a reverse ethnoarchaeology (Roosevelt 1989). I began to test ethnographic theory with archaeology.

Amazon social anthropologists, oddly, were not at all offended at this approach, though it refuted some of their explanations. Sydel Silverman, then President at Wenner-Gren, put that foundation behind my approach by funding an international conference where I could try to persuade them that the archaeology results required changes in ethnographic interpretation. The result was that they developed an intense interest in archaeology, and we published a book together (Roosevelt 1994). Even Levi-Strauss, who might have been expected to object, jumped on the band-wagon, when he read our and others' books.

Later on, I further integrated ethnography and archaeology in my thinking. My discussions with Jim van Stone at the Field Museum helped me refine my thinking about their relationship (and he also taught me how to hunt with a harpoon).

Sedentary foragers and complex tribes

The archaeology of the tropical lowlands that emerged from our work brought several other theoretical revelations that together fundamentally altered

my view of human evolution and human society. Through the Lower Amazon findings in the 1980s, I became aware that there had been a long, complex history of cultural adaptation, not just one 'tropical forest culture', as consensus had held. It was clear that the environment had not limited people to one particular adaptation and that human interactions were important in causality. Four counterintuitive conclusions emerged:

- 1 the first pottery makers there were fishing people, not agriculturalists
- 2 the early complex societies were not agricultural
- 3 those societies were not organized as states or centralized chiefdoms
- 4 some later complex cultures, however, were both stratified and centralized, and
- 5 they were agricultural.

Most of these findings went against the then-current beliefs about the history of human occupation in the humid tropics, beliefs that I had shared when I did my dissertation work. Many other researchers have come to similar conclusions since the 1980s.

Forest foragers and human evolution

Later, at Caverna da Pedra Pintada in the 1990s, I realized that the tendency for anthropologists to project ethnographic patterns into the past had obscured another important facet of human evolution. Ethnographic evidence about living humans in rainforests had shown that all peoples have relied to some degree on farming. This evidence persuaded many anthropologists that, therefore, true foragers could not survive in such habitats. Part of their reasoning was that early hunter-gathers would be big-game hunters, and, admittedly, there's not a lot of big game to be found in the forest.

But in prehistoric times hunting and gathering people had indeed lived in tropical rainforests. At Pedra Pintada we found that late Pleistocene people had lived without agriculture for more than a thousand years, foraging for tropical fruits, nuts and small fauna (Roosevelt et al 1996). Thus at least some early tropical foragers were not big game hunters and so not necessarily provisioned by males. Despite the numerous identified plant and animal remains, there was not a scrap of mastodon or sloth: just fish, shellfish, turtles, toads, lizards, rodents, and other smaller animals, all foods of which women and children tend to make the greatest harvests, among living groups.

I guess all ideas have their eras, and just about the time I was working at that cave, several other archaeologists – Mike Moseley, Susan de France, Richie Burger, Dan Sandweiss, and others on the south Peruvian coast, for example – had also found evidence for distinctive local adaptations by broad-spectrum foraging, rather than big-game hunting. These unexpected results motivated me to begin an intensive review of the record of the early peopling of the Americas. I read the Paleoindian literature, visited many sites throughout the Americas, including Clovis, looked at many research collections, and, with collaborators John Douglas, Linda Brown, and Ellen Quinn, and others, took a good, hard look at the statistics of dating. In the end, my ideas coalesced into a new view of the first Americans, one I call 'Clovis in Context' (Roosevelt et al 2002). By this view, Clovis was not the ancestor but just one specialized big-game hunting culture among many other Paleoindian cultures most of which were broad-spectrum foragers.

Furthermore, the biological remains and isotopic chemistry at Pedra Pintada and other, later, sites suggested that the ancient people had had impacts on their habitats. As Bill Balee has argued from ethnocological data and Nigel Smith from soil data, Amazonia's forests are to some degree a human artifact. According to the archaeological sequence I obtained, the nature and degree of impact varied. Societies with larger populations and more stratified organization had more impact. Small scale, locally based societies had less. These variations have obvious relevance to resource conservation and development in the tropics (Kawanabe et al 1999; Roosevelt 1995a). If a threatened forest community has been occupied by humans for thousands of years in prehistory and yet there were no known species extinctions, then eliminating native people from the area is not going to solve its current problems. One must look instead to the region's relations with the outside world. As Steve Schwartzman and others have shown, conservation problems are closely linked to the presence and interference of entities from the outside: urban migrants, government agencies, and companies or banks based in other parts of the country or the world. There's no use and no justification for vilifying the local people, whose own cultural and biological survival is itself in danger. Their participation is necessary and beneficial.

Finally, our and others' findings about paleoecolo-

gy in the Amazon have tended to refute the well-accepted theory of Pleistocene tropical forest *refugia*. We have found absolutely no evidence for disappearance of the forest in Amazonia during late Pleistocene glacials. There are no remains that would indicate a grassy, open, cold savanna. Rather, the pollen, macrospecimens, sedimentology, and isotopic chemistry all indicate continuation of the habitats of tropical forest and river, albeit with varying hydrology and community composition.

These results were what motivated me to go over and interfere in African paleoanthropology, even while continuing my project in Amazonia. The big archaeological gap on the map in Africa is the Congo basin, where assumptions about the hostility of the rainforest for humans and for archaeological sites seemed to have discouraged all but a few archaeologists, most of whom had bought into the *refugium* theory. When I looked at the literature for the continent, I found that a number of researchers outside the central basin already had made discoveries that threatened the traditional story of savanna big-game hunting hominid ancestors. They include Andrew Hill, Stan Ambrose, Jean Sept, Alison Brooks, Raymonde Bonnefille, Ron Clarke, Phillip Tobias, Alan Walker, C Rayner, Charles Brain, Frank Brown, and others. It was especially interesting to me that some of them were finding evidence that even the east and South African hominid sites were in humid tropical forests at the time of their occupation. What had not yet been done, though, was to go to the very core of today's tropical rainforest in the Congo basin. If the early ancestors had been reliant

on tropical forests, and if continental climate had fluctuated, then the longest and most continuous developmental sequences ought to be found there, where high heat and humidity create a habitat well-buffered from climate change even during glacials.

My experience in Amazonia gave me a certain advantage. I knew how to live and dig in the rainforest. We started to work in Africa in 1997. Since then, we have surveyed in Central African Republic, Republic of Congo, and Democratic Republic of Congo and have dug several seasons at Ngolio near Bayanga in Central Africa Republic. The preceramic component there has not yet been dated, but it's likely that either it or some other nearby site is datable. We had another Great Moment when we were digging at that site. The layer of early-Acheulean-like lithics there turned out to be embedded firmly in a strange, soft, layer. 'What is this stuff?', I asked Bertin Mbongo, our geologist. He looked at it with his loupe and said, 'Volcanic ash!' It was a tuff that had fallen shortly before the lithics were deposited. Not just Ngolio, but many sites in this part of Africa turn out to have such layers, which probably are ash blown east from the arc of volcanoes in which stands Mt Cameroon. The tuffs' presence means that options for dating the human sequence in the central Congo are much better than had been assumed. Sites earlier than radiocarbon dating can in principle be dated by potassium/argon or argon/argon isotope ratios in the tuff crystals.

So began another saga for me, the search for the role of the tropical forests in early human evolution.

SELECTED BIBLIOGRAPHY

1972. Handedness in Prehistory: A Study of Tools from Ancient Chile. *Indian Notes* 9(4): 98–109. New York: Museum of the American Indian.

1980. *Parmana: Prehistoric Maize and Manioc Subsistence along the Amazon and Orinoco*. Studies in Archaeology. New York: Academic Press.

1981. (second author with van der Merwe, N & Vogel, JC). Isotopic Evidence for Prehistoric Subsistence Change at Parmana, Venezuela. *Nature* 292(5823): 536–538.

1984a. Population, Health, and the Evolution of Subsistence: Conclusions. In Cohen, M & Armelagos, G (eds) *Paleopathology at the Origins of Agriculture*. New York: Academic Press:559–583.

1984b. Problems Interpreting the Spread of Cultivated Plants in the New World. In Stone, D (ed) *Pre-Columbian*

Plant Migration. *Papers of the Peabody Museum of Archaeology and Ethnology* 76:1–18. Cambridge: Harvard University.

1987. The Evolution of Human Diets. In Harris, M & Ross, E (eds) *Food and Evolution: Toward a Theory of Human Food Habit*. Philadelphia: Temple University Press:565–578.

1988. Microcomputers in the Lower Amazon Project. *Advances in Computer Archaeology* 4:41–53.

1989. Resource Management in the Amazon Basin Before the European Conquest: Beyond Ethnographic Projection. In Posey, D & Balee, W (eds) *Natural Resource Management by Indigenous and Folk Societies in Amazonia*. New York Botanical Garden. *Advances in Economic Botany*, No 7:30–61.

1991a. *Mound-builders of the Amazon: Geophysical Archaeology on Marajo Island, Brazil*. Studies in Archaeology. San Diego: Academic Press.

1991b. Determinismo Ecologico na Interpretacao do Desenvolvimento Social Indigena da Amazonia. In Neves, W (ed) *Origens, Adaptacoes, e Diversidade Biologica do Homen Nativo da Amazonia*. Belem: Museu Goeldi:103–141.

1991c (first author with Housley, R, Imazio da Silveira, I, Maranca, S & Johnson, R). Eighth Millennium Pottery from a Prehistoric Shell Midden in the Brazilian Amazon. *Science* 254 (5038):1621–1624.

1992a. (Reprinted 2001) Secrets of the Forest: An Archaeologist Reappraises the Past – and Future – of Amazonia. *The Sciences* November/December: 22–28. New York: New York Academy of Sciences.

1992b. Sociedades Prehistoricas do Amazonas Brasileiro. In Farias, F & Diaz, J (eds) *Brasil nas Vesperas do Mundo Moderno: Brazil e Africa*. 2 vols. Lisbon: Comissao Nacional para as Comemoracoes dos Descobrimentos Portugueses. Vol 1:17–45.

1992c. (Second edition 1998) Arqueologia Amazonica. In da Cunha, MC (ed) *Historia dos Indios no Brazil*. Sao Paulo: Companhia das Letras: Secretaria Municipal de Cultura: 53–86.

1993. The Rise and Fall of the Amazon Chiefdoms. In Taylor, A-C & Descola, P (eds) *Le Remontee de l'Amazone: Anthropologie et Histoire des Societes Amazoniennes*. *L'Homme* Special Issue 33 (126–128): 255–284.

1994. *Amazonian Indians from Prehistory to the Present: Anthropological Perspectives*. Tucson: University of Arizona Press.

1995a. Educating Natural Scientists about the Environment. In Arcury, TA & Johnson, BR (eds) *Anthropological Contributions to Environmental Education*. *Practicing Anthropology* 17(4): 25–28.

1995b. Early Pottery in the Amazon: Twenty Years of Scholarly Obscurity. In Barnett, W & Hoopes, J (eds) *The Emergence of Pottery: Technology and Innovation in Ancient Societies*. Washington, DC: Smithsonian Institution:115–131.

1996 (first author with Lima Costa, M, Lopes Machado, C, Michab, M, Mercier, N, Valladas, H, Feathers, J, Barnett, W, Imazio da Silveira, M, Henderson, A, Sliva, J, Chernoff, B, Reese, D, Holman, JA, Toth, N & Schick, K). Paleoindian Cave Dwellers in the Amazon: The Peopling of the Americas. *Science* 272:373–384.

- 1997a. *Excavations at Corozaal, Venezuela: Stratigraphy and Ceramic Seriation*. New Haven: Monograph, Yale University Publications in Anthropology, No 83.
- 1997b (author with Jasanoff, S, Colwell, R, Dresselhaus, MS, Goldman, RD, Greenwood, MRC, Huang, AS, Lester, W, Levin, SA, Linn, MC, Lubchenco, J, Novacek, MJ, Taylor, J & Wexler, N, in alphabetical order). Conversations with the Community: AAAS at the Millennium. *Science* 278:2066–2067.
- 1997c (first author with Lima da Costa, M, Brown, L, Douglas, JE, O'Donnell, M, Quinn, E, Kemp, J, Lopes Machado, C, Imazio da Silveira, M, Feathers, J & Henderson, A). Dating a Paleoindian Site in the Amazon in Comparison with Clovis Culture. *Science* 275:1950–1952.
- 1998a. Ancient and Modern Hunter-Gatherers of Lowland South America: An Evolutionary Problem. In Balee, W (ed) *Advances in Historical Ecology*. New York: Columbia University Press:190–212.
- 1998b. Paleoindian and Archaic Occupations in the Lower Amazon, Brazil: A Summary and Comparison. In Plew, M (ed) *Festschrift Honoring Wesley Hurt*. Lanham, Md: University Press of America:165–192.
- 1998c (appeared 1999) (third author with Morbeck, ME & Bateson, MC). Lita Osmundsen (1926–1998). *American Anthropologist* 100(3): 753–756.
- 1998d (last author with Michab, M, Feathers, JK, Joron, J-L, Mercier, N, Selos, M, Valladas, H & Reyss, J-L). Luminescence Dates for the Paleoindian Site of Pedra Pintada, Brazil. *Quaternary Geochronology* 17(11):1041–1046.
- 1999a (third editor with Kawanabe, H & Coulter, GW). *Ancient Lakes, Their Cultural and Biological Diversity*. Ghent, Belgium: Kenobi Publications.
- 1999b. Twelve Thousand Years of Human-Environment Interaction in the Amazon Floodplain. In Padoch, C, Ayres, JM, Pinedo-Vasquez, M & Henderson, A (eds) *Diversity, Development, and Conservation in Amazonia's Whitewater Floodplains, Advances in Economic Botany*, Vol 13. New York: New York Botanical Garden:371–392.
- 1999c. Dating the Rock Art at Monte Alegre, Brazil. In Strecker, MA & Bahn, P (eds) *Dating and the Earliest Rock Art*. Oxford: Oxbow Books:35–40.
- 1999d. The Maritime-Highland-Forest Dynamic and the Origins of Complex Society. In Salomon, F & Schwartz, S (eds) *History of the Native Peoples of the Americas. South America, Part 1*. New York: Cambridge University Press:264–349.
- 1999e. Ancient Hunter-Gatherers of South America. In *Cambridge University Encyclopedia of Hunter-Gatherers*, edited by R. Lee and R. Daly. Cambridge: Cambridge University Press: 86–92.
- 1999f. The Development of Prehistoric Complex Societies: Amazonia, A Tropical Forest. For Bacus, EA & Lucero, L (eds) *Complex Polities in the Ancient Tropical World*. Archaeological Papers of the American Anthropological Association Number 9:13–34.
- 2000a. (Reprinted in 2002). Who's on First? There's Still No End to the Controversy over When and How Humans Populated the New World. *Natural History*, October/November, 7: 76–79. New York: American Museum of Natural History.

2000b. The Lower Amazon: A Dynamic Human Habitat. In Lentz, DL (ed) *Imperfect Balance: Landscape Transformations in the Precolumbian Americas*. New York: Columbia University Press: 455–491.

2000c. New Information from Old Collections: The Interface of Science and Systematic Collections. In Hitchcock, A (ed) *Museum Issues and Trends. Cultural Resource Management* 23(5): 25–29. US Department of the Interior, National Park Service, Cultural Resources.

2002a. Gender in Human Nature: Sociobiology Revisited and Revised. In Nelson, SM & Rosen-Ayalon, M (eds) *In Pursuit of Gender: Worldwide Archaeological Approaches*. Walnut Creek, CA: Altamira Press:355–376.

2002b (first author with Douglas, J & Brown, L). Migrations and Adaptations of the First Americans: Clovis and Pre-Clovis Viewed from South America. In Jablonski, N (ed) *The First Americans: The Pleistocene Colonization of the New World. Memoirs of the California Academy of Sciences* No 27. Berkeley: University of California Press and the California Academy of Sciences:159–236.

2002c. Early Amazonian. In Peregrine, PN & Ember, M (eds) *Encyclopedia of Prehistory*. New York: Kluwer Academic/Plenum Publishers and HRAF:75–77.

2002d. Late Amazonian. In Peregrine, PN & Ember, M (eds) *Encyclopedia of Prehistory*. New York: Kluwer Academic/Plenum Publishers and HRAF:289–293.

2003a (second co-author with Bevan, BW). Geoarchaeological Exploration of Guajara, A Prehistoric Earth Mound in Brazil. *Geoarchaeology*. 18(3): 287–331.

2003b Transcience. La Ciencia Transdisciplinaria *La Vanguardia* 6 November 2003 (Spain). Trans-scienza: La Scienza Oltre I Proprio Limiti. *Quark* 9: 34 (Italy). Transdisciplinari Veda: Prolomeni Banier Mezi Obory. *Vesmir* November (Czech Republic).