CHAPTER TWENTY-ONE

Roots of Conflict (Hawai‘i and Maui, 2001–2009)

In early February 2001, the National Science Foundation (NSF) put out a call for proposals under a new program, Biocomplexity in the Environment, which would support research on “dynamically coupled human and natural systems.” The program seemed to be a perfect fit with the research I had already been conducting on the relationships between people and island ecosystems, especially in Mangaia and Hawai‘i. Moreover, it was generously funded. Why not submit a proposal, I asked myself. Nothing ventured, nothing gained.

Knowing that a successful application would have to be multidisciplinary, I immediately thought of Peter Vitousek across San Francisco Bay at Stanford University. I had met Vitousek—who, like me, grew up in the islands—during a sabbatical year at the Center for Advanced Study in the Behavioral Sciences at Palo Alto in 1996–1997. Vitousek worked on the flow of nutrients in Hawaiian ecosystems, something we could link to my own work on ancient Hawaiian agricultural systems.

I telephoned Vitousek and asked him, “Peter, have you seen this call for proposals in NSF’s new program on human and natural systems?”

“Yes,” came the reply. “I was just getting ready to call you myself.” The two of us had already bandied about vague ideas of future collaboration. Now that a funding source was evident, we agreed to pursue the opportunity.

NSF’s new Biocomplexity in the Environment program called for “quantitative modeling,” meaning we would need someone who could bridge empirical fieldwork with theoretical models and computer simulation. Vitousek suggested that I contact Shripad Tuljapurkar, a population biologist about to join Stanford’s Biology Department. As ignorant of Polynesian archaeology as I was of mortality and fertility schedules, Tuljapurkar was nonetheless intrigued by the larger issues of human population and landscape interaction that we wanted to tackle. Meanwhile, Vitousek called Oliver Chadwick, a “renegade” soil scientist at the University of California, Santa Barbara, who studied Hawaiian soils. A knowledge of Hawaiian soils would be critical to understanding how the intensive Hawaiian farming systems had operated.

I wanted to focus on Kahikinui, where over six years I had built up a database on Hawaiian land use and agriculture. Vitousek suggested that we also include the Kohala District of Hawaiʻi Island, where he and Chadwick had both been working. Kohala's broad leeward slopes had once been thickly inhabited by Native Hawaiians. As a Hawaiian proverb states, "*Leʻi o Kohala i ka nuku na kānaka*" (Covered is Kohala with men to the very point of land).[1]

I was aware of Kohala's potential for studying traditional Hawaiian agriculture and land use. In 1968, Richard Pearson of the University of Hawaiʻi had discovered that a vast dryland field system once extended over leeward Kohala, which was densely planted in sweet potatoes, dryland taro, sugarcane, and other crops. This flourishing agricultural system was indelibly inscribed over the Kohala landscape by a reticulate grid of closely spaced, low stone-and-earth embankments bounding ancient fields. The field embankments were crosscut by curbstone-lined trails that run up- and downslope delineating *ʻili* and *ahupuaʻa* boundaries.[2] Covering roughly sixty square kilometers, the Kohala field system is one of the most remarkable archaeological landscapes in all of Polynesia (Fig. 21.1).

Paul Rosendahl had mapped part of this grid of ancient garden walls and trails, along with residential features, in upland Lapakahi, for his University of Hawaiʻi doctoral dissertation. Rosendahl showed me—when I visited him in the field in the early 1970s—how the stone-and-earth embankments sometimes ran under, but at other times abutted with, the curbstone-lined trails.[3] I incorporated this Lapakahi evidence in my 1984 book *The Evolution of the Polynesian Chiefdoms,* using Rosendahl's map to define several phases of development in Kohala's agricultural landscape.[4] I showed how an early pattern of a few large fields was transformed over time into one with more fields of smaller and more uniform size, a process known as agricultural *intensification,* or increased production per unit area. Such intensification, I argued, was linked to the rise of stratified polities in Hawaiʻi. As the landscape was divided into a grid of increasingly uniform parcels, the *konohiki* or land managers were able to control production—and the collection of surplus—more efficiently.

In the late 1990s, Michael Graves of the University of Hawaiʻi had recommenced fieldwork in Kohala. Technological advances in global positioning (GPS) and geographic information systems (GIS) made it possible for Graves to survey on a larger regional scale. Teaming up with Thegn Ladefoged, an expert in GPS and GIS applications in archaeology, Graves investigated the field system over large parts of leeward Kohala.[5] Knowing of their work, Vitousek and I invited Graves and Ladefoged to join our project.

Our team drafted the NSF proposal, which focused on the intensive dryland agriculture that had underwritten the staple economies of the emerging archaic

Figure 21.1. The Kohala field system, covering sixty square kilometers, consists of a reticulate grid of earthen and stone embankments demarcating ancient sweet potato and taro fields, crosscut by stone-lined trails. A small part of the system is visible here, from the summit of Puʻu Kehena, as the late afternoon sun creates shadows behind the low embankments.

states of Hawaiʻi and Maui Islands in the centuries leading up to European contact. We argued that the development of these rain-fed agricultural landscapes was a key to understanding how Hawaiian society had been transformed from complex chiefdoms to archaic states. We would combine field studies of the agricultural systems and their "biogeochemical gradients" with computer simulations of agricultural production.

We argued that the lessons to be learned in Kohala and Kahikinui had implications beyond local history. Hawaiʻi was a kind of "model system," we wrote, for investigating linkages among population, land, intensive agriculture, and sociopolitical organization. The processes that had driven intensification and sociopolitical change in ancient Hawaiʻi might be broadly applicable to many other parts of the world. In short, Hawaiʻi was a microcosm of the world.

Our proposal, "Human Ecodynamics in the Hawaiian Ecosystem, 1200 to 200 Years before the Present," had to be approved by research administrators at five universities, a minor bureaucratic feat. I was sure that the competition for this major program would be stiff, steeling myself for a decision of "reject and resubmit." To our surprise and delight, four months later we were informed that the review panel had ranked our proposal as one of the highest. With a budget of $1.4 million it was by far the largest research grant I had ever received. Now, the challenge was to show that our multidisciplinary team could deliver the goods.

The two Jeeps crawled up a rarely used four-wheel drive track on the steep lava slopes below Puʻu Pane, a cinder cone perched at 3,900 feet in the Mahamenui region of Kahikinui. Sitting next to me was soil scientist Chadwick. Driving the Jeep behind us was Chadwick's postdoctoral student Tony Hartshorn. For the past few days we had been digging soil pits spaced out along two transects over the Kahikinui landscape. One transect—which Chadwick called our "chronosequence"—ran at an elevation of about 1,500 feet, allowing us to sample a variety of lava flows of different ages, holding elevation (and hence also rainfall) constant. Dave Sherrod of the U.S. Geological Survey had given us a copy of his map showing the ages of these flows. The second transect, the one we were now working on, was a "climo-sequence" running from the coast to the uplands, all on the same 226,000-year-old lava flow. It would show the effects of elevation—and increasing rainfall—on soil development, holding geological age constant.

Over the hum of the Jeep engine, Chadwick and I chatted about our sampling strategy. "I want to get as high up on the old Kula surface as we can this morning," Chadwick told me. "We got some good samples yesterday near the coast and in the intermediate zone. If we can sample a location up where the fog drip has a daily effect, we'll be able to determine how soils of the same age develop in relation to water input."

I nodded in agreement. "I'll get us as high as this road goes. Then we'll have to hike up the slopes on foot."

"How far upslope have you guys found evidence for Hawaiian habitation and cultivation?" Chadwick asked.

Over a number of years of walking the rugged lava landscape of Kahikinui, I had determined that the Native Hawaiian population had been densely concentrated in a zone between about 400 and 600 meters' elevation. "The main zone of intensive land use starts to peter out around 2,000 feet elevation," I replied. "Above that you get occasional small features, and virtually nothing at all above 3,000 feet."

Just why Hawaiian land use in Kahikinui had been narrowly concentrated in a band between 400 and 600 meters had been puzzling me. The reason for a lack of cultivation and permanent habitation *below* about 400 meters seemed obvious:

the lack of water, either for growing crops or for other human needs. But why had people not moved farther up the mountain slopes, above 900 meters? Fog drip and rainfall increase as one moves higher up the face of Haleakalā. Sweet potato, a crop originally domesticated in the Andes of South America, readily tolerates low nighttime temperatures. So why had the Hawaiian population not expanded farther inland, up to say 1,500 or even 2,000 meters? Had the process of inland expansion been truncated by the devastating effects of Western contact? Or was something else responsible?

Chadwick seemed to be reading my thoughts. "Over in Kohala," he said, "I've found that there is a nonlinear relationship between rainfall and soil nutrients. Instead of just declining gradually as rainfall increases, soil nutrients hit a threshold—an invisible cliff—at about 1,500 mm of annual rainfall. Above that rainfall level leaching increases dramatically and soil nutrients plummet."

"Right, Oliver," I responded. "I remember the diagram you showed us when the team met in Kohala earlier this year. It really looks like that threshold closely matches the upper limit of the Kohala field system. I hope that the soil samples Peter is collecting in Kohala this summer will allow us to see whether the field system had reached its physical limits to expansion."

The jeep track was about to cross a ravine where periodic flash floods had cut into the lava slope. The track was little used, and the tire ruts were partly obscured by thick lantana. Always cautious, I made sure I could see bare rock ahead for my vehicle's tires to grip onto. The Jeep lurched as I crossed the dip, but we made it over without mishap.

A couple of seconds later I heard the metallic crunch of steel on lava. Looking in my rear-view mirror I could see Hartshorn's Jeep stopped at a peculiar angle, the left front lower than the right. He had driven a few inches too far to the left, into the obscuring clump of lantana that hid a three-foot dropoff. The chassis of Hartshorn's vehicle rested on a lava shelf with the left wheel spinning in the air. It took us an hour to gather up lava rocks and build a platform under the wheel and then jack up the vehicle so he could get it back onto the track again. Fortunately, no serious damage was done.

We spent the rest of the day digging the last soil pit of our climo-sequence transect. That evening, over glasses of wine at the ʻUlupalakua Ranch field house, we continued our discussion about the interactions between rainfall and the geological age of the lava flows that Hawaiian farmers had cultivated. When Chadwick had started his research into Hawaiian soil genesis, he had not been thinking at all about ancient Hawaiian land use. He simply wanted to understand how soils develop over time. He was curious about the fundamental physical and chemical processes involved. The sequence of "pedogenesis" or soil formation that his field and laboratory research had unveiled, however, was now proving to be of critical significance in our efforts to explain why Hawaiian farmers had intensified their field systems across the mid-elevation slopes of leeward Kohala and in a similar band across the face of Haleakalā on Maui.

The key was the relationship between rainfall—which is correlated on the leeward sides of islands with elevation above sea level—and the geological age of the island surface. Given a lava flow of the same age, rock-derived nutrients in the soil that forms on the flow surface are progressively leached out as rainfall increases. Conversely, with a given rainfall level, soils on younger flows will have a higher nutrient value than those on older flows, where there has been more time for leaching to occur. In addition, rainfall was an essential variable for dryland (rainfed) farming. Sweet potato, for example, needs at least 760 mm of rain per year to grow but does not do well if rainfall is much higher than about 1,270 mm. Dryland taro prefers a somewhat higher rainfall range, up to as high as 2,500 mm annually.

"It seems that the Hawaiian farmers were looking for the right combination of soils and rainfall," I mused, sipping my wine as I looked out at Kahoʻolawe Island, illuminated by the light of a nearly full moon. "Kind of like Goldilocks," I said. "Not too old, not too wet, but *just* right."

Chadwick chuckled. "Yeah. They were after the sweet spot."

Over the next two years, our team teased out the relationships among soils, rainfall, and the archaeological evidence for intensive cultivation in Kahikinui and Kohala. Chadwick and Hartshorn's analyses of the soil samples from our Kahikinui transects showed that the Hawaiian farmers had indeed found the "sweet spot" that ran right across the face of Haleakalā, between about 400 and 600 meters' elevation.[6] Edward Craighill Handy had called this "the greatest continuous dry planting area in the Hawaiian Islands."[7] Vitousek summed it up during one of our team meetings: The Hawaiians were "farming the rock," he said. Not the youngest rock, of course, but lava flows of the right age that had abundant nutrients, located within the right rainfall range.

The sweet spot running across the face of Maui's great volcano was not uniform in its soil properties. The best soils for sweet potato cultivation were on lava flows with ages of between about 25,000 and perhaps 100,000 years. Younger surfaces, such as the 10,000-year-old Alena flow, were simply too rocky, with little fine sediment in which sweet potato tubers could grow. In contrast, older flows such as the 226,000-year-old Kula surface, which makes up most of the land in the eastern part of Kahikinui, had good tilth (workability) but significantly reduced nutrients. Although easy to work with using a digging stick, those older soils would not have supported intensive cropping, year after year.

These results went a long way toward explaining the settlement pattern that my doctoral student Lisa Holm had uncovered in her survey of two large tracts in Mahamenui and Manawainui in eastern Kahikinui.[8] Unlike Kīpapa, where habitation sites and *heiau* were densely concentrated on lava flows of between 25,000 and 50,000 years old, in Holm's survey areas sites were far fewer and more dispersed. This suggested a lower population density on the older land surfaces. Radiocarbon dates from Holm's test pits confirmed that eastern Kahikinui had not been farmed until roughly the final century prior to European contact. Aware that these older soils were not as fertile (something they may have discovered through trial and error), the Hawaiian farmers avoided them until the crunch of growing population finally made it necessary to expand onto those lands.

Over in Kohala, Vitousek had collected soil samples along five parallel transects running from *mauka*-to-*makai* across the Kohala field system. Plotting out the values for soil nutrients in relation to the ancient field system, it became clear that the Kohala farmers had likewise found their sweet spot for sweet potato farming. The *makai* edge of the field system closely followed the 750 mm rainfall isohyet; below this it would have been too dry to grow *ʻuala*. The field system's upper boundary corresponded with rainfall of between 1,500–2,000 mm, the "threshold" where soil nutrients drop off precipitously.

The implication was obvious: The Hawaiians in Kohala had extended their vast field system over the full extent of terrain suitable for intensive agriculture, pushing the system to its geographic limits.[9] This confirmed what I had long suspected but had not been able to prove—that by around AD 1600 "nearly all suitable land" on Hawaiʻi Island had been brought under intensive cultivation, not only in Kohala but also in other regions such as Kona and Kaʻū.[10] Having reached the limits of dryland cultivation, I surmised that the Hawaiians worked these field systems harder and harder during the seventeenth and eighteenth centuries, partly driven by increasing chiefly demands for tribute, especially in the form of sweet potatoes and pigs.

Our new evidence indicated that the field systems had been heavily intensified during the two centuries prior to European contact. In Kohala our team dug trenches across field embankments, obtaining samples from open field plots and from undisturbed soils capped by the stone-and-earthen embankments. Vitousek and his student Molly Meyer analyzed pairs of soil samples from under the embankments and in the fields, finding that the samples from the cultivated plots had significantly reduced nutrient values.[11] Hartshorn, Chadwick, and I found similar evidence for nutrient depletion in intensively gardened areas in Kahikinui.[12] This quantitative evidence supported my hypothesis that yields—and most likely available surplus—had been declining on Hawaiʻi and Maui Islands prior to European contact. Such declining surplus could have been a major impetus to inter-island conflicts. Hawaiian oral traditions indicated that wars between the islands became increasingly common in the seventeenth and eighteenth centuries, especially between the Hawaiʻi and Maui kingdoms.

Not all of the Hawaiʻi Biocomplexity Project's work was focused on soils. Tuljapurkar and his postdoc Charlotte Lee developed a computer model of sweet potato production in the Hawaiian field systems. Computer modeling allows one to run thousands of simulations, exploring how slight differences in rainfall or variations in soil nutrients would have affected crop yields. The simulations gave us a better idea of the spatial variation within the sweet spot of the Kohala field system, revealing differences between its core and marginal parts. *ʻUala* production would have varied substantially from year to year, depending on rainfall. In wet years farmers could have expanded their gardens *makai,* whereas in drier years only the higher elevation gardens would have yielded crops. To deal with this risk, the Hawaiian farmers had used a "bet-hedging" strategy, planting crops at both lower and higher elevations, a strategy that matched their territorial pattern of long, narrow *ahupuaʻa.*

In 2004 we published our initial conclusions in the international journal *Science.* Observing that intensive, rain-fed field systems such as those of Kohala and Kahikinui were largely confined to the geologically younger islands of Hawaiʻi and Maui, whereas the older islands such as Oʻahu and Kauaʻi boasted large areas of irrigated taro lands, we wrote:

> The resulting contrast in the agricultural bases of societies on the younger versus older islands (rain-fed dryland versus irrigated wetland) influenced the archipelago-wide pattern of sociopolitical complexity that emerged late in Hawaiian prehistory. In comparison to irrigated wetlands, dryland agricultural systems are more labor-intensive, yield smaller surpluses, and are more vulnerable to climatic perturbations—features that probably contributed to the development of the aggressive and expansive chiefdoms that arose on the younger islands.[13]

In other words, the very nature of Hawaiian sociopolitical organization was closely linked to the agricultural potential of older and younger landscapes. Other articles presented the details of the Kahikinui investigations and the computer simulations.[14]

With these publications in prestigious journals our team confidently reapplied in late 2004 to NSF's Biocomplexity Program for a second phase of research. But in spite of all the successes of our first phase, NSF turned us down. The NSF administrators wanted "new" projects, they said. To me this reflected the shortsightedness of government bureaucrats, abandoning a productive research endeavor before its full potential had been achieved. Fortunately, during a meeting at NSF headquarters in Washington, DC, the director of a different program, Human Social Dynamics, approached Vitousek, encouraging us to submit a proposal. In late 2006 we received funding through that program for another three years of research.

It was important to delve more deeply into the relationships between the demography of Kohala's ancient farmers and the process of agricultural intensification. This required getting a better handle on the long-term history of population growth and obtaining information about how the region's economy had operated at the household level. I suggested that we apply the approach of "household archaeology," which I had pioneered in Kahikinui. By sampling residential sites in Kohala, we would be able to estimate the number of households over time; their relative status; their access to marine resources, domestic pigs, and dogs; and other aspects of their social and economic life. This new research direction also pushed Tuljapurkar and his team to develop new theoretical models linking agricultural production with the key variables of human demography, fertility, and mortality. Theory would meet history when we attempted to test the predictions from these models with our archaeological data.

In early 2007, around the same time that we received word that the NSF would support another three years of our project, I was faced with a major career decision. Yale University had discreetly approached me, asking whether I would leave Berkeley to join its Anthropology Department. There were both "push" and "pull" reasons to take this overture seriously. While Berkeley had been suffering from declining financial support from the State of California, privately endowed Yale was hiring top faculty and building new facilities. Yale was willing to equip a state-of-the-art laboratory designed to my specifications.

I had also become disillusioned with the way in which the sociocultural anthropologists at Berkeley had jumped onto the "postmodernist" bandwagon. The older cohort of scientifically oriented anthropologists who had once put Berkeley in the top academic ranks—scholars such as Elizabeth Colson, Brent Berlin, Gene Hammel (all members of the National Academy of Sciences), and others—had retired. Those now in control no longer regarded anthropology as a holistic science of human evolution and culture. Instead, they viewed the goal of anthropology as the "critique of science." One senior Berkeley professor went so far as to inform his graduate students that *nothing* written prior to 1986 (the year in which George

Marcus and James Clifford published an influential postmodern critique of ethnography[15]) was worth reading! How absurd, I thought to myself when I heard this, to dismiss out-of-hand the incredibly rich and nuanced ethnography of someone like Raymond Firth, whose writings on the Tikopia had been such an inspiration to me during my own fieldwork. Or the writings of Bronislaw Malinowski, Ward Goodenough, or even Marshall Sahlins, to name just a few scholars whose cumulative research provides the foundation for our understanding of Oceanic cultures. Although my archaeology colleagues at Berkeley had not gone quite this far, some were also flirting with postmodern, "interpretive" approaches. I found it disturbing when I heard from the graduate student teaching assistants that some of these colleagues denigrated or mocked scientific archaeology and an evolutionary approach in their lectures.

I phoned Berkeley's Dean of Social Science, George Breslauer, telling him we needed to meet. A respected political scientist, Breslauer was aware of the intellectual tensions in anthropology. In his cramped Campbell Hall office I told Breslauer of the Yale offer and of my frustrations at Berkeley. "You're one of our stars, Pat," Breslauer told me. "We don't want to lose you. Would you be happier in another department?" I decided to pursue the opening Breslauer had just broached.

Ever since the Mangaia project (see Chapter Seventeen), and increasingly with the Hawai'i Biocomplexity Project, my research had become closely entwined with that of colleagues in ecology, palynology, botany, paleontology, and related fields. I met with David Lindberg, a senior professor of paleontology in Berkeley's Department of Integrative Biology. Over coffee at the Free Speech Cafe, Lindberg urged me to shift my faculty position over to Integrative Biology. "We're very open intellectually," Lindberg told me. "I think my colleagues would be happy to have you join us."

I arranged to give an afternoon lecture to the Integrative Biology faculty and students. Soon after they voted unanimously to make me a full member of their department. I politely informed Yale that I was declining its offer. In the end, I decided to retain a 25 percent appointment in the Department of Anthropology, because I still had graduate students housed there. I also kept my lab in the Archaeological Research Facility. But the appointment in Integrative Biology allowed me to develop new courses, such as Human Biogeography of the Pacific and a worldwide review called Holocene Paleoecology: How Humans Changed the Earth. I also became a member of the team-taught intensive field course in Geomorphology and Biogeography of Tropical Islands, taking small groups of students to the Richard Gump Research Station on Mo'orea Island in French

Polynesia. I am grateful that even as anthropology was becoming increasingly narrow-minded and antiscience, my colleagues in the biological sciences were happy to open up their department to an anthropologically trained archaeologist.

The morning United Airlines flight from San Francisco to Kona descended low over ʻUpolu Point on its final approach to Keāhole. From my window I could see the red cinder cone of Puʻu ʻUlaʻula, along with Kamilo Bay to the south. It was early June 2007. A few days earlier our team had begun work there; I wondered what they were finding.

Julie Field met me at Keāhole Airport. I tossed my bags in the back of the Ford F150 pickup and hopped into the cab. I had hired Field as a postdoc under the new NSF grant; her task was to lead the excavations in Kohala. Field, who had finished her PhD at the University of Hawaiʻi the previous year, was eager to apply our strategy of household archaeology in Kohala.

"I think you're going to like the site we're digging," Field said as we headed up the Kaʻahumanu Highway toward Kohala. "It's a stone enclosure with adjoining U-shaped structure that might have been a canoe shed. There's some branch coral on it. I think the enclosure might have been a fishing shrine."

"Or, maybe a men's house," I replied. "Or both. Those functions weren't necessarily mutually exclusive."

Arriving in Makiloa *ahupuaʻa,* Field put the Ford into four-wheel drive and headed down a rutted track. A few minutes later Field pulled up next to a grove of *kiawe* trees. It was baking hot there on the leeward coast, an abrupt transition from the foggy Bay Area I had left a few hours earlier. The dry air sucked the moisture from my skin. I slathered on sunscreen, then walked over to see what Kathy Kawelu was finding. Kawelu had recently finished her dissertation under my direction at Berkeley and was helping Field with the excavations. Kneeling in a test pit inside the enclosure, Kawelu pointed to the outlines of a stone-lined fire pit. She pointed out the pieces of branch coral set on and into the walls, likely evidence of ritual use.[16]

Getting out of the test pit, Kawelu dumped the contents of a plastic bucket into her screen, shaking it vigorously. The powdery sediment rapidly passed through the ⅛-inch mesh, a size considered standard in archaeological work. I could see many mollusk shells in the screen, along with some basalt flakes. There were *Nerita, Drupa,* and *Cypraea* shells, as well as fragments of the prized *ʻopihi* limpets (*Cellana*). But no fishbones. "Have you been recovering much fishbone, Kathy?" I asked, puzzled.

"None at all," came the reply. "That's crazy," I said. "We're just a few feet from the shore. There ought to be fishbone in this deposit."

“Well, we haven’t found any so far, Uncle Pat.” Kawelu, born and raised in Keaukaha, Hawai‘i, like many Native Hawaiians had fit me into her “fictive kinship” system, calling me “Uncle.”

“Wait a minute. Lift up that screen. I want to take a look at the dirt pile.” I poked at the dirt with a *kiawe* twig. Sure enough, there were a few tiny fishbones, so small that they had passed right through the sieve. “Look at this,” I said, handing Kawelu a tiny bone just a few millimeters across. “It’s the pharyngeal grinding plate from a parrotfish. But it’s so small that the whole fish can’t have been longer than my hand.” *Uhu* are often a foot to two feet in length, so we were dealing with the remains of really small fish here.

“OK, on the way back to the field house this afternoon we’re going to stop at the hardware store to buy some window screen,” I told Field. “That’s the only way we are going to get an adequate sample of these tiny fishbones.”

With our new sieves fitted with window screen, we started recovering lots of tiny fishbones—more than 84,000 of them before we were finished—along with abundant mollusk and sea urchin remains. The Hawaiians inhabiting leeward Kohala had heavily exploited their inshore marine environment. Barely a half-million years old, the Kohala coastline, with its low, rocky cliffs indented here and there by small bays, lacks a true reef. Coral heads dot the offshore rocky bottom, providing food for herbivores such as parrotfish and wrasses. But the biomass that can be supported on this kind of incipient reef is much lower than for a more mature reef such as is found along the leeward coast of Moloka‘i or around much of O‘ahu and Kaua‘i.

The large population that had once occupied leeward Kohala, supported primarily by the intensive cultivation of sweet potatoes, had to depend largely on this immature reef ecosystem for its supply of protein. The farmers did raise pigs and dogs—we found the bones of these domestic animals in our excavations—but most of those animals were destined for the households of the *ali‘i* and *konohiki* or were consumed at special temple ceremonies. For their daily *i‘a,* their flesh food to accompany their staple *‘ai* of sweet potato and taro, the commoners had to harvest fish and shellfish. With a population that likely exceeded twenty thousand in the century prior to Captain Cook’s arrival, the pressure on leeward Kohala’s delicate inshore marine ecosystem was intense.

The minute fishbones in the Makiloa men’s house were just the first of many discoveries that we made over the course of three summers of fieldwork between 2007 and 2009. We sampled household sites in two areas, each with both coastal and inland components. One set of sites was located in the adjacent *ahupua‘a* of Kaiholena and Makeanehu. Tuljapurkar and Lee’s computer model indicated that these *ahupua‘a* were within the central core of the Kohala field system, with the most productive lands and least risk of crop failure due to drought. Our second set of sites was in the Kalala, Makiloa, and Pahinahina *ahupuaʻa,* near the southern margins of the field system where the risks of periodic drought were greater. Comparing household sites between the core and the periphery of the field system, we hoped to see differences in the local economies. After Thegn Ladefoged and his students from the University of Auckland conducted GPS surveys of the archaeological features within each sample area, Field and I selected sites to excavate. By the end of the 2009 field season we had dug in fifty-seven precontact residential features, sufficient to give us a statistically valid sample of house sites in two study areas.

While Field and I worked on the household excavations, Tuljapurkar and his team fine-tuned their new models linking agricultural food production to key variables in human demography, especially fertility and mortality. Central to their work was the concept of "food availability."[17] When the Hawaiians had first settled an area such as leeward Kohala, population density was low and land was freely available. The main limiting factor to food production would have been available labor to clear, plant, and tend the fields. Several centuries later, after the population had grown to thousands of individuals, suitable land for farming—not labor—was now the limiting factor. Thus, as population density increased and the limiting factor shifted from labor to land, food availability would have decreased, as more and more mouths had to be fed on a finite amount of arable land. Another way of putting this is that over time people became increasingly hungry more often. And as food availability decreased, feedback loops would have affected women's fertility at the same time that they increased the death rate among the most vulnerable: infants and the elderly.

Tuljapurkar's postdoc Cedric Puleston ran computer simulations using the new models. I was fascinated by the nonlinear trends they projected. Such nonlinearity is, after all, what makes the study of "dynamically coupled" human and natural systems so intriguing. The model simulations predicted that population growth in an area such as leeward Kohala would have initially followed an exponential growth rate, with population sizes doubling over short time intervals. Simultaneously, the food availability ratio would have declined, following a negative exponential curve. As the food ratio approached and then dropped below a value of 1 (the equilibrium point at which the population replaces itself without growing or declining), the negative consequences for fertility and mortality would have kicked in, slowing the rate of population growth dramatically. Although this may have kept the population in balance with the agricultural system's capacity to produce food, the final outcome would have been a population constantly on the margins of hunger.

Mulling over these results, I recalled the Hawaiian *moʻolelo* or traditions that spoke of repeated attempts by the Hawaiʻi and Maui kings to break out of their island kingdoms, to conquer new lands where their people would have better farming land and resources. I had long been intrigued by the fact that it was the Maui and Hawaiʻi polities that had been the most aggressive and warlike in precontact Hawaiʻi. I thought that our new models revealed some of the complex dynamics that lay behind this political history.

Field's painstaking excavations in the sample of household sites allowed us to test Tuljapurkar's and Puleston's computer simulations. Radiocarbon dates from a series of habitation sites confirmed that there had been exponential population increase in leeward Kohala over a 400-year period. The maximum population had been reached between AD 1650–1800, just prior to European contact.[18] But within the core of the field system (the Kaiholena-Makeanehu area) the population reached its peak earlier, by about AD 1650. Continued population growth had been absorbed by expanding the Kohala system into marginal zones, including the Kalala-Makiloa-Pahinahina area to the south. The intensive agricultural system of Kohala had, in its late stages, been pushed to its limits.

The theoretical models that we tested in Kohala with empirical, archaeological data were not just academic exercises. In fact, they are central to a debate that has raged in Western intellectual circles ever since Thomas Malthus published his provocative *Essay on the Principle of Population* in 1798.[19] Malthus observed that, whereas the natural reproductive potential of humans is geometric—as in the series 1, 2, 4, 8, 16—the ability of food production systems to increase output is arithmetic, as in 1, 2, 3, 4, 5. All things being equal, human population growth will inevitably outstrip our ability to feed the exponentially increasing mouths. Using European history as his gauge, Malthus argued that disease, starvation, pestilence, and ultimately war were the forces that kept human numbers in check. Malthus's *Essay* had a major influence on Charles Darwin's thinking, contributing to the latter's theory of natural selection.

But Malthus always has had his detractors, those who argue that human creativity and innovation can blunt the Malthusian "scissors" as they slice away the excess population. Economist Ester Boserup argued in her widely read 1965 book, *The Conditions of Agricultural Growth,* that population growth was in itself a powerful force driving agrarian innovation.[20] Her theory seemed to be borne out by the so-called Green Revolution in which high-yielding rice, wheat, and other crop varieties developed through new genetic technology dramatically boosted world harvests (until those gains leveled off, as they now have). The debate over the limits to population growth was most famously played out in the 1980 wager between Stanford ecologist Paul Ehrlich (author of *The Population Bomb*) and economist Julian Simon.[21] That Simon won the $1,000 bet emboldened many conservative economists, strengthening their anti-Malthusian views.

Our Hawai‘i Biocomplexity Project research in Kohala and Kahikinui tested the Malthus-Boserup models with empirical data spanning four centuries in the "model system" of Hawai‘i. Both Malthus and Boserup were partly right and partly wrong. Malthus's insight that the human potential to reproduce outstrips the pace of agricultural production is correct. At the same time, humans do have an amazing capacity to innovate. In Kohala, this capacity was evident in the highly intensive field system that the Hawaiian farmers developed over more than sixty square kilometers. But there *are* limits to innovation. The Hawaiians were discovering those limits in the late eighteenth century, as their soils were increasingly depleted of nutrients, as they pushed their fields to the maximum. Their chiefs sought solutions in wars of conquest and territorial acquisition. There are lessons in the story of what transpired in late precontact Hawai‘i if we care to heed them—lessons from the model system of the Earth's most isolated archipelago.

In the late 1960s, Roger Green's settlement pattern approach moved archaeology in Hawai‘i and the Pacific out of an older "culture history" paradigm, launching a new period of research into island societies. The Hawai‘i Biocomplexity Project in a similar way took Hawaiian archaeology to a new level, creating another paradigm shift centered on the dynamic interactions between human populations and their ecosystems. Our team demonstrated how multidisciplinary research—with archaeology as a core integrating discipline—can develop and test models with implications that extend beyond the histories of individual islands. It is not too much to claim that the linkages among population, agriculture, food, and society that the Hawai‘i Biocomplexity Project has addressed are directly relevant to problems that have beset humanity for the last ten thousand years and that will become even more crucial in the decades that lie ahead.