

EXPLORING THE ENTANGLED BANK

SYSTEMATIC RESEARCH ON THE SEPIK COAST OF NEW GUINEA

John Edward Terrell
Field Museum, Chicago

The title of this symposium could be read as implied criticism of the conduct of archaeology in the Pacific. Isn't all archaeological work systematic? Haven't there been decades of systematic research in places such as New Zealand, Australia, and Hawai'i—not to mention the more recent work in other places such as Tonga, Vanuatu, and New Caledonia? Why focus on "systematic approaches" to understanding Pacific prehistory? You will be hearing many answers to this question this morning. Here's my own.

Systematic is as systematic does?

Dictionaries say *systematic* means "presented or formulated as a system; methodical in procedure or plan." A fair but boring definition. I think being methodical in doing *what* we do and *how* we do it is not as strategic as being methodical in *why* we do what we do. Please let me explain.

I often have the feeling that archaeologists are too easily seduced by what they find. We too easily think that what's important about what we find is that something has been found. And having been found, we are duty bound to explain what we have found. I think this "reconstructionist" logic of "putting found things first" encourages all of us to spend too many hours debating "What can we say about this or that?" Investing a lot of time worrying about how well we can "read the past" can make it hard for us to acknowledge that maybe—and maybe more often than we would like to admit—we may not be able to say very much about what we find, or at least not much of interest.

Now in our defense, it is commonly acknowledged that archaeologists are duty-bound to "push the evidence as far as it will go." This admonition may be right, but how are we to avoid turning the pursuit of archaeology into little more than an academic's Ouija board (something which I think some who write about political economy in prehistory, for example, have already done in their own special way)?

Admittedly I'm being a bit sneaky here. I'm definitely *not* saying that archaeologists ought to throw away everything they find that they cannot immediately say something wonderful about. Hey, I've been a museum curator for almost 30 years. Museums are full of things that we can't say much about since museums keep many things on speculation—on the hope that someone will come along someday who needs what's on the shelves in our storerooms.

But mere possession of something is not its own reward (unless you are a true-blue "collector"). Museums keep things even when they don't really know what to say about them because curators hope that one day these "kept things" will prove to be the perfect "grist" for some particular scholar's intellectually exciting "mill."

Ask not Why not? but Why?

But let's get back to archaeology. Let's take the problem of gender, for instance. Surely here the good question for archaeology isn't "*What can archaeologists say about gender?*" Instead, isn't the first question "*Why do archaeologists need to talk about gender?*"

Until we have a good answer to this primary question (and there is a good answer in this case, isn't there?), how could we even begin to figure out *how* we might be able to contribute systematically to the resolution of gender's more pressing problems?

Here's where being "methodical in procedure or plan" about asking research questions is so important. As others have sermonized before, in archaeology as in everything we do, our strategy should be *problems first; solutions second*. As a former U.S. presidential candidate liked to say, "it's that simple."

In practice, of course, thinking systematically—questions first, answers second—is rarely simple. Which may be why some archaeologists would seem most content when they are in the field and are "just digging" (to quote the famous British archaeologist Sir Mortimer Wheeler). And why some of us may be so easily seduced by what we find. Instead of being pensively "proactive" in archaeology by first asking carefully constructed questions and *then* designing research projects to find things that might help answer them, aren't we often "reactive"—get data; *then* ask questions? (Or perhaps alternatively: (1) *ask really big NSF questions*; (2) *get data*; (3) *then try to figure out how on earth what we have found may help answer the really big questions we started off saying we were asking?*)

Asking good questions

It may be obvious at this point that what I have just said is far too polarizing. As archaeologists, we all ask questions both *before* and *after* we dig, measure, analyze, and compute. But here's my big question. *Are we asking good questions? Are we being systematic in asking the questions we ask?*

Now it may be obvious where we are headed. You know I am about to ask: *Where do "good questions" come from?*

Take Lapita pottery, for example—surely the Pacific archaeologist's favorite conundrum (and whipping boy?). Does it truly make much sense to ask—as some do—if Lapita was a "fast train," an "express train," a "speedboat," or (nowadays, some say) a "slow boat" to Polynesia?¹ Aren't these descriptive tags for the "Lapita phenomenon" actually little more than headline "hooks," journalist clichés, figures of speech, catchy metaphors?

Here's what I think. I don't think these ways of talking about Lapita pottery have been inspired by well-specified (and thus testable) scientific models. I don't think these clichés help us ask good questions about Lapita. I think these journalistic hooks are misleading. I think wasting ink on debating these (frankly false) analogies is a waste of good time. But if I'm right about this, then where are the good Lapita models and the good Lapita questions that scientific models can provoke us to ask that are worth their weight in gold (or at least in federal research funding)?

¹ Or even an "entangled bank."

Saying this differently, has anyone actually spelled out in truly useful ways what difference it makes to say “Lapita peoples” rather than “Lapita people”? If we put a gallon of gas in what *SCIENCE* has recently told us is the current “most preferred” model of Lapita—namely, the “triple-I model” (*Intrusion, Innovation, Integration*)—how far down the road do we get? Can we even call this particular model of Lapita a scientific model? Doesn’t “intrusion, innovation, integration” sound more like the headings at the top of a textbook table—or perhaps at the top of a weird Chinese menu (“with today’s *Lapita Dim Sum Special* you can pick two from *Intrusion*, two from *Integration*, and two from *Invention*, plus you get two spring rolls”)?

Or maybe the Lapita 3-I model is a Denny’s breakfast special?

Finding better questions

Having used what I hope you will hear as humor to walk you down the garden path as far as we have now gone, you probably know exactly why I am going to invite you to sit down for a moment. From now on, I will have nothing more to say directly about Lapita and its problems (or about gender). Instead, I want to talk about me, or rather about what the New Guinea Research Program at the Field Museum of Natural History in Chicago has been doing in the Pacific since the late 1980s.

My museum is lucky enough to have one of the greatest collections (~ 60,000 items) in the world of things (“material culture”) from the Pacific Islands. The vast bulk of this collection was assembled before the First World War. A good portion of this material was once exhibited on the museum’s ground floor. The rest was hidden away in storage (where sadly most of it now is). Until recently, few people paid any systematic attention to all this remarkable stuff.

In 1987, I asked my colleague Dr. Robert L. Welsch—who is a cultural anthropologist—if he’d help me decide (a) whether “real science” can be done with this old museum collection, and (b) whether it would make sense to go back to the Pacific to document better what the museum has under its wing, so to speak. (As you might expect, our collections from the Pacific are old enough that we then had little detailed catalog information on most of our holdings.)

Welsch soon discovered that the best documented part of our collection (over 14,000 items) was put together by my predecessor, Albert Lewis, during the Museum’s 1909-1913 Joseph N. Field Expedition to Melanesia. Like most museum collections from this era, our records include provenience information documenting where Lewis had collected each of the objects (“specimens”) he brought back to Chicago. But Welsch found something else, too. Lewis’ notes on the objects he collected on the Sepik coast of New Guinea also tell us *where* the things he collected had been made. Welsch discovered that these two kinds of provenience information—*where made* and *where collected*—told us an unexpected story. Many of the things that Lewis collected on the Sepik coast did not come from where he collected them, but instead from other villages on the coast, in the hinterland, and on the small offshore islands sometimes hundreds of kilometers away.

It soon became obvious, in other words, that “trade” among these many small communities was a prominent feature of life on this coast back at the turn of the last century. This was unexpected since something like 60 different languages are spoken on this coast along the 700 km between Jayapura in West Papua and Madang in Papua

New Guinea. Even more astonishing (at least to me), these many languages can be grouped into possibly as many as 24 different language families belonging to five unrelated language phyla—Austronesian and at least four non-Austronesian groupings.

Why on earth were language and culture on the Sepik coast so out of step with one another at the beginning of the 20th century? Why is this also true today?

The tempo of discovery

This was when “science” began to click in. To make a long story short, we now know that the Sepik coast is a real anthropological puzzle. In their ways of living, villagers here are bound to one another far and wide by culturally structured and possibly quite ancient relationships of friendship, marriage, commerce, and shared social responsibility. Yet in their ways of speaking, they are divided into scores of separate speech traditions. How can this be so?

Welsch and I concluded that if we could answer this basic question, then maybe we could discover if language on this coast has much to tell us about the history of people living there. However, phrasing the issue this way may obscure that this is a version of one of the most famous questions in anthropology—namely, how “tightly” or “closely” have “race, language, and culture” marched to the same drummer down through the many millennia of human history?

In spite of what we may teach in Anthropology 101, many people see language as a sure sign of a person’s ethnic identity and origins; many assume that language differences create barriers that isolate people from one another. On this basis—as Franz Boas skeptically remarked many years ago and yet some still tell us today—language history ought to parallel culture history (and also the biological history of human populations). And some people, even in our own profession, continue to endorse the commonsense ideas that “different peoples speak different languages” and, therefore, linguistics can be used to chart hereditary relationships *between* societies the way that kinship has often been used in anthropology to chart human relationships *within* societies.

While anthropologists, archaeologists, linguists, and other social scientists have frequently argued against these commonsense notions, unfortunately scholars have often done so in ambiguous, even ambivalent, ways. (If you don’t believe me, go to <http://www.Greenwood.com> and buy a copy of my new book, *Archaeology, Language, and History: Essays on Culture and Ethnicity*.)

Rob Welsch and I spent much of the last decade of the 20th century both in the Field Museum and in New Guinea trying to solve what we call the “Sepik conundrum,” that is, the striking lack of fit between language and culture on this coast. We have published much on our work. We have sometimes inadvertently (and possibly sometimes “advertently”) stirred up a hornet’s nest or two. (And believe me, the wrath of those who insist that race, language, and culture “go together” can be astonishing.) But you can read about all of this at some other time if you feel inclined.

Instead, I think it may be simply said that (1) Welsch and I have been building on the strengths (and weaknesses) of one of Field Museum’s greatest ethnological collections; (2) we have been systematically combining museum-based research with new field work; (3) we have been building on what we have discovered; and (4) while our initial research question—our original “why”—was the seemingly humdrum question

“Can we do real anthropology with old museum anthropology collections?,” we soon switched horses to tackle one of the basic issues in anthropology using museum collections, ethnographic field methods, and dirt archaeology.

In doing so, we have had to confront some of the main implications of this major issue in archaeology, anthropology, and human biology. For instance, what defines a human community? Do people who speak different languages belong to fundamentally different human lineages? How well can we use the human achievements that archaeologists and museum curators label “material culture” to judge how people in different places are (or are not) historically related to one another? What, in short, are the determinants of human relatedness?

How far we have got in helping to resolve these basic anthropological concerns is for others to judge. In the minutes remaining this morning, I want to sketch where we are now headed. I will say nothing about our growing ArcView GIS project, or about the particular details of our current field research plans. Instead, I want to concentrate on the “big issue” we have decided to tackle, an issue that has grown—dare I say the word?—systematically out of all that we have so far done (and all the questions we have been asking) at the Field Museum and in the field.

The “ancient lagoons” hypothesis

A fundamental concern in archaeology and anthropology is understanding the relationships between population growth, the development of so-called complex societies, and the shift in prehistoric times from foraging to food production. It is not yet widely recognized, however, that the subsistence practices of many communities in the region that van Steenis called eastern *Malesia* (the Celebes, Moluccas, and New Guinea) cannot be placed unambiguously along a typological spectrum running between hunter-gatherers and agriculturalists.

Modern research has shown that people in this region have long been following broad-spectrum approaches to subsistence that have evidently made agriculture or horticulture the mainstay only where (and when) local circumstances have narrowed the range of subsistence choices. Today, for instance, the lagoons at Sissano, Malol, and elsewhere on the Sepik coast are complex, highly productive habitats (fish and shellfish as well as extensive mangrove and sago swamps) that support some of the largest village communities on the coast. In this subsistence arena, “garden produce” and domesticated animals supply little more than what in Tok Pisin is called “grease,” that is, tasty side dishes for hearty meals of sago pancakes (or boiled sago balls) and seafood.

Our continuing work on the coast is encouraging us to rethink the role of “domestication” in human history. We distrust the scientific value of linear models that oppose “foragers” and “farmers” as the end-points on a single axis of historical variability. The real story can’t be that simple. We are skeptical that human-induced morphological (and genetic) change is the best hallmark of “domestication.” The ecology movement in the 1960s convinced many of us that our impact on the earth has been much greater than simply that.

Contrary to conventional wisdom, creating productive human environments—as the economies of people in Greater Near Oceania illustrate—does not always require focusing time and effort on only one or a small handful of species. In fact, “managed”

landscapes can sometimes be even richer in species diversity than “wild” ecosystems (if you are doubtful, visit any botanical garden).

Hence a good down-to-earth way to think about domestication and track the history of domestication archaeologically is to focus research not only on how human beings have changed the genetic profiles of individual species, but how they have changed the species profiles of the places they have come to inhabit. The true story of domestication is a tale about our continuing impact on the earth’s species diversity, not just a story about the morphological and genetic modification of particular species that are nowadays seen as being of primary economic importance. As Charles Darwin told us, if you change the balance of nature, you change the genetics of species—to a greater or a lesser degree. And not just the genetics of the particular species that we have come to really care about.

Our work on the coast is also leading us to rethink the mid Holocene foundations of human subsistence throughout Greater Near Oceania. We now hypothesize that beginning around 6,000 years ago, newly forming lagoons along New Guinea’s long northern coastline (and elsewhere in this vast region of the Pacific) may have started to be productive enough—in combination with already well-established local practices of tropical agroforestry—that they helped fuel significant demographic growth that had far-reaching ramifications and that ultimately led to the colonization of the central and eastern Pacific.

During much of the 20th century it was conventionally said that Asia was the ancient source of “civilizing traits” in the Pacific—including agriculture and its focal plants and animals. Indeed, it is still widely claimed that Asia was the main source of much that transformed Oceania after the Pleistocene.

Scholars have argued in favor of this interpretation in a variety of ways. The ethnobotanist Doug Yen’s phrasing of this popular interpretation catches one of the main threads. New Guinea, he has said, was “the site of the blending of two independently developed agricultures,” a coming together of Asian and New Guinea species that established the biotic and cultural foundations for all Oceanic subsistence practices thereafter.

There are several reasons to be skeptical about this widely endorsed reconstruction of what happened here in prehistoric times. I want to note only two. Many people in New Guinea, as I have observed, cannot be pigeonholed even now as living an agricultural way of life. If so, then is searching for the origins of Pacific agriculture here, there, or anywhere as central a question in Pacific archaeology as some believe?

The second reason is equally straightforward. Asia and the Pacific are not separate isolated worlds. There is much to be learned about how people in Greater Near Oceania may have been involved with one another perhaps from the earliest days of human settlement 30,000-60,000 years ago. But current ignorance about prehistory in this part of the world is not good reason to think that people in Asia and the Pacific were once so isolated from one another that it makes sense to talk about the prehistoric blending of two *independently developed* agricultures even if we accept that searching for the origins of Pacific agriculture is a worthy pursuit.

Sea levels and shifting coastlines

This last observation does not mean, however, that the opposite of human isolation is promiscuous panmictic human intercourse. The northern coastline of New Guinea—the second largest island in the world with a land area of 808,000 km²—runs from northwest to southeast between 0°–6° south of the equator along the edge of the Australian Plate where it strikes into the West Pacific Plate. The mountain ranges of New Guinea are folds in the earth's crust that have been thrust up by the impact of these two great continental plates. The northern coast is a zone of continuing tectonic instability leading to frequent earthquakes and landslides in the mountains.

Experts argue about the magnitude of the eustatic drawdown of sea level during the last ice age, but estimates of 120-130m are common. It is not clear how inviting this coast was when the sea was so much lower than it is now. Archaeologically we know that people were on this coast 35,000 years ago. However, our working hypothesis is that this part of New Guinea was only sparsely inhabited until around 6,000 years ago when global sea levels had risen to within a few meters of their current stand. We now suspect that much of the genetic and cultural distinctiveness of New Guineans today may be a reflection of these Pleistocene circumstances.

To phrase our suspicions colloquially, we think that during a large part of human history in the Pacific, New Guinea turned a cold shoulder to Asia. If relatively few people were living then on the northern beaches of this great island, New Guinea's links with islands elsewhere in Oceania must have been much weaker than they would have been under other, more favorable circumstances.

This hypothesis, however, is about Pleistocene New Guinea. We think that by mid Holocene times—that is, around 6,000 years ago—living on the northern coast was much more inviting. In fact, as I have already suggested, we now think that living conditions there had improved radically enough to alter the give-and-take between people in the southwestern Pacific.

Where do we go from here?

At present we are designing new archaeological and ethnobotanical research to (1) chart the history of coastline progradation on the Sepik coast and study the development of coastal lagoons over the past 6,000 years; (2) locate buried midden deposits that may help tell us—as they did Pamela Swadling in the inland Sepik River basin—about human settlement and prehistoric economics; and (3) document modern agroforestry practices and lagoon fishing so that we can begin to model more accurately how changing shorelines during the Holocene may have changed the very fabric of social and cultural life in the Pacific.

But what is my take-home message for you this morning? I think being “systematic” in archaeology does not only mean “being orderly in method, plan, or procedure.” Nor does being systematic just mean being orderly in figuring out *why* you are asking the questions that you are asking. Our work on the Sepik coast has shown us that good questions in archaeology are *not* just good questions *about* archaeology. Our work has also shown us that there are different ways to tackle the same good questions, some archaeological, some not.

Saying this is not being post-modernist. Let's face it, nobody makes archaeologists ask only questions that they alone can answer. So when we go about

being systematic *in how we ask* questions, and systematic also *in how we frame* (or “model”) the breadth of likely answers to them, let’s also be systematic *in how we call upon* other fields of expertise to help find the “good answers” to the “good questions.”