

# CARTERFACTS

## GEORGE CARTER ON DIFFUSION

### INTRODUCTION

*During the last forty years, hearing the very word “diffusion” has raised the blood pressure of “independent inventionists”—making them see red.*

*During a lifetime of challenging accepted wisdom, the late Professor George F. Carter never shied from asking tough questions or pointing out the Emperor’s sartorial condition.*

*In this collection of musings mixed with a wide spectrum of solid evidence—evidence ranging from botanical (peanuts, potatoes, cotton to hibiscus), crustaceous (cowrie shells), to animal (cats and chickens to elephants)—George Carter has brought the “D” word out of the closet, giving transoceanic diffusion a star role in the peopling of the Americas.*

### ON EVIDENCE

From the “Epigraphic Controversy, Setting the Record Straight”, *Epigraphic Society Occasional Publications (ESOP)* Vols. 19 & 20, 1990, 1991.

I have read of a Chinese lamp found in Mexican archaeology, which was rejected as evidence *because* it was obviously a Chinese lamp. In Peru dozens of classic Graeco-Roman oil lamps have been found, but to my knowledge only one has been published (Ibarro-Grasso and Dick Edgar).

A bronze spear on Monhegan Island, Maine, where an inscription was also found is also ignored. The bracelets with the Bat Creek find were misidentified as colonial copper, but are a rare brass used in the Mediterranean only at the time the accompanying inscription indicates and for which there is also a confirming C-14 date (McCulloch 1988). How much more has been swept under the academic rug (ESOP 19-58)?

Epigraphers are specialists, hammering away at their special inquiry. They have the burden of good presentation of their data. The critics have the burden of both showing that the epigraphers are wrong, and that there is no expectation that such material should be in America (ESOP 19-58).

If I seem to meander along, that is exactly the course of thought and of discovery. We mislead our students when we teach that there is a fixed methodology that leads inevitably and systematically to self-evident truths. Discovery is quite

otherwise. We stumble and fumble around and ideas grow, sometimes by chance events (ESOP 20-239).

### INVENTION, DIFFUSION, AND RACISM

“You See What You are Prepared to See.” *Anthropological Journal of Canada*, Vol. 19 No. 1, 1966, pp.10-12.

Ideas are strange things, when you come to think about it. They seem to spring from nowhere, and to have a life of their own. Very often, they persist in the face of a total lack of fact or logic to support them. Attributing the label “racism” to the diffusionists is just such an idea. Not only is it totally wrong; it could be just as easily—and as erroneously—applied to the inventionists.

The current theme can be stated thus: “Those who think that the growth of American Indian civilizations was enormously influenced by overseas contacts are *racists*, because they are in this way denying the American Indians the genius to develop their own civilizations.” With great emotion this view has been expressed to me, both by anthropologists and by informed laymen. It can be tested for validity, however, on several grounds.

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First, there is the question of meaningful contacts. My own rather extensive research on this subject has not been limited to *plant geography*, but it is in this area that we find the most telling arguments for transoceanic contacts. Plants, obviously, are far removed from any possibility of independent invention by the hand of man. Plants, beyond reasonable doubt, show that deliberate voyages were made to and from America, and that useful plants were carried both ways across Atlantic and Pacific waters. The names of plants (and this is even more striking, if we extend consideration to chickens) show that such voyages were made by different peoples, and presumably at different times. That they were meaningful contacts is a certainty, since the exchange of useful plants involves rather complicated learning processes. As for the chicken, the associated *complex* of rituals and beliefs would seem to leave no question at all that an entire body of related ideas was transferred across the ocean.

The indisputable evidence of such things that cannot be reinvented simply closes the door on any argument that the voyages could not have been made. The fact is that they began at least as early as Lungshanoid times—that is, prior to 1500 B.C., when the American peanut arrived in China. The American Hibiscus (*H. rosasinensis*), too, reached China well

before the time of Christ. As reported in this *Journal* (Vol. 18: 3, pp. 17-23), a Chinese-style anchor, with a manganese coating that suggests, though does not prove a 3000-year date, has been found off the California coast—right on the great circle route to America.

Beyond the plants, moreover, lies the rich realm of cultural similarities. The most rewarding has been in the comparison of art forms, styles and motifs—reaching from Alexander von Humboldt’s writings, around A.D. 1800, through Covarubias, Heine Geldern, and Ekholm, to the still more detailed work of recent scholars. While these have focused on Asiatic-American similarities, some attention has been given to transatlantic parallels also, notably by Jose Alcina Franch of Spain. References to cover this field, together with the related cultural areas, would require book-length treatment. It should here suffice to say that there is hardly an aspect of the realm of culture for which evidence of influences from overseas cannot be shown to have reached America.

The question we must address is this: Does the evidence of overseas influence make the American Indian out to be an inferior being, a kind of second class man, incapable of inventing things or building civilizations for himself?

First, let us tackle the misnomer, “American Indian.” That there is or was a uniform race in the Americas is a complete myth. Almost every race known on the face of the earth was here, at one time or another. The earliest arrivals for whom we have skeletal evidence are represented by slab-sided, ridged-roofed skull types of at *least* two kinds, and they are not Mongoloid. One might guess, however, that they were Bering Sea entrants.

Eventually, various people began crossing the oceans to America, and evidently in sufficient numbers to affect the racial make-up in different parts of the two continents. For instance, the eastern United States was strongly influenced by Europeans, and parts of South America by Negroids, as exemplified by the Botocudos.

I will not try here to defend these assertions. One can read Roland B. Dixon’s *Racial History of Mankind*, or delve into blood groups (see *AJC* Vol. 11:4, pp. 18-35), or ear-wax studies, and quickly find the evidence. Now, if most of the races of the world are present in America, what then becomes of the racist argument?

Let us try the question of the origins of civilizations outside America. All of them show the effect of the inflow of ideas. Not one of them grew up in isolation. We can even state this in reverse: no isolated region ever developed a civilization. Australia, Tasmania, and Tierra del Fuego are classic instances of the absence of civilization in isolated areas. There are many others. It would be curious, indeed, if the inhabitants

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of America, in total isolation, developed *their* civilization without any outside stimulation. That such was not the case is clearly indicated by the massive evidence of everything from alphabets to zodiacs.

One may ask, of course, if the obvious contacts were early enough to be formative, or were simply added onto the already developing, perhaps already flourishing civilizations. At the moment, all of the Amerind civilizations seem significantly later than Old World counterparts, and have indications of Old World influences right from the start. As for the key element, agriculture, I have shown elsewhere that just as good a case can be made for American agriculture resulting from stimulus diffusion from the Old World, as can be made for an independent origin—an argument that Donald Lathrap has also made. I am not suggesting that such a thing is proven. But the notion that the American Indians were some sort of geniuses in the matter of plant domestication is an exaggeration. Actual *counts* of domestic plants show them to be laggards in this respect, and would be far behind the people in the broad belt from eastern Asia to the Mediterranean.

Is all of this *racist*? Let’s try it on the British, those exemplars of Nordic racial supremacy, if we were to believe such writers as Lathrap Stoddard, Madison Grant, and Adolf Hitler. (We could equally well take the Germans, the French, the Swedes, or others.) They domesticated no plants or animals. Nor did they invent pottery, the wheel, numbers, or alphabets. Rather, they borrowed all of these fundamental building blocks, and did so belatedly at that.

But agriculture was well underway by 7500 B.C. in the not-so-distant Near East. It did not reach the benighted British until around 2500 B.C. One could say, then, that they were not even very ready borrowers. Yet, at a later time, they were borrowing movable type, gunpowder, the compass, fore-and-aft-rig sails, rudders, and were even stimulated by the Chinese to manufacture porcelains. The list can be made embarrassingly long, and the British made out to be a group of uninventive donkeys. In the end, though, the British put all of these borrowed elements, and many more, into a new configuration. They created a distinctive culture all their own, and became one of the world’s leading people. It would be difficult to attribute all of these happenings to racial change. And if one were to discuss China or Japan, or Greece, or Rome, only the details would change.

If the American Indians were startling exceptions—a peculiar race different from all the rest of mankind—able to start from nothing, yet suddenly erect vast civilizations that could duplicate in virtually every detail the civilizations of the rest of the world, they would be the supreme geniuses of all time. The elevation of a mythical Indian race to such cultural heights is the *real* racist position—in substance, a Hitlerian creation

of a super-race. It is the independent inventionists, then, and not the diffusionists, who are racists.

I write this brief comment somewhat tongue in cheek. Those with whom I have argued this case are not racists, even if they are misguided in their labeling of the diffusionists as racists. They have been beguiled by a seductive proposition, and more easily led to accept an indefensible position by their emotional commitment to the purity of the American cultural realm.

#### ON FANTASTIC ARCHAEOLOGY

From "Fantastic Archaeology at Harvard," *ESOP* Vol. 18, 1987, pp.280-284.

I learn that Harvard's department of archaeology offers a course with the title of Fantastic Archaeology, Professor Steven Williams as master of ceremonies. It is a strange mélange.

I am featured in lecture (the title is crank personalities). Two examples are given. The first is Le Plongeon, a nineteenth century man who worked on the Maya. He is said to have seen Mayan influence around the world. Some today see influences on the Maya from around the world. I do.

On the second example, George Carter, I am somewhat of an expert. The profile of a crank is given as: (1) works in total isolation from his colleagues, (2) has a tendency to paranoia, (3) considers himself a genius, (4) colleagues are ignorant blockheads, (5) is unjustly persecuted and discriminated against, (this is paranoia, see three above), (6) focuses attacks on great scientists and best established authorities, (7) writes in complex jargon.

I work with those that let me, and I find in my address book of active correspondents 55 archeologists, all interested in the early man in America. On the diffusion question there are 41. There are 38 on the list of epigraphy and linguistics. This hardly sounds like working in isolation.

It is hard to answer persecution and discrimination except to cite cases, so here is one. A University of Arizona anthropologist recently was in Texas bragging that "they" had kept George Carter out of all major journals for 25 years. Sounds like discrimination to me.

I don't consider myself a genius. I am lousy at math, poor at philosophy, and have other lamentable gaps. There are difference men and similarities men. I am a similarities man. I tend to put things together. It is a well known fact that the two minds do not get on well together, and that it is that the difference man seldom understands the similarities man. It is sad, for we need both.

As for attacking established theories and the men that defend them, of course. No progress is made by endlessly accepting the established notions. If we did that, we would still maintain that the sun goes round the earth, that there are no such things as germs, that continents are not in motion, and so on. When great scientists hold such notions, and their present equivalents, their views are subject to attack, if attack is the right word.

There is much more. Professor Williams is able to read men's minds and knows why they do what they do. I was taught to stick to the facts and not impugn motives. It is a sound doctrine.

#### ON FELL

From Fell, "Epigraphy and the Anthropological Monroe Doctrine," *ESOP*, Vol. 22, pp.22-25, 1993.

The ferocity of the attack on his work measures the impact of his findings on not only Amerind prehistory but on much of the world history and on ideas concerning the growth and development of civilizations. The import of his linguistics is simply enormous, and the resistance is proportional. Actually, the evidence is overwhelming for transoceanic diffusion of knowledge before A.D. 1500, indeed reaching back at least to 3000 B.C.

Every kind of evidence that one might want is there—from alphabets to zodiacs. Both the biogeographical evidence and the linguistic evidence are immune to the independent inventionist claim. They change totally the assessment of the cultural evidence, and in combination force a complete reassessment of the view of cultural dynamics underlying the growth of the Amerind civilizations.

#### ON PRE-COLUMBIAN DISCOVERIES OF AMERICA

From the *Anthropological Journal of Canada*, Vol. 19, No. 2, pp.10-17, 1981.

A popular pastime, in the present day, is to label people having different opinions as to the nature of evidence and a different understanding of it, as "cultists." Thus, those who consider that evidence now before us, and in part long available to us, is an indication that there have been extensive and long-continued contacts between the Old World and the New, are so described and usually linked with the lunatic fringe. It is even hinted, at times, that they have psychological problems that lead them to their diffusionist tendencies.

To me, these seem to be deplorable terms in the realm of scholarship. It is true that there are divisions between those who see much diffusion in the world and those who see only limited amounts, but it is only a degree of difference. No one

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denies that diffusion occurs, and most people would agree that it is the dominant feature in the affairs of mankind. One finds extremists, of course, on both sides of the fence.

I recently read about a particular trait, megalithic structures, in Europe. The author had it reinvented at least *six times* in western Europe! This I would call an extreme view. Whether it was invented in northwestern Europe and diffused to the southeast, or invented in the western Mediterranean area and diffused to the northwest, this very specific trait is a classic case of diffusion. It is no different from Gothic architecture: an unmistakable art style in building, having a particular origin in time and place, then spreading widely.

That brings us back to the word “cultist”, as a term to designate a group defending or espousing any particular idea, set of ideas, or points of view. Cult usually is identified with religion, and especially a particular subdivision of a religion. It carries a connotation of religious fervor. In my opinion, this is the particular meaning implied by those using the term so freely today. Yet I am unaware of any more fervid, passionate, emotional, and irrational writers than those “cultists” who oppose diffusion in general, and any such connections between the Old and New Worlds in particular.

Curiously, it is this very group who slyly hint about diffusionists having some sort of psychological need. They seem quite oblivious to the fact that one can just as easily—indeed, much more easily—point to an apparent emotional drive, indicative of some psychic need, that obviously leads them to *their* excesses.

The recent writings of John Cole provide excellent examples of this frantic attack on the “cultists”, meaning those who disagree with him. In Britain, one need only look to *Antiquity* to see even more emotional attacks on the diffusionist position. If emotion is the measure of cultism, then it’s the anti-diffusionists who are today the cultists.

If we examine the meaning of diffusion, we find that it is simply the spreading of ideas, technologies, and so forth from one person to another, one people to another, one area to another. Essentially, it is a learning process, and it is perfectly clear that in mankind it is the paramount factor in personal, cultural, regional, and national formation.

A. L. Kroeber once calculated that in going from tribelet to tribelet in California, one could *at the most* credit each with the invention of 10% of its culture. Elsewhere, Kroeber clearly considers diffusion as the master process, with invention a relatively rare occurrence. This principle he defends and exemplifies worldwide, except when it involves the crossing of a major ocean, e.g., to America, whether by the Atlantic or the Pacific. He looked upon great oceans as virtually absolute

barriers to the spread of knowledge.

This emotional rejection of transoceanic diffusion is rampant today, and calls for some explanation. Why is it not simply a matter of one more controversy concerning the careful examination of data? Unfortunately, such an inquiry requires a reading of the other person’s mind. This, of course, leads all too easily to the attributing of motives, and such judgments may not be true.

It is, therefore, with extreme diffidence that I make a few suggestions. To begin, America has been viewed as a sort of laboratory in some of the social sciences. It was a *tabula rasa* for the Europeans: hence the expression “New World.” Everything started over again, almost as though in a laboratory experiment in which the purity of the glassware, the medium in which the test is to take place, must be controlled. If many influences were permitted, whether in the experiment of man entering America at an early date or in the long interval between such entry and the post-Columbians, the picture then became not just a simple laboratory experiment, but an enormously complex and “messy” problem. One can only guess how much of this attitude, whether consciously or otherwise, has entered into the ferocious rejection of evidence for a plurality of important cultural influences from elsewhere in the world.

It is further true that the text books have been written, articles published, and reputations staked on positions taken. Lecture notes have those positions firmly fossilized in them. Thus it is that generations of anthropologists in America have been raised on the anti-diffusionist model, and taught to view as “cultists” those who espouse diffusion as a major factor in cultural growth.

This practice is fostered and continued by the lampooning of works performed by some of the great pioneers and researchers, or the lumping together of charlatans or idiots with gifted and sincere researchers. Not uncommonly in today’s world, we may see Heyerdahl paired with Von Daniken, although they appear to have little in common, from my point of view. Sir Grafton Elliot Smith is dismissed by nearly everyone as a nut who believed that everything started in Egypt.

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I was raised on the “G. Elliot Smith is a nut” myth at Berkeley, when training in anthropology. Naturally, I avoided reading Smith—I had not the time to waste on idiocy. But years later, while pursuing the question of elephants in America, I felt compelled to look at Smith’s *Elephants and Ethnologists*. I was astonished to discover that it is witty, well informed, and makes Smith’s opponents look foolish. Further works by this man revealed that he did indeed start out with

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an “Egypt-as-center” complex. But, as the years went by, he broadened enormously, with Egypt retreating accordingly. Smith is well worth reading, although one has to remember that we have learned a bit in the ensuing half-century.

We are supposed to learn, are we not? Kroeber, in the early 1930s, espoused the notion that one small band of hunters entered America about the time of Christ, and that the total racial and cultural picture found in America in 1492 had developed—in that brief period of 1500 years—from that single inoculation.

I was then an undergraduate student in anthropology, and remember this pronouncement in his classroom lectures. Lest I be too hastily charged with a faulty memory, we might note that he also published it in *The Maya and Their Neighbors*. It is hardly possible that Kroeber would hold to that notion today, but I am sure that he would view with great caution any suggestion of extreme antiquity for man in America, or the evidence for diffusion across the major bodies of water.

It is abundantly clear, within the whole world of knowledge, that *resistance to new ideas is most extreme inside the field by those presumably most expert in that particular area*. T. S. Kuhn, in writing on this problem, drew numerous parallels from chemistry and physics. In the medical field, one has only to review the resistance met by the germ theory and vaccination, to see the medical men of that time acting just like the anthropologists now, furiously resisting change. The greater the proposed change, the stronger the resistance. Most fanatical of all will be those securely entrenched within the field where the change is proposed.

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We find a curious ambivalence in the demands for evidence of diffusion. At times, just one trait or one kind of evidence is dealt with, and it is insisted that this and only this one item be discussed. To clutter our minds with a number of other things is considered bad scholarship. Yet, on other occasions, it is required that a whole complex of traits be produced. It is true, of course, that by piling up many irrelevant things, all or many of which are only possibly or probably true, one can arrive at some very odd conclusions. But it is also true that if one trait turns up, it is very likely that there are others.

Americans speak English, for instance, suggesting to us that there has been some connection with that distant land across an ocean. The presence of bicameral government, common religions, and some racial similarities strengthen the probability of a real cultural connection between England

and the United States (as, of course, we know there is). It would be a complex of this sort, had all records been lost, that would assure us there had once been such a connection, and would even let us know something of its nature. One would surmise that there had been massive and continued influence—colonization, in fact. If we happened to find similar relationships elsewhere in time or space, it would seem natural to consider that the causes were similar. Or so it seems to me.

Before going further, it seems necessary to consider what kind of model we are using. Kroeber’s model—and he can stand as a useful example of American anthropologists in the first part of this century—involved a late arrival of one limited type or segment of mankind, with rapid development thereafter of the full breadth of American cultures, without any outside influence. Slowly and reluctantly, there has been grudging acknowledgment of some slight influence, principally Asiatic, on the growth of the civilizations of America. I am acutely aware of this movement, for I have participated in it. As a product of Berkeley anthropology in the 1930s, I was an unquestioning follower of the isolationist camp. When my understanding began to broaden a little, I conceived of an arrival—late in time, from across the vast sea—a single Polynesian canoe, its occupants picking up a sweet potato, pausing barely long enough to learn its name, and carrying it back to Polynesia. So it was that I was startled one day to hear Robert Heine Geldern begin linking different waves of cultural influences in the Americas to specific peoples, kingdoms, and dynasties on the continent of Asia. It now appears that his model, far more complex, is the better one; i.e., many influences, at various times, from different parts of Asia, spreading to separate areas in America. And now, in recent years, we begin to add equally numerous and weighty influences, perhaps, coming across the Atlantic. The model that some of us are now working with is poles apart from our starting position.

If the model is as complex as it now appears to be, then numerous difficulties confront us. One cannot progress very far by simply piling up a list of similar traits in the Old and New Worlds. There are such lists, of course, with traits from all over the globe, and from immense spans of time. They suggest massive diffusion, to one school of thought; to the opposite, immense inventiveness in mankind.

More is accomplished with lists, however, when they apply to specific areas. Rowe’s published (1966) list of Andean-Mediterranean traits—which, as Jett and Carter pointed out, probably proved the reverse of what Rowe thought—is an example. With its very specific traits that are common to two areas, it is very suggestive of a Mediterranean influence in Græco-Roman time, with some indications of Egyptian influence, e.g., a very specific type of axe, apparently known only in the Andean region and in Egypt. The exercise of ad-

equate care might allow one to segregate cultural complexes that are specific as to time, as well as place.

Rowe's list includes a whole set of Græco-Roman religious rites, together with specific items of clothing. Should one not then ask, "Is there still more?" Indeed there is! Dick Edgar Ibarro-Grasso, for one, has pointed to the figuring of a bronze-age sword, the presence of a classic Roman oil lamp, and other items. Heyerdahl has published on classic portrayals of European racial types. Does one now ask, "What has become of these classic Europeans in Peru?" Yes, one might reasonably ask such a question—if he were ignorant of such classic studies as Carleton Coon's *Races of Europe*, in which it is documented that within a century or two an invading minority simply disappears into the racial majority, even when bringing great cultural influences. What, then, remains to tell us that an alien group was ever present? Language, of course—but even in that direction, what is one to expect? We may profitably look at some historical models.

The Iron Age Celts overran the British Isles in at least two waves, obliterating the preceding language, whatever it was. Then came four centuries of Romans in England. In turn, their influence was wiped out by the succeeding Germanic invasions, yet leaving Celtic survivals to the west and north. Next, with the Norman conquest, there came a renewed Romance language influence. All of this 4000 year history is still evident in Great Britain today. An overly simplistic presentation, perhaps, but it may be a fair model for us, being based as it is on reality.

In the Andean area where Rowe assembled what he took to be a "meaningless" list of Mediterranean traits, I am now hearing of the presence of Indo-European speakers. The published material extends beyond mere vocabulary, right into syntax and grammar. Comment on this work by another linguist is that while some of the emphasis and details are wrong, the work is basically correct: there *is* Indo-European speech in the area!

How should we react to such information? Must we insist that the evidence for Indo-European speech be absolute and perfect in every detail, and that pure Indo-European speakers be demonstrated? That, surely, would be to deny the reality of other situations. To seek a parallel, one might point to traces of Celtic speech in Cornwall, where pure speech groups are no longer present. May we turn, then, to adjacent, tangential, or corroborative (choose your own words) evidence?

If linguists can recognize Indo-European speech, and cultural anthropologists can point out specific cultural traits, even clusters of traits appropriate to Indo-European speakers in the Andean region, is there no significance in the growing pile of evidence? As I have pointed out before (1976), there are Chinese characters and art in the Andean region, and bits of Polynesian vocabulary. What does one do with such material?

The meaning of it all is quite simple, as I see it: the Andean region has been reached at various times by various people, and with varied impact. The case seems hardly different at all from the pre-Celtic, Celtic, Roman, Germanic, Norman influences we have seen in Great Britain. What could possibly be called "cultist" about this observation?

Part of the answer to such irrational and unscientific denial of fact is found in the unthinking pronouncements, "It is well known that men could not cross the oceans," or the companion piece, "If they got to America they certainly could not get back!" Let us consider this question in greater detail.

The earliest suggestive evidence that people had some sort of watercraft capable of crossing bodies of water seems to lie in cultural relations on the two sides of the Strait of Gibraltar. These date to about 200,000 years ago. The kinds of watercraft used are not known, but I would hazard a guess that it was some form of raft, perhaps a reed bundle. Next oldest, one may suppose, would be the peopling of Australia, which would require water passages of 15 or 20 miles. Small boats today seldom go that far out to sea, except under special conditions. Australia was certainly populated by 40,000 years ago (some brave souls even suggest 100,000 years). I find no support for the theme of late invention of watercraft, in these circumstances.

There is a vast area of study in the matter of drift voyages. People can survive on wrecks, or on rafts, for six months or even a year—and that is sufficient time for crossing any ocean, even by drift alone. Some passages, in fact, are notably favorable, especially the transatlantic route. The currents and winds would sweep all craft toward America—rapidly, steadily, and dependably. As experts in primitive watercraft tell us, the problem is not navigation, but *survival*; anyone adrift at sea is bound to hit a continent.

We have greatly underestimated human ability to survive at sea. Caucasians are not particularly good at it, being neither psychologically prepared, nor culturally adapted. In many cases the skin is much too fair for withstanding exposure. To judge other races or peoples by our standards is quite misleading. Aside from the matter of protective pigmentation, many groups moving about in the remote past must have been more like some of the Polynesians, in their superb knowledge and adaptation to the sea.

The evidence, as it is known to me, suggests that people used rafts (reed bundles, or multi-log types), and perhaps even bark-shelled boats, far back in time. Just how early the start was made, of course, is quite unknown, despite hints of minor water crossings some 100,000 to 200,000 years ago. Neither can it be said at just what time it became feasible to transport by water sufficient numbers of people to exert significant influence of a cultural nature.

One might surmise that by Middle Paleolithic times—something in the order of 100,000 years ago—there were simple watercraft that would allow planned crossings of water gaps, in fair weather, when the target or objective could be seen. Inevitably, storms would catch some of the voyageurs, sweeping the craft away. Some, certainly, would make fortuitous landings on distant shores, in such circumstances.

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In Upper Paleolithic times, one could expect that there were dugout hulls with added planks, and these would lead to the construction of sizable vessels. The number of people carried, and the range of both deliberate and accidental voyages, could have taken a very large saltation.

With the advent of the Neolithic, the smaller oceanic gaps might have been crossed with some frequency. Outer islands may have been reached, and even such relatively easy passages as from the Canaries to America might have been undertaken.

I do not put forth these suggestions as established facts, but simply as speculations. Yet they may not be too far off the mark. Moreover, they tend to reduce the theme of “no trans-oceanic voyaging of any significance before 1492” to a ludicrous position.

At the present moment it seems relatively easy to provide almost any kind of evidence that might be desired, to show the spread of ideas to America, either across the Atlantic or the Pacific. We are on the verge of being able to assemble much of this evidence into units grouped in space and in time. These cultural packages will signal the arrival of meaningful contacts of varying magnitude.

What most of us have not yet grasped, curiously, is the very great complexity of what is about to emerge from all of this study. It is not a case of this people or that, this time or that. Rather, it is a matter of many people at many times and in many places. A single trait, such as the stamp and cylinder seals of Mexico and Central America, carries the implication of separate arrivals: one from the Mediterranean, the other from southeast Asia. The ultimate origin of the trait, presumably, was in the eastern Mediterranean. In both cases there are extensive associated data. For instance, in Central America there are specific shapes of manos, and specific ways of making bark cloth both pointing to southeast Asia, the source area for the type of cylinder seal found in Central America.

Well, one could go on at book length. What astounds and saddens me is that such inquiry should be called “cultist”—the work of lunatics—and that the majority of a supposedly sane, learned body, American anthropologists and many

of their colleagues abroad, should take such an emotional stand about the whole matter. They seem to think that the diffusionists have psychological problems. I submit that it is far more likely that in historical perspective it will emerge that it was the isolationist-independent inventionists, in fact, who suffered mental difficulties.

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## ON JOHN HOWLAND ROWE

A comment on Rowe’s “Diffusionism and Archaeology” (with Stephen Jett), *American Antiquity*, 1966, Vol. 31:867-870.

We must express our regret that an article (Rowe 1966) in a scholarly journal should be filled with such imputations directed at professional colleagues as “strident claims,” “fantasies,” “evasion,” “biased selection,” “nonsense,” “sixteenth century thinking,” and so forth. We must agree with Rowe’s (1966:334) statement that “the general public tends to assume, with a touching innocence, that any scholar with a Ph.D. can be trusted to present the evidence fairly.” We feel that Rowe’s article amply proves this lack of fairness (though doubtless in a different fashion that Rowe intended) by perpetuating the same lack of objectivity which he describes in “doctrinaire diffusionists,” by which he apparently means those sug-

gesting any cultural diffusion (especially within the New World) over long distances, particularly by ocean voyaging. His allusions of Ecuador and Japan, etc., demonstrate that he includes among such “doctrinaire diffusionists” even such careful and cautious workers as Meggers, Evans, and Estrada (1965) (See NEARA Journal Vol 36 no. 1). We agree with Rowe’s (1966:377) statement, “What is needed is an impartial general inquiry into the significance of cultural parallels,” but we believe that the remainder of his article belies this professed attitude of objectivity.

The diffusionism-independent invention controversy has for too long been a conflict in which, as Hamblim (1964:70), in describing another controversy, has put it, “scholars have put forward one doctrinaire type of explanation or another, and then (have) sat back with cocked typewriters in a sort of literary *High Noon*, ready to fire at any other typewriter that moved.” We agree that many fanciful diffusionist theories have been put forth (as well as many imagination-straining claims of independent invention), but we feel that there has been too much reflex reaction to new ideas which threaten entrenched ones. Attachment to the ideas of one’s teachers, one’s colleagues, or one’s own has too often led to unreasoned resistance to innovation.

Rowe seems afraid that young archaeologists in the United States are being seduced from the traditional isolationist viewpoint by biased presentations of diffusionist theories and warns (1966:337) that “any innocent archaeologist who comes away...convinced that transpacific contacts are the wave of the future is sadly deluded.

#### KILMER’S LAW

From “Kilmer’s Law, Evidence from Plants of Early Voyages,” *Oceans*, Vol. 12 No. 4, 1979.

I drifted into the study of plants as evidence of man’s early transoceanic explorations quite accidentally—serendipitously—for it arose from a study of the domestic plant geography of the American Southwestern desert. Seldom has an interest in oceanic voyaging begun in so arid an area. The study led me to realize that domestic plants are among the most intimate of man’s belongings. By studying the Indian’s corn, beans, and squash I could discover where their ancestors had come from. And from their names for certain plants I could tell how recently they had obtained them, and from whom. For example, I was once handed some pumpkin seed and told that this was *mormonvatna*.

*Vatna* is pumpkin; and *mormon* is simply—Mormon. The Mormons had given the Indians of the southwestern desert some seeds of the Hubbard squash or pumpkin, an American plant. The botany told the story, but so did the name, or at least the name should have clued any curious person to the fact that this was an introduced plant.

Not long afterward I received a manuscript that said in effect that there was something strange about the American cotton story. The wild cottons in America had a simon-pure heritage, but the domestic cottons were suspected of contamination by Old World cotton. The cotton experts who sent this material thought that man must have carried this Old World cotton across the sea, that it had hybridized in America with the American wild cottons, and that from this came the best commercial cottons in the world. But the transporting of the cotton to America would have to have been at a very early time, for good American domestic cottons were known to have been in the Western Hemisphere for thousands of years.

This set off a furor that has still not died down. We looked for evidence of cotton of the right sort at the right time in southeast Asia for, until recently, all our thoughts about men crossing the seas to America focused on the Pacific. But cotton was late to appear in southeast Asia. As research on cotton proceeded, it appeared more likely that the cotton which reached America to crossbreed with indigenous cottons probably came across the Atlantic from southwest Africa. This is a little difficult to explain, for we do not think of the Africans as early agriculturists, or of having good boats. Maybe we have something to learn.

There are other plants whose transoceanic transfer is in question. One such is the sweet potato, which also has a long and stormy history in the academic sea. The first botanist to do serious work in the Pacific collected sweet potatoes in Polynesia and recorded their names as *uwala*, *umara*, *kumara*, changing from island group to island group. This is similar to variations in Latin, German, and English, such as *pater*, *vater*, *father*; *mater*, *mutter*. This degree of divergence took two or three thousand years, and similar deviation in Polynesia suggests a comparable period there, too. To the botanist’s surprise, when he reached America he found that on the coast of northwestern South America the name for the sweet potato was *kumar*. But this caused a near panic among American anthropologists who had long thought the sweet potato to be indigenous to America. And no one was supposed to have reached America and supplied the American Indians with such things as names for useful plants. This was a breach of the Monroe doctrine of anthropology.

For a while the sweet potato got a rough reception. Roland B. Dixon, a Harvard professor, concluded that it had been in Polynesia for at least 500 years, and that the Polynesians were quite capable of carrying the plant to America. A concerted effort was made to have the sweet potato declared “un-American.” The good standing of the sweet potato was restored by the work of two researchers (one in Japan, other in New Zealand) who studied the cytology and the taxonomy and proved it to be American all right after all. The embarrassment was just beginning, however, for as early as the 1890s



a scholar had investigated the name kumara and pointed out that it is neither an American nor a Polynesian word, but it is an old Sanskrit word; it came from India.

Now that poses a whole set of other questions. Under what conditions is a foreign name conferred upon a native plant? It is most unlikely that a casual stranger coming along would say to a native people, "Here you idiots, don't call that plant *apitchu*, call it kumara," and have them say, "Yes, boss," and promptly change it. It usually works the other way around. The stranger meeting a new plant asks, "What do you call this?", and then adopts the name that he is given. That is why corn is called maize in most of the world, for that is the name the Spaniards learned from the Caribbean natives from whom they received their first corn. But that raises the interesting question of why English and their colonial descendants call it corn. The difference is that Anglo-Saxon immigrants inundated a land had been emptied by disease, and instead of adopting the native name for this new grain, they bestowed an English name on it. Wheat, or barley, or oats meant a specific plant, so they took a word that meant any small grain. One such small grain is corn.

Another vegetable wanderer that points back across the Pacific to Asia is the coconut. O. F. Cook, in the early part of this century, was convinced that its home was in America and that it had been carried to the Indian Ocean. The evidence today appears to point to the other way around. If the coconut went from America to Polynesian Islands and thence to the Indian Ocean, it must have been traveling with the winds and currents all the way, but even then there is some question of whether it could survive such long drifts at sea. But if it came to America against wind and current, then there is no possibility of its doing so on its own. Desperate attempts have been made to explain this, using the eastbound Equatorial Counter current to transport coconuts. However, the countercurrent is intermittent in time and space and can carry nothing to America. Lately the problem has been fed into a computer and the answer was: impossible, man must carry it. So, men brought coconuts across the wide Pacific to America, and took sweet potatoes back.

The list of transoceanic plant migrations tends to grow over the years. Peanuts are strictly American. They originated somewhere near southern Brazil, and made their way to the Peruvian coast a few thousand years ago. The record of them is nicely preserved owing to the great aridity of the coastal desert, and we can trace the changes in peanuts throughout the centuries. Peanuts first appeared in Chinese Archaeology in the pre-Shang period, that is before 1500 B.C. Moreover, the peanuts that the Chinese grow today are not at all like America commercial peanuts. They are like

the peanuts that were grown more than a thousand years ago, but were no longer cultivated there when the Spanish arrived. This curious fact was first observed by a botanical economist at Harvard, Oakes Ames. He noticed that the peanuts sold in Chinatown were not from American commercial strains, but were good old Chinese peanuts imported to suit homesick customers. Ames determined to find out when and where such peanuts were now, or whether they had once been grown in America. It was long after his death that the archaeological evidence of peanuts in China 4,000 years ago showed up.

A plant that particularly interested me was *Hibiscus rosa sinensis*. This is the showy red trumpet-flowered hibiscus so beloved by Hawaiian maidens. As the name implies, the botanists considered it an ancient endemic plant of Asia, and probably China. But a Dutch ornithologist noted that in the East Indies it was not naturally pollinated by the local flower birds, and he made the suggestion it was really suited for pollination by the hummingbird, a strictly American species

Evidence of the plant's American origin finally turned up in Chinese annals. The northern Chinese were introduced to the red-flowered hibiscus by the southern Chinese, and were sufficiently impressed to write poems about the flower.

They recorded that the southern Chinese said they had obtained this flower from a vast [land]

below the eastern horizon. Well, that is where America is, and that is where the home of the hibiscus, that needs a hummingbird to pollinate it, is.

The list can be extended, but this should be enough. Useful plants, even flowers, were carried back and forth across the Pacific. The peanut seems to have been transported before 1500 B.C., and the hibiscus was being exported to Persia from China in the first century B.C. Should one ask for a written record of this, one has but to refer to the Chinese legend of the land of Fusang, for it probably alludes to America (*Oceans*, Vol. II, No 3).

Plants in the Pacific have played a considerable role in supplying the evidence needed to support theories on man's travels. Scholars have long pointed to various arts and technologies that were similar in Asia and America, but their views were always brushed aside with the contention that people everywhere were alike, and so it was to be expected that they would invent similar things. Similar architecture, similar calendars, similar myths—the list is very long—but to the independent "inventionists" it did not matter. Men could invent anything at any time and so long lists of similarities were inevitable. But no one can invent a plant. I have long enjoyed calling this Kilmer's Law:

**When plants can be proven to have been on both sides of the Pacific, the world's greatest ocean, it seems very likely that man carried them.**

Hypotheses are made by fools like me, but only God can make a tree, sweet potato, peanut, hibiscus. When plants can be proven to have been on both sides of the Pacific, the world's greatest ocean, it seems very likely that man carried them. This is especially true for poor swimmers and flyers such as the sweet potato and the peanut. When one further considers that the name is the same on both shores, as is the case of the sweet potato, then it is virtually impossible to deny that men from some quite early time were making deliberate voyages back and forth across the Pacific.

The main role of all this plant evidence was in reopening the inquiry during the early part of the twentieth century into the evidence that men had crossed the world's greatest oceans early, deliberately and easily. In the face of the adamant refusal to believe unless presented with hard facts that could not possibly be explained away by independent invention, the plants have played their part. They were carried; they cannot have been invented. I invoke Kilmer's law.

### ON SHELLS

From "Shells as Evidence: G. Elliot Smith and J. Wilfrid Jackson." *The New Diffusionist*, June 1976, Vol. 6, No.23 pp.50-57.

The Manchester School of diffusion, indelibly linked with G. Elliot Smith, has for long been thrown out of the body of scholarship. It is a classic case of throwing the baby out with the bath water, and we may well review some of these early studies to see just what merits they might have. Of course, they have faults, but that is true of any work that is fifty or more years old, as it will be of current work in fifty years time. My aim is to salvage some of the good, and deal lightly with the scholarly sins that emerge.

Smith points out in the preface that, worldwide, shells are not just ornaments, but have arbitrary meanings which give them value. Perhaps this seems a bit sweeping, but it certainly has a huge element of truth in it. As his prime example, Smith cites the cowry and makes a typical claim in his introduction: "If one asked the question, 'how did these remarkable qualities come to be attributed to certain shells,' the answer is plainly given by the collection of data brought together by Mr. Jackson."

Jackson assembled the data on the shell-purple industry, in the most exhaustive work on the topic to date, perhaps still unsurpassed. He reviewed Pliny's and Aristotle's detailed descriptions of the preparation, including the description of the particular organ of the animal that produced the dye. He cites authorities for Crete around 1600 B.C. as the earliest source for this dye. The Phoenicians spread it widely, and Sidon and Tyre were early noted for their purple production. The Phoenicians sought this shellfish widely, not only down the Mediterranean, but out onto the Atlantic face of Europe, even to the British Isles, where occur great heaps of *Purpura*

*lapillus* shells broken in the right way to extract the dye. He cites here Wood-Martin on the *Lake Dwellings of Ireland* who names Tigearnas as introducing this dye about 1000 B.C., and gold smelting also. This dye industry persisted in Britain up to the eighteenth century. One has then, from shells, a nice case of diffusion and persistence.

Equally interesting is the case of shell-purple industry in America—southern Mexico to Peru—for it is clearly pre-Columbian. The American shellfish is easily utilized without killing it because, when taken up, it pulls back into its shell and exudes a small quantity of liquid which, when exposed to the sun, changes from cream, to green, to purple. It is easy to envision the independent invention of shell-purple through the accidental wetting of clothing by such material. The American case as an example of diffusion must be considered unproven though the possibility of European shipping arriving intermittently in America cannot be overlooked.

Surprising little attention has been paid to Jackson's data on occurrence of *Cyprea moneta*, the money cowry, in America. This shell has a natural distribution around the Indian Ocean and extending into Polynesian section of the Pacific. It does not occur on the American side of the Pacific nor is it in the Mediterranean or Atlantic. Yet this particular species is, and has been for centuries, a sacred object among the Ojibwa and Menomini Indians of North America, and "is employed in initiation ceremonies of the Grand Medicine Society" (Jackson:1840).

Jackson noted the identity in the practices of the spitting out of cowries as practiced by the Ojibwa and the Togo priests of West Africa. We seem to be looking at a complex of curing, resuscitation, and resurrection associated with the cowry in both Old and the New Worlds and specifically focused on a shell not native to the latter.

Jackson also has fun with the *wunder glaubers* as he comments on some ridiculous attempt to explain these shells away. "After all these imaginary wanderings and episodes on sea and land, the cowries should have eventually come to rest in the heart of the American Continent and, 'of course purely by accident', have been linked up with the identical beliefs and fantastic practices with which they are associated in Africa, India and Eastern Asia" (Jackson, p.188.).

Experts in the archeology of the eastern United States could refine this early time-placement somewhat by reviewing Moore's pottery finds, but the most probable date for these cowries would remain around the time of Christ, and of course, if the association is actually Adena, it could be earlier, even back to 1000 B.C., the time suggested for shell-purple in Britain.

This then seems to establish an Old World bio-artifact in the New World, without the possibility of psycho-unity ex-

planation. This shell had to be imported. The probability that associated ideas accompanied it would seem rather high.

I will not extend this further to shell trumpets, pearls, and gold as discussed by Jackson. He presents mountains of data. It is a rich source book and the effort here is to show that we should not have overlooked it. We have plenty of unknowns, e.g. where *did* the Adena culture and northeastern United States pottery come from? Careful compilations of data such as Jackson assembled have a permanent value and should not be ostracized.

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#### ON CATS

On "Norse Importation of Domestic Animals into North America," *ESOP* Vol. 17, 1988, pp.65-70.

A most interesting case has emerged concerning Norse cats. An Icelandic geneticist undertook a study of cats. He established that there is a race of distinctively Nordic cats. These are very different from English cats. They are larger, stronger, have denser and longer fur, different color patterns in their coats, etc. When inquiry into American cats was initiated, Blumenberg looked at cats in the Boston-New York area. To everyone's surprise these cats proved to be Nordic cats, NOT English cats. It is the conclusion of the geneticists that the Norse brought them, that the cats either jumped ship, or were traded to the Indians, and that a feral cat population was established in the New England area. These large fierce cats then killed off the English male cats when they were introduced and established their genes in the area.

The authors of the cat paper saw some problems. There is no mention of cats by the earliest explorers and settlers. They suppose that the cats were unimportant to the Indians, if they had ever kept them. The probability of cats having gone wild, they think, is quite great. A feral cat population would be almost invisible to the settlers until they imported cats and one of the pussy cats came in heat. The Norse males would then soon be evident, and being much larger and stronger than the English cats, would contribute their genes, while possibly killing off some of the English males. One might also note that explorers often did not note the commonplace. Nordenskiöld, a student of ethnology of South America, noted that if you depended on the reports of the earliest reporters, then you would never know that the Indians had dogs. They seemingly were thought of as too unimportant to merit mention.

Cats were established in the folklore of Ireland in the seventh century. In Greenland around A.D. 1000, a seeress had a lambskin hat lined with white cat's fur, as well as white cat fur gloves. In Iceland around A.D. 1200, cat pelts were lawful currency and worth three fox skins. From this it is concluded that cats were quite likely taken along on ships to control rats, and as valuable trade items.

It is interesting to see these scientists handling the data and then proceeding to speculate. They reject the Kensington Rune Stone, the Newport Tower and all other evidence for Norse contact with America, except the L'Anse aux Meadow site in Newfoundland and the Viking age coin from Naskeag Point in the Blue Hill Bay in Maine. As re-examination of the Kensington Stone indicates, it is much more probably genuine than fraudulent.

**It is the conclusion of the geneticists that the Norse brought [the cats]...and that a feral cat population was established in the New England area.**

The authors also struggle with the coin in Maine. They suggest that it was traded through a chain of tribes all the way from Newfoundland to Maine. Occam's razor would call for the assumption that it was introduced right where it was found. Or, now that there is evidence for Nordic cats in the Boston area, have the coin traded north from that nearby place.

In the far north it now appears that extensive evidence of Norse contact is being found over a wide area in the vicinity of Ellesmere Island. Of particular interest is the presence of nests prepared for eider ducks. The Eskimo did not use eider down, but the Norse did. The eider duck nests indicate Norse residence—not mere trading contact. Farther south there is evidence of Norse settlement on the Ungava Peninsula, on the east side of Hudson Bay. Still farther south the Ingstads found a Norse site at L'Anse aux Meadow in Newfoundland. Although spotty, the evidence suggests wide contact and probable settlement in northeastern North America.

What is slowly emerging is that there was much more important Norse contact than that mentioned in the sagas. This should really not be surprising. The sagas are the account of the exploits of one family, the Ericsons and their in-laws. It is a freak accident of history that these verbal accounts actually were written down, and it is rather unimaginative to assume that that was all there was to the Norse contact with America

Archeologists should take note of much of this. The finding of the bones of domestic animals of European origin in sites should not be taken as proof of post A.D. 1500 date. It

could well be evidence of Norse introductions. With the insistent demand that evidence be produced of European contact in parallel with the epigraphic material, it appears that this domestic animal evidence supplies some of that data. One does wonder if a good deal of such evidence has not turned up and been swept under the academic rug. I recall in particular the finding of a horse skull in an Indian mound in the Great lakes region. It had six carbon 14 datings determined on it. They all placed it between A.D. 500 - 700. It is rarely mentioned in the literature, but I would think that it was a very important datum.

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## ON ELEPHANTS

From "Elephants and Ethnologists: Fifty Years Later," *The New Diffusionist*, Vol. 5, No.21, October 1975.

The anthropologists have been most sensitive to criticism and have responded to much of it by hurling thunderbolts at their critics. It is a bit sad, for one always expects scholars to be demi-gods, calmly accepting the frailties of mankind and moving majestically on with the careful weighing of evidence. Unfortunately, scholars are all too human. Their reputations and their careers are tied up with their professional adherence to the paradigm of the day, and they band together to hound those with the temerity to stray from the accepted path. One of those that dared stray was G. Elliot Smith. This has made his name anathema, and as a young anthropologist I was told that he was a madman, and that there was no point in reading him. So, decades went by from the time of my receiving an A.B. degree in anthropology at the University of California under A.L. Kroeber, Robert Lowie, E.W. Gifford, and Ronald Olson, until I finally read something by G. Elliot Smith.

As so often happens in scholarship, I reached Smith by a circuitous route. One of my interests is in the problem of antiquity of man in America. This led eventually to considerable interest in the elephant in America. At first I was drawn into the controversy over whether or not man and elephants had lived in America at the same time. One has to go back in the literature only 30 years to find great anthropologists, even so wise a man as Loren Eiseley, furiously attacking the notion

that this was true. Today of course it is commonplace to find the remains of an elephant kill, complete with the artifacts used to slay the animal. While the greatest concentration of such finds is in the Clovis period about 10,000 to 12,000 years ago, there are much earlier finds, such as those on Santa Rosa Island where men were barbecuing elephants about 30,000 years ago (Orr, 1968), and much later ones that may bring the elephant within the range of folk memory (Johnson, 1952).

Following elephants led me to look at the stone with an elephant inscribed on it from Bucks County, Pennsylvania, and to the shell with the inscribed elephant from Delaware, and to put my students to research on American Indian recollections of the elephant (Johnson, 1952). The gist of these studies indicate that the elephant survived in America long enough to have been retained in the folklore of at least of the Indians of the eastern United States. While pursuing these lines of inquiry I was led to look at evidence for elephants elsewhere in America. Uhles' mastodon was a case again of a late survival of elephants in South America (Carter, 1968).

The Verrills' find of a statue of an elephant in Central America, however, was clearly something else again, for if it were valid, it was a case of an Asiatic representation of an elephant finding its way to America (Verrill and Verrill, 1953, plate 2). (FIGURE 1) This led me to look at representations of elephants in Mexico, and to the stela at Copan (FIGURE 1A) and to the elephant head affix in Mayan writing and the elephant masked figure published by von Humboldt from an Aztec manuscript, and finally, to reading G. Elliot Smith's *Elephants and Ethnologists*. I was stunned. The man was surely not mad, and he was surely well-informed, and he surely should not have been put on the Anthropological Index—those books that no anthropologist is supposed to read. Further, he most emphatically was *not* saying that everything came from Egypt. Quite the contrary, Smith was pointing to India and



Fig. 1a. Elephant stela at Copan (Mexico).

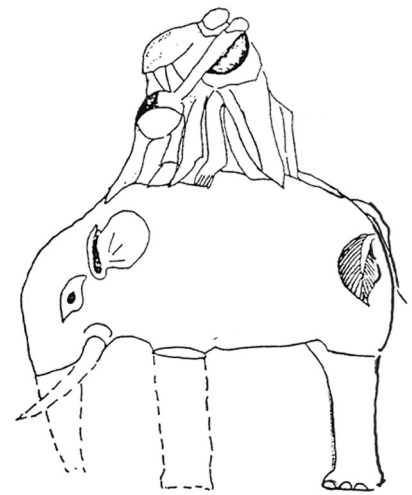


Fig. 1. Elephant figure from Panama (of unspecified date).

especially Cambodia. This is the current thought of people like Kelley (1960), Kirchoff (1964), Ekholm and Heine-Geldern (1964). So the man was not mad. He was misrepresented. He had a lot to say, and much of it is relevant today.

A somewhat lofty view taken by a reviewer of *Man Across the Sea* (Riley, et al., 1971) was that diffusion is not important in modern anthropology because the focus in recent years has been the study of cultural systems and the processes of cultural change. The archaeologist as anthropologist is now said to be concerned with the processes of culture change and seeks to learn of the ramifying effects of new cultural traits on cultural systems, rather than of the origins of the traits themselves. I find it a bit strange that archaeo-anthropologists are uninterested in whether a culture system generates its own traits or imports them or, if there is some of both, then which are which, and which are more important and what the sudden introduction of a new set of ideas does to a culture. Probably no one was more interested in the ramifying effects of Hindu-Cambodian cultural traits in the cultural systems of Meso-America than Smith. For that matter, some of the contributors to *Man Across the Sea* were process-oriented too.

Smith is quite interesting in his dealing with Tylor, whom he clearly respects. He points out that Tylor was not originally a dogmatic independent inventionist, and perhaps never became one. Smith notes that Tylor considered the Mexican game *patolli* to be a case of diffusion of the Indian *pachesi* to America, and that he had pointed out that the Japanese and Mexican journey-of-the-spirit beliefs proved a connection. He quotes Tylor, "...that the appearance of analogues so close and complex of Buddhist ideas in Mexico constituted a correspondence of so high an order as to preclude any explanation except direct transmission from one religion to another." And, "...on these cumulative proofs, anthropologists might well feel justified in treating the nations of America as having reached their level of culture under Asiatic influence (pp.35-36)." These were strong words indeed for the allegedly arch independent-inventionist Tylor to utter.

Smith reviews at some length Stempel's examination of the elephant problem. Stempel was a biologist and argued from biological grounds, and concluded that most of the long nosed representations were indeed referring to elephants, and not to tapirs. As noted earlier, Stempel wanted to attribute these to late surviving elephants in America as models. This is by no means as impossible as it once seemed (G.F. Carter on Uhle's elephant, 1968), but it fails of conviction when the whole complex of Indonesian features are considered.

Differing cultures see different things in the moon—an old man, or a rabbit. The shadows on the moon are so vague that what one sees is culturally patterned. Here, where vagueness is not the case, what one sees is still culturally patterned—tortoise, macaw or elephant? A properly trained

early twentieth century anthropologist could see anything except an elephant, but that was because he *knew*, having been taught, that the American Indian didn't know the elephant and hence couldn't portray elephants. This is indeed the kind of faith that moves mountains—or removes elephants.

Elephant headdresses deserve special note, as Smith clearly saw. He showed the headdresses that resulted from Alexander's contact with India as reflected on Greek coins. (FIGURES 25 AND 26) One might postulate that baby elephants were used for these headdresses, but this is impossible since tusks are shown. Or one could suppose that adult skins were shrunk down to human size, but the tusks would then be disproportionately large. It seems more probable, that these head ornaments were carefully constructed artificial elephant headdresses.

In a chapter on the involved mythic beliefs concerning the elephant in India and America, Smith reviews a mass of data that only someone immersed in the religion of India and of America could evaluate. Only recently we have had a few scholars attempt such wide straddles, notably Kirchoff (1964) and Kelley (1960) with their studies of calendars, deities and religious transfers from India to America. Most of their work



Fig. 26. Elephant headdress from Palenque. Drawn by Waldeck. (This looks very much like Elliot Smith's Pl. 14 from a Greek coin showing elephant headdress worn by a Hellenistic king.)



Fig. 25. Elephant headdress from Asia.

**Elephant headdresses in Asia and America. The clearest portrayals of Asiatic elephant headdresses are those inspired by the Alexandrian contact with India. Elephant headdresses thereafter appear on coins. Note the characteristic trunk position and the tusks on Fig. 25.**



Fig. 27. Ganesha is the elephant-headed god of India and, strangely, elephant-headed men also appear in America. This one is from Ecuador.

tends to support Smith's insights of fifty years ago. Whole religious complexes *were* transferred to America. Since the elephant plays a very prominent part in Indian religion, it becomes expectable that it too would be transferred to America, and the presence of Ganesa figures is further evidence that it was. (FIGURE 27)

A chapter on the *makara*, a type of dragon, is one of those neglected vignettes. The Maya country in particular is filled with strange figures: part animal, part human, part fish, and part some other animal. In India these are makaras, the progenitors of the dragon. In Asia and also in America, men are often shown emerging from the mouths of makaras. Von Humboldt, as so often is the case, had seen these resemblances, and even noted the identity of the association with the same sign in the zodiac in India and Mexico.

The trouble then with *Elephants and Ethnologists* is not G. Elliot Smith, but the time in which it appeared. Americanist thought was in a regressive mood. We had withdrawn inside our shell much like the Japanese withdrawal after their first contact with the west. Smith correctly saw that American Indian civilizations did not develop in isolation from the Old World, but under some considerable degree of influence, and that the elephant at Copan signaled this. He did not stop there, but by his studies of the makaras and other significant material clearly showed the Indo-Cambodian origin of what is now coming to be seen as one of the major influences on American Indian civilization. It is true, of course, that when he started in 1910 he saw it all as Egyptian. But by the time he wrote *Elephants and Ethnologists* he saw things in a far wider frame. It is a pity that his critics didn't grow proportionately, and even more of a pity that the dogmatic biases of fifty years ago still echo in our university halls and discourage later generations from reading this stimulating thinker.

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## ON CHICKENS

From "Pre-columbian Chickens in America," *Anthropological Journal of Canada*, Vol. 9, No. 3, pp.2-5, 1971.

After nearly 25 years of desultory accumulation of notes on chickens in America, I began three years ago a fairly intensive pulling together of the data. It is an absorbing and apparently endless story that illustrates how severely our preconceptions handicap our perceptions.

The chicken was domesticated somewhere in Southeast Asia, and perhaps over a fairly wide zone from India to Malaysia. The real time is unknown, but may have been around 3000 B.C., for the chicken is said to be present in Mohenjo Daro. Firm evidence for the chicken appears with its introduction into China, Mesopotamia and Egypt around 1500 B.C. In Egypt the chicken appeared briefly and then disappeared from the record for 1500 years.

From the evidence it is clear that eating chickens and chicken eggs was an afterthought, and that the original domestication must have been for ritual and cockfighting, probably in that order. In the West, eating of chickens and chicken eggs was a Greek contribution dating to around 300 B.C. They had gotten the bird at least two centuries earlier from the Persians who, of course, had gotten their birds from India. The Persians got a special kind of chicken, one with a large comb, bare feet, rather small in size, of nervous temperament, with tight feathers, and with white-shelled eggs. This is now known as the Mediterranean race, since it was spread throughout that area and from there into northwest Europe. Until well into the nineteenth century this seems to have been the only kind of chicken known to the west Europeans. At some time, probably earlier, Asiatic races of chickens reached Eastern Europe; an instance is the chicken known as the Transylvanian Naked Neck.

One can treat chickens in terms of race just as one treats man. Thus there is the Mediterranean race, just described. There is also a Sinitic race, although it is usually referred to as the Asiatic race. These chickens are large-bodied, fluffy-feathered, flat-combed, feather-footed, phlegmatic, and always lay brown-shelled eggs. I prefer to refer to these birds as Sinitic in order to differentiate them from another Asiatic race, the Malays. These are very large, muscular, ground-dwelling birds with a hawk-like visage, and frequently have large patches of bare skin. One distinctive type of the Malay race has a featherless neck. This Naked Neck chicken is very distinctive, easy to recognize, and has come to have an important role in the historiography of the chicken. There are also bantam chickens of two types: dwarfs of established breeds, and true bantams, i.e., birds that occur only in diminutive size. True bantams may be Indian in origin. Then there are special breeds such as the melanotic silkies. These birds are black throughout, including the skin, blood, flesh, and bones. The feathers are imperfect and the bird looks as if it were hair-covered rather than feathered. The feathering may be snow white or jet black. In India they have their own distinctive name: *karaklwtil*. In

some way they are especially involved in magic and ritual.

The point of this brief catalogue is to show that chickens are readily divided into distinctive races that, until fairly recently, occupied quite separate parts of the world. The present mixed situation is largely a matter of the last 100 years, following upon the nineteenth century Europeans discovering something of the variety that existed in the world and becoming fascinated with these beautiful and useful birds.

None of this has been of much concern to Americanists. It was assumed that the chicken was brought to America by the Europeans, and almost all ethnological reports brush chickens aside as an unimportant post-1500 introduction. This is probably wrong, and the indications have been in the record for a long time. Acosta in 1590 noted that while the Indians used Spanish names for animals that the Spanish introduced, *they had their own names for chickens and eggs*. It struck him as suggestive of pre-Spanish chickens in America.



The early accounts are filled with mentions of chickens (*gallinas*), but some of these refer to chicken-sized tame birds, while at other times the turkey has been intended. The possibility that at still other times real chickens were meant has been generally overlooked. Capa (1890), however, plainly stated that although some of the references were not to chickens, in some areas, notably in the Argentine and Paraguay, real chickens were described by the first Europeans into the area.

Latham became interested in the chicken in South America that lays blue eggs. He was convinced of its pre-Columbian presence, but erred in attributing the blue-eggshell to crossing with a local bird. Sauer reviewed the evidence and supported Latham in his conclusion of a probable pre-Columbian presence. Indeed, almost the lone dissenter among those few who have investigated the question was Nordenskiöld. It seems to have escaped notice that Nordenskiöld did not investigate that part of the question. He merely assumed that the chicken had been introduced in A.D. 1500 and then used it as a case study in rates of diffusion.

It is a peculiar case, for it involves “instantaneous diffusion” from eastern Brazil at a rate of 100 miles per year (or more), to put the chicken in Peru so early that the last Inca and his uncle were both named chicken (Hualpa). I stress this rate of spread for it has always seemed far too rapid. Several studies of rates of diffusion of traits on a “neolithic level” have been made. They almost uniformly show that items spread at about one mile per year. I have compiled the

data for the chicken in the Old World (Carter, n.d.) and, to the extent that they will allow us to judge, shown that this is just the rate that it spread there. Why should the rate in America be 100 times as fast?

When the races of chickens in the hands of the American Indians are examined, a startling fact emerges. They are mostly Asiatic. If the Spaniards brought chickens, why should this be? How could it be? Naked Neck chickens are all over Mexico and South America. Melanotic silkies, and such strange breeds as Araueanas—the blue-egg-laying chicken—are widespread in South America. In fact, the surveys of types of chickens made by a master cock fighter (Finsterhusch) pointed out that all of South America was dominated by Asiatic breeds.

One could dream up all kinds of answers, and imaginative folk have done so. They were brought by Dutch pirates from Indonesia. The Portuguese brought them from their colonies in the last Indies. The Spanish imported them via the Manila Galleon.

Would this also account for the usage of these chickens? The American Indians, by and large, would not, and in some cases still will not, eat chickens and chicken eggs. This is true over wide areas in the Amazon, parts of Mexico and the American Southwest. This is certainly not a European outlook on the chicken, or at least not a sixteenth century outlook. Around the time of Christ, on the other hand, it would have been appropriate for most Europeans. But in the sixteenth century, Europeans, Dutch pirates, Portuguese traders, and Spanish settlers all considered chickens primarily as food. Not so the American Indians. For them the chicken was for magic, for ritual, and for feathers.

We have no real depth of knowledge on these uses of chickens by the Indians, because it has been considered unimportant, post-1500, and “un-American.” How ridiculous! If all this ritual sprang into being among the Indians so rapidly, it is a phenomenon well worth studying.

Does it mean that there was some preceding bird ritual that was transferred to the chicken, or does it mean that the chicken was non-European in source, and that these attitudes were brought in from that non-European source? The latter has been one of those unthinkable thoughts. But, when one looks to Asia, one finds just the kinds of attitudes that reappear in America and the role of the chicken in ritual is found to be very important. Some Asiatics still will not eat chickens or chicken eggs. Unfortunately, we know almost as little about chicken rituals in Asia as we do in America.

There have been several revealing observations. Both Kelley and Foster noted that there is a widespread and deeply embedded egg ritual in the folk culture in Mexico. Foster noted with surprise that there was no source for this in Spain and commented on how odd it was that a ritual such

as this should spring up so fast and become so important. The other possibility, that it was ancient, seemingly never occurred to him.

A somewhat differing outlook is revealed in *Fiestas in Mexico* (Mexican Tourism Department). For San Andres Chamula, a pre-Hispanic cock ritual is described and stated to be found throughout Meso-America. In Tlaxcala it is stated that “... they whip cocks, which are hung up one after another, until they are dead...” and again it is noted that this is a *very old pre-Columbian custom*. For Yucatan it is reported that dancers enter the dance area carrying turkeys, and that as they dance they kill the turkeys and deplume them. No evidence is given for the pre-Columbian status of these rituals. Clearly there was a very extensive bird ritual, and it included domestic fowl such as turkeys. Was there a rapid transfer to the chicken or was the chicken anciently included in such rituals?

I have examined masses of evidence. It all points the same way. Chickens probably were in America before the Spanish arrived. They were Asiatic chickens and they were used in Asiatic fashion. They were at times known by Asiatic names, e.g., in South America where melanotic silkies are widespread, one finds among the Amazonian Arawak that the names for chicken are variants of *karaknatl*, the (Asiatic) Indian name for the melanotic silky.

Only the archaeological evidence will convince the ultimate skeptics, and since that data is for others to publish, I shall do more than mention that it exists, that the archaeology is as sound as the Rock of Gibraltar, and that the identifications are probably good, but are being minutely checked now.

Meanwhile, I am planning to spend the spring in Mexico mapping the races of chickens still in the hands of the Indians, trying to learn more about the rituals and to find out why at least someone in Mexico was perceptive enough to state that the chicken rituals are “very old pre-Columbian customs.”

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