

Confessions of a Cultural Diffusionist¹

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Of course, America had often been discovered before Columbus, but it had always been bushed up.

Oscar Wilde

As an undergraduate majoring in geology at Princeton University in the late 1950s, I heard in my classes about the German meteorologist Alfred Wegener and the theory he had proposed in the 1920s called "continental drift." On the basis of the shapes of the continents plus stratigraphic, fossil, and phytogeographic distributions, he concluded that the Americas and Afro- Eurasia had once been joined, and there had once been a super-continent that subsequently broke up, its pieces then floating, over the ages, to their present positions. But, said my professors, virtually no reputable scientist had ever accepted the theory of continental drift, because no plausible mechanism had ever been proposed that could account for the continents' plowing through thousands of miles of solid oceanic crust, a phenomenon that, in any case, had never been observed.

They said that geologists' initial rejection of drift theory had not been much altered over the years even in light of post-Wegener findings concerning mineral paleo-magnetism in the rocks of the continents, paleo-magnetic alignments that were everywhere identical in very late rocks but became consistently more divergent among the continents the further back in time one went. I couldn't believe what I was hearing. There seemed to be no way in which all these phenomena could be accounted for other than by the continents having split and moved. There had to be a mechanism -- the evidence showed that -- we just hadn't identified it yet; there could, in my young view, be no reasonable basis for rejection of the notion of movement. What was wrong with all these doubting geologists?

As it turned out, and as we students were also informed as our studies proceeded, the chairman emeritus of our own department, Harry Hess, had, with colleagues, recently come up with a theory as to a possible mechanism of movement: convection currents in the plastically deformable mantle of the earth that, as they diverged at the tops of their rising plumes, split continents apart and, like snail's-paced conveyor belts, carried the resulting pieces slowly in opposite directions. This concept set the stage for the new paradigm of "plate tectonics" that accounted plausibly for a host of otherwise inexplicable phenomena and that revolutionized geology. Wegener with some modification and much amplification -- was redeemed, following the long duration of what Naomi Oreskes (1999) has called the twentieth century's "great failure" in geological theorizing. As physicist Thomas Gold (1999:37) put it, "a person who thought that continents or parts of continents might have moved in the past was ridiculed before 1960, despite the existence of good evidence from magnetic rock measurements. After 1965 anyone who did not believe in such movement was again a subject of ridicule."

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I seemed to be destined to encounter questions of intercontinental connections in my schooling. When I interviewed for admission to the Ph.D. program in geography at Johns Hopkins University, I talked with Professor George F. Carter, a one-time student of the great Berkeley cultural geographer Carl O. Sauer. Carter spoke of his interest in the question of ancient transoceanic Old World cultural influences on certain Native American societies. My education had been conventional regarding this matter, and my reaction was a

bit incredulous. "Aren't the cultures of the two hemispheres generally considered to be independent?" I asked. If there really were something to this notion, I wondered to myself, wouldn't I have heard about it?

Actually, I *had* heard that the absence of known jade deposits in Mesoamerica had caused a few people to speculate that this material, widely used among the Maya and others, must have been imported from Asia. Carter didn't put great weight on that supposed absence (a jade source had, in fact, recently been found in Guatemala); nevertheless, here he was, talking about all kinds of other evidence of contacts, especially the pre-Columbian sharing between the hemispheres of a number of cultivated-plant species. I was polite but highly skeptical; I "knew" that, until Christopher Columbus' day, everyone thought that the earth was flat, ending with an edge off which one risked falling, that naval architecture wasn't capable of creating ocean-crossing ships, and that the absence of the magnetic compass prevented sailing safely far out of sight of land. So, I wondered if Carter was "all there" intellectually. I'm afraid that the analogy to the resistance to Wegener's ideas that I had decried in geology escaped me at the time: the notion that cultural similarities across the seas, no matter how detailed, *had* to be independent, because no *means* of contact -- adequate watercraft and navigation -- existed in pre-Columbian times. But, it didn't much matter whether Carter was off-base, because I was going to Hopkins to study geomorphology, not culture history.

While still at Princeton, I had taken the University's one (temporary) anthropology course, with sociologist Melvin Tumin and visiting professor of anthropology Paul Bohannon, and had audited a course in linguistics. I had received very strong training in history in high school and, although I had been a geology major, my first aim as an undergraduate had been acquisition of *breadth* of knowledge. Accordingly, I had enrolled in many courses in what could collectively be called "culture history:" the natures and histories of art and architecture, music, world religions, and philosophy -- including a course in the philosophy of science with Carl Hempel, the philosopher most often invoked by those that wished to make the social sciences "scientific." Thus, when I took Professor Carter's advanced cultural-geography course at Johns Hopkins, I found that I was intellectually "pre-adapted" to his cultural-historical approach. My plans for pursuing geomorphology soon dropped by the wayside.

Carl Sauer and his Berkeley colleague, the pre-eminent anthropologist Alfred Louis Kroeber, had greatly influenced each other, and Carter assigned Kroeber's (1948) geographically oriented text *Anthropology* for his advanced cultural class. I was dazzled by the book's information and ideas. This was what I was *really* interested in, I discovered, but hadn't known about as an organized approach to knowledge until then. My then-new focus is best termed "culture-historical geography:" the study of the spatial aspects of the origins, diffusions, distributions, and artifactual and landscape manifestations of cultural phenomena as they have developed and changed through time.

Some of Carter's ideas -- widely considered radical and widely dismissed -- were certainly astounding, but I found those relating to transoceanic influences on the Americas to be increasingly persuasive as a whole. Carter permitted me to browse his personal library of books and offprints of articles. Most riveting to me was Thor Heyerdahl's (1953) *American Indians in the Pacific*. My parents had read Heyerdahl's (1950) *Kon-Tiki* aloud to me as a lad and I had found the adventure intriguing. But here, in *American Indians*, was an incredibly **[end p. 172]** massive compendium of multifarious lines of evidence bearing on the question of ancient circum-Pacific connections -- what the *Kon-Tiki* raft voyage had *really* been about rather than adventure alone. I stayed up late night after night devouring this material, which included plant evidence, physical-anthropological comparisons, and cultural indicators.

We students were required to produce research papers for Carter's courses. I wrote one on the discovery and settling of the Polynesian islands, in connection with which I read works other than Heyerdahl's, such as Andrew Sharp's (1957) *Ancient Voyagers in the Pacific*, which argued for west-to-east accidental settlement of the islands, not intentional voyaging from the Americas as per Heyerdahl. I found that I concurred with some

of both these authors' ideas and dissented from others. A second term paper was inspired by Erland Nordenskiöld's (1931) *Origin of the Indian Civilizations of South America*, which had been brought to my attention by Kroeber's book. This paper involved comparing ancient Indonesians and tropical South Americans, especially their shared weapon, the blowgun.

Thus, I was on my way -- on the slippery slope, some might say. My general training in geography -- the spatial science," in which diffusion and distribution are integral concepts preconditioned me to thinking in terms of movement in geographic space, and gave me background in earth-sun relationships, climatology, oceanography, and other matters relevant to the question of ocean voyaging and navigation. And I have endeavored to introduce to the study of possible long-distance contacts a level of rigor that has all too often been wanting in such work.

My main goal all along has been simple: negatively, it has been to avoid 1) trying to "explain" similarities solely by describing their respective cultural functions; and 2) untested deductions as to how specific cultural similarities in distant areas *must* have arisen according to evolutionary theory -- that is, elicited by pre-existing cultural structures, economic and demographic considerations, and so forth. Rather, my positive aim has been, and remains, to proceed from detailed empirical investigation and analysis to application to the data obtained, of carefully formulated evaluative criteria intended to ascertain, to the extent possible, *what actually happened in the past* -- specifically, attempting to distinguish between those similarities best accounted for on the basis of contact and influence and those more likely to be of independent origin. But, despite the above remarks the approach is not atheoretical, for another aim is to examine the general implications of the "revisionist" view of culture history that emerges: i.e., what all this may mean in terms of our understanding of how and under what conditions humans innovate, and how and why their cultures change or remain relatively static. The transoceanic contacts question may not have as much potential for producing paradigm shifts as did the issues addressed by Wegener, Hess, et al., but the hemispheres being to some extent united in what I call a "global ecumene" (Jett 1995) does have far-reaching implications for better comprehending humankind and its history.

Carter was no doubt pleased that I took an interest in these matters and, after I was employed at the University of California, Davis, he apprised me of a special session on trans-oceanic contacts being organized by Heyerdahl for the forthcoming 1966 International Congress of Americanists in Mar del Plata, Argentina. I submitted a proposal to present my Malaysia- tropical America study, and it was accepted. I worked hard to tighten and augment it; having asked a friend and colleague in anthropology to read it for comment, I received a one-word suggestion: "Recant." Nevertheless, I duly gave the paper at the meeting and it was published in the proceedings (Jett 1968). Attending the conference, besides Heyerdahl, were the Smithsonian Institution's Betty Meggers and Clifford Evans, who had recently published papers suggesting transpacific inputs in early Ecuador (Estrada and Meggers 1961; Meggers et al. 1965). Meggers said to me, "You're even more radical than we are!" Also in the audience was a well-known archaeological coprolite analyst. He [end p. 173] was so upset by my suggestions that he rose, physically trembling, and basically said that everyone knew that such voyaging as I suggested was impossible and that I should repudiate these ideas (he later apologized to me for his outburst).

Yet what was so radical about this paper? In it, I showed that the interior peoples of Borneo and the Carib and Arawak of Amazonia were physically more similar to each other than either was to immediately neighboring groups; that the complex of swidden-raised cultivated plants of the two areas was very similar, with some evidence suggesting a pre-Columbian sharing of a number of these; and that the cultural complexes of the regions in question were closer to each other than to those of anywhere else -- at least in terms of Nordenskiöld's (1931) list of "oceanic" traits. My conclusion was that a series of Indonesian migrations -- similar to those to Madagascar in the opposite direction -- probably reached and influenced tropical South America millennia ago.

In my youthful inexperience, I felt that I had carefully assembled and presented a mass of evidence and that the force of this would inevitably make an impact -- perhaps even revolutionize thinking on the subject of transoceanic contacts. However, despite presentation of three different kinds of evidence, including biological, all suggesting influence from Asia, I recall seeing the resulting article cited only a few times by anyone other than myself, much less its transforming thought in the field!

In the aforementioned work, I looked particularly closely at the blowgun complex and found dozens of detailed points in common between the hemispheres. This led to a global survey of the complex published in the *Annals* (1970); I was being considered for tenure and there *was* pressure not to appear too radical, so I merely raised the possibility of inter-hemispheric connections, without elaborating or taking a position -- for which restraint I was gently chided by George Carter. But I did attain tenure! Years later, I assembled much additional data on the blowgun and explicitly explored the concept of post-Pleistocene sea-level rises having set off overseas migrations of Malaysians (Jett 1991b).

Despite being isolationists for the most part, a group of anthropologists and geographers at Southern Illinois University, including Campbell Pennington, were sufficiently fair-minded to feel that the transoceanic question was serious enough to deserve major re-examination. Accordingly, they organized a symposium on the subject in connection with the Society for American Archaeology's 1968 annual meeting in Santa Fe. Interestingly, the organizers had difficulty in finding more than a few people willing publicly to represent the majority, anti-contacts position -- perhaps owing to the difficulty of proving a negative. Probably because of Carter's influence, I was invited to participate. The resulting paper (Jett 1971) on methodology relevant to the issue became Chapter One in the organizers' *Man across the Sea: Problems of Transoceanic Contacts* -- a University of Texas Press product. This volume became something of a classic and a good seller for the publisher, and was republished, in part, in Japanese. But its influence has not been as great as the contributors might have hoped. However, in assembling information and examples for my chapter and in considering the methodological issues in detail, I personally became increasingly convinced that abundant evidence existed to support the idea of important pre-Columbian contacts.

At a later date, I was invited to organize a session at the University of Calgary's annual Chac Mool Conference, and assembled several speakers on both sides of the controversy. Parts of the proceedings subsequently were published (Duke et al. 1978).

Then, on the basis of my *Man across the Sea* article, archaeologist Jesse D. Jennings asked me to contribute the final chapter to a compendium text he was editing, *Ancient Native Americans*. In this chapter (Jett 1978, later revised and reprinted), I attempted to synopsise and, to some extent, evaluate all the principal theories that had been forwarded concerning possible Old World cultural influences on pre-Columbian America. This daunting task gained me a very wide familiarity with the published evidence and argument on the subject[**end p. 174**] (had I realized how vast the literature was, despite the "untouchability" of the subject (see Sorenson and Raish 1996), I might not have had the presumption to commence the project).

Since that time, I have been involved in other relevant conferences. I was a co-organizer of Jon Polansky's 1988 World Cultures of Native America conference at the University of California, San Francisco; and for the 1992 annual meeting of the Association of American Geographers, I organized two special sessions in honor of George Carter stressing biological and linguistic evidence for inter-hemispheric contacts. In the same, Columbian quincentennial year, I also contributed a paper on Old and New World dyestuffs to the New England Antiquities Association's (NEARA) America Before Columbus conference (Jett 1993a), which was later revised to become a book chapter (Jett 1998a), followed by a second, John Sorenson *festschrift*, paper comparing resist-dyeing methods in Asia and Latin America (Jett 1999). The Columbian-encounter observances also resulted in an invitation to present a Forum Assembly address on ancient inter-hemispheric

inter-influences at Brigham Young University (Jett 1993b). With geographer Mark Blumler, I also co-organized a session on diffusion at the 1995 national meeting of the American Association for the Advancement of Science (Blumler and Jett 1995; Jett 1995). In 1996, I joined the boards of the post-Barry-Fell Epigraphic Society and the post-Joe-Mahan Institute for the Study of American Cultures, in efforts to put those organizations on a footing of rigorous scholarship.

In 1996, I organized another AAAS session, called "Cultural Connections on the Ancient Pacific Perimeter" (Jett 1997, 1998b), and Carl Johannessen (1998) was one of the presenters. The Early Sites Research Society asked me to be founding editor of its *Pre-Columbiana: A Journal of Long-Distance Contacts*, so I prepared the AAAS papers for the first issue, which appeared in June 1999. The AAAS session also led to my being asked to write a major book on transoceanic contacts, for Copernicus, a division of Springer-Verlag, and it should appear within a year or so (Jett n.d.a).

However, my personal history in these matters is of only limited general interest. One may ask, then, what is the significance of these kinds of studies in the larger scheme of things?

That the voyages of Christopher Columbus initiated transformation of the two Western Hemisphere continents by Europeans is undeniable. But the question of whether or not there were earlier voyages across the seas that resulted in significant cultural influences between the hemispheres is perhaps the most controversial of any issue among students of the pre-Columbian culture history of the Americas, especially of Middle and South America (Fingerhut 1994). This question is also of supreme significance for our comprehension of both the past per se and historical processes—that is, for empirical and particularist culture history and for theory regarding culture change. As Mesoamericanist archaeologist Gordon Ekholm (1950:344) once put it,

This is perhaps the most important question confronting those working in the field of American archaeology and seeking to make known the true history of the American Indian. It is also a question of outstanding significance to our general understanding of how civilizations came into being, of how simple and primitive cultures develop into more complex ones.

Note

1. This paper was presented at the annual Conference of Latin Americanist Geographers (CLAG), Austin, TX, January 2000, in a special invitational session organized by Daniel W. Gade; it is drawn from the preface of an in-progress book (Jett n.d.a) and an in-progress article (Jett n.d.b).

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