

1491: In Search of Native America

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We know of scarcely any record of destructive exploitation in all the span of human existence until we enter the period of modern history, when transatlantic expansion of European commerce, peoples, and governments takes place. Then begins what may well be the tragic rather than the great age of man. We have glorified this period in terms of a romantic view of colonization and of the frontier. There is a dark obverse to the picture, which we have regarded scarcely at all.

—Carl O. Sauer (1938)

W. GEORGE LOVELL

How Latin America is written about in the U.S. media can make for frustrating, infuriating, and at times disheartening reading, especially for any geographer who believes that past experiences shape present predicaments. Twenty years ago, for instance, when Nicaragua and El Salvador, not Afghanistan and Iraq, consumed U.S. attention, the vast amount of print generated by political turmoil in Central America seemed to roll off the presses in a directly opposite correlation to grounded comprehension of the key issues. When Henry Kissinger (1984) was entrusted with the job of investigating for the U.S. government the causes of

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the upheaval and how to deal with them, my despair and disbelief hit an all-time low. Perhaps Dr. Kissinger's decision to relinquish chairing the inquiry into the events and circumstances surrounding September 11, 2001, will spare the United States the déjà vu of what the Mexican writer and diplomat Carlos Fuentes (1985) aptly called "the historical amnesia" of the Kissinger Report on Central America.

It's certainly not the case that no reliable sources exist, good journalists among them, to help the United States better understand its Latin neighbors. When I was a graduate student, my supervisor, the late John F. Bergmann, bought me a subscription to the *Christian Science Monitor* in order for me to read and think about what James Nelson Goodsell had to say. "Your head's full of left-wing British nonsense," he told me as he forked out for the remedy. The high regard I developed for the *Monitor's* Latin American correspondent in the 1970s was echoed in the 1980s and '90s by my appreciation of Ray Bonner, Stephen Kenzer, Susan Meiselas, Alan Riding, and Jean-Marie Simon, to name but a handful of reporters who filed gripping and illuminating stories and went on to write books of enduring worth about Latin America. These days I'd be lost without the *New Yorker* missives of Alma Guillermoprieto, and so would my students.

Fuentes' diagnosis of "historical amnesia," however, continues to haunt me, which is one of the reasons why I found Charles Mann's "1491" essay in the March 2002 issue of the *Atlantic Monthly* so compelling.* There, in a dozen pages that reached some half-million subscribers, and who knows how many readers, is not only sheer good writing but also investigative journalism at its critical best. Mann provides succinct, accessible, and incisive analysis of what the Americas are thought to have been like on the eve of contact with Europe and how native life changed dramatically in the New World as a consequence of intrusion by the Old. Distilling the essence of a vast body of literature, he portrays aboriginal America, in certain favored areas, as densely settled and ecologically transformed, a manifestly cultural landscape at odds with earlier depictions of it as sparsely occupied and little touched by human action.

Though Mann's interpretive grasp extends north of the Rio Grande, a forum organized in Tucson in January 2003 as part of the Conference of Latin American Geographers focused its attention south of the border, down Mexico way and far beyond. To help stimulate discussion of ongoing, controversial matters, I invited three scholars whose

* Mann has recently expanded his *Atlantic Monthly* essay into a book; see Mann 2005.

research findings figure prominently in Mann's essay to critique his reconstruction and to comment on how their investigations have been drawn upon and synthesized. Contextually, not just specifically, Henry F. Dobyns concentrates on issues pertaining to Native American population history, William M. Denevan on what he provocatively terms the "pristine myth," and William I. Woods on the tricky business of time and chronology in contact and postcontact scenarios, as well as human-induced environmental change in Amazonia.

The remarks that follow have been amended and edited in order for presentations made orally to have coherence and integrity on the printed page. While an attempt has been made to retain the timbre of original discourse, references have been furnished to give readers an inkling of published sources extant on the subject. After, in turn, Dobyns, Denevan, and Woods have their say, Mann responds. As in any worthwhile debate, questions are raised as much as answered. Advances have been made, but much remains to be done.

HENRY F. DOBYNS

Charles C. Mann's "1491" is a skillfully written, stellar example of fine science reporting for the general reader. It is the kind and quality of science reporting that I always aspired to when I wrote for newspapers, before the awkward prose and jargon of social science graduate study entrapped me. "1491" connects our intellectual images of Native American numbers and cultural actions to our premises governing environmental conservation policies. By making these connections, "1491" presents a fresh scientific synthesis.

At the same time, "1491" is a mini-review article on the course of research and controversy regarding the magnitude of the Amerindian population of the Americas on the eve of European contact, citing both "high" and "low" counters (Henige 1998). Mann's imagery—"the fight about pre-Columbian populations has already consumed forests' worth of trees"—is marvelous. And most fitting.

What do I think about how Mann treats me and my work in his Atlantic article? I must admit that I think Mann treats my work generously. He writes, in reference to my article "Estimating Aboriginal American Population" (Dobyns 1966), that "his argument was thunderous." I have not heard the thunder myself, unless that is the proper label for the pejorative criticisms of David Henige (1998). The loudest

noise I have heard personally is from colleagues who tell me, "You forced us to think differently." I am content with that, because it signals what Thomas Kuhn (1962) identifies as a paradigmatic shift.

Mann quotes historian James Wilson (1998) as stating that my colleagues "are still struggling to get out of the crater that paper left in anthropology." Perhaps; certainly the imagery is flattering. In my book *Their Number Become Thinned* (Dobyns 1983), and also in a related article (Dobyns 1991), I did at least try to outline a general theory of the socio-cultural consequences of human population implosion—one almost diametrically opposite to the body of social science theory about "development" derived from studies of expanding populations, to which I also have contributed (Dobyns 1951a, b; Dobyns, Doughty, and Lasswell 1971). Depopulation theory is grim theory, and deals with unsettling events and disturbing socio-cultural trends. It is not pleasant to think about. With some notable exceptions—the work, for example, of Woodrow Borah (1951, 1962) or David Fischer (1996)—most people choose *not* to think about it, even though policymakers in nation-states whose populations are declining (Spain, according to Betty Smith 2003) and aging (Japan, according to James Webb 2003) sorely need accurate theory to govern well. On the other hand, the problems almost everyone wants social scientists to solve today are consequences of runaway human reproduction. I perceive extreme reluctance to face the reality of population implosion in Africa resulting from epidemic HIV disease. Medical specialists in the United States only relatively recently began warning us that illegal migrants from Latin America all too often carry tuberculosis bacilli resistant to existing antibiotics. Pathogens simply do not respect social and cultural boundaries; they are biological organisms, and behave as such.

On that point, Mann writes that the argument I presented in 1966 "was simple but horrific." Infectious Old World diseases "could have swept from the coastlines initially visited by Europeans to inland areas controlled by Indians who had never seen a white person." Mann then lists as examples Peru (see Dobyns 1963 and Dobyns and Doughty 1976) and the Pacific Northwest (see Boyd 1990, 1992). Mann might also have mentioned that smallpox decimated inland Aborigines in the wake of the famous first voyage to Australia and coastal landing in 1789. Yes, I did advance this biological concept.

Mann states further that, in 1966, my "insistence" on the historic role of disease in Amerindian affairs "was a shock to [my] colleagues." Today, on the other hand, "the impact of European pathogens on the New

World is almost undisputed,” an outcome of the investigations of researchers such as Noble David Cook and W. George Lovell (2001), Alfred W. Crosby Jr. (1976), Robert H. Jackson (1985, 1992), Daniel T. Reff (1991), and Anna C. Roosevelt (1999). With this advance I am well content. Reconstructing the process of biological decimation of Amerindians shifted the paradigm. The precise quantification of that decimation, however, remains a recalcitrant problem. Mann asserts, most correctly, that “the fight over Indian numbers continues with undiminished fervor.”

In terms of Amerindian numbers, Mann presents my contact estimate of between 90 and 112 million (Dobyns 1966). He writes most dramatically about huge earthworks in the Amazon River basin, particularly in the Beni of Bolivia. The geographer William M. Denevan (1966) published a pioneering report of large-scale human-made earthworks in the Beni years ago. Denevan’s monograph, along with my personal observations of central Andean terraces and massive Inca stonework, influenced me to stand by my contact estimate of the magnitude of the South American population.

If the round forest islands in the Beni were indeed made a millennium or more ago, their construction implies a large, dense population with a sociopolitical organization capable of mobilizing and supervising the labor of a great many workers. Raising the earthworks—particularly straight berms miles long—required, I infer, impeccably precise timing. Though the annual floods drain slowly, because of the slight slope of the basin, waters several feet deep still build up tremendous hydrostatic pressure. That pressure would bear on any impediment to water flow. If the water mass found even a fractional nick in the berm under construction, its hydrostatic pressure would force flow across that nick, which would increase flow velocity by its constriction—and there would go the berm! In brief, the leaders of the society that constructed the berms must have been (or commanded) superb hydraulic engineers as well, probably as dictatorial administrators. As for the contact population of the Beni, the maintainers and beneficiaries of the enormous fishing weirs (Erickson 2000) seem to me unlikely to have been a few hunter-gatherer-fishers. Mann is quite right to focus attention on the Beni complex as another piece of *physical* evidence of relatively dense pre-Columbian Amerindian population and massive landscape creation.

For me, a key passage in “1491” is when Mann paraphrases Professor Denevan: “No definitive data exist,” he states, “but the majority of the extant evidentiary scraps support their [the high counters’] side.” A

good example of this is Elizabeth Fenn's *Pox Americana* (2001), which reconstructs the 1775–82 smallpox pandemic in North America as deadlier and farther spread than I outlined it in 1966. In another example, I recently attempted to reconstruct the historic decline of Pueblo settlements in Arizona and New Mexico, only thirty-two of which survived into the nineteenth century. I arrived at a total of 320 pueblos occupied immediately prior to the A.D. 1520 smallpox pandemic, which originated during the Cortesian conquest of the Aztec capital city (Dobyns 2002). That is a 90 percent settlement attrition rate through three centuries. I did not attempt in that article to translate settlement numbers into individual Pueblo numbers, because too few Pueblo ruins datable to circa A.D. 1500 on ceramic grounds have been excavated to allow for calculating occupants and population. The settlement numbers themselves make clear that the human landscape was at least ten times more occupied in 1520 than in 1820.

I would like to clarify one passage in “1491,” presumably the result of the pitfalls inherent in conducting an interview over the telephone. My clarification concerns Mann's statement that, while I was working for “a few months in northern Mexico,” the connection between Spanish intrusion and Indian demise hit me “like a club right between the eyes.” I did not spend even a few months in northern Mexico poking through crumbling leather-bound ledgers in which Jesuits recorded local births and deaths. I drove with Paul H. Ezell (1961), then a fellow graduate student, and Alden Jones, then chief clerk of the Bureau of Indian Affairs agency at Sells, Arizona, over narrow dirt roads to Altar, Sonora. There we enlisted the cooperation of the parish priest. Records from several colonial missions in the river valley had at some time been concentrated at the Altar parish office. I carried with me a crude copier that I could use during the six hours or so when the municipal generator functioned each day. The disparity between the number of recorded burials and the number of recorded baptisms was immediately evident to Ezell and me. The more numerous burial entries occupied more physical space on the register pages than did the few baptismal entries, even though the latter were typically longer. The metaphorical club that hit me between the eyes did so over the course of a single weekend.

I close by expressing my gratitude to Mann for having made me “famous”—at least among attorneys who represent parties adverse to the Gila River Indian Community, for which I provide assistance, in state and federal water rights litigation in Arizona. During a federal hearing

in Tucson in April 2002, two such individuals came up to me during a court recess to congratulate me on being “famous.” Me? Famous? What are these guys up to? I wondered. When I inquired why they thought so, they told me that they had read about me in the *Atlantic Monthly*. I then went to the library and found a copy of the March issue, and read Mann’s article with surprise, gratification, and admiration. The attorneys, for once, were right. Fame at last!

WILLIAM M. DENEVAN

Charles C. Mann’s “1491” is remarkable in that the related topics of Indian numbers, sustainable agriculture, and environmental impact in the Americas just before the arrival of Columbus receive extended attention (the cover and twelve double-column pages) in a mainstream magazine with an enormous circulation (Mann 2002a; see also Mann 2000, 2002b). Mann’s article triggered nearly one hundred comments on the *Atlantic Monthly* website, and the author himself received more than three hundred items of correspondence in response to its publication. “1491” is written for the general public, but has a solid academic grounding. It makes reference to the research findings of scores of scholars, including anthropologists, historians, biologists, and geographers, many of them interviewed by Mann in the course of his investigations.

Mann opens “1491” by reporting on his own observations of pre-historic earthworks in the Bolivian Amazon, which recalls my own bush-plane flights over the region in 1961–62 (Denevan 1966). Particularly impressive are hundreds of thousands of raised fields in the swamp-lands. Similar fields have also been found in the savannas of northern Colombia, coastal Ecuador, the Guianas, and the Orinoco Llanos, as well as in the Andes and Mexico. Other surviving agricultural earthworks, especially terraces and canals, are widespread in the Americas, evidence of past food production for large numbers of people (Denevan 2001; Doolittle 2000; Whitmore and Turner 2001).

Mann gives considerable attention to the debate over Indian numbers in the Americas at the time of Columbus. Given inadequate data, this is a topic that will never be resolved, but a variety of forms of physical, archaeological, and documentary evidence indicates much higher populations (at least fifty million) than generally thought prior to 1950,

when totals of only eight to fifteen million were accepted by the likes of Alfred Kroeber, Angel Rosenblat, and Julian Steward (Denevan 1992b, xxix, 3). Even someone as contemptuous of “high counters” as David Henige (1998, 210) considers a contact estimate of forty million “not unreasonable.” On the other hand, Henry F. Dobyns (1966, 415) argues that the total exceeded ninety million. As Mann (2002a, 44) himself points out, “[T]he dispute shows no sign of abating . . . partly because the subject is inherently fascinating . . . [but also] due to the growing realization of the high political and ecological stakes.”

One of the main themes of Mann’s article is the “pristine myth,” a term I coined a decade or so ago (Denevan 1992a). The case for environmental impacts by Native Americans, however, is much older and may be found in work published by Karl W. Butzer (1990), William Cronon (1983), Gordon M. Day (1953), Stephen J. Pyne (1982), Carl O. Sauer (1956, 1963), and Omer C. Stewart (1951). The notion of a “pristine myth” has also been advanced by Arturo Gómez-Pompa and Andrea Kaus (1992) and has been elaborated upon by Shepard Krech (1999) and others since 1992.

The central argument is that, by A.D. 1492, most of the New World had already been transformed ecologically by Indian activity for thousands of years. This transformation varied in form and intensity, and of course, was absent where people were absent. Where people were present, they could not survive without using natural resources, and in the process they changed those resources—vegetation, wildlife, soils, hydrology, and landforms.

Others have countered by arguing that this thesis is an exaggeration (Vale 2002). Some contend that stating that most so-called wilderness is already humanized gives license to environmental destruction, a point made by Mann in his final paragraph (2002a, 53). This argument, I believe, is a weak one. Environmental protection does not need a “wilderness” foundation for it to be justified.

About one-third of “1491” concerns Latin America, and most of that is on Amazonia, one of the regions where pre-European human impacts previously have been considered to have been minimal. Mann provides abundant evidence to the contrary. In the lead-in to “1491,” Mann (2002a, 41) states that we are led “to a remarkable conjecture: the Amazon rain forest may be largely a human artifact.” How could this possibly be so, given the immensity and remoteness of much of the Amazon forest? Mann furnishes indigenous examples: the earthworks

of the Bolivian Beni, of Marajó Island, and of the Upper Xingú; forest modifications by clearing, burning, and selective extraction; semi-domesticated and managed wild plants; and anthropogenic soils—dark earths or *terra preta*. In addition, there were fluvial alterations—the digging of canals and shortcuts.

Are these isolated instances? The balance of research suggests otherwise. Findings from diverse disciplines include those of anthropologists William Balée (1987, 1994), Darrell A. Posey (1985), Clark L. Erickson (1995), Gustavo Politis (2001), Anna C. Roosevelt (1999), and Peter W. Stahl (1996); biologists Charles R. Clement (1999) and Charles M. Peters (2000); historian David Cleary (2001); geographers Geoffrey A. J. Scott (1978), Nigel J. H. Smith (1995), and William I. Woods and Joseph M. McCann (1999); and various soil scientists and paleoecologists.

A conclusion increasingly reached by prehistorians of Amazonia is that the indigenous people who lived there were not simple shifting cultivators with small villages (Meggers 1996). In many places, the evidence indicates otherwise—villages were large, cultivation intensive, social organization relatively complex (chiefdoms), and environmental impacts significant (Stahl 2002); a decidedly revisionist perspective.

But wasn't Amazonia sparsely populated? Well . . . yes and no. The bordering stretches of the main rivers, extending inland from the bluffs, were often densely settled (Denevan 1996). In the upland interior, population was generally sparse but with dense pockets. Settlement sites measuring hundreds of hectares have been discovered (Denevan 2003). High population densities, however, are not required for discernable environmental modification.

Hunter-gatherers, widely dispersed, modify the numbers and distributions of game animals and useful plants. A single family will hunt and forage over a radius of about ten kilometers' walking distance from their camp, which equals an area of 315 square kilometers, rapidly depleting available resources and then moving on. One family!

There are a few inflations in Mann's article. He says that in 1492 the hemisphere was "thoroughly dominated by humankind" (2002a, 41). Actually, there was a continuum of degrees of human modification, from mostly invisible or ephemeral to a fully cultural landscape. On the *Atlantic Monthly* cover, visible on magazine stands throughout the United States in March 2002, is the comment that "America before Columbus was . . . a more livable place than Europe." Such an assertion

depends on one's criteria: health, diet, longevity, wealth, peace, or whatever. A study with specific comparisons would be interesting. In his final sentence, Mann (2002a, 53) suggests that the continent contained the "world's largest garden"—a dramatic, big-bang ending. But if the New World was not a vast garden, much of its vegetation had been modified to make it more productive for human inhabitants. And there were indeed great expanses of actual cultivation, often intensive and permanent (Denevan 2001; Doolittle 2000; Whitmore and Turner 2001).

In the past, many of us have had unfortunate experiences with the media—print, radio, and television—in which our names have been misspelled, our findings and words have been twisted around, and we have been misidentified as to our professional discipline. Science writer Charles Mann has proven to be an exception. He made an effort to get it right, to contact proponents of alternative viewpoints—Betty Meggers (1996) and David Henige (1998) among them—to check back with his sources, and to treat them fairly. As a community of scholars engaged in an unending search for knowledge, we have been well served by him.

WILLIAM I. WOODS

I choose to respond to "1491," Charles C. Mann's articulate reconstruction of native America, by discussing aspects of the joint imperatives of time and space. To answer the question of New World population density and quality of life vis-à-vis Europe before contact, one needs to ask "where?" and, most decidedly, "when?" Answers are then markedly variable. I will address two key points: (1) the fact that different regions in the New World experienced different trajectories, in which the year A.D. 1491 may or may not have been the cultural "high point"—in other words, that what really made A.D. 1491 at all significant was 1492; and (2) the relationship of *terra preta*, or dark-earth soils, to scenarios of settlement, subsistence, and sustainability in pre-European Amazonia.

Cultures are dynamic systems in their application. Periods of stability, instability, and even catastrophe, we must remember, are the norm. Retrospective narration of these complex stories is difficult but essential to understanding the conditions that may have prevailed at one point in time, at one particular place. When I saw the first film of the *Lord of the Rings* trilogy, I was reminded that, in developing his tales of Mid-

dle Earth, J.R.R. Tolkien had been strongly influenced by Norse sagas and Anglo-Saxon myths. Was similar lore present in the New World in A.D. 1491? Of course it was! Everywhere there was a rich legacy of the past. Like that of their northern European counterparts, the folklore of Native Americans at contact had its basis in prior realities of cultural history. In order to comprehend the hemispheric meaning of A.D. 1491, therefore, we need to look at specific regional and local contexts.

In highland and lowland Middle America, as well as in Andean, west desert coastal, and Amazonian South America, complex societal units existed at the time of contact. In the Mesoamerican scene, Tulum, Cempoala, and Tenochtitlán inevitably come to mind, but what of Tikal, La Venta, and Teotihuacán? Whenever we look, we find nuanced local histories that are anything but linear in their development.

Though our focus in this forum is Latin America, allow me to make a point about chronology by turning to an eastern portion of North America. At Cahokia, in the middle Mississippi River valley, a sophisticated developmental sequence is locally conspicuous. Passing over at least 10,000 years of prehistory to A.D. 600, we find the introduction of the bow and arrow and, 200 years later, the widespread acceptance of maize as a dietary staple. Both developments provided greatly increased productive capacity. By AD 1000 we have evidence of a planned city with clearly defined administrative/ceremonial zones, elite compounds, discrete residential neighborhoods, and even suburbs (Dalan et al. 2003). A high degree of social organization and great expenditures of labor are indicated by a huge central plaza and numerous immense earthen mounds, including Monks Mound, the largest prehistoric structure north of central Mexico (Woods 2000). With a peak population of perhaps fifteen thousand or more, Cahokia covered an area in excess of thirteen square kilometers and existed, in its heyday, for approximately three centuries. By at least one hundred years before A.D. 1491, however, after a period of disruptive, horrible trauma, Cahokia and its environs had become largely abandoned in terms of permanent occupation.

A.D. 1491, then, was certainly not the high point for Cahokia, nor for other settlement systems in the New World. But what about Amazonia? Unlike Mesoamerica and eastern North America, the prehistory of Amazonia is only now becoming known. Localized diachronic situations here have rarely been worked out in detail. Indeed, there is fundamental disagreement about many key issues, and academic debate is intense.

When discussing quality of life and population estimates, subsistence considerations become paramount; people consume food. It is increasingly clear that Amazonian responses to problems of food production involved utilizing a variety of cultigens and semi-domesticates; the practice of agroforestry; focused manipulation of local ecologies, including insects; and large-scale modification of soil conditions. Patches of exceptionally fertile, anthropogenic dark soils occur throughout lowland portions of the basin. These have been used to support numerous theories concerning pre-European settlement patterns, population densities, and cultural development. Heightened biotic activity and nutrient-retention capacity brought about by deposition of ash and organic material appear to be principally responsible for the remarkable persistence of these soils long after their cultural manipulation has ceased (Woods and McCann 1999).

Why have these dark earth soils been overlooked? The culprit is clearly the tyranny of scale. No maps of the Amazon basin depict these soils; individual expanses rarely exceed more than a few square kilometers and usually encompass much less area. When taken together, however, their cumulative expanse is staggering. A century ago, Friedrich Katzer (1903) projected more than fifty thousand hectares of *Schwartzerde* in the small upland zone between the Tapajós and Curua Uná rivers—roughly 7 percent of the total area of this lower Amazon heartland, the cradle of Santarém culture. This is not a unique example; dark earths extend well up the Tapajós and westward up the Amazon. Wim Sombroek (personal communication), based on a recent reconnaissance in the estuary of the Xingú, estimates the presence of ten thousand hectares of dark earths there, or about 3 percent of the total area. Dirse Kern (personal communication) has concluded that similar densities prevail near the Atlantic coast, along the lower Amazon, and in the Trombetas. So, too, have Michael Heckenberger, James Petersen, and Eduardo Neves in the upper Xingú and in the lower Río Negro (Heckenberger, Petersen, and Neves 1999; Petersen, Neves, and Heckenberger 2001). Reports from Marajó, Acre, Bolivia, Colombia, and Suriname all point to the widespread distribution and immense cumulative area of these anthropogenically enriched soils. If only 1 percent of the lowland portion of the Amazon basin consists of dark earths, that percentage translates into approximately sixty thousand square kilometers or six million hectares—an *enormous* amount of fertile soil.

Did this huge productive capacity translate into the reality of production? Yes—but to an unknown degree. Even with such equivocation,

however, the implications for population estimates are profound. On the eve of contact, the New World thus had its rich Urzeit narratives of Middle Earth, which encompassed better or worse conditions than in A.D. 1491 or its calendrical contact equivalent. Modes of production and population levels in many systems at many locations were both more sophisticated and more numerous than we have generally thought. Efforts from an increasing number of dedicated researchers are beginning to demonstrate the specifics of this hitherto incomplete picture. The engaging description and interpretation of these findings by Charles Mann, and his dissemination of them in a respected, mass-circulation monthly, only serves to accelerate the process of understanding.

CHARLES C. MANN

I am honored by the attention given to my article “1491” by the Conference of Latin Americanist Geographers. Indeed, I am enormously flattered by it.

One of the great pleasures of journalism is the opportunity it provides to meet interesting people. The contributors to this forum are no exception. I thank them for providing me with hours of intellectual stimulation. I greatly enjoyed speaking with them and reading their works.

How did I, a non-geographer, come to write “1491”? Why did I choose to take the tack that I did? What is my background, and how did it prepare me for this particular job? I have been a correspondent for *Science* and the *Atlantic Monthly* for the last eighteen years or so. I’ve also written or co-written four or six books, depending on whether one counts writing the texts for books of photographs. My work has concerned many subjects, but almost all of it in one way or another has been concerned with scientific research. I feel very lucky: My work is like all the fun parts of grad school, plus I get paid.

The seeds of “1491” date back, at least in part, to 1966, when our family, including twelve-year-old me, moved from the suburbs of Detroit to a small town outside Seattle. Soon afterwards we visited museums in Seattle and in Victoria, B.C., which have wonderful collections of Northwest Coast Indian art. I was not an artistically knowledgeable child, but the calm ovoids and surreal multiple perspectives of the Bella Coola and Kwakiutl masters immediately engaged me. I thought they were fully as sophisticated and elegant as the works in those same museums from Europe and Asia. I hold this opinion still.

The genesis of the article may also be traced to the day in 1982 when, on a NASA plane that was monitoring the atmosphere, I landed in Mérida, on the Yucatán Peninsula in Mexico. Between 1979 and 1981 I had lived in Rome, Italy, where naturally I visited many of the famous ruins. The atmospheric scientists had a day off in Mérida, and we all took a decrepit Volkswagen bus to Chichén Itzá. I knew nothing about Mesoamerican culture—somehow the true inventors of zero had been skipped in my math classes. But in purely aesthetic terms I thought these ruins were more interesting than those I had seen in Italy. On my own—sometimes for vacation, sometimes on assignment—I went back to the Yucatán five or six times. For the German magazine *Geo*, photographer Peter Menzel and I made the fourteen-hour drive down a one-lane dirt road to the then-unexcavated city of Calakmul. We stayed in a *chiclero's* shack in the midst of broken stelae. I still remember my amazement when our Maya guide, Juan de la Cruz Briceño, emerged from the forest with a wild turkey that he had caught by sneaking up to it and lopping its head off with his machete.

The point I am trying to make here is that the article stems from a long-standing though rather formless personal interest. This interest only snapped into anything resembling focus in September 1992. By chance I saw the Smith College Library displaying the special quincenary edition of the *Annals of the Association of American Geographers*, which Karl W. Butzer (1992) had edited and which contained Bill Denevan's powerful manifesto, "The Pristine Myth" (Denevan 1992a). A year or two later, at the annual meeting of the American Association for the Advancement of Science, I attended a forum called something like "The Genesis of the Amazonian Forest," which featured William Balée of Tulane University and Anna C. Roosevelt, then of the Field Museum in Chicago. In his fascinating talk about anthropogenic forests, Balée mentioned the explosive impact of someone named Dobyns, whose work sounded interesting enough to send me back to the library.

"Gee," I thought, "someone ought to put this stuff together. It would make a really interesting article." I kept waiting for that article to appear. The wait grew more frustrating when my son entered school and was taught the same things I had been taught—ideas that I knew had long been sharply questioned. Since nobody else appeared to be doing it, I finally decided to take a stab at writing the article myself. "1491" is the result of my efforts, part of a larger work in progress (Mann 2005).

In “1491” I tried to be fair, but I gave more attention to what might be called the revisionist position. To judge by the three hundred letters I received in response to “1491,” arguing in favor of large, sophisticated pre-Columbian populations pleased those you might expect and displeased those you might expect. Let me try to justify the point of view I adopted on three grounds: pseudoscientific, pseudosociological, and pseudopsychological.

First, it seemed to me that the revisionist picture was stronger than the sum of its constituent parts. That is, one could question the logic of working backwards from estimates of disease mortality to arrive at large initial populations. And one could attack the evidence from early accounts and dendrochronology and soil science that the Great Plains were in large part created and maintained by fire. And one could challenge the data from plant genetics indicating that humans bred and propagated the peach palm and many other Amazonian trees. But when these and other new ideas are viewed together, they reinforce each other. Like the standard model of elementary-particle physics—the subject of my first book—the revisionist view gains explanatory power from a distance. The remark of the late Columbia physicist Gary Feinberg is relevant. “None of the arrows is conclusive in and of itself,” he said of the standard model, “but when you stand back it seems so obvious that most of them point in the same direction that you begin to believe.”

I hesitate to mention the second reason because it may seem mean—so mean that I referred to it in my article only indirectly. Like many people my age, I read Thomas Kuhn (1962) in college. Among other things, he makes the social-constructivist claim that the outcome of scientific disputes is always determined by the age of the protagonists. Rightly or not, the point of view championed by younger researchers invariably wins. (By “young,” I mean young in academic terms—under the age, say, of fifty.) Working for *Science*, I have seen this demographic process many times. And in the case of “1491,” the younger the archaeologist, anthropologist, or geographer, the more likely he or she was to endorse the revisionist position.

The third reason is entirely personal. My parents passionately supported the U.S. civil rights movement. They told me as a child that human beings are profoundly similar in their personal qualities despite differences in skin color and custom. People of all sorts, my folks said, are assertive and wily; they like to figure things out, rearrange their environment, and sometimes just wreck stuff for the sheer, giddy joy of it.

My parents themselves exemplified this: My father was always surrounded by pieces of old cars that he was reassembling, my mother was constantly training dogs for hunters. On an intuitive level, the revisionist picture was closer to what I imagined a hemisphere of active, inventive people would be like.

In a marvelous book, *Indians and English: Facing Off in Early America*, historian Karen Ordahl Kupperman (2000) emphasizes how seventeenth-century natives and colonists were mutually dazzled by each other's technology. As has often been proclaimed, European steel tools amazed the Indians. What the history books have not said until recently is that the British were equally amazed by Indian agriculture, Indian construction methodology, and Indian nautical engineering.

So superior were Indian canoes to European boats, Kupperman tells us, that British colonists often simply abandoned their shallops and begged Indians for canoes. In 1605, a canoe of three Indians was so much faster and more maneuverable than the heavy boat carrying explorer James Rosier and eight oarsmen that the natives paddled around them in circles, laughing at the Englishmen as they struggled through the swells. Rosier was delighted, chagrined, and amazed by the scene. In "1491," I tried to convey to contemporary readers the flavor of his response to that now-distant world. ✦

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