

TERRA INCOGNITA NO MORE ... AND NEVER WAS

Roderick James McIntosh

Too long (and far too soon) to be an epitaph: “I am a steel boy. When cut I bleed Yale blue. And I am a proud member of a not insignificant corpus of scholars ‘qui aiment trop le Mali’ (who love Mali too much).”

Admittedly, this simply leaves out too many critical moments and underplays the career serendipities. However, this is not altogether bad as a gloss on my intellectual trajectory. Formative years in a hardscrabble steel town provided the insights to parse the unusual patterns of authority at the alternate polities and cities of prehistoric West Africa. The unusual breed of archaeologists who taught me at Yale set me on a path through the *terra incognita* of ancient Africa to find—to echo the words spoken at a highly evocative moment elsewhere in the archaeology of Africa, by Howard Carter in the Valley of Kings—“Yes, wonderful things” (not just in Mali, but in Senegal, Ghana and South Africa, too).

I was born in Bethlehem, Pennsylvania. It is only somewhat facetious also to say that I was conceived an archaeologist; more on that later. Bethlehem was the North American center of one of scores, perhaps hundreds, of utopian experiments founded during the first three centuries of the settling of the eastern colonies. The Quaker William Penn granted many charters to utopians in the colony given by the English king to his admiral father, making Pennsylvania quite uniquely heterogeneous, linguistically diverse, fractious, and skeptical of traditional authority. The Moravian Brethren, or more formally, the “Unitas Fratrum” or “Unity of the Brethren,” who marched some seventy miles beyond settled lands to found Bethlehem on Christmas Day of 1741, were deep into the then frontier.

Roderick James McIntosh, PhD, University of Cambridge, is the Clayton Stephenson Emeritus Professor of Anthropology, formerly curator-in-charge of anthropology at Yale’s Peabody Museum, and is Honorary Distinguished Professor at the University of Pretoria. His major interests are in African and Old World comparative prehistory, palaeoclimate, and emerging urbanism. He was involved in the birthing of the first bilateral accord banning the import of antiquities into a “market” nation (US) from a “source” nation (Mali). His on-going field research concerns the early urban landscapes of the great Niger and Senegal floodplains. Professor McIntosh continues to head Yale’s archaeomagnetism dating laboratory (high-resolution dating from archaeological sites). His bio-archaeology lab discovered the first ever way to identify malaria in ancient bone. Professor McIntosh has been a Guggenheim Fellow, twice a Fulbright Senior Fellow (Senegal and Mali), and has held a fellowship at the Center for Advanced Study in the Behavioral Sciences at Stanford.

Most of us growing up in Bethlehem, even non-Moravians like myself, could recite a few lines of the poem composed by Count von Zinzendorf (patron of the Moravians, domiciled at their world headquarters of Herrnhut, near Leipzig, in eastern Germany) who himself traveled to the wilds of the new continent for the settlement's founding:

<i>Nicht Jerusalem</i>	Not from Jerusalem
<i>Sondern Bethlehem,</i>	But from Bethlehem
<i>Aus dir kommet, was mir frommet</i>	From you comes what gives me joy

172

The Moravians (as we'll call them here) were an unusual brand of Pietist utopians. Like most other eighteenth-century Pietists, they tried to replicate Christ's life "experientially" in their own lives. Unlike most other Pietists, the Moravians followed the teachings of the sect's guiding figure, Jon Hus, in the belief that God's plan for the world was best revealed in beauty—beauty as the clearest window into the soul of God. For decades before 1741, missionizing "scouts" were sent throughout the world from Herrnhut, searching for the most beautiful locales to settle their headquarters for each continent (such as Bethlehem), near and far support communities (Nazareth, Emmaus, etc. nearby in the valley of the Lehigh River; Salem in North Carolina), and even smaller hamlets for native peoples converted to Protestantism by the resident missionaries (Gnaddenhuetten of the famous 1755 massacre of Moravians from Bethlehem and their Lenni Lenape followers; "Cape Colored" Genadendal, oldest of sixty-four Moravian mission communities in South Africa). The obsession with beauty of the landscape was peculiar to the Moravians' flavor of Pietism; however, it was their take on the Pietist duty to replicate Christ's life that was to open a brilliant chapter in the New World's experiments with alternatives to Europe's sclerotic habits of labor relations – and by extension, provided the foundation to my own career in archaeology.



1755 map of Moravian Bethlehem, Pennsylvania. The Colonial Industrial Quarter occupies the western segment of the town.

We know all this from diaries. Or rather from the Diary, supplemented by hundreds of individual diaries. Back to Jon Hus: Hus, like John Wycliffe and most pre-Martin Luther reformers, insisted that the Bible should be preached in the vernacular. This, indeed, was one of the reasons why the Council of Constance decreed in 1415 that he should be burned alive, (apocryphally?) on a pile of the book containing his Bible translation, entitled *The Book*. After this, his long-persecuted congregation, the Hussites, took this heresy a step further by insisting that all congregants be literate: men, women, and converts. (An aside, my own northern Scottish family were Free Church of Scotland, who also insisted that all should be able to read the Bible.) Among the eighteenth-century Hussites, now called Moravians, not only was literacy universal but also everyone was expected to keep a daily diary. The individual named diarist for Bethlehem, with a term usually of a year, wrote generally of the daily activities and leadership decisions of the community; the diarist also composed the funeral eulogy for each departed soul by harvesting entries from that individual's diary. These diaries are scrupulously kept at the Moravian Archives in Bethlehem, representing a precious and quite unique source of information about the structure – and rationale – of labor in this industrially precocious community. In these we have a generous and penetrating record of who worked where and with whom and also of the independent and highly diffuse decision-making relations between the many trades and industries, among themselves and with the central Church.

Most official histories of early Bethlehem concern the religious leadership “Elders’ Council” (lauding the ability of women to hold the highest ranks and perhaps ignoring the lack of membership of Native American and freed Black congregants). Secondary sources have yet to cover the parallel (and in the early nineteenth century, increasingly powerful) *Berufen* (trades and Industries) or Warden’s Council. A bit of background: early Moravian society and economy were communalistic: personal property was held privately, but all land and (in principle) all output of community trades and industries was the property of the Church. Ideally, up to one-third of the community would be away doing the work of the mission, or Pilgrims’ Choir (echoing Christ as missionary). The rest of the community (the Home Choir) labored (echoing Christ the Carpenter) to support the Pilgrims’ Choir, being divided up into “residential” Choirs along sex, age, or other status criteria (Widows Choir, Single Brethren’s Choir, Married Couples Choir, Friedenshuetten Choir – for unbaptized Native American converts, etc.). Importantly for the breakdown of the original communalist General Economy by the 1820s, many practices within the choirs were not ideal in modern terms (infants were removed from their mothers and tended by the Widows Choir in Nazareth; marriage partners for the Single Brethren were decided by lot (wasn’t that a source of great hilarity among us boys and girls growing up in Bethlehem!). But importantly, one could choose the trade or industry to be trained in. And within the fifty-one trades practiced in Bethlehem by 1758, leadership was by merit and not by position within the Church. It is worth emphasizing that, unique to the Moravians, we know intimately the industrial practices, organization of labor, and decision-making within each trade

or industry because we have the archive of each individual's diary. We know not just what a diarist was doing on a certain day, but what she or he thought of the task.

(Not entirely pertinent here, but fascinating, nonetheless, the third facet of Christ's life for the Moravians was "homiletics," interpreted as "making a joyful sound unto the Lord." Everyone played a musical instrument, voice included (my mother delighted in singing in the Bethlehem Bach Choir). Over half of all classical music written in North America during the eighteenth century came from Bethlehem, most printed on Benjamin Franklin's presses in Philadelphia.)

174

The Moravians built to stay. Unusual for the raw frontier and for territory still bloody, having recently been wrested by the Iroquois Confederacy from the Delaware, most of the choir buildings and the Gemeinhaus (early communal dwelling and chapel) were built of stone and brick and still stand today. And this applies also to the several extant buildings housing the many trades and industries of what we now call the Historic Bethlehem Colonial Industrial Quarter (CIQ) in the valley of the Monocacy Creek (source of the indispensable water power). While religious hierarchy (albeit inclusive of women) and choir life prevailed at the residential quarter on the overlooking limestone heights, one can only call the lack of hierarchy of prestige among the Moravian trades and the communal decision making within each industry as something approaching "distributed authority." In the minds of those of us growing up in Bethlehem, those with an historical bent at least, this organization of labor could not have been any further from the guild-based structure of master-journeyman-apprentice of Europe or from the highly hierarchical corporate structure of Gilded Age American capitalism.

And what does all this have to do with African archaeology? Conceived as I was an archaeologist (seriously, I never imagined doing anything else), the Colonial Industrial Quarter is where I learned to dig. And to love suffering while digging. I was extremely fortunate that the 1960s witnessed a grand campaign of excavation and reconstruction of the waterworks, tannery, spring house, smithy, and Luckenbach mill within the CIQ (and exposure of foundations of several other trades and industrial buildings along the Monocacy). The complete "recovery" of what was the largest industrial park in North America in the decades before the Revolution has been frustrated by the rise of the water table because of upstream over-development. Nevertheless, the CIQ is the site of continued civic efforts to preserve and highlight this landscape of labor and innovation of techniques and specializations that hosted two score "cutting edge" (for the time) industries within a decade and a half of the community's founding.

Excavation protocols that I have brought to West Africa and southern Africa are the enduring legacy of the director of those historic Bethlehem excavations of the 1960s. (One of the finest individuals I've ever known and one of the best quantitative archaeologists, Albert Spaulding of UCSB, was fond of saying "If you can't say something nice about someone, just shut up.") That director, Dr. Vincent P. Foley, was a



Rod at age twelve at his first Historic Bethlehem excavation season. With him is the director of research, Dr. Vincent P. Foley.

complicated soul who suffered an acute case of what we would now recognize as PTSD due to being severely crippled by a mortar round in Korea. (This being a steel town, you can imagine that many of my friends' fathers came back from WWII with deep scars and violent tendencies – we all thought this was normal.) Vince was quick with a blow from his left-arm prosthesis at the slightest infraction: lapse of discipline in the pits or just horsing around. However, I did learn the lesson that the best excavations are models of paramilitary organization and discipline (thank you also General Pitt Rivers). The basic protocol of note-taking and structure of standardized forms on all of my digs have Vince Foley's handwriting all over them. It is not an exaggeration to say that three generations of Senegalese archaeologists and two generations of Malians all use the historic Bethlehem-born LRFs (level record forms), feature and radiocarbon forms, all organized by the head excavator's Master Workbook (Maître Cahier). Perhaps these technical innovations do not count strictly as intellectual trajectory – but they surely make it possible to return to the records as much as a decade after backfilling to put together the obligatory site-report monograph.

Decades later, once back at Yale, I ran the field excavation component of the Archaeological Studies Council's obligatory Field Methods course at Bethlehem's CIQ. We would travel from New Haven to Bethlehem four or five weekends in the fall, spending the rest of the semester and the spring analyzing the artifacts and

writing the final site report (“No one receives a final grade, including graduating seniors, until a publishable quality site report is submitted to the executive director of Historic Bethlehem Sites and Museums”). The CIQ excavation protocol was learned by perhaps seventy American, Mongolian, Chinese, South African, etc. students. All those students delighted in filling out those LRFs, feature forms, and learning to double-string the units, all as a tribute to Vince.

Sadly, there is one negative lesson of the Foley excavations at the CIQ: he left Bethlehem somewhat under a cloud, never producing final reports. Decades later I was asked by the management of Historic Bethlehem Sites and Museums (the current keepers trying to revitalize the Moravian historic buildings, museums, and the industrial landscape) to attempt to recover the data from the 1960s. Notes had gone missing (or, unappreciated, had been tossed out), artifacts removed from uniquely numbered bags, etc. so the sweat, strained muscles, and “wooped upside the head” indignities suffered at Foley’s tantrums were as if for naught. A lesson for all archaeologists.

But we did it all – all that today would make OSHA turn apoplectic! First season we walked in front of front-end loaders and graders removing a good meter of WPA-era fill that covered the original Moravian grade within the CIQ. We heaved massive limestone building stone that infilled collapsed structures; we drank far too little water during a nine-hour day; we worked two stories below grade in the Tannery with fume-spewing sump pumps at our feet (where I took ten stitches just below the left eye from an errant shovel). We were young, we worked to exhaustion – and we loved it. And no question about it, the best part of those five summer seasons (having started at the age of twelve) was the comradery. (Comradery despite unpleasant conditions – still the best part of being a dirt archaeologist.)

The kids on the crew were from all the ethnic neighborhoods of Bethlehem. If their fathers or brothers worked in the mills across the Lehigh River, we knew exactly where they fit into the productive organization of “The Steel” (pronounced “the *Stil*”). For Bethlehem at the time was The Steel and we were proud of it. If I was lucky to have had the formative opportunity to cut my teeth as an archaeologist at Historic Bethlehem, I was doubly lucky – very sadly – to have that source of summer employment because ours was the first generation not to have practically assured summer employment on the shop floors of the Bethlehem Steel Corporation. The writing was on the wall by the early 1960s; the days were numbered in the United States for the fully integrated steel production (from iron ore, coke, and limestone in the blast furnaces, to pig iron, to steel to be rolled into various shapes, structures, and commodities). The town mourns still that eighteenth day of November 1995, when The Steel went cold. What was lost that day was something that made the steel town of Bethlehem unique amongst steel towns in America and something that had a profound influence on my intellectual trajectory.

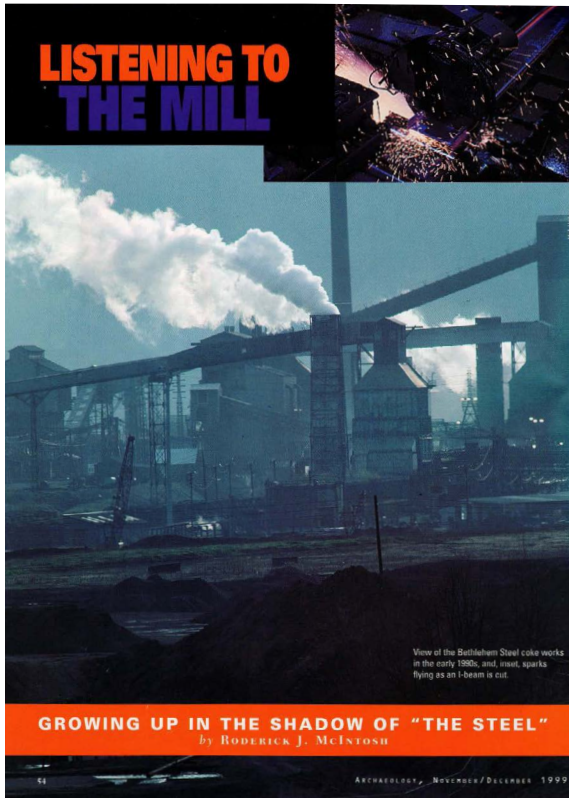
That something was the division of labor along ethnic lines. The Bethlehem Steel Corporation was as corporate, as hierarchical in the upper levels of decision-making



Bethlehem Steel Works 1881, by Joseph Pennell, captures the majesty of, and forebodings about, early American industrialization.

as any other industrial behemoth of the time. But at the level of the shop floor (and in important ways in hiring), The Steel was vastly different (and perennially a crow in the gullet of the likes of Bethlehem's great rival, Andrew Carnegie). My connection to The Steel was that my grandfather and father were steel company engineers (head of the sheet piling division and chief engineer of projects, respectively), so the family was thoroughly marinated in the ethos of steel-making and in the particular pride of the ethnic *mélange* that was South Bethlehem (where ethnic neighborhoods revolved around the signal church – St. Cyril-Methodius Slovak, St. Nicholas Russian, St. Peter and Paul Greek, St. Stanislaw Polish and so on, with separate churches and neighborhoods for Windish, Italian, Mexican, Irish, etc.) – where the parish priests were instrumental in hiring to bring together those members of large families still in the old country. My father used to joke that, no matter what his status in the corporation as a chief engineer, he was not welcome on the shop floor of the blast furnaces because he could not speak Hungarian. When, at the end of my last year of university, I was *finally* offered a summer steel job as assistant boiler-maker, that offer was *only* because I had an Irish girlfriend (whose father liked me!). (I had already committed the summer to farm work in North Carolina instead – another story.)

Years later, in 1999, *Archaeology* magazine asked me to reflect upon the influence of “Listening to the Mill: Growing up in the Shadow of ‘The Steel’” on my career path. I



Cover to Rod's 1999 article in *Archaeology* magazine, "Listening to the Mill: Growing Up in the Shadow of 'The Steel'"

had to put a personal gloss on the demise of the corporation. By that time, the rusting blast furnaces, the crumbling rolling mills at Bethlehem were morphing into historical (industrial) archaeology. I wrote:

What makes this the great American industrial tragedy is that workers and management alike were enormously proud of what made the town of Bethlehem and the flagship plant so distinctive: ethnic heterarchy; Hungarians ran the blast furnaces, Slovaks the specialty mills, there were German machinists, Native American high-altitude fabricators. Bethlehem, town and plant, was a honeycomb of interconnected cells.

The saddest thing about the termination of iron smelting and steel rolling at the Bethlehem plant is that the four-mile industrial behemoth was already organized along horizontal lines of authority that might have been its salvation.

I still, in the abstract, cannot fathom Bethlehem without the pride that each took in his place within a massively complex, productive, and otherworldly whole fabricated out of shops organized by ethnic group: horizontal authority. Yes, all steel towns in America shared a (grandiose) ethos: "Steelworkers believe their skills set them above anyone else in manufacturing. Robots can assemble automobiles. But it takes uncommon talent, a strong body, and a mind that knows no fear to be able to transform

piles of red dirt and scrap into molten metal that is poured, rolled and pounded into the various shapes that support the mainframes of civilization,” to quote the former editor of the local newspaper in his book, *Crisis in Bethlehem*.

This is not the place to dwell on the demise of steelmaking in Bethlehem. Unfortunately, nor is it the place to examine the fascinating question of the mid-nineteenth-century transformation of the town from Pietist, Pacifist (however complex the Moravians were in practice), Communalist, and characterized by distributive authority (at least in their industries) to a town that can be characterized as a “gilded age” sire of the military-industrial complex (recalling the visit in the 1880s by the CEO and engineers of the Bethlehem Iron Company to the Navy Department with the promise that steel from Bethlehem could transform the US Navy from being, at the time, the world’s sixty-fifth blue-water navy—twelfth in the Western Hemisphere), plus hosting the sprawling complex of mills “that built America” (echoing the boast from the late 1940s that “if all the H-beams from Bethlehem disappeared, 80 percent of the skyscrapers of Manhattan would collapse”). Rather, now is the time to turn to the next important locus of my intellectual evolution, led again by my grandfather and father: sacred Yale!

Talk about brainwashing: In the Bethlehem houses of my grandfather (Class of 1914S) and my father (1944), there were always copies of the *Yale Alumni Magazine*. (My son graduated from the Whiffenpoofs, oops, from Yale, in the Class of 2009, and my daughter was accepted into the genetics program of Bio-Anthropology.) I came to Yale in the fall of 1969 by way of the Lawrenceville School, near Princeton. Sadly, the Bethlehem public schools were not up to grade and the exposure to the sciences and math I received during three years at Lawrenceville was rigorous. But the single-sex boarding environment? Oh, my, how happy I was to be a member of the first fully co-ed Yale class. Delighted to have been part of a tight circle of eight men and six women friends at Davenport. Proud to be of the first class that looked like America. That said, it was a difficult time for America, with the recent assassinations of the Rev. Martin Luther King and Bobby Kennedy and, at Yale, the trial of the Black Panther Bobby Seale. But Yale, under the capable helmsmanship of Kingman Brewster, survived. (Time for a quick story: At my forty-fifth reunion, Bobby Seale was brought back for a retrospective roundtable. My son, then employed by the Yale Alumni Association, ran the credit card machine for the sale of Mr. Seale’s book, *Power to the People: The World of the Black Panthers*. He bought a copy to give me and asked Mr. Seale to write a dedication. He wrote, “To Rod, Bobby Seale, Sorry about the tear gas.” He then looked up at my son and said, “Why am I apologizing for the tear gas? I was the one in prison!”)

I had superb, but unorthodox, training in archaeology at Yale. First, I was one of the four majors allowed from the Class of ’73 in the Archaeological Studies program. This was an experimental, multi-disciplinary major that recognized that archaeology strode with one foot in the humanities, one in the social sciences, and one in

the sciences. I had intensive exposure to classical archaeology and Egyptology. And who would have thought that, decades later, I would have had to call up my training in the history of art to write a dozen or so articles in the campaign to convince my Africanist art history colleagues to consider abstaining from writing about (thus legitimizing) terracotta statuary looted from ancient sites? My main intellectual seat was in anthropology, but courses in geology and geophysics were the background to teaching future courses in geomorphology, the human evolution component of African prehistory, and to setting up a geophysics laboratory (archaeomagnetic dating) upon my return to Yale.

As important and enduring as these non-anthropology experiences were, Yale's main influence on my intellectual trajectory came through my exposure to – and eventual friendship with, I dare say – the prehistoric archaeologists Irving (Ben) Rouse (responsible for the incomparable Caribbean collection at the Peabody Museum and for creating Caribbean archaeology wholesale), Michael Coe (the great Mayanist and patron saint, if you will, of the Olmec), and K.C. Chang (revered by Chinese archaeologists as the founding father of modern, scientific Chinese prehistory). This was the time most large anthropological archaeology departments taught the supremacy of the six great “Primary Civilizations” (Mesopotamia, Mesoamerica, Egypt, etc.) where urbanism and stratified society (complex society) arose without influence from the outside. Movement towards complexity in all other societies was disdainfully thought to be due to direct influence or stimulation (diffusion) from these big six. Chang, Coe, and Rouse taught us that this was bunkum. Citing the great parts of the globe as yet untrod by archaeologists, they encouraged us not to write “yet another dissertation on yet another medium-size house at Chan Chan,” but rather to go into the great *terra incognita* of global prehistory. They also took an interested but skeptical position towards the great interpretive movement sweeping archaeology at the time, the so-called New Archaeology. Some elements of this movement of the 1970s – rejecting the humanities roots of archaeology, embracing certain strains of the philosophy of science (and too many statistics and analytics just simply inappropriate to the inherent ambiguities and sampling unknowns of archaeological data) – have not stood the test of time. Others endure, such as making hypotheses explicit, creating and testing globally comparative causal models, and especially, embracing scientific laboratory techniques tailored to the realities of archaeological data. How wonderful that Yale archaeology is today in the forefront (at least in America) of experimenting with new laboratory and scientific applications to the past – a major legacy of the New Archaeology.

One other Yale influence needs to be broached: my introduction to Africa. Entering Yale, I really did not know which part of the world I would end up in; hence I foolishly heeded advice to take the three scholarly languages simultaneously (I survived). I took several eye-opening courses in African history. I'll mention South African History with Leonard Thompson, particularly, for the background I possessed when in 2001

I accepted an invitation to reform the deeply Apartheid-implicated archaeology department at the University of Pretoria (more on that later). And, most tellingly, in my sophomore year summer, I answered an advertisement in a professional journal for a field method season at the “medieval” site of Begho in Ghana.

At Begho I gratefully caught the Africa bug – and the first case of malaria of so many more that I’ve lost count (see below about my “ancient malaria” archaeo-histology lab!). At Begho we worked under the thoroughly decent Merrick Posnansky. I still miss my Ghanaian friends from Legon and from the village of Hani. Tough times for Col. Acheampong’s Ghana and for West Africa during the early days of the 1968 – 1985 Sahel Drought. But I see my three Begho field seasons (two more for a Cambridge M.Litt. while awaiting permits to dig at Jenne-jeno) as transitional: transitional from the intellectual rigor and excitement of Yale to the intellectual “Potemkin-villagism” (to coin an awkward term) of the dusty, dour archaeology department at Cambridge; transitional from the vibrant undergrad life and friendships of Yale to the grey, gritty, dark – and dour – Cambridge of Mr. Wilson’s England (and mind you, we of the McIntosh family, cleared twice from northern Highlands homes, normally delight in the dour); and transitional in that while I learned the challenges of doing fieldwork in the rain forest, I began there to cast my eyes to the north, to the vast and unexplored prehistoric landscape of the Niger River’s inland delta, the Middle Niger. And at Cambridge I met my future wife, Susan Keech McIntosh, with whom collaboration continues on Malian and Senegalese projects long after our separation.

It is time, perhaps, to dig deeper into the title of this essay. During the colonial period and in the decade after independence, so little archaeology had been done in sub-Saharan Africa that it is fair to call the sub-continent’s prehistory to be a *knowledge terra incognita*. Of course, the location and local significance of certain sites like Jenne-jeno or even Timbuktu never were incognita, strictly speaking, to Malians themselves. This fact will explain the “And Never Were” of the title. Even so, the dating and larger importance of their prehistory and earliest history were certainly unknown to the world. To make things worse, the earliest written sources about these peoples and lands were penned by chroniclers writing far, far away, never visiting the Western Sudan, and bringing to their narratives a mess of biases (including the conflation of “Black peoples” and “Lamlam” – or pagans), and a malignancy of ulterior and exploitative political agendas. Lack of attention to these prehistoric unknown territories by western historians and prehistorians was only a symptom of a deeper intellectual blindness – and the scales could only be ripped from the eyes of western scholars with the doing of systematic, extensive archaeology. Regarding West Africa, we can call the source of blindness the thesis of “The Golden Trade of the Moors,” with its corollary, “Arab Stimulation.”

Encapsulated by the popular book of that name by E.W. Bovill (1958), the thesis of *The Golden Trade of the Moors* was the conceit that anything “civilized” in West Africa (what we would now call complex society – long-distance trade, specialized

craftsmanship, cities, even states) appeared because of the trade by Muslim North Africans for sub-Saharan gold. Desired that gold certainly was. By the late Classical period, the usual pan-Mediterranean sources of gold (Egypt's eastern desert, the Balkans, Spain) had largely played out. By the later "medieval" period that saw burgeoning trade with China, easily the majority of newly struck Islamic (and increasingly, European) currency was made of West African gold. The sources of that gold were of intense interest to court chroniclers in Damascus, Baghdad, and Cordova, as were the itineraries along the grueling trans-Saharan trade routes. The gold fields were famously kept secret at the sources themselves. A consensus grew among those Arab chroniclers, passed on to later historians, that this trade would have begun no earlier than, say, the eighth or ninth century CE, with influences percolating south along the Saharan routes only when North-African looking, Berber-occupied southern termini towns were founded south of the Sahara in the ninth, tenth, or eleventh century (Awdaghost, Tunbuku, Tadmakka, inter alia.).

Hence the corollary to the Golden Trade of the Moors (as we now know), the intellectually shackling idea of "Arab Stimulation." This corollary in its essence was built upon the premise that the pagan Blacks of the Western Sudan, the grasslands and savannas south of the Sahara, were inherently so conservative, so uninventive that they had to wait until the eleventh century for elements of complex society (especially cities and states) to arrive in the form of the aforementioned southern termini towns. Thereafter, ever so slowly, the concepts behind urbanism, statecraft, long-distance exchange, etc. could creep glacially southward to the coast through the mechanism of stimulus diffusion. The southern Berber-founded towns were roughly at 22° to 17° north latitude (southern Sahara and northern Sahel). The "Arab Stimulation" reckoning was that complex society would have reached the southern Sahel (roughly to 14° N.)—including the Jenne-jeno region of the Middle Niger—no earlier than 1250 CE. Towns and states were not to be expected in the rain forest to the south until after 1400 CE. This became the received wisdom of books of all genres about African history. Thus wrote the doyen of francophone African archaeology, Raymond Mauny, in his influential *Siècles Obscurs de l'Afrique Noire* (1970), that (paraphrasing) before the arrival of Arab traders, West Africa was stuck at the developmental level of the European Hallstatt (early Iron Age) and that there was little trade beyond perhaps a fifty-kilometer distance of the small villages where everyone lived—and certainly no cities or polities beyond the chiefdom level. (Raymond Mauny becomes one of the heroes of this story—see below.) Even as progressive a scholar as my dear friend Nehemia Levtzion, in his equally influential *Ancient Ghana and Mali* (1973), cleaved to the notion that contact of southern, Black sedentary agriculturalists with Berber merchants was the prime mover for the development of states south of the Sahara. Consensus revolved around a date of sometime between the eighth and tenth centuries, at earliest, for this stimulation event to have taken place.



Excavating a unit at Jenne-jeno, at ca. 5 meters depth (around the turn of the present era).

In retrospect, this dating consensus is all the more surprising because of Jenne-jeno's location in the southernmost, most fertile basin of the Middle Niger. The social and political historians theorist Karl Wittfogel developed the "Hydraulic Theory" purporting to predict where the earliest, "primary" cities were most likely to emerge. Look to broad floodplains of arid lands, through-flowing rivers. The Tigris, Euphrates, Nile, Indus, or Yellow Rivers – here despots could force servile populations to create vast irrigation systems to water the fertile floodplains and thus provide the surpluses of grain to feed the growing city and to obtain the exotic luxury items that would mark the wealth and status of the elite. Mesopotamia boasts some fifty-one thousand square kilometers of potentially irrigable land, the Nile Valley some thirty-four thousand square kilometers. The Middle Niger is now only fifty-five thousand square kilometers of equally rich floodplain (as recently as ca. 3000 BCE we believe the middle Niger inundations covered some one-hundred-seventy thousand square kilometers). Why, before the Jenne-jeno excavations, did prehistorians not include the Middle Niger on the list of Primary Centers of urbanism? Recall that consensus revolved around a date of sometime between the eighth and tenth centuries, at earliest, for the secondary Arab Stimulation beginnings of West African urbanism to have taken place.

Imagine our surprise, delight—and shock when the first set of radiocarbon determinations from the first season of excavation at Jenne-jeno in 1977 came back with dates in the first centuries CE. These were not even from the foundation levels.



Rod on the surface of Jenne-jeno with burial urn. One to three individuals would be buried, in serial, within.

To backtrack a bit, as was known historically, Begho was the forest entrepôt where gold from “artisanal miners” in Akan country further to the south was assembled for donkey and porter caravans to Jenne, whence it was loaded on boat for the trip to Timbuktu, whence loaded onto camel caravan for the trip to North Africa. The same western Sudanese-born chronicler, al-Sa’di, who in his 1655 *Tarikh es-Sudan* said of Jenne that “It is because of this blessed city that caravans come from every point on the horizon,” who mentioned that there was a prior city, Djoboro (or Jenne-jeno, ancient Jenne). Now, if there was a city, a true city, *before* the present town (that grew because of the trans-Saharan trade, that is after ca. 1250 CE) *and* if West Africans *before* 1250 CE were just as savvy, just as innovative as Malians today, could it possibly be the case that ancient Jenne emerged fully urban for reasons other than the gold trade? There was only one way to know – by excavating. After years of waiting for the research permission, we sunk our first unit (JF1) in a region of the vast thirty-three-hectare tell (abandoned occupation mound) with scores of burial urns and exposed human bone on the surface. That season we put in three other units – with pathetic graduate student funding for the eight months in the field. Every day was a surprise. Since no excavation had been done before, we could not know that the six-meter-high mound was entirely anthropogenic, as opposed to a natural geomorphological element of the floodplain, such as a beveled levee or remnant dune, for example. Day after day I expected the deposits to peter out; day after day we kept digging deeper and deeper – of course, not

knowing at all the date of the deposits, of the ceramics, slag, bone we were cataloging in the field camp each afternoon. Imagine our shock with the report of the first set of radiocarbon dates.

And here I happily acknowledge the scholarly integrity and generosity of two elders, in particular. How presumptuous, two mere graduate students with four three-by-four-meter units sunk into Jenne-jeno to presume to say that the dating and by extension the whole intellectual foundation of the Arab Stimulation thesis was incorrect. Two senior Africanists spoke up. Thurstan Shaw, founder of the archaeology faculty at the University of Ibadan, by this time retired to Clare Hall, Cambridge, had long had a curious problem. His meticulous recovery of Fabergé-like bronzes from Igbo Ukwu, in the forest of Nigeria, was accompanied by consistent ninth-century radiocarbon dates – centuries too early according to the Arab Stimulation thesis for these signatures of complex society. Thurstan coined the term “suspense account” for a suspended acceptance of the Igbo Ukwu dates (I knew of no one who would gainsay his excavation skills). Clearly, our Jenne-jeno dates and these Nigerian forest dates made more sense if the premise of the Arab Stimulation thesis was just wrong. And so Professor Shaw became our biggest supporter in the anglophone Africanist world. We’ve already met Raymond Mauny, head of the Centre des Etudes Africaines at the Sorbonne, the undisputed master of Africanist prehistoric studies in the francophone world. Virtually immediately he said that, to paraphrase, “everything I wrote (in *Siècles Obscurs*) was wrong.” The opposition to accepting early Jenne-jeno, if there was any, crumbled. I frankly believe that the Africanist scholarly world was more than ready for the (frankly, not-a-little racist) Arab Stimulation thesis to go away. It was dumb luck, really, that I just happened to be at the right place, the right time, with the basic skills in excavation, a system of note taking, and (embarrassing as it is to say) a delight in stratigraphic interpretation (thank you, Vince Foley and the decayed mud walls of Begho and Hani). Three more better-funded seasons at Jenne-jeno (1981, 1988, and 1999–2000) (and one large unit put into Jenne itself), twenty-two units dug into the tell (eighteen dug to sterile), and three Malian co-directors – and we now are secure in the dating of the uninterrupted, fully indigenous, locally evolving urbanism at Jenne-jeno from 200 BCE to abandonment at 1400 CE.

It has been a long journey from the Moravians and Bethlehem Steel, those deep roots of my intellectual trajectory, to the deeply vexed question of what did the early urban society of Jenne-jeno look like. But here was the conundrum that only increased as we spent more and more years digging house after house, craft atelier after workshop at the ancient site – there were none of the classic *expected* signatures of what prehistorians had always thought should at least accompany, if not serve as the cause of, cities and “complex society” – that is, kings (or individuals at the apex of centralized, despotic authority) with a supplementary cadre of elite bureaucrats, police, merchants (to channel the flow of goods and information up, and decisions down the socio-political hierarchy). And – if the Arab Stimulation model held any

validity—there should have been evidence of “influencers” from the north, archaeologically visible in the form of any or many objects of clear Mediterranean, Islamic, Roman or Punic origin.

Jenne-jeno at 200 BCE to 1400 CE: *too early* (by a good thousand or so years); no outsiders; no sign even of long-distance trade with the North Africans who were supposed to bring the light of civilization across the vast wastes of the Sahara (to paraphrase an earlier Raymond Mauny); no king or trappings of a despotic control structure (expected at the time to be the *sin qua non* of early states and cities, according to classic Neo-Evolutionary theory); and no sign of a hierarchically stratified society (also the expected feature of any emergent city).

This last point, lack of material signs of hierarchical access to power or exotics, was further complicated by regional survey. Each excavation season was capped by several weeks of progressively more intensive regional survey until all seventy sites in Jenne-jeno’s vicinity (some one-hundred seventy hectares) were surface mapped, surface collected, with the ceramics from the last occupation dated. Eventually, thirty-five soundings of various depth were sunk into eleven satellite sites (including a nine-meter-deep, six-by-five-meter unit into modern Jenne itself). Within the limits of resolution of our dating methods, almost all satellite sites were abandoned around 1400 CE, implying a longish period of simultaneous occupation of the landscape (and much higher population than thirty-three-hectare Jenne-jeno itself). Differences in occupation-related artifacts and features on the surfaces of various sites led me to posit the hypothesis of the urban cluster: a web of interacting communities; specialists drawn together to take advantage of proximity of consumers and economies of scale, yet determined to maintain physical separation. Because site clustering is widespread in all basins of the Middle Niger, we now speak of urbanism at Jenne-jeno not as a single-site phenomenon (as ancient Mesopotamian urbanism is often characterized), but as the Jenne-jeno Urban Cluster.

Urban clustering was hardly original: many informants in Jenne from different occupation groups insisted that in the past their forebears lived on named satellites in the Jenne-jeno cluster. Talking to them I was transported back to the ethnically-specific shops and mills of Bethlehem and to the separate church-focused ethnic enclaves of South Bethlehem. And what of the apparent lack of king and despotic political apparatus (“City without Citadel”) and apparent lack of expected hierarchical social organization? Hierarchy: a centralized, vertically organized, multi-tiered model of power that concentrates decision-making in the hands of the few. The Jenne-jeno data just could not be forced into that theoretical straightjacket. So, I did not coin the term “heterarchy,” but the cumulative Middle Niger prehistoric story soon became the much-cited case study. Heterarchy: a complex organization in which relations between elements are unranked vertically but may be highly stratified horizontally. Multiple authorities are overlapping, counter-imposed and competing. To my mind, at least, this recalled the distributed authority prevailing at the Moravian Colonial Industrial

Quarter. Maybe one can't go home again, but home does embed in the structures of the mind. Pattern recognition.

All the while we were excavating in and near Jenne-jeno, or traveling to other Middle Niger localities (Dia, Timbuktu, Akumbu in the Mema) as well as the Senegalese tumuli and megalith fields and the tells of the Middle Senegal River floodplain – the elusive Takrur of the Arab chroniclers – a dark shadow fell over the ancient sites of Mali. The high-inflation regime of Western economies of the late 1970s and 1980s boosted the prices of all art and antiquities. The illicitly obtained, looted terracotta statuettes of the Middle Niger were no exception. Looting was barely visible on the surface of Jenne-jeno during the first 1977 season. By the long 1981 season, however, there were multiple large but shallow looters' pits. Throughout the 1980s many sites in the near vicinity were gutted to 90 percent of the surface. Sites as far away as Gao (Gao Sané) were disturbed so badly that archaeologists can scarcely find a spot on the surface to sink a proper unit. We were faced with the real possibility that the prehistoric past of Mali would be erased by the covetousness of the international, illicit market in art and antiquities before we could even begin scientifically to investigate it. This is the cue to introduce the first of my Malian co-directors.

Alpha Oumar Konaré had just returned from finishing his doctoral degree (on medieval cathedral architecture!) in Warsaw when we finally obtained permission to dig in 1977. He came out with us and supervised the excavations of the JF2 unit – but soon was turned unwillingly from archaeology when he became minister of culture, then head of the ADEMA party in opposition to the dictator Moussa Traoré, and finally in 1992 the first democratically elected president of Mali. Konaré was the first African president voluntarily to step down after two five-year terms (then going on to be president of the African Union). He never dug again, but he never lost his rage at the destruction by looting of Mali's archaeological heritage that we were just beginning to see in the Jenne-jeno environs. Long story short, with Konaré orchestrating things in the background and me liaising between the Malian Ministry of Culture and the US Embassy (where we were blessed with a series of solid cultural affairs officers, as well as David Rawson as ambassador and confidant of Konaré during the brutal last days of the Traoré dictatorship; Rawson was always a friend of Malian archaeology) concrete steps were taken to confront the looters and their international enablers. In 1988, at Konaré's orchestration, UNESCO declared "Old Towns of Djenné" (Jenne, as well as Jenne-jeno and several of the satellite sites) to be World Heritage sites. Nice, but that did little to stem the looting. Then in 1993 Mali and the US signed the world's first bilateral protection agreement under the umbrella of the 1970 UNESCO Convention on the Means of Prohibiting and Preventing the Illicit Import, Export and Transfer of Ownership of Cultural Property. All subsequent bilaterals around the world have used the language of the 1993 Mali-US accord as their prototype, something of which Mali remains justly proud. The bilateral of 1993 (an emergency import restriction) became an expanded, permanent import restriction in 1997 and has been renewed

seven times (after obligatory review), most recently in 2023 – and we have recorded only one shallow looters' pit at Jenne-jeno since 1993.

We broke the back of the illicit trade in Middle Niger ancient terracotta (in President Konaré's signature phrase, "C'est sûr et certain!"). This legal process and a none-too-subtle campaign to appeal to Africanist art historians not to legitimate the trade by valuations or by writing about known looted pieces were the subject of far too many articles in my career (and the book, *Plundering Africa's Past*). But what satisfaction!

188

I would be digging Mali still, but for the 2012 Tuareg revolt in the north, leading to multiple coups and horrible conditions for the citizens of Jenne, Timbuktu, and Gao, indeed for all Malians north of the capital. On the bright side, my recent shift to the Middle Senegal Valley (MSV) (where I had before directed an eight-month "Search for Takrur" campaign of tell excavation in the early 1990s) led to two generations of exciting Senegalese students (including some fifty or more trained in excavation, survey, and archaeomagnetism sampling basics during the 2016–2018 field seasons) and to the infusion of "historic Bethlehem" standards of digging and recording into the enormously rich prehistoric archaeology of Senegal. This then is the time to wrap up this tale of my intellectual trajectory by recounting the overlap of my post-Mali field work with the intellectual changes wrought upon my return to Yale in 2006, after twenty-seven years teaching at Rice in Houston (dreaming of cold water beaches and lobster boats – both of which I see every day outside my Branford shoreline home! Home again).

Coming back to archaeology at Yale meant being embedded in a happy department with really wonderful prehistorian colleagues. We train each others' graduate students as a collective, sending our own to each others' field area, especially to work on other graduate students' sites. We teach many courses jointly (most notably the department's Early Complex Society course required of all undergraduate and graduate students) and have jointly made the Yale Archaeological Studies Council a standout in the US in promulgating the laboratory sciences. And I have been able to teach advanced courses in non-field archaeology topics on which I've published abundantly: palaeoclimate and human response, ethnohistory, and advanced archaeological theory. However, I will end these reflections on intellectual trajectory by looking to the future; one year into formal retirement (and with four graduate students still to see to the end), Yale has allowed me to keep my two thriving labs. And I am still active at the University of Pretoria, Faculty of Geesteswetenskappe (how Hegelian!), as Buitengewone Professor in Argeologie (Extraordinary Professor of Archaeology; how embarrassing!).

We have been working since 2016 at a MSV site, Walaldé, with curious iron-smelting furnaces yielding radiocarbon dates of the early first millennium BCE. This puts them as some of the very earliest properly excavated iron-producing furnaces in sub-Saharan and right at or before the received dates for introduction of iron into the subcontinent. These furnaces look nothing like their Mediterranean putative-forebears.



Terracotta statuette from Jenne-jeno (ca. 1200 C.E.). Head possibly removed ritually to desacralize the object.

To complicate matters, the early Waladé carbon-14 dates fall within what is called the Hallstatt Plateau, that 800–400 BCE period of the calibration curve wonkiness within which it is impossible to resolve dates closer than 800–400 BCE. This problem does not apply to another, more experimental dating method, archaeomagnetism. Indeed, since collaborating in the early 1990s with Dan Wolfman, the deeply mourned pioneer in the application of palaeomagnetic dating techniques to burnt archaeological features, we have been taking archaeomag samples from multiple sites, from 1000 BCE to ca. 1500 CE, along the Senegal River. Upon my arrival, Yale built me a shielded archaeomagnetism lab, filled with super-sensitive magnetometers (e.g., the Alternating Gradient Magnetometer, or AGM, at 1.0 nanoTesla resolution), at present the only dedicated archaeomagnetism lab in the western hemisphere. The goal is to create a dating reference curve, of high resolution, of the last few millennia before present, for all of western West Africa.

Further, there is something of a revolution going on in bio-archaeology. We are now applying laboratory techniques too expensive or just not available just a decade ago, routinely, to problems of prehistoric disease, palaeodemography, palaeoecology, and historical ecology. Because of the serendipitous acceptance of a graduate student with a bio-chem background and a latent interest in the evolutionary epidemiology, we now have a robust method to identify malaria in ancient bone. In close collaboration with the welcoming “malaria mafia” in the Yale School of Medicine and School

of Public Health, my archaeo-histology lab has used (hemozoin-bearing) bone from the Peabody's Caribbean collection, from fetal bone from Umbria, from archaeological sites from Senegal, etc., to address questions such as was malaria in the New World before Columbus (yes! *Plasmodium vivax*). And, in a spectacular moment of serendipity, my archaeomagnetism grad student and my ancient malaria grad student put samples of the iron-rich bio-crystal hemozoin in the AGM – and we now have a way to distinguish the responsible malaria-bearing parasite *Plasmodium* by species. (Some call this the holy grail of malaria research.). All this because of that first case of malaria (*P. vivax*) from Begho in Ghana (and many later cases of Malian *P. falciparum*).

Let us finish in the future: My fondest hope is to continue my association with archaeology at Pretoria and, indeed to continue to take archaeomagnetism samples in South Africa. Briefly, in 2001 I was invited to the Universiteit van Pretoria to reform the department at that deeply Apartheid-implicated university. The ANC had shut down archaeology at Stellenbosch and threatened to do the same at Pretoria. Six years later (coming for their long winter term each year) the old faculty was out. I had recruited a whole new faculty (including the first Black professor and head of department), and the department is now out of the shadows and doing dynamic prehistory freed of the intellectual shackles of *Volkekunde*. My dream: to take a passel of Pretoria archaeology students, Black and Afrikaner, Colored and Indian (and maybe some eager Yalies for color) to the most beautiful site in the world (in my humble opinion), Klasies River Mouth Cave(s). After joining the pods of dolphins surfing the nearby Indian Ocean, we would take a long series of archaeomagnetism samples at Klasies from the fifty-meter-long profile of deposits dating from the emergence of *Homo sapiens sapiens* to the end of the Last Glacial Maximum. Now that would be a truly monstrous archaeomag reference curve!

Dreaming at Klasies: And at night around the braai fire, I would put the Pretoria students deeply to sleep with stories of H-beams screaming as they were cut at The Steel, of utopians fulling linen, annealing ornamental iron, and crafting their signature tile stoves deep beyond the Pennsylvania frontier, of deep excavations at Malian tells, and mincing around crocodile nests during surveys along the banks of the Senegal. One might awaken and ask, “Do you have visions when you peer into lands unknown?”

“Yes, of wonderful things.”