


THE WAY OF HISTORY: THE ATLANTIC JOURNEY OF AN AFRICANIST INTERVIEW WITH JOSEPH C. MILLER

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Joseph Calder Miller, or as he likes to be called “Joe”, is one of the foremost current Africanists historians⁵. He got his PhD from the University of Wisconsin, home of many of the researchers who helped consolidate the field of African history, he was advised by Philip Curtin and Jan Vansina. His research field was the History of Angola, in particular the Imbangala, a community that lived independently of Portuguese rule until the twentieth century.

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5 Shortly after the interview was approved, its authors received the news of Professor Miller's passing. We do not want to write an obituary, as more qualified people have written and will write texts about Miller's immense contribution to history in general (and History of Africa in particular), but the authors would like to add a sentence they heard about Miller recently and summarizes what they thought of him: “Joe Miller was one of the giants in the history of Africa.”

From his extensive work, *Way of Death* became an outstanding classic, and may perhaps be defined as an economic history of the slave trade in Angola, but also a history of the peoples connected through this trade. This classical and necessary work will finally be published here in Brazil, thanks to the collective effort of several Brazilian researchers. He is currently investigating the global history of slavery, contributing to understand this social relationship (or “strategy”, as he himself points out) beyond the traditional theoretical frameworks (slavery as an institution within the frontier of the National State).

We could list here his works, awards and achievements accumulated in a fruitful scholarly career, but we believe it is better to let the author speak for himself. After all, in this interview, reproduced in full thanks to the editors of *Almanack*, we have a synthesis of his career and a lesson about history and epistemology that do not require any panegyric.

1. To start, tell us about your career and your choices as a would-be scholar. Why History and African History? Who were your main scholarly inspirations in that field?

You may be asking politely about how I became historian of Africa, which is how I think of myself. The work that I have done on the history of the slave trade and slavery world-wide I view as secondary. And both of these “interests” were entirely by accident. I sometimes say to people who ask your question here “It’s not my fault!”

I came from a mid-twentieth-century middle-class family in a middle-sized city in the American “middle west”, which is not a background that particularly pointed me to end up in the *Baixa de Casange* in the midst of a war of liberation in 1969. My grandfather and father had been directors of a department store in that town (Cedar Rapids, Iowa), and I was raised to become the third generation head of the firm. I expected to spend my entire life in the city where I had been born, as had both of my parents and all of their parents.

But I had traveled, by sea, to Sweden as a summer exchange student during my secondary schooling, and I wanted to see a bit more of the world before returning to settle in. So I left town for my undergraduate education at a small men's college in Connecticut, Wesleyan University, in a very different part of the U.S. The educational philosophy there was what was called "liberal arts", that is, ranging widely across all the academic disciplines. I don't think that you have an equivalent in Brazil, where you earn your first degree in a defined subject, such as history. In the U.S. colleges (which focus on undergraduates) and even universities with graduate research programs – masters degrees and doctorates – require their undergraduates to take a wide range of courses with only a partial concentration (about a fourth of the total, one year out of four) in a "major" subject, such as history.

But I wanted to explore different fields, since – as my mother always said – I was curious. The Wesleyan curriculum required me to register for a "major", so I picked the least restrictive one I could find, which was history. The history faculty allowed me to count courses in art history and music history and the history of literature all toward my major. I was succeeded at branching out so well that I earned a degree in "history" without having taken enough conventional history courses to know what the subject might actually amount to. I was a dedicated dilitantte. But academic superficiality didn't matter, because I was going back home to manage a family business, and I could use a little knowledge about a lot of things at cocktail parties.

That practical objective led me to earn an MBA, two additional years of professional training in accounting and finance and other aspects of managing a business unrelated to history. But I should have suspected something when the capstone requirement, writing an "industry study", led me to write a 20,000-word essay on the history of the airlines in the United States. I think the faculty were expecting 2000 words and a recommendation to "buy" or "sell".

I had married a young woman from my home town, whose grandparents had been friends of my grandparents, and our fathers had been business associates for their entire adult lives. The two of us

returned home, for us both, and I began working at the family store, with the possible burden of being a “boss’s son”. But the employees were quite welcoming, and I began to inherit three generations’ worth of civic responsibilities, from two families. I don’t think I was ready.

Then one day my wife ran into a friend of hers from secondary school on the street. Both women had one-year-old daughters on their hips, and they decided to give the babies a “play date” together. The two mothers also enjoyed seeing each other again and decided to arrange a dinner for the husbands to meet also. That invitation was the fateful moment. My wife’s friend’s husband was an instructor at the local college – where all four of our parents had graduated, but which I knew nothing about. Except that my father had been on the college board of trustees, and I remembered him coming home to dinner after meetings of that board and complaining vehemently about how “difficult” the faculty were.

That was the evening that I learned better. My wife’s friend had included us in an evening where with a half dozen members of the college faculty, and their spouses. Well, it was 1963, and so the spouses were all wives, able, talented, creative, but unemployed. Their conversation electrified me! I felt like a light bulb had turned on in my head. These intellectual people excited me! My home town and the family business felt more like a duty...an obligation...even a burden.

But what to do? Obviously, I needed to leave business and to graduate school to become a professor and spend the rest of my life with smart, witty, engaged, politically savvy friends and colleagues. I wasn’t being paid much, boss/s son or no, and I had a wife and a child and a mortgage on the house we had bought. I turned to them for advice on whether I could afford to leave. What subject should I study? I had no idea, but because my undergraduate degree was in “history”, however little about the discipline I had learned, I would have a better chance of being admitted to a graduate program in that subject.

But that initial decision only led to another choice, for which I was equally unprepared. What field of history? My new friends advised me wisely: “Don’t even consider U.S. or European history; there are already too many PhDs in those fields, and with your financial

responsibilities you will need a job.” Then they said, rather uncertainly, “Why don’t you consider one of these new ‘non-western’ fields?” Remember, in 1963, every field other than Europe and the U.S. was brand new. I thought “Why not? I’m curious ...”.

Then I had to select a university. I chose three Midwestern universities because my mother suffered from Parkinsonism, and I felt I should be somewhere close enough to her to be available if needed. Among the three, every one selected for personal reasons having nothing to do with academics – of which I knew nothing – I had heard that the University of Wisconsin had a fine department of history. What I didn’t know was that the reputation I had heard about was from before World War I, the high point of midwestern progressivism. I was only half a century out of date: not a good start for a would-be historian.

So I applied. When I filled out the application form, I didn’t understand the question at the bottom of the first page. It asked “What seminar do you want to be in?” And then it listed all the regional fields that the program offered, starting with the U.S. (still that department’s main claim to fame), then Europe, and Ancient history, followed by quite a list of “new non-western” fields: China, Japan, Middle East, Southeast Asia, Latin America, India, and Africa. My reaction, literally, and I grit my teeth every time I remember that moment, was “Africa? I didn’t know that Africa had a history.” I had no idea that Hegel had raised the same doubts, and much more seriously, 150 years earlier.

But, I thought, it’s only a “seminar”, which I understood to be a one-semester course. I was, as ever, curious, and so I thought “Why not?” and checked the Africa box. I would have plenty of time, I thought, to look around at the other options.

Of course, “seminar” was a hang-over from the pre WWI language of their curriculum, a relic of Victorian English and the first decade or two of professional training in history in the United States. So their language was as out-of-date as were my expectations.

They admitted me to the Africa “seminar”, for reasons I have never understood; I certainly had no obvious qualifications. I didn’t

even realize that I had applied for a PhD in African history, never mind whether Africa even had a history. You can imagine my surprise when I arrived in Madison and realized that I was committed for the “long haul”.

But the good news was that I had ended up at the (arguably) best program in African history in the world, led by Professors Philip D. Curtin and Jan Vansina, two eminent founders of the field in the U.S. Along with no fewer than eleven other new students, nearly all of them returnees from volunteer service in Africa and coming to graduate school as a way of softening their re-entry into U.S. culture. Contrast their worldliness with my studiedly parochial background. The culture shock was mine! Fortunately, among them was my long-time and admired friend Mary Karasch.

Expected or not, I was fascinated and never looked back, even for an instant. Fifty years later, that group of students has gone on to lead the profession of African history in the U.S., and some of them internationally – including Allen Isaacman. We are known as the “Madison Mafia”, and I count myself as extremely privileged to have somehow ended up in just the right place at just the right time. Entirely by accident, through no “fault” of my own.

But there were still more choices to be made. Prof. Curtin was a busy man, and I think that he wasn’t so sure of the prospects in Africa for a former businessman from Iowa, and one whose foreign languages were extremely limited. Remember – I was going into business, and everyone did business in American English, and my accent was Midwestern.

Curtin had brought Vansina to Wisconsin only a few years before. He had not yet published *Oral Tradition* or *Kingdoms of the Savanna*, and so must not have been attracting many students. So I was assigned to him as my mentor; Isaacman had been assigned to him the year before. But Vansina, born in Belgium (on the Flemish side) was francophone, and I thought he would be a good guide to learn French. He was, of course, a polymath, speaking a dozen or fifteen languages (including African ones) and publishing stylishly in six of them.

As I would study with Vansina, my research would focus on central Africa, and so I requested a research topic in some francophone part of the region – most of it, of course, from Cameroun through (former) l’Afrique Equatoriale Française to Congo. So I thought it a bit odd when he pointed out the difficulties in pursuing every topic I proposed. Finally, he suggested the Chokwe (Quiocos) in nineteenth-century eastern Angola. “But,” I protested, “I’m trying to learn French and I don’t know a word of Portuguese.” At least I knew that the Chokwe might involve Portuguese. “Oh!” he replied, “Portuguese is easy. I learned it in two weeks while I was studying Italian. Just read a grammar, buy a bi-lingual dictionary, and do it.”

Naïveté can have its advantages. Vansina had a truly wonderful gift of making all his students feel like they could do anything. He seemed to know everything, literally beyond our collective comprehension, except for one blind spot: our limitations. But, what did I know? So I thought, “OK, I’ll just do it.” Of course, he hadn’t mentioned all the German travelers’ journals that I’d also have to read, or the sources in Italian (which I didn’t try to learn over a weekend while I was studying Portuguese). The result was a master’s thesis that won the the Wisconsin African Studies Program’s A.C. Jordan prize of and became my first publication and has circulated fairly widely over the years.

There are still a lot of stories to tell about improving my barely adequate reading knowledge of Portuguese to levels of oral comprehension and speaking that I could use in doing field research in Angola, but the question was “How did I end up doing research on Angolan history”, and the answer, again, is “through no fault of my own” – with the added emphasis that I could not have been more fortunate. My lusophone colleagues have been generously welcoming and supportive, in Portugal, Angola, and Brazil, without a single exception. I have learned more from them than I had ever imagined there might be to learn, and I thank you all. One more undeservedly fortunate outcome.

As for my intellectual inspirations, I have been extremely fortunate to have known two of the defining figures in African history so well, as mentors and as friends. It was the perfect combination in

practice as well as in principle, since Vansina and Curtin were so different in every way, personally, intellectually, and as mentors. I think that they weren't always entirely comfortable with these differences, as well as with their fundamental similarity in wanting to be in charge of whatever situations they found themselves in. Even Madison wasn't large enough for both of them in the same room, and so they alternated the years in which they went away on research leave. As a result, for both my MA thesis and my PhD dissertation, I worked out the topics with Vansina but wrote the texts for Curtin. With any luck, I managed to combine Vansina's insights into Africa with Curtin's dedication to writing clearly and to service to the profession. Their joint mentorship added Curtin's accomplishments in economic history, quantitative methods, and slavery to my own training in finance and combined those with what little I could manage of Vansina's multi-disciplinary versatility in languages, linguistics, anthropology, and archaeology, as well as pure historical method.

Most of these eventually converged in my *Problem of Slavery as History* (Yale, 2012), which is dedicated to both of them and tries to replace the conventional comparative thinking about "slavery as an institution" with an epistemologically rigorously historical focus on "slaving" as practice, as a strategy motivated and enabled by slavers' positions in their contexts of time and place. Their strategies thus emerged from ongoing processes of change and then altered those trajectories by bringing in resources (human) external to the preceding dynamics. It's Curtin's topic, treated in a historiological vein inspired by Vansina's unrelenting dedication to method – his heritage from his own training as a medievalist. History in early Africa is inherently presents a challenge to most thinking, intuitively in the conceptual fields of the modern social sciences. "Slavery", a sociological abstraction rather than an action, is a prime example – and routinely qualified explicitly as an (imagined) "institution".

Subsequently, I am inspired by colleagues intent on understanding Africans' distinctive ontologies. Jane Guyer (Johns Hopkins, *emerita*) is an economic anthropologist worthy of anyone's attention, for her all-but-unique ability to adapt formal liberal economics, the

most modern of intellectual discourses, to explain radically differing economic strategies in Africa. Steven Feierman (U. Pennsylvania, *emeritus*) an anthropologically trained historian, similarly bridges the gap between early Africa and our modern selves, articulating each side to the other. And David Schoenbrun (Northwestern), a linguistic historian, and particularly his students, have advanced historical linguistics from a technique of classifying languages to a method of recovering historical motivations, the starting point of “doing” any history, and doing so on the incontrovertible evidence of what ancient people were saying about what they were doing. You can use words to lie, but the words you use to lie can’t lie about what you were talking about. Selecting these three, for epistemological reasons, implies no lack of appreciation of the many other colleagues from whom I have also learned enormously.

I should add that I am constantly inspired in more immediate ways by the size and sophistication of the Brazilian historical academy, and particularly in its many practitioners treating the subject of slavery in your country. You train your students rigorously on an unparalleled wealth of intriguing documentation and orient them appropriately toward sophisticated theory and international historiography. I attempt to keep track of your proliferating academic conferences and the hundreds of research papers they produce every year, as well as a cascade of monographs and collections of research papers. And you post nearly all of this scholarship accessibly on line and keep track of it publically in multiple formats. Brazil is a model of engaged, responsible, and accessible scholarship. My thanks!

2. You were in Angola in the 1960s to carry out research field, in the middle of the wars for independence; how was your experience there? Was

there any trouble stemming from the colonial authorities or the guerrilla?

Ahhh – Angola! Another learning experience, “extreme learning”, one might say.

A bit of background: I have finished with those Quiocos, I thought. Now to settle into francophone central Africa somewhere and learn French properly. But Vansina had other ideas. Once again, he tolerated my search for PhD research subjects in Congo and Cameroun, but he always had reservations. Until one Saturday afternoon, in a casual encounter outside the usual frame of academic collaboration, he sighed, “What a pity that you don’t want to stay with the Portuguese and Angola. There’s a kingdom there, called Kasanje, that I will take up for my next book if you don’t want to research its history for your dissertation.”

End of discussion. I replied, “Yes, sir, I see what you mean. I’d better start learning paleographic skills.”

My assignment was clear, and, to me, it meant significant trust in me on Vansina’s part. His *Oral Tradition* had set the standard for the promise of Africans’ oral traditions as sources for the histories of the vast undocumented regions of central Africa. He had worked out that formal statement of historical method from his time in Rwanda and Burundi and in the Kuba polity in the western Congo – all in Belgian colonial territories, where he had been employed as a researcher for the government’s *Institut pour la Recherche Scientifique en Afrique Centrale* (I.R.S.A.C.) – based on procedures for documentary criticism that he had learned in his formal training as a historian of medieval Europe. But he had no document-based confirmation of the historical processes he had inferred.

Angola presented the prime opportunity to compare oral traditions in the Kasanje area, recorded since the nineteenth century by several European travelers, including the eight massive volumes of the Portuguese mission led by Henrique Augusto Dias de Carvalho in the 1880s, with Portuguese documentation of their occupation of the coastlands and subsequent commercial development of the trade in enslaved people to the Americas. My job was to study the docu-

mentation, collect whatever additional traditions might still survive about the history of the Kasanje polity, and confirm the historicity of the latter. That strategy meant as lengthy a period as possible of residence among the Imbangala (Bângalas in the ethnic schema of the Portuguese) of Kasanje. The opportunity to test one's mentor's famous historical method sounded very promising, to say the least.

But Vansina didn't mention Salazar, the dictator in Portugal, or armed liberation movements or the PIDE (state police, *Polícia Internacional para a Defesa do Estado*), the fascist secret police who had kept Europe's longest-surviving dictatorship in power, by that point, for more than forty years.

Nor did he bother to point out that the first violence, and brutal Portuguese government repression, of what became Angola's wars of liberation had started in late 1960 in the Baixa de Cassange, exactly the area of the kingdom I was supposed to study. Eight years later, for a lot of survivors the memories of that carnage were very much alive.

And then there was the sensitivity of Portugal's luso-tropicalist government to English-speaking (or writing!) visitors. Their unfortunate experiences with visiting anglophones dated back to the famous Scottish missionary David Livingstone in the 1850s. He had trekked north from the Cape of Good Hope to arrive in Luanda, where he was lavishly welcomed and treated very generously and respectfully. Then he walked back east to the Portuguese posts on the Zambezi River and returned to Britain to write his famous abolitionist journal - *Missionary Travels and Researches in South Africa*, (1857) - in which he severely criticized his Portuguese hosts' long devotion to the Angolan slave trade.

Portugal may have been Britain's "oldest ally", but by the end of the nineteenth century the triumphant British had become quite disdainful of a small country in Europe with large territories in Africa that they coveted. Those tensions culminated to the diplomatic crisis of 1890 in the Zambezi area and to Cecil Rhodes' invasion of the areas between Angola and Moçambique that Lisbon regarded as Portuguese. Then the British chocolatier William Cadbury wrote a scathing critique of the forced labor practices employed to send *serviçais* to São

Tomé to cultivate the cocoa used to make Cadbury confections. Liberal (in this case, Quaker) British were proving unwelcome guests in Portuguese Africa. The criticisms continued with the League of Nations Ross report (1925) on conditions of forced labor in Angola, and international criticism had been given academic currency at the very foundation of modern African studies in the 1950s, with American anthropologist Marvin Harris, British journalist Basil Davidson, and U.S. Portuguese literature Professor James Duffy's outraged documentations of the decidedly illiberal coercive methods of mobilization of African labor for Portuguese colonial projects in Africa.

Americans were becoming implicated as well, particularly after Africa's "year of independence" in 1960 and the election of John Kennedy as president of the U.S., the Katanga crisis in independent Congo just across Angola's borders, and disputes over U.S. air force uses of Lajes airfield in the Açores as radical protests against the military in Viet Nam grew in the U.S. in the 1960s. The CIA had been rumored to be involved with some of the liberation movements in still-colonial southern Africa, not least with the FNLA in Kinshasa, and rumors were circulating of American Africanist scholars gathering intelligence under the cover of their academic research. It was an acutely political sensitized era, and Madison, Wisconsin, had been one of the most active centers of protest, even violence, in the American anti-war movement. I and my friends had several times been routed from our classes owing to the tear gas from the street demonstrations seeping into the university halls.

What could authorities charged with the defense of Salazar's *estado novo* expect from a young American graduate student? Particularly from one intending to do field research in a highly sensitive war zone, and on the kingdom that for more than two centuries had been the main supplier of slaves to Portuguese in Luanda? The very subject of the slave trade, an embarrassment since Livingstone's exposé, in Salazar's time was eliminated from Portuguese academic history. "Angolan" history was the study of selfless missionaries, governors, conquest, and a "civilizing mission", a *missão civilizadora*, as well as Gilberto Freyre's luso-tropicalism, with its genius for racial harmony.

In this constrained context, it was safer to edit and publish documents than to attempt interpretive narrative history. Which turned out to be good news for a young Anglophone historian from Wisconsin, since so many of the key documents were carefully transcribed, annotated, and published and so many historical questions unasked. I have enormous respect for the dedicated and excellent work by the scholars of that era, Father António Brásio, Father António da Silva Rego, and the former government official António Carreira, just to mention a few who survived a fascist regime so creatively and productively.

But a young American was another story, even accompanied, as I was, by a wife and two children, aged three and five when we arrived in Lisbon in 1968. Portugal was not particularly problematic for foreign researchers, although the Arquivo Histórico Ultramarino usually required several hours to deliver the *caixas* of documents that we requested, and some of them arrived suspiciously empty. Whether or not the archive staff, or its PIDE complement, was “sanitizing” the parts of the collection we were allowed to see, we were being watched closely. The only way to obtain a visa to go to Angola (or Moçambique) was to allay the suspicions of unseen authorities by spending long, dark, cold, winter hours, in the unheated stone palace where the archive was housed, paging through collections of papers we knew had been emptied of whatever they imagined we were interested in. “We” were a small group of British and American PhD researchers, more of them working on Brazil than on Africa; there were no Portuguese or, in 1968, Brazilians, for reasons that your readers in Brazil will know better than I.

The understanding – and it was a tacit one, never articulated – was that if we kept our noses in the *caixas* and didn’t associate with people the PIDE regarded as dangerous, who included any African, we would eventually be informed that it was appropriate to apply for visas for Angola. Since one never knew whom the government might suspect, we socialized mostly among ourselves, huddled in a small expatriate enclave. The great loss was how few Portuguese we were

able to meet, but it was a time of well-advised caution, wariness, for them as well.

Eventually, an academic contact whom I had met, someone who was well-connected in official circles, informally suggested that visas for me and my family might be available. I think that the PIDE had decided that CIA spies didn't travel with wives and children. Or that I had demonstrated sufficient folly and futility in that long, cold winter in the AHU that I couldn't possibly be a threat to anyone but myself. Or that I was so naïve that they would be able to convince me of anything and turn me into a propaganda coup when I returned. I might become their revenge for Livingstone's insults.

I won't try to describe the mystifications of conducting any sort of business with a police state, particularly with its state police, but somehow we received our visas – for only thirty days – and packed up for the long flight to Luanda. Since TAP, the Portuguese national airline, was barred as an agency of a colonialist regime from landing anywhere in independent Africa, or even overlying its new nations, they had invested in the first generation of long-range 747s, and so making that long flight all the way out around the western coast of Africa before turning back southeast toward Luanda was another learning experience.

But land we eventually did. I wanted to pay appropriate respects to authorities in Luanda and then move directly on to my research site in the eastern parts of the District of Malanje, the inland terminus of the Luanda railroad and accessible by a tarmac road, part of a recent build-out of highway infrastructure in the colony to enable the logistics of the Portuguese army deployed there to defend Portuguese interests from three liberation movements operating in isolated pockets in the territory as guerrillas. The road system still consisted mostly of dirt tracks, rutted and dusty in the dry season and impassably muddy during the rains except for four-wheel drive Landrovers or other heavy equipment.

A vehicle suitable for transporting a family was essential to my plan, since I expected to be seeking out African experts knowledgeable in the oral traditions of the former Kasanje polity, many of them

living in villages located as far as possible from the colonial government's rural administrative posts and roads. The premise of a remote, pristine "traditional Africa" untouched by modernity was absurd, of course, anywhere in the continent, and utterly impossible in an area organized for four hundred years around sustained contact with Portuguese armies and slave-traders. But my proposed method of using Portuguese documents to confirm the historicity of African oral traditions presumed independence of the two kinds of source. I was in Angola to discover an illusory "traditional Africa" that the logic of my project absolutely contradicted.

That unrecognized incongruity turned out to be the least of the contradictions of living for six months in the Angolan *mato*, and also to be a drastic underestimate of the resilience and integrity of the African world that I was about to enter, in spite of centuries of slaving and military repression of the Imbangala in a series of late-nineteenth-century military expeditions sent to the area and nominal defeat of the polity before 1910.

Somehow – I have forgotten the exact steps – I ended up in the office of a man involved in unspecified "consulting" work in Luanda, perhaps for BP, the British oil production company then developing offshore drilling in Cabinda. Luanda was already showing signs of being a petroleum boom town. When I inquired about finding a vehicle, he, seemingly very generously, offered me his own. I protested. He insisted, and so we ended up with a small, light British Opel – not designed for backcountry explorations, but at least a start.

Officials in Luanda, also with unexpected cooperation, gave me a formal letter of introduction to the governor of Malanje District, where I intended to work. And off we drove, with our small children and our small vehicle loaded filled with enough equipment for a family for a year.

The Malanje district governor, clearly under instructions from higher authorities, welcomed me with elaborate Victorian formalities of the sort that prevailed in Salazar's Portugal, seemingly frozen in time since before the Estado Novo. The national tourism board had promoted the country in international newspapers by beckoning

Americans to “Come see Portugal, Europe before it changed”. Lisbon felt like a step back in time, open electric street cars (*elétricos*), and all. Rural Portuguese Angola struck me as caught in another time-warped, a violent frontier just like the nineteenth-century North American “wild west”.

The district governor had arranged for my research, based in a small administrative post called Quela, perched on the top of a towering sheer wall of rock that dropped 500 meters to the low-lying valley, the *baixa* of Kasanje, and the Kwango River, the westernmost large north-flowing river of the Congo River system. Kasanje had derived its leading position in the slave trade from control of the crossings of the Kwango that led to the major eighteenth-century slave-raiding polity in western central Africa, the so-called “Lunda empire”.

Quela was also the site of the processing facility of the chartered monopoly cotton company (Companhia Geral dos Algodões de Angola, COTONANG), whose ruthless practices of forcing African cultivators to cultivate cotton had led to the 1960 revolt in the valley, and to subsequent brutal military repression. The town also housed the camp of a company of the Portuguese army. It was the nerve center of the Portuguese colonial occupation in the region. And of the Angolan Catholic Church. It was a town of a few dozen Europeans surrounded by *favelas* (in Angola called *musseques*) home to a few thousand Africans.

The civilian government administrative officer in charge had been instructed to prepare for my arrival. As we drove into the town late one afternoon, sweaty and covered with red dust, the sight that greeted me was an assemblage of all the government-appointed *sobas* (village chiefs) in the region, in full formal regalia, sitting on the steps of the government administrative building waiting for me, ready to talk, complete with an African government messenger (*cipaio*) to interpret what they had to say.

I was entirely unprepared for this overwhelming encounter, but I made the best of the occasion – which was not very much. The compact cassette tape had been invented in 1964, and Professor Curtin had been alert to the possibilities of this new and seemingly marve-

lous technology for making storable records of performances of oral traditions in Africa for later historians to consult. We were to create oral archives of Africa's traditions before – as everyone worried – their bearers in the villages died, without leaving successors among newly liberated generations already moving on toward modern futures in the cities. I was prepared, Sony machine and dozens of new cassette tapes among my equipment.

The *sobas*, like everyone else along my way, were under orders to tell me whatever I wanted to hear. I was alarmed to find myself treated as if I was one more in an endless series of government “ethnographers” or other commissions of inquiry, in effect a colonial spy, gathering intelligence that might help the Portuguese maintain control in one of the last remaining colonies in otherwise independent Africa. So the *sobas*, and I, did our best to make something worthwhile out of a profoundly awkward, artificial situation. I knew enough about oral performances to realize that they were deeply contextual, products of their moments, of the people present, of the purposes of their telling. The chiefs present were all from the highlands around Quela, none from the great valley of Kasanje. And I was not yet adept at operating my recorder, which had an annoying tendency to lose battery power at crucial moments.

In this suffocatingly official context, surely with PIDE “observers” present somewhere in the crowd of perhaps fifty men, what they were prepared to perform were the “praise names” attached to the positions they held and that entitled them to be present on this occasion. I explained something about these “praise names” in the book that developed from this dissertation research, a fine Portuguese translation by Maria da Conceição Neto as *Poder político e parentesco*, so here I'll mention only the pragmatics of struggling through trying to capture what I could on tape. These verbal formulae were compilations of verses composed to commemorate all the predecessors of the present occupants of their titles, memorized verbatim, and so preserving archaic language and obscure allusions to otherwise forgotten issues and events going back centuries, at least three, perhaps five or six. And they were largely unintelligible, incomprehensible to

their tellers, who performed them by starting in loud tones at a measured pace but then speaking faster and lowering their voices as they moved into the less intelligible earlier portions, and trailing off into an inaudible mumble. One after another ... until the sun was setting and my batteries exhausted. We agreed to return the next morning at 9:00.

In the meanwhile, my wife was attempting to unload our belongings from the car and manage a three-year-old and a five-year-old, and move into the accommodations they had prepared for us. It was the servant's quarters in a vacant cement-block house, one of the structures that the government maintained for officials posted to the town. To complete this picture: four people in a single room, perhaps four meters square, with a small kerosene burner to cook and a bathroom, and a single bed. We were experienced campers and prepared to "rough it" as part of the full experience of "Africa". But these circumstances seemed a bit rudimentary.

The next morning I rejoined the region's assembled African officialdom back in the administration building. I'm sure they tried their best. I know I did. But by time for coffee it was clear that it would be pointless to try to continue. They went their ways and I was left to figure out how to start over.

At this point it became clear that the government's hospitality was meant to speed me on my way – perhaps one night in town. I suspect that they were applying a familiar model of tolerating "visiting experts" whose inquiries were superficial at best and who spent more time drinking with local officials than consulting with the subjects of their researches. To government civil servants, and doubtless also the PIDE, the idea that Africans had anything resembling a history was literally incomprehensible. They were consummately polite in tolerating my "fantasies" about recovering a history of the kingdom, but utterly baffled; they could not imagine what I might be doing. For them "history" in Angola began with the arrival of Diogo Cão at the mouth of the Congo River in 1482 and told the story of their country's subsequent "civilizing mission".

As it became clear that I meant to stay indefinitely, they faced a problem: I was so naïve, and they were so fearfully suspicious of the Africans among whom they lived, but whom they did not begin to understand, that they worried that I could get myself into trouble that would embarrass them. Their reasoning for allowing me into the Angolan *mato* was to demonstrate the falsity of their scandalous reputation as colonial rulers. Anglophone observers, since Livingston, seemed to command the attentions of the international media, and I, treated with suitable generosity, might return to the real world and correct the prevailing misimpressions about the 1960-61 insurrection in the *Baixa de Cassange*. They had nothing to lose, and so they pretended they had nothing to hide. And I had to be made to feel more than welcome. That is to say that they couldn't expel me without undercutting the basic reason they had allowed me to come and had gone to considerable expense to make me feel at home.

As they prepared to wait me out, they moved us into the main part of the house where we were first accommodated, if you can say that, in the servant's room. The dining room in the main house was where I asked the government *cipaio*, my official interpreter (and spy, no doubt), to come and try to transcribe the praise names I had recorded in that first chaotic session. He must have been very uncomfortable being invited into a family space totally off limits to Africans in Portuguese homes.

He was a native speaker of the Quela dialect, not the dialect of the valley, and certainly not familiar with the archaic language used in the praise names. His Portuguese could best be described as "command bureaucratic" and utterly unrelated to the poetic allusions and political subtleties of the praise names, even without taking into account the archaic vocabulary and the calculatedly obscure articulation of their more sensitive components. These powerful names were designed to be heard and understood only by those qualified, trained, and trusted to hear them. Words were powerful. And this poor man was a lackey of the government that had bombed and staffed the villages, women and children, of the rebels of 1960-61. It was not a promising collaboration.

The prescribed method for creating an oral archive of the traditions was, first, to transcribe the words recorded, from tape to paper. But no dictionaries of these languages existed, and there was no orthography for writing them. I was not a linguist trained even to hear their phonology, let alone try to create the phonetic symbols to represent their phonetic features, even in a pointlessly crude format based on English phonetics, or Portuguese orthographies, or the conventions of the Sanskrit-like “International African Alphabet” adopted during the interwar years by the wave of colonial ethnographers who had descended on the continent aiming to write standardized descriptions suitable for systematic comparisons of all of what were then seen, dimly, as its “tribes”. That was an exercise born of colonial-era ideas of African “societies” that we now see as well-meaning projections of modern European ideas of social order. However, this “ethnographic survey” served its purpose of rescuing Africans from the chaos attributed to them at the height of European racial arrogance.

Needless to say, I failed to entice my interpreter to articulate what had been spoken into my tape recorder in sounds that I could attempt to transcribe, unable even to discern the elemental semantic units (distinct words) in Bantu language syntax that works by extending root words with layers of embedded suffixes and prefixes. He tolerated me, as he was surely under strict orders to do. But I thanked him for his services, and he disappeared, probably transferred to another government post, as I never saw him again. Or he had been brought to Quela from a posting elsewhere in order to keep track of me but reported the futility of his assignment.

But then, inexplicably and unexpectedly, my prospects brightened. The Catholic mission in the area, the *Missão does Bângalas*, Holy Ghost Fathers, the *Ordem do Espírito Santo*, was located down in the *baixa* right in the heart of one of the major components of the Kasanje polity. The Catholic priest in Quela, very quietly open to my project, hinted that his mission colleagues – lay brothers, all Basques, and thus quite outside the spheres of illusion in which the Portuguese seemed to be living – knew an individual who seemed well informed about the oral traditions of the region. One of the brothers was

an amateur ethnographer in his own right and was learning the local language and collecting the proverbs that were an important discursive mode. These dedicated men were my kind of people, endlessly generous and hospitable, and educating about 120 boys in the mission's residential school.

Because of official Portuguese concerns about me, I needed to remain entirely visible to them. I told them everything I intended to do before I did it; I even offered them the opportunity to see copies of the research notes I was sending back to the United States, to prove that I was actually talking about history rather than, say, politics. But, since they were posing as open and trusting hosts, they could not admit to being suspicious of me, and so they always refused my invitations to observe – at least to my face. I will never know who might have opened those packets of notes as they made their ways back to the U.S., although every one of them (several dozens) arrived intact. But if I left Quela to spend time at the mission, with other foreigners, would I be getting myself into trouble?

For all these reasons, it was out of the question even to consider living in a village with the people I had come to Angola to understand. I had failed even to be able to hire anyone to teach me the language, as the PIDE would be suspicious of a foreigner talking to Africans in their own languages, which few Portuguese in Angola could understand. Without knowledge of the language of the traditions, the first, elementary step that any serious researcher would take, I was significantly limited before I had even started. Over time, I worked out a special purpose “idiolect”, that is, an ad hoc private combination of relevant Kimbundu vocabulary and Portuguese grammar to talk about the content of the traditions. We had a certain advantage in the content of the traditions, which were mostly genealogical lists of names, which did not require translation as such.

My family and I decided to visit the mission, packed up and drove, very slowly, along a single-lane, twisting, deeply rutted dirt trail down the steep cliffs and into the valley. The Bângalas mission became our “home away from home”, partly because the brothers there were so hospitable, but primarily because the knowledgeable gentle-

men living in the area turned out to be the living embodiment of the kingdom I had come to study. Literally: he personified the polity, which was how African politics functioned. He was trained as the “court historian” of the regime, an office that the Imbangala had preserved, out of sight of the Portuguese, for six decades after they had officially removed its last king. He was a walking, talking “archive”. For my purposes, he was “the mother lode”. For his purposes, which were no less important, I’m sure that he saw my tape recorder and then pen and notepad as his only opportunity to preserve details that he had been trained since childhood to know, or rather, to “be”, and be able to recall as needed, but that he had no successor to carry on.

But the antiquity of some of the information in chronological terms that intrigued me was not what he valued. Some of it surely certainly dated to the seventeenth century, and I estimated that the oldest elements referred to people and processes of the 1400s. We think of those times as long ago, vanished, no longer present. But for him, and most rural African Angolans, the past lived on in the present. The living embodied their ancestors. What I thought of history for him constituted life itself. The kingdoms that the Portuguese had thought they had defeated and dispersed in 1900 continued on, with new people taking the places of their defeated and banished predecessors. It was alive and well in 1969, and my expert guide took me to the villages where its present personnel lived, as often as he was free to lead me, while supporting himself and his family as a farmer. I flatter myself to hope that we established a relationship of appropriately professional respect, with him as my mentor. I was extremely fortunate that he seemed to accept me as his student.

He and I settled into a routine, in which he would make an appointment for me with a holder of one of the positions inherited from the past, always a mature gentleman, sometimes extremely advanced in age. We would then drive, often many kilometers along dirt trails, to the village of the designated office-holder, who would greet us with full formalities due his position, and mine, as – I suppose – an apprentice “court historian” with literate and electronic technologies. In these official occasions, not unlike formal diplomatic

conferences in the world of nation-states, he embodied – much more substantial than merely “representing” some abstract thing imagined as separate – the accumulated accomplishments of his ancestors.- He appeared with the emblems of his full presence, or accumulated past, and he spoke in the first-person present tense about deeds many generations earlier, always in the presence of as many of the people in his kin group, on their behalf, and so subject to their acceptance of what he said. His words articulated them all; they were his words, as custodian for the assembled generations. No one else could give them the reality that they constituted.

And anyone who dared to intrude was a danger to all. The words were proprietorial. I made a large mistake, as I was just beginning, by pressing someone to speak on a subject that violated the sanctity of another invested speaker. I learned later that this violation had led to a fistfight in the local tavern that evening. From then on, I was careful to try to sense the subtle guidance of my collaborator, the “court historian”.

He was himself a kind of trustee of the realities that the custodians alone were capable of manifesting – “authorized” would not capture the ontological substance of speaking in that official capacity. In fact, not many of the elderly gentlemen remained able to speak these truths by 1969, or in their advanced years, few still knew the traditions they were responsible for embodying. The historical reality of the kingdom was fading before our eyes, badly in need of the rescue that I might bring. The “court historian” knew all the revered lore but never presumed to speak for others. Instead, he would gently “encourage” or “suggest” or otherwise prompt the legitimate custodians to convey what he wanted me to write down. These dignified and discrete performances of propriety and respect were moving to behold. I think I was present for official occasions that – except for my quiet presence – echoed an etiquette refined and continued over centuries.

In this professional way, I had the privilege of spending quite a few, very productive, days in villages far from the Portuguese network of military roads. I can only guess why the omnipresent and never-visible PIDE allowed these consultations to continue, unless

the officials were preoccupied with matters closer to the paved roads. These remote, highly personal settings were too intimate for an informant to remain undetected. Informants can survive only in contexts of anonymity, among strangers, or among people engaged only in limited, superficial contexts.

To return to your question about doing research in a totalitarian police state, the PIDE were still watching. Within two weeks after we had settled into our house in Quela, a very friendly young man moved into the previously vacant house next-door to ours. He has served in the Portuguese army then struggling to keep control of the west African colony of Guiné-Bissau. He said that he had come to Quela as an employee of COTONANG, the cotton company with the processing factory in the town. But we never saw him in the vicinity of the mill; he seemed always to be lounging around his house, and available to advise us on routine matters of living or how to fix the single-burner kerosene stove on which my creative spouse was cooking meals for four people, or how to prepare the live chicken I received as a gift for dinner. I won't go into those details here.

And then, there was the question of our thirty-day visas. To renew them, every month we would drive 100 kms, fortunately most of it on paved roads, to the PIDE post in the district capital town of Malange, hand-in our passports for processing in Luanda, and expect them back the following week with new visas stamped in them. We needed to make these trips to town to buy supplies and to enjoy a restaurant meal or two. After three or four of these calls on the PIDE staff, the formalities seemed routine.

Until the next time, when we went to pick up our passports, they had not come back from Luanda. We were assured that the delay must be a normal sort of thing and that we should expect to have them returned the following week. Except that, when we went back a week later, they still hadn't turned up. Inexplicable, but not to worry. So we didn't.

Until one Saturday afternoon back in Quela, I was walking down the sandy main street when I saw one of the Malange PIDE officers walking toward me. Portugal in 1969 worked on the legendary "se-

mana inglesa”, that is, nothing official ever happened after 1:00 pm on Saturday. So the unexpected presence of a Malange-based PIDE officer in Quela in the mid-afternoon was pointedly off the record. When he saw me, he feigned surprise and said “Senhor Miller! O que está fazendo por aqui?” So I answered, “Senhor Adjutante, you know that I’m researching the history of the Baixa. And my work is going very well, thank you.” “How very odd,” he said, “I thought you had left.” “On no,” I responded, “I’m planning staying as long as I can, probably until the rains start.” “How very odd,” he repeated and continued walking down the street.

Then, when I attempted to arrange contacts with African authorities through the government *chefes de posto*, they would assume me that they would make my intended contacts available. But when I drove the 100 kms or so to meet them, they were “momentarily away”, though expected to return very soon, perhaps that very evening. So I waited, and hours turned into days, and no one appeared, even though they were perpetually expected “very soon”. I found myself wasting days on end waiting for people who never came; fortunately I was able to work productively on my notes. But the delays were becoming suspicious. I can only imagine how those *chefes*, whose families entertained me hospitably the whole while, were laughing behind my back.

The pace of the game was quickening, as they then began to try to incriminate me. The Baixa was one of the diamond-bearing regions in Angola, controlled by a private international corporation based in Belgium (DIAMANG, Companhia de Diamantes de Angola), and independent to the point of operating its own police and running on a time zone half an hour ahead of Portuguese time in the rest of the colony.. Walking along in the sand, one occasionally caught the glint of small diamonds in the sand when its surface was disturbed, but it was illegal bend over to touch them. The company’s monopsony was total.

But gemstones were far from my mind one bright day, during the lunch hour, once again on unofficial time, when someone knocked on the door of our house. I opened it, to find an African man wearing a suit coat, in spite of the hot sun. When I asked what I could

do to help him, he glanced furtively from side to side and wondered if I wanted to buy any “beans” (*feijão*). Not expecting to be offered food at my front door, I was confused and, not trusting my comprehension of his accent, asked him to repeat. “Feijão!” he answered, with emphasis. Still uncomprehending, I asked him to repeat again. This time he put his hand in the lapel of his coat and to reveal the top of a brown beer bottle: “Senhor ... FEIJÃO!”

At that point, I finally understood. One of the sideline businesses of some Africans in the region, always conducted under the cover of euphemisms and illusions, was to fill old beer bottles with broken glass and attempt to pass them off to unsuspecting tourists as diamonds. I was so surprised to recognize the scam that I exclaimed, rather loudly, in broad daylight, on a public street, “Oh! O senhor quer dizer diamantes!” The man disappeared, running down the street at top speed, before I could declare my utter disinterest in the trap he was attempting to set. In a land of illusions, it was important to keep one’s eyes wide open.

Their next move was to involve the military. The off-duty soldiers in the company stationed in Quela often came out of their walled compound to stroll around the town, and so I wasn’t too surprised, one evening, when three or four of them turned up at my house to invite me to go along with them on a night-time hunting expedition they were planning. Big game, mostly antelope, some of them as large as ponies, were still plentiful in Angola in 1969, and they were nominally protected by game laws that prohibited hunting them at night with sealed-beam torches. Everyone, including government officials, hunted at night with torches, regardless of the law. The “wild-west” ambiance was palpable in this casual disregard for firearms and game laws. Guns were everywhere.

I had observed, or rather accompanied, my civilian friends on more than one of these dry-season midnight expeditions. They would fuel up their Landrover and then drive out across the open bush shining a sealed beam torch around the horizon. When an antelope heard the noise, it would look up, and its eyes would catch the light, thus locating it for the hunters. Blinded by the light, the animal usually

froze, allowing the hunters to approach it to well within the range of an average marksman with a high-powered, large caliber rifle. Some of these expeditions returned toward dawn with the cargo areas of their Landrover piled high with carcasses. But I was careful never to touch a gun, much less fire an illegal shot.

When the troops invited me, I thought it was be interesting to see how military-grade weapons performed in these circumstances, and so I assured them that I would be interested in going along “for the ride”. We set the date for two nights from the agreement. The following night, the soldiers returned to assure me that the plan was proceeding and that they had managed to obtain my gun for me. Not wanting to inconvenience them, or expose them to misusing army-issue equipment, especially a lethal weapon, I explained, “Oh no, that’s not necessary. I don’t hunt, but I am interested in observing your tactics and marksmanship.” They said nothing and returned to their post.

Until the following morning, when a single soldier showed up at my door, filled with apologies. The captain, it seemed, had ordered them to go out on an official mission on the night we had planned our hunt: “que pena!”. By this point, I was exercising more and more caution in assessing every hospitable invitation that came my way, and I suspected that I had “dodged another bullet”, so to speak, in declining to fire a weapon in the company of the defenders of the colony.

Then the chief administrator of the district, in an authoritative and sometimes authoritarian position in the civilian administration, and the one who had assembled all the government chiefs to greet me when I arrived, saw me out on the street one evening, again “after hours” and so a completely friendly, chance encounter, nothing official at all. “Hello Senhor Miller,” he greeted me, and then immediately remarked, “I understand that you don’t have visas to be here with us.” I acknowledged that we didn’t. He continued “Do you know what we do to foreigner in our country without permission?” and laughed. Before I could speculate, he followed up on his private joke with an invitation to join him at his home for a glass of whiskey. And so we dissolved the incident in an alcoholic haze of deniability.

But I was getting the hint that it was time for us to volunteer to leave. As we were officially welcome to remain as long as we wished, they could not expel us or even advise us to move along. So we needed a reason to go. In fact I had prepared for a departure by making it clear that our car was not capable of moving on the dirt tracks I was using when the rains started. Angola has a six-month rainy season in the southern hemispheric summer, November to April, and I had been there for the absolutely rainless dry season during the other half of the year – hence the dusty roads that I have noted. At that point, we began “praying for rain”, as the English expression goes.

But that year the rains were late. And so they gave me more reasons to get going. My missionary friends advised me that they had been questioned about me, and I could not risk endangering them. The situation was growing urgent.

And then the assistant administrator invited me to join his family for lunch one Saturday afternoon – again, after “business hours”, and the sort of occasion that we had several times enjoyed with friends and seen turn into affairs lasting well into the evening, and in one delightful instance into a five-day trip into the interior. Whatever the official suspicions, the personal hospitality we enjoyed was generous to the point of overwhelming. And in that spirit I accepted his invitation. Also because, in the web of soft surveillance woven around me, he was the “good cop”, the friendly, helpful face of the civil administration, to offset the “bad cop” posture of his rather stern superior.

The lunch was a relaxed leisurely family affair, with his wife and young children, and the usual alcoholic accompaniments. As we finished, he invited me into the family’s parlor, in Portugal a private space reserved for kin and trusted friends. He added to the air of confidence by pouring glasses of imported scotch whiskey, not the Angolan product usually available, and not the locally distilled counterfeit “Johnny Walker”. With other accents of intimacy the conversation moved into personal experiences of our respective careers, for me a possible peek behind the pervasive posturing. And as the sun was setting, he casually wondered “What are you *really* doing here?”

It was a moment that revealed the profundity of the illusion of living in late-colonial Angola, where the threat of a popular uprising like the one in the Baixa in 1960, or its sequel months later in northern Angola, which had murdered a considerable contingent of Portuguese living and working in the region, many of them colleagues of the survivors I was among in Quela. The liberation war also loomed constantly. The PIDE could be anywhere and seemed to be everywhere. And they could not recognize that I was doing exactly what I appeared to doing: talking to old African men about their history. After six months, since they couldn't see what I was doing, their suspicions only increased.

Finally, the rains began to fall, and we hastened to pack up to return to Luanda, and where I expected to begin research in the Arquivo Histórico de Angola, in a voluminous collection of government records dating back to the 1740s, reading them in the light of what I had learned about the oral traditions. When we announced our impending departure, we were greeted with a chorus of regrets that we were leaving so soon. Everyone wished that we could stay longer. But the rains fortunately continued to fall, justifying our "reluctant" departure. Besides, our visas had still not arrived at the PIDE post in Malange, and one of the officers there had suggested, helpfully, since the local staff had had no luck in penetrating the bureaucracy in Luanda, that it might make sense for us to go there and inquire about them in person.

Even with our car packed to overflowing with all our belongings and headed west, they took no chances. A rather spectacular waterfall, Quedas de Duque de Bragança (now Kalandula), was located some ways off the asphalt highway, and we thought we could take an advantage of our rather forced evacuation to detour to visit the site. The Angolan government was promoting tourism, in spite of the liberation war, and had built a large *pousada*, convention center, restaurant, hotel, meeting rooms, in a spectacular location right on the edge of the falls. We anticipated a good lunch with a dramatic view, and so we set off down yet another dusty, rutted dirt road to relax, see the sights, and enjoy the hospitality. We felt we had earned a break.

Until our dangerously overloaded car, quite the worse for the wear of driving down all those trails through the bush in the Baixa de Cassange, blew a tire. I managed to unload enough of our baggage to find the spare tire and mount it on the car, reload our belongings, and drive on, still in time for lunch. When we arrived at the *pousada*, the vast parking lot was empty, except for a single vehicle. We were afraid that the facility was closed, but we have driven so far that we thought we would get out and see if the doors were locked. In fact, they were open, and we were greeted by a full staff of host and waiters, ready for us. The dining room was enormous, more than a hundred tables. Only one of them was occupied by a couple, evidently the owners of the single car we had seen in the parking lot. The staff must have been eager to welcome another set of patrons.

We were taken to the table adjoining the one occupied, where the couple greeted us enthusiastically and invited us to join them as their guests. They were the commanding officer of the military company stationed in Quela, a captain, whom I had met several times over the preceding months. "What a surprise! Such a coincidence that we and they should find ourselves at this rather remote *pousada* together!" At that point in the performance, I was anything but surprised. But they were entertaining people, and we enjoyed spending the entire afternoon together, exploring the waterfall. We genuinely lamented having to end a pleasant occasion as the sun began to drop toward the western horizon.

Driving back to Malange, with the sun setting, we paid again for having overloaded the car, when a second tire blew out. This time, I had no spare to replace it. We were marooned on a deserted dirt road on the edge of an African village, with two children, as darkness descended, and no help in sight. That is, until a dust cloud appeared on the horizon and soon materialized as the automobile we had seen in the *pousada* parking lot, driven by our gracious host and new friend, the captain. They stopped, of course, and the officer took command of the situation. (I could not have imagined a script that played out as true to form as events unfolded.)

It was Saturday evening, and prospects for finding a mechanic willing to repair the tires were vanishingly small. But our commanding officer took charge. He loaded the tires and my wife and children in his car and drove them into town to a hotel. I stayed with my vehicle and all our belongings. He somehow located a mechanic to repair the tires and drove them back out into the bush where I was waiting, by then, long after dark. He mobilized the men of the village to lift up the car by hand and replaced the wheel with the repaired tire. And we then drove, very slowly, into town for a late and very appreciative dinner together.

The positive side of official concerns about me could not have been clearer. They were keeping track of me to take care of us all. The hazards of driving an overloaded car rutted roads must have been obvious to everyone but us. The unaccounted-for young man who had moved into the house next to ours right after we arrived, we learned later, had been “transferred” away within days after we left. The commanding authority of the Portuguese army in Angola could be deployed to protect a dangerously naïve American researcher from himself as well as to protect occupying Portuguese from stealthy guerrilla fighters for independence from them.

The remainder of the drive to Luanda proceeded without further distraction. There, my priority was to clarify our visa status, as I had been gently advised. But I anticipated that recovering our passport would require considerable effort and frustration. The secrecy of a “secret police” like the PIDE extends to their visible presence, or rather invisibility. The public service section of their large and prominent headquarters building in Luanda was a room with a counter with a high frosted glass barrier hiding their personnel from the public, people like me who needed them to handle all manner of legal paperwork. All communication passed through a round hole at chest height, perhaps 10 cms. in diameter. I would have to lean down to speak through the hole and then turn my head to the side to position my ear to hear a response, sight unseen. At the bottom on the glass, on the surface of the counter, was another opening, wide enough for papers to be passed through it. The awkwardness of the arrange-

ment was designed to keep inquirers, already wary of bureaucratic lethargy, calculated or not, off balance. For me, facing this barrier and communicating in Portuguese, a language in which I was comfortable in conversation but not so much in formal settings, was all the more intimidating.

So I approached one of the holes in the glass with a wariness heightened by months of deceptive hospitality and calculated generosity. I spoke carefully through the opening to identify myself and state my business, “Boa tarde, sou Joseph Miller...” Before I could finish, the voice behind the frosted glass responded, “Sim, senhor, eram quatro passaportes americanos, não?” And our documents appeared through the slot at the bottom of the glass, with visas stamped in them. I have no idea whom, behind the glass, I may have thanked, or why I should have appreciated the entire elaborately staged performance that culminated there, in front of, and behind, the daunting screen. I may have been grateful mostly that I had gradually glimpsed at least some of the reality behind the illusions, enough, at least, to have finally recovered our passports.

We were welcome to remain in Luanda for as long as we had planned, another six months of productive research in the Arquivo Histórico, disturbed only by the day that a missionary somewhere in the interior rescued a goat from a seven-meter-long python by crushing the snake’s head with a sledge hammer. Thinking that the natural history section of the museum might want to exhibit the carcass he had put it in his Landrover and brought it to the staff, unloading this huge reptile in the driveway of the building to display its truly impressive size.

Except that he hadn’t managed to kill the snake. The first I heard about these developments was when one of the assistants in the archive ran into the reading room screaming “A cobra fugiu!” I gave up research for the rest of the day and went to join the staff searching for the massive beast, which – given its size – was not too difficult to find. But then someone had to restrain it, in order to approach close enough to finish the job of killing it. A huge 18th-century cannon, and a large palm tree were available as anchors for a hawser tightened

around the head and tail, and the snake by that point was too weak to break it. Even so, by evening, the body was still writhing.

The end of the story about Quela officials allowing us to leave we learned later when the captain who had saved us from a night in the bush in a broken-down car turned up in Luanda, on leave. We met, and without saying as much, he allowed me to deduce that a munitions caravan of MPLA guerrillas had been moving from their base in Zambia toward a pocket of comrades isolated in a region to the west of the Baixa. The military was preparing to attempt to eliminate them as they passed through. They didn't want witnesses to what they anticipated having to do, least of all Anglophones. I wasn't informed of whatever the military had eventually had to do.

And that was as close as I am aware that I came to the liberation struggle, although the tight control that the freedom fighters forced the Portuguese to maintain pervaded our months in the *mato*. My research was more productive than I could have planned, thanks to the guidance of the Kasanje "court historian" and his colleagues in the shadow polity that they had preserved, entirely unseen by the Portuguese, for more than two generations. I learned enough that I finally recognized the subject with which I should have started, by asking the last question I ever raised with one of my historian-collaborators: "But what about the witches?" He nearly dropped through the concrete floor of the room where we were talking. "How did you learn about *them*?" he stammered. And he ended the conversation, as I had finally reached level I had been looking for, and it was too sensitive a subject even to mention within earshot of the civil authorities, or the missionaries, or uninitiated Imbangala. Late-colonial Angola was a tangled morass of deceptions, on every side – except mine. And every time I had touched truth, someone fled.

As for the witches, I've been working on them ever since, and they were – and still are – everywhere.

3. In Brazil, you are renown due to your historical works, specially based on Portuguese Empire's sources; however, your first book, Kings and Kinsmen, is based on an intense ethnological research among Imbangala people, when you devised a methodology to reconstruct historical events out of oral sources and traditions and comparing them with written sources. Could tell us about this methodology and the relationship between Ethnology and History in your research?

I've gone into some detail about the ontological qualities of the oral traditions as the implementation "in the field" of my attempt to follow them out provoked the attentions of the PIDE. But I can elaborate here a bit more in relation to what you call "ethnology".

As you remind me, my mission in Angola was to confirm the "historicity" of the oral traditions by using the Portuguese documentation covering the same geographical areas in the same four centuries. The "good news" turned out to be that two peoples, Africans and Portuguese, who had lived there together, often interacting intensely, trading, fighting, intermarrying, sometimes in terms of formal diplomatic correspondence, recorded two entirely separate histories, unrelated to each other. Africans and Portuguese were living in parallel worlds, in the technical mathematical sense of lines that never intersect.

This compatible parallelism, as it could be called, is – of course – familiar to ethnographers, who have wryly ironic phrasings for it, in English, as a "working misunderstanding" or as "dialogues of the deaf". I call it the "universal 90/10 rule", by which – for example – stereotypes can't be refuted, because they are about 10% valid, but the problem is that they are taken as 90% true. "Working misunderstan-

dings” “work” because each party is able to perceive about 10% of the other’s world and then proceed innocently on the assumption that they have understood 90%. For books, the 10% is the cover; or, only 10% of an iceberg rises about the surface of the water. You see what I mean about its universality. The potential for trouble is enormous, as Angolans of all persuasions constantly lamented.

The same is true of oral traditions as historical sources. At first, to a historian trained to work with documents, they look like familiar written evidence from the past. Or – it might be better to say – at first a documentary historian will be able to notice only the similarities – the 10% of the actuality. These innocent historians don’t know what they don’t know about the rest. I required months of bafflement to finally recognize that Africans describe their political systems – the “kingdom” I had intended to study – in terms of the existential personal relationships that maintained it rather than in terms of the abstractions we use – “kingdom” and “state” and “empire”. When I asked about “o reino de Cassange” they responded with genealogies, “so-and-so married so-and-so and begat so-and-so”. It sounded like the book of Genesis. But the males in the genealogies were political authorities, and the females were the people (reproducing communities of kin, hence female in significance) who provided the holders of that authority.

And this political discourse was accurate, not a metaphor. The polity wasn’t a “kingdom” like those in early modern Europe but rather a network, a confederation, or a “composite” of the reproducing groups in it, as linked by the genealogies they recited. The genealogies were in fact the formal constitution of the polity and as vital to the political/collective identities of the people composing it as national citizenship is to us; we are defined in law by the nations in which we are citizens, and we live in a context defined more by law than by familiarity, at least publically. Bângala identities of this political, competitive, sort are literally vital, and Africans’ reliance on them, and intense sense of belonging in them, explains the sensitivities I described, including the fistfight in the tavern.

So the Africans were telling one, entirely coherent and adequately comprehensive, experienced reality of being in Angola between 1560 and 1960, and the Portuguese present in the same abstract space and time were living in another, parallel existence. Because they were parallel, not intersecting, people on either side didn't confront each other in a systematically conflictive way. As a result, no one on either side had to take account of much more than 10% of the other to proceed in their respective worlds, even interacting. These were NOT mental spaces comparable to the directly conflictive falsifications currently designated as "alternative facts".

For me, as a historian, this compatibility was an opportunity to triangulate from the two 10%'s acknowledged in each to infer the remaining 90% of the other. As it turned out, composite polities must maintain sequenced lists of the men whom they entrust with the positions constituting the polity, and particularly the central ones around which the networks revolve over time. Those constituting sequences are what ethnography, and historians, have called "king lists". Some have attempted (very crudely) to infer chronology from them, by assuming some average "reign length" and multiplying the positions of the "kings" in the list to guesstimate calendar dates for them. But the central figures are not "kings" in the sense of the unilateral personal power attributed to early-modern European monarchs. Rather, they are trustees of the legacies of their predecessors, who must be remembered, that is, introduced into the present, for the current trustee to be effective by embodying their predecessors' accumulated deeds and powers. And they are correspondingly constrained by the same precedents, what we see as the past.

As it turned out, the Portuguese documents had recorded most of the names in the oral traditions, and in the same sequence in which they were recited. That confirmed the deliberate historicity of the traditions, as I had been assigned to demonstrate. However, to do that I had to learn what aspects of them to read as referring to what sorts of events, or – more likely – processes. The traditions did not record individual perceptions of events, as a document might describe them, although they presented them as narratives of humanized

personae, like the names in genealogies referring to groups and political titles integrated into the polity. Rather, they were retrospective, spontaneous consensus interpretations of the significance of what people had experienced together, the lasting shared meanings of what people had done as lessons preserved to guide the lives of later tellers and their audiences.

I take the sense of your question about “ethnology”⁶ as asking about the ways in which anthropological insights, or models of human behavior, including what is seen as “culture”, enabled me to recognize this alternate compatible coherence. I can’t say how I recognized the African sensibilities, but the anthropology I had learned – for example, modeling of kinship – was only a starting point, though a vital one to alert me to several aspects of the Africans’ world with no direct counterparts in my own – or Portuguese – culture. So it gave me questions, but no answers. Nearly all of the several strains of anthropology, as developed by 1965 or so, were structural, thus static, timeless, without the ability to articulate change of any sort, and least of all the incremental initiatives and inadvertent outcomes that characterize change of a historical sort.

My dissertation and the subsequent monograph (*Kings and Kinsmen/Poder político e parentesco*) took only the first step, of integrating what had been modeled as mutually exclusive alternatives (“states” and what were somewhat redundantly and negatively characterized “stateless societies”): hence the “kings and kinsmen”, the same contrast but seen as interacting in the same political systems. But combining abstract “types” was only a preliminary step toward a more thoroughly historical epistemology, since it sequenced static models

6 In English “ethnology” refers to a kind of descriptive ethnography, focused on material culture and looking for patterns of it in space, characteristic of pre-WWI Germany – and surviving by 1960 mostly among a rather constrained circle of ethnologists in Portugal. “Ethnography” refers to interwar British and French participant-observing inherited from Malinowski, seeking social structures for the British, and mental structures for the French (including Lévi-Straussian structuralism). I used what I knew of all varieties of anthropology to alert me to possibilities, but I saw no significant compatibility between its emphasis on abstracted modeling and my interest in human-created processes of creating historical change.

or types rather than showing how preceding ones had generated their successors through an infinite number of incremental steps and where ideology (which is what “structures” or “institutions” are, in historical terms) fit into that process. The combination has now matured into the polities that I characterize as “composite”, and with much more intricate political and processual dynamics, and my interpretations of the data are resolutely historical.

History and ethnography, or anthropology, are different disciplines for good and substantial epistemological reasons. A historian of early Africa must know both well enough to keep them separate. Muddling them dulls the analytical cutting edges of both.

4. On the origins and even the existence of the “people” named Jaga, a historiographical controversy rose among you and other outstanding Africanist historians; recently, Mariana P. Candido critically approached the Jaga label regarding the “sobas” in Benguela region. How do you see this debate nowadays?

The debate over the “death” of the Jaga remains very much alive, often led by Brazilians, including the expatriot Cândido. She is exactly right to recognize the legal/political significance of the way later Portuguese authorities used the term “Jaga” to designate uncooperative African authorities in Angola – *sobas* – as unredeemable savages, and hence legitimate targets of a “just war” that would yield slaves. Her sensibility to the legal aspects of Angolan history is exemplary; the Portuguese, whatever their irregularities as slave raiders, always operated by the book, covering their tracks with legality, long before the Brazilians invented “para o inglês ver”. The law mattered to them because enslavements had to be legitimate in order to establish clear title to property in human beings, a title that they could

convey securely when they sold the captives – which, of course, was the whole purpose.

But Cândido is looking at the eighteenth century and doesn't go back to the archetypal "Jaga" alleged to have attacked Kongo in 1568. They were the ones I attempted to argue had never existed in any form resembling the savage cannibals that were portrayed as being. Instead, I suspected a São Tomista-Kongo (the Catholic regime that they had restored two years later) conspiracy to cover up their own intrusive, and illegal, placing of a collaborator on the throne of what was legally a "kingdom" equal to the monarchies of Europe. Kongo was also supposed to have been a triumph of Catholicism, that is a polity that the Tomista Portuguese should have respected rather than subordinated to their primary interests in slaving. One could not legally enslave a fellow Catholic, but many Kongo counted as Catholics were being enslaved.

John Thornton, of course, attempted to breathe life back into these "cannibal Jaga" by treating them as an "ethnic group" invading from somewhere in the interior, and Anne Hilton made a different case to reach the same conclusion. However, a Belgian missionary-scholar, François Bontinck, joined in the ongoing debate on my side. I had declared a "requiem for the Jaga"; Thornton declared a "resurrection"; I responded with a reflection on death ("Thanatopsis"). Hilton contented herself with an unspecified "reconsideration", but Bontinck took up the march of allusions to mortality by publishing "une mausolée pour les Jaga". However, the "Jaga" were far from dead in the minds of historians.

But there were other confusions, owing to the Portuguese strategic generalization of the "Jaga" label, which they had first applied to similarly warlike and invincibly destructive raiders in the vicinity of Luanda, thirty years later and hundreds of kilometers to the south. An extraordinary document attributed to an English sailor who had lived with one of these bands of raiders reported that they called themselves "Imbangala". One of these groups later became the progenitors of the "Bângalas" of Kasanje in the Baixa.

But Kasanje oral traditions recorded by Henrique Augusto Dias de Carvalho in the 1880s had attributed the Kasanje polity elsewhere, to an exiled “prince” from the Lunda “empire” far to the east. In search of a date for the “foundation of a Lunda empire” imagined entirely in euro-centric terms of a “monarchy”, Jan Vansina and David Birmingham had attempted to use the known date of Imbangala near the coast to infer a date for an event in Lunda that could never have happened (since African composite polities consolidate gradually and have no “founding date” as such). I had extended this debate in its original terms by offering my understanding of the oral traditions thought to have contributed to it. But none of these Imbangala or Lunda had any historical connection with whatever had happened in 1568 in Kongo, other than Portuguese calculated labeling of enemies as enslaveable. Fábio Baqueiro Figueiredo has written a very clear and comprehensive summary of these debates about dates, in Portuguese.

Several brazilians have contributed thoughtful reflections on the other “Jaga”, of Kongo by looking for what additional hints the same primary sources might yield, under further intensive scrutiny, though not always clearly distinguishing the 1568 “Jaga” from the later, quite separate Imbangala and Kasanje (related to Lunda, or not),. These efforts have yielded little new insight, continuing to justify one or the other of the two initial positions, Miller’s and Thornton’s, and tending to favor Thornton’s mysterious horde of savage invaders over my straightforward proposal of the possibility of an internal revolt that I had attributed to the failures of Catholic conversion to stop the wave of slaving violence washing over Kongo. In general, few scholars have taken the slave raiding growing throughout the region seriously as the truly traumatizing shock it must have been, one capable of producing an interpretation of slavers as “cannibal” consumers of people. This exact image is reported over and again from every part of Africa.

But the “smoking gun” has now been discovered, and in the least likely of places. A Portuguese historian working on Macau, Paulo Jorge de Sousa Pinto, discovered a petition from a priest, candidate

for appointment as bishop of Malaca in 1588. The priest, by way of supporting his candidacy for high ecclesiastical office with a recital of his many prior services to the Crown, cited his presence accompanying the army that had rescued the Catholic regime in Kongo from “quase de sessenta mil alevantados no dittos reinos do Congo”: an eyewitness explicitly describing the disorders as a domestic uprising. His petition had escaped the notice of Africanists because it was not in Lisbon or Rome but in Madrid, by then the seat of the Hapsburg “dual monarchy” that had succeeded Lisbon, and in files not from Africa but from southeastern Asia. A new book by Jared Staller, *Converging on Cannibals*, will rewrite the history of Kongo as a political composite rather than a European-model Catholic monarchy and detail both the political dissension in Kongo and the subsequent creation of the myth of “Jaga” as legally serviceable “cannibals”.

Oddly, Pinto, who is not an Africanist, did not notice what this unique source actually said, as he was reviewing the complex historiography very thoroughly to support Thornton. Staller understood what the document in fact said and called it to my attention, knowing that I would be pleased to find this final nail from Malacca driven straight into the heart of the vampire myth of the “cannibal Jaga”. Nearly forty years later, the “Jaga” have finally been laid to rest. The question seems closed, but historians – and not least Brazilians – being as creative as they are, may well raise the shades of the “Jaga” yet again, and I will follow their wispy trail with enduring interest.

5. In *Kings and Kinsmen*, you tackled the origins of the Angolans (precisely Mbundu) kingdoms from the perspective of institutional change, showing how the rising of those kingdoms was a product of institutional transformations and adaptations taking place among the several peoples living in West-Central Africa, which went beyond kinship. More recently, in *The Problem of Slavery as History*, you cast doubt on the usefulness, for African History, of the very concept of kingdoms and empires and strongly underscore slaving in the process of political integration. May we say that your reasoning on this subject underwent some transformation?

The direct answer is “absolutely yes”, as I’ve been detailing in my responses to your previous questions.

First, my compliments on the very acute statement of the logic of *Kings and Kinsmen*. But I regard my further thinking not as a “transformation”, which implies a holistic substitution of one whole thing for another. In this case, that could be replacing abstract structures – such as “kingdoms” and “lineages” – for the historical processes that I have come to understand as much more realistic ways of understanding change. Since *K&K* was framed in terms of those “institutions”, which are static, the only way to trace change is to contrast them and sequence them. You gave me at least some credit for “adaptations”, presumably attributed to the Mbundu who would have made them in the sixteenth century, but I don’t think that I gave enough attention to this elemental aspect of accounting for the changes that people make in their lives. I appreciate the benefit of the doubt.

But the same understanding of change as historical process – that is, minimal accessible incremental adaptations of what people have inherited from the past to new conditions beyond their control – applies to what I did in *K&K*, I had anthropological structures (“kingdoms:” and “lineages”) to work with; in fact, of course, these political and kinship aspects of life were only a portion of the frameworks of relationships that those Mbundu had inherited from perhaps thirty generations of their ancestors’ inventive responses to a millennium

of meeting historical challenges – less than half, perhaps only 10%. So I had reduced the full richness of their lives to an overly rational schema. And I didn't replace what I had derived from the literature but only combined bits of it in a new way. It was, thus, an incremental step, one that was as much a product of the past as it was also a step toward the future.

But it was a move in the right direction, that is, "right" for me, as I was moving beyond the structuralism of the "social history" of the 1960s and the anthropological models that had enabled the first generation of historians working on Africa to infer patterns (as modeled, in the abstract, in general terms) from the very scattered evidence for the unwritten past that we had at the time. The models gave us narrow beams of light into the obscurity. They were the starting point available to us.

Since then, everything I have done has strained in the direction of a more historical epistemology, an emphasis on change as the product of human actions motivated and enabled by the specific contexts in which people were living. "Kingdoms", abstract entities that could – in the discourse of the 1960s – be "founded" or "formed" in a vacuum, have disappeared from my understanding, and gradual historical processes, with people working with whatever means they have at hand (including their own, limiting pasts) to meet half-perceived new challenges. That is "half-perceived" in a precise sense, since the novelty can be perceived only in aspects of it that maintain familiar features, recognizable to people as products of their own pasts. The other half is not perceptible, or will be distorted in terms of the familiar. Life itself is a "working misunderstanding" of our emergent presents based on our obsolescent and selectively remembered pasts.

If you understand history in this existential way, it's quite amazing that we ever make any progress at all. But, of course, progress happens less by design than by accident. Outcomes are seldom what we intended, or seldom happen the way anyone intended them. If a historical understanding of change teaches one lesson about life, it would be humility, with a strong overtone of irony.

If we are to contrast African political dynamics – not static “kingdoms” – with background social dynamics, then we must start with people living in small, intimate, face-to-face communities, in which oral (and visual, tactile) communications are richly sufficient. They operate in terms of familiarity and are dedicated to reproducing themselves, significantly through marriage relationships managed through social categories created by kinship rules, “marriageable” and “not-marriageable”. When these reproductive strategies succeed in increasing their numbers, they lead to shortages of local resources, which means that the reproducing communities have to intensify their utilization of their own terrain and also engage more distant partners in trade. That is, partners beyond the range of the in-marrying neighborhoods, who are “strangers” in these terms.

But strangers are a problem because they are anomalous, uncontrollable from the existing contexts of familiarity and community. So people extend their categories of human relationships to apply, in defined aspects, to foreigners whom trade relations bring them into direct contact, episodically. When these new trading relationships, managed in terms of the old categories of kin, succeed, they bring new problems of competition and potential conflict among stranger-traders far from home and supportive kin. (I am simplifying by omitting clients, in-laws, and other human resources developed over time.) They cannot imagine relationships outside these frameworks of obligated kin, and so remote groups, people who do not interact on a face-to-face basis routinely, work out analogous terms through which they can collaborate, at a distance, even in the absence of the others, and avoid misunderstandings, betrayals, and conflict. A few of the peoples in these situations work out viable understandings, supplementing their inherited kin-based communities but not replacing them, since the objective is not to create an overarching political system but simply to protect wherever they were coming from.

If they work out an arrangement that succeeds, its advantages then lead others to join voluntarily, and some of them will be welcomed for their unique contributions to the emerging collective. Some of these ad hoc understandings fade out with the passing of the gene-

ration that created them, for purposes of the moment, which may not last. But ongoing propitious circumstances may lead their successors to extend familiar rules of inheritance in kin-based communities to regulate access to positions in each group designated to represent it to its counterparts. This political aspect of their lives – distinguished from “social” as the abstract rules of comity that enable people who are otherwise strangers to engage one another productively, or successfully – thus emerges as an extension of the social/familiar aspects of their lives that they want to preserve. You will have noted how an incipient significant change, addition of an operative – even, on occasion, a significant – “political” aspect to lives lived in villages and neighborhoods, emerged contradictorily from conservative efforts to preserve a status quo of familiarity in a new context of crowding and incipient conflicts among strangers, that is, a failure created by their own successes of the past. Nothing (in history) fails like success!

Any number of circumstances at that point can then provoke the neighborhoods and villages to intensify the limited arena of collaboration they have created: a drought provoking conflicts that others may unite to contain ... or the emergence of an ambitious and charismatic individual who takes advantage of the incipient network to promote himself as conciliator and arbiter in a prolonged period of conflict...or some external threat to all...or an opportunity to control, and tax, flows of trade goods from regions external to the incipient political collectivity.

Such extensions leave residues of precedents and processes that later people invoke to integrate and regulate further the lineage components, or nodes of the network, increasingly around a shared central node, declared independent of any of them and thus a neutral figure capable of arbitrating disputes among them. Not every expanded neighborhood found it necessary to take this further step, and few welcomed its costs. An authority independent of any of them was anomalous, by definition uncontrollable, hence potentially a threat to the pasts they meant to preserve, a step taken only under duress, as the lesser of two evils. We might think of central Africa, as of the tenth or eleventh century CE, as having many of these inchoate poli-

tical systems, but circumstances sufficiently compelling to take this drastic step toward consolidating a polity might converge only rarely. Parts of western Africa had encountered these compelling moments several centuries earlier. But whenever the moments were opportune, people would have ended up having consolidated enduring political composites out of earlier (kin-based) contexts. These polities would be coordinated confederations, not unified “kingdoms”.

There is a lot more to be said about the perfectly sensible solutions to recurring challenges that people in Africa resorted to and that, one way or another, left their heirs with political arrangements capable of mobilizing imposing numbers. To regularize the first phase of *ad hoc* understandings, they would create oral constitutions, which – over time – their successors, who had not been present at the creation or known the people who created them, synthesized in the memorable verbal formulations that we think of as “oral traditions”. These retrospective collective consensuses often attributed the autonomous powers conceded to the central figures – always carefully limited, never absolute or comprehending – to a source external to the composite. Representing them as outsiders – wandering hunters endowed with strong powers over the wilderness, or scions of a political authority far enough away to appear invulnerable – declared their independence of the component factions of the polity.

I’ll stop there, although there’s much more to be said, other than to highlight the incoherence of the usual account of creation by conquest. “Conquest” was neither sensible nor logistically possible. The logic of “conquest” also presumes the powerful military polity that it is supposed to create. African polities were not significantly militarized, other than in the cavalry/horse-based exceptions of sudanic latitudes. I’ve emphasized the slow historical processes, essentially conservative, that leave later generations in much more politicized contexts than the inadvertent creators had known. The political innovations always incorporated the social pasts from which they were generated. Their historical world – like ours – was constantly in motion, driven by the constantly renewed dialectical tensions of life. The only “structures” in it were the ideological “constitutions”, alwa-

ys retrospective and schematized models of acceptable, consensual, behavior.

One final methodological point: without literacy, how can historians access enough evidence of these processes to posit them as “history”, a knowable past? Historical linguistics can reconstruct the words that people invented, or adapted, to talk about what they were doing, which is direct testimony from the past about generalized processes of the same logical order of what I’ve said here. The specific individual and events are lost within three generations or so, that is, with the deaths of the eyewitnesses. But eye-witnesses seldom perceive significance, only partial impressions of recognizable details.

The second source is modern ethnography. If you think of the routinized behaviors that anthropologists call “culture” not in their structural terms but rather as accumulations of innovations and adaptations inherited from the generations of the past, you open the possibility of distinguishing particular components added at particular moments in the past, as reflections of responses to historical moments that can be reconstructed in general terms, then arraying them according to that sequence of contexts, and using these contextualized changes to infer additional aspects of their past times not evident in language. The past lives on in the present, in the form of its residues in consensual behaviors.

But that’s surely enough. If you know *Kings and Kinsmen*, you can see how my present stress on historical processes fills in the gap between institutionalized moments left by its formulation of the key change as a structural “transformation”. I suppose that I’ve been part of the “post-structural turn”, in a historical voice rather than a literary or philosophical one.

6. *Way of Death* is your most renowned work, in which you clearly try to construe the whole of Angola's slave trade and its relationship with capitalism, and several societies that interplayed through the business of selling people, following the best tradition of the *Annales*' "total history". How would you depict the intellectual context when you wrote the book? And why that "Atlantic" approach, if we may say so, once you former works focused mainly on the African people?

Another intelligent, relevant, probing question; thank you.

The historiography when I began the book needs to be considered in three sections. One was, of course, African history. It was still mired in "kingdoms" and other unappealing "institutions" and was being drawn strongly toward recent times through Terence O. Ranger's overwhelmingly popular studies of "resistance" to colonial rule. The field would move on into the colonial era, 90%, as its archives opened up, and graduate students flocked into them. Colonial Africa was also less demanding than earlier African history, which Wyatt MacGaffey once aptly called "the decathlon of the social sciences".

The second is Angolan historiography, which was flourishing under the prodigiously able scholarship of Beatrix Heintze and John Thornton, as well as Jill R. Dias in Lisbon. But Thornton and Heintze worked mostly on the sixteenth and seventeenth centuries, and followed the documents quite closely. Dias was starting to develop an African history of the nineteenth century. No one was working on the eighteenth century, and so *Way of Death* began to discern the outlines of that "middle era" from 1730 until about 1830. It was also the first attempt to integrate the themes of African history in the broader Atlantic context. Thornton and Linda Heywood, in particular, have subsequently elaborated that framework, though using the concept of "Atlantic creoles" that I do not find helpful in understanding Africa – always my "bottom line". With "Atlantic creoles" we see Africans mostly as they could be argued to have resembled Europeans; another example of the deceptive appeal of the "90/10 rule".

The third was the first stirrings of what later became Atlantic history, and that was Philip Curtin's *Atlantic Slave Trade: A Census* (1969), which had set the quantitative tone for at least a generation of imitative studies aimed at mining the archives of Europe (less so Brazil, some contributions from Angola) for data to document a trade that had always been assumed to lie beyond the reach of scholarship, conducted surreptitiously and leaving no paper trail for historians to recover. How wrong that assumption was, as we know fifty years and 36,000 documented voyages later! No one had ever looked – or perhaps wanted to look at a shameful episode in a still-progressive historiographical context.

So the tone of that first round of counting the people exported from Africa was not well historicized. It was about finding numbers and then punching them into computer cards to feed into the then-only-initial versions of university main-frames running on huge reels of magnetic tape. The published versions of these studies consisted of tables reproducing the numbers that the computers generated and then describing them in prose; that was pure redundancy, but it was probably necessary, since not all historians fully understood the tables that the computers produced. It was a rather euphoric “cliometric” phase of data and quantification. These “cliometricians” seldom contextualized either the sources from which they extracted the numbers or the historical contexts that had produced the sources. It was a failing exercise as history, since it did not significantly (analytically) explain what produced the outcomes it described. Looking at evidence from the past without context is not history.

Because of my graduate degree in business administration, I could read the numbers well enough to wonder about the missing contexts. And the four (could be five) embedded contexts of *Way of Death* were the result. Of course, the outcome surprised me when I realized that I had replaced an economic history mode of quantification with a business history emphasis on finance and strategy, thus the motivations and the means. When I left business and sank into oral traditions in the Baixa de Cassange, I thought that, if I had gone any further away, I would have been coming back again. But there

I was, a business historian of Africa. So far as I know, I'm the only historian of Africa with an MBA. You can run from your own past, but you can't escape it. And African historiography acknowledges that; progressive history would like to deny it.

And you in fact answer your own question. *Annales* "total history" means understanding people and specific events in their full historical contexts. And the "Atlantic approach" in *Way of Death* simply contextualized a primary aspect of history in western central Africa – that is slaving – out to the scale of the Portuguese Atlantic. People in the center of central Africa were reacting to the consequences of decisions being made in Lisbon. I couldn't understand what Africans were doing unless I could appreciate the full context in which they were living. So I didn't leave African history; in fact, I was digging in deeper.

Way of Death has – as you're well aware – four parts: the Africans' inherited worlds, how they drew on those to engage European traders (as intentional buyers of imported goods hoping to acquire people rather than losing some as they unintentionally ended up doing), the sets of traders, obligated by the debt they took on to acquire the goods they imported, and then the large merchants who financed all these steps in the process, avoiding direct ownership of the fragile human beings who died from their sufferings in numbers that motivated the financiers, whose wealth put them in control of everyone else, to devise a legal framework that insulated them from "o risco dos escravos", as they quite pointedly acknowledged.

Each part is the context for the preceding one: starting with a boy captured on the banks of the Zambezi River, caught up in the political and economic of African authorities collaborating with bush traders bringing the imported goods to the heart of central Africa. The bush traders (*funantes*, *aviados*) had borrowed the goods they carried on credit from importers in Luanda and Benguela, debts that they paid off with the people they collected, and these traders on the coast thus formed a strongly motivating, as well as enabling, context for them. The coastal merchants, in turn, were working in a southern-Atlantic commercial context based operationally in Brazil, where

they obtained imports, again on credit, and paid for them with captive Africans. And merchants in Brazil operated in an imperial framework based in Lisbon and focused on gold from Minas Gerais.

Everything that anybody did with regard to the slave trade was embedded in these concentric spheres of context. I couldn't understand, say, the polity in Kasanje, which had been my starting point back in Quela and the Baixa, without probing the *praça do comércio* in Lisbon. To understand the full history (*l'histoire totale*) of Kasanje, to do Africans' history properly, I had to include the Atlantic. So it wasn't the change that you imply. I thought of it as chapter 2 of the history of Kasanje that Vansina had sent me off to Angola to discover; *Kings and Kinsmen* was chapter 1 of the planned dissertation, in effect to set the local context from which Kasanje had emerged. I might just now, thirty years later, be starting to sketch elements of chapter 3 – which would be to trace the Kasanje specifics of the general processes that I designated as “composite polities” in answering your preceding question. It was not a “kingdom” at all, although the Portuguese tried to treat its central figure as “their Jaga”. But you already know that that, in turn, was yet another story, a potential chapter 5.

Let's just say that my planned dissertation is still “in progress”, with your questions evoking my current thinking on several of what would be its eventual components. Of course, *Way of Death* itself was incomplete, as you know better than I. Lisbon was working the capital markets in London, as was most of the non-French Atlantic by the eighteenth century. With 750 pages needed to reach Lisbon, I didn't have the energy to expand the context to its final pan-Atlantic sphere. The book should have been subtitled “From the Banks of the Zambezi to the Bank of England”. Fortunately, the two of you are now embarked on writing that final section, and I hope it takes you many books and articles to follow it out – where you will eventually end up sailing the Indian Ocean – a looming Part Six.

I am delighted also that you recognize what I did as “Atlantic history”, since I finished writing the book in 1985, and the rest of the profession didn't engage the limitations of the discipline's national or nation-state framework for another twenty years. And twenty

years after consolidation of “Atlantic history” as a field, most scholars in the field still retain derivatives of the nation-state framework, which – by definition – remains euro-centric. So we recognize that “empires” are “entangled”, or that people moved “trans-nationally” or that people in the Americas and in Africa had histories of their own, at least insofar as they “resisted” Europeans. But that’s only a first step, of the usual historical – and also historiographical – sort. It still needs to be de-centered, or rather, multi-centered with the people on all the shores of the Atlantic given equal initiative. Then we will see that Europe didn’t “expand” but instead leveraged local and regional historical dynamics in Africa and the Americas to serve purposes of their own, and that those purposes were only a part, sometimes very small, of the full regional and local historical dynamics.

7. How do you see the reception of *Way of Death*, particularly in Brazil?

Well, that’s a question I should be asking you.

I have been extremely grateful for what seems to have been significant appreciation, in spite of the challenges of struggling through a very long text in English. My initial welcome was a bit overwhelming. I had arrived in Rio to attend an international meeting, a bit late, and Ambassador Alberto da Costa e Silva, a very distinguished historian, was giving the opening plenary lecture, praising someone’s recent book on the slave trade. As I found a seat, I realized that the ambassador was talking about *Way of Death*, and very generously. After he had finished, I introduced myself and became acquainted with not only a fine scholar but also a gracious and refined gentleman.

Ambassador Costa e Silva has remained a firm advocate on behalf of the book. More than ten years ago he arranged for a translation into Portuguese, which unfortunately remains unpublished, although efforts continue to make it available. Quite a number of Brazilian

historians seem to have read it, I suspect because of its integration of African history into themes of the history of Brazil thirty years ago.

Elsewhere, the original English edition remains in print, and students and scholars – not all of them Africanists – seem to continue to refer to it in complementary terms. But it does not seem to have provided a model for others to follow in integrating the history of Africans into Atlantic and world-historical narratives. Two very strong new fields – the “second slavery” approach of Dale Tomich and his associates at the Universidade de Sao Paulo – and the “new history of capitalism” in the United States – have recognized the historical significance of capital that is a core theme of *Way of Death*. I like to remark that the “second slavery” group uses industrial capital to explain slavery in Cuba and Brazil, and the “new history of capital” people use slavery to explain capitalism in the United States. But both approaches limit themselves to nineteenth-century processes, virtually defining themselves around that century’s phase in the global history of capital, and they seem not to look to my book on the eighteenth century for inspiration.

However, I am making progress in showing the significance of classifying human beings as personal property as the collateral for the loans needed to finance the start-up phases of investments throughout the Atlantic, from the fifteenth century onwards. How else did planers finance the rapid growth in coffee production in the Paraíba Valley? I hope to provide the background to these nineteenth-century debates and to do so in an epistemologically historical mode, showing the incremental extensions of former stages at the threshold of each new one. I first published this accent on the Atlantic as an incremental historical process in Portuguese in Brazil in *Áfro-Ásia*, now reprinted in a *colectânea* celebrating the fiftieth year of that fine journal’s publication.

8. In *Way of Death*, the slave trade's dynamics is related to peasant demography, resembling a "neo-malthusian" thesis (which can be linked to the French historiography, as Ladurie's works, for instance) and to the slaving frontier thesis. Recently, other historian's works problematize or criticize those two arguments – even though they have your work as a milestone – ideas which we deem as the cornerstone of your interpretation. What can you tell us about these critics? Is there any point of your argument that you see as subject to revision?

First, let me say how impressed I have been with the contributions to earlier Angolan history that younger scholars here in Brazil have been making. They are producing a torrent of first-rate scholarship, and they linger less and less over the conventional questions centering on the Portuguese and more and more probe the lived worlds of the Africans. I compile a bibliography of this scholarship, insofar as I can keep up with it, and it contains dozens of names and hundreds of titles. I hope that all this new historiography will critique the older works, including mine. One makes one's contribution in one's own time, and then others should explore the limitations inherent in everyone's work and move beyond.

The "other historians" you mention as criticizing are young Brazilians, though trained in North America and now making careers here. They are fine scholars with distinguished careers ahead of them, but their reading of the "slaving frontier" is somewhat partial, understandably so, I think. They focus on the extreme violence, organized raiding and warfare that are conventional emphases in studies of the origins of the war captives, some of whom were eventually sold as slaves. War is what historians seem to understand, and particularly with regard to producing slaves in Africa. It is not intuitive for most to realize, as I've argued, that Africans enslaved one another, in many ways for many reasons, beyond the still-looming shadow of what are called *as guerras angolanas*.

Consequently, I worry a bit that my principal arguments about the origins of the people sold from Angola as slaves may elude readers inclined to focus selectively on the violence. My purpose was to account for processes far more complex, and locally rooted, than “kingdoms” and “warfare”, which, for me, carry unfortunate overtones of the pejorative stereotype of brutal savages in Africa, constantly locked in “tribal conflict”. The book attempts to acknowledge the extreme violence of slaving but to contextualize it by locating it as a specific, initial moment, localized in spaces that moved inland over the centuries between 1580 and 1850. That is to say, the violence was historical, not inherent in some mythical violent African character.

One of my critics uses her very thorough and creative research on the area inland from Benguela, the smaller, more southerly, of the two ports in Angola through which the slaves were sent off on the middle passage, to note continuing incidents of violence near the coast at a time when the “slaving frontier” had moved well inland. She claims that these ongoing disruptions nearer the coast at least severely qualify my hypothesis. I find this logic rather tenuous, as my argument was that the extreme violence characterized the opening phases of slaving in a broad sequence of regions, farther and farther inland, over three hundred years. I did not say that violence disappeared in its wake, but rather that the uses of force were of a different character, less chaotic, more organized, more systematic, as slaving became a way of life for the survivors. And she finds exactly that sort of violence.

I also emphasized that the “slaving frontier” appeared particularly clearly in the Luanda hinterland, where most of the Atlantic capital was being invested. That financing supported strategies behind the “slaving frontier” based less on violence and more on debt in the form of trade goods sold on credit, with ordinary people being enticed to borrow more than they could afford, and the inevitable defaulters, and their entire families, seized and enslaved to pay what they owed. (Several scholars have documented the indebtedness in detail, with records of the so-called *Tribunal dos Mucanos*, a colonial judge where subjects of the Portuguese could plead the injustice of their enslave-

ment, often for debts.) That's a low-level form of violence nearer the coast that grew, initiated by commercial agents rather than by warlords.

I also contrasted the hinterland of Benguela with this Luanda shift toward forced foreclosures on debts, though tentatively, as I had not had the advantage of the fine research that my critic has subsequently done, as relying less on indebtedness and more on continuing violence. Theft and plunder, in the form of war, is always cheaper than investment in creating a commercial economy. I understand that the financial logic of this argument may not be as clear as war to an Africanist trained on the cliché-d logic of prisoners of wars and that she may not have taken this level of the argument fully into account. I see her research as confirming what I had only hypothesized and, respecting her abilities as I do, I would welcome a more refined critique of the thesis as I proposed it.

The second debate depends more on interpreting the detailed returns from the first Portuguese census counts taken in the areas around Luanda that they controlled by the mid-nineteenth century. These counts enumerated the slaves living there individually, by name and "origin", by analogy with Portuguese civil standing defined by the parish of one's birth, with baptismal records previously being the equivalent of modern birth certificates. Most of the people listed in these counts as slaves showed "origins" within Portuguese territories. The author asserts, rather reductively, that the "slaving frontier" hypothesis would predict that these counts should show high numbers of people from the zones of violence, which by that late date were located far in the very heart of the continent, beyond the eastern border of modern Angola, and far beyond the areas the Portuguese then controlled.

This historian was not trained primarily by an Africanist and may not be sensitive to the analytical focus of my book on the African aspects of the historical processes that had brought people to wherever they were counted. It's a complex book, with several themes interwoven, and it would be possible to read it in terms of two or three of its levels and not take full account of one or two others. Again, I see

him emphasizing the remote violence to the point of neglecting the financial means of generating slaves nearer the coast through seizures for indebtedness. His interpretation also ignores the numbers of people enslaved who were kept, particularly in the Portuguese areas, to replace others lost to slaving or to mobilize to participate in the trade. That is, the Luanda hinterland had probably been repopulated by slaving several times over since the first wars near Luanda in the 1580s. The population was in constant, forced flux, and people there had probably kept more captives for themselves than they had sold to the Portuguese.

The people counted as slaves by the 1850s could well have included mostly descendants of captives in earlier generations from elsewhere, including the “slaving frontier” zones. We know that the captives coming from the “slaving zone” itself by the 1850s were continuing this pattern, being retained in large numbers by the Chokwe east of the Kwango River, by the Ovimbundu on the central highlands, and elsewhere beyond the reach of the Portuguese officials taking that census count. I think that the observed geography of the “origins” of the people classed in the census as “slaves” raises more questions than it answers, and that finding those answers will depend on greater contextualization of the resulting lists of names that we have. I look forward to further critiques of these very valuable documents.

As for Malthus, I interpret your question to refer to one more of my efforts to contextualize population numbers in Africa, which Africanists have debated for years, largely without setting them in demographically relevant contexts. You’re referring to an article I wrote, and to a parallel essay by the late Jill R. Dias, highlighting constant references in the documentation for Angola to “drought, disease, and famine” and to the political and economic sensitivity of feeding the population of the city of Luanda. These Malthusian concerns also included provisioning the thousands of starving captives being driven to the ships in its bay. It was a major preoccupation of everyone there, with smallpox epidemics originating in the wake of droughts in Angola being carried by afflicted cargoes of slaves to touch off epidemics in Brazil. But this basic fact – “Malthusian” in a loose sense, I

suppose – was barely acknowledged in histories devoted to the usual wars and governors and abuses of the slave trade.

I simply compiled a list sequencing these events, which showed that the wars often coincided with droughts and that the smallpox epidemics followed them. Once again, timing and historical processes showed clear patterns in events that otherwise seemed merely random or “natural”. The final confirmation of the importance of the climate dimension of African history came from reports of how various communities of Africans had emphasized the same facts hard of their lives. Africans, to their credit, had managed to maximize the numbers they could support with the resources optimally available to them. They had achieved full occupation of their lands. Disruptions – most often failures of the rains in Africa’s tropical climate regime–forced significant adjustments and, in extreme cases (which have recurred at least one each century for a very long while), collapse. I was thinking more in terms of climate history than Malthusian dynamics, but citation of them is accurate.

I was thinking in terms of climate at the time because the general historiography of Africa had begun to take account of these historical reactions to changed climates as the severe droughts in the western African sahel in the early 1970s focused attention on environmental instability. Others were working with major century-long tendencies toward wetter and drier regimes, mostly in western Africa. So, on the one hand, I was only extending their very important insights to the unusually clear and consistent Portuguese documentation for Angola, while on the other I was focusing in on short- and immediate-term changes that influenced specific events, rather than long-term general patterns. Clearly established climate changes over six or seven millennia (and more) are now basic frameworks for understanding the famous “Bantu migrations” in central Africa, and much else.

In a political economy of people, where numbers both constitute wealth and enable political power, everyone sensibly tries to maximize the population they can mobilize. The resulting political and economic tendencies toward demographic growth may generate a special historical case of Malthus’ general model. Progressive opti-

mists, or optimistic progressives, have never accepted this implicit critique of their faith in the boundless possibilities of capitalism to improve the lives of all, sooner or later. It's the basis of the present depletion and pollution of the world's resources: limitless wealth, regardless of context. For Africa, this issue centers on the consequences for the continent of the loss of 15-18 million people to all of the export trades in slaves – Atlantic, trans-Saharan, and Indian Ocean. By assuming abstract rates of growth of potential populations, one can count every person exported as a demographic cost, inhibiting market growth and economic development. So the famous “underdevelopment” theses attributing modern Africa's lagging economic development to the trades in slaves in fact rests on the assumptions of classical liberalism.

My emphasis on the actual historical contexts of realistically limited resources and competitive human ambitions doesn't lessen the tragedy of slaving for the enslaved, but it recognizes that African populations have oscillated, periodically decreasing and then returning to hopeful periods of increase, which – carried to extremes – made the larger populations they created vulnerable to the next collapse. Slavers preyed on the unfortunate scattered, vulnerable refugees of these crises, creating more captives than the Europeans could otherwise have provoked. To some extent, or phrased more historically, changing climates and human ambition, or politics, contributed in specific ways to the large numbers of survivors sent off into slavery.

9. Back to the relationship between capitalism and slavery, there have been relevant historiographical controversies since *Way of Death* was issued. How do you see this debate, which seems unavoidable since Eric Williams' seminal work?

I have outlined several phases in these important ongoing debates as I've responded to preceding questions. William's brilliant insight was a historical one, contextualizing the process of industrialization in Britain within its persistently created imperial context, enabled as an active contributor rather than left a passive object of British aggression. He also overcame the prevailing conceptual isolation of evolutionarily opposed abstract "modes" or "models" or "cultures" that was inherent in both liberal-progressive and Marxist schemes of global history. Capitalism and slavery were not logically opposed and thus somehow unrelated. He recognized the relevant historical question: creating change is expensive. Someone paid for British investment in industrial production. He also recognized that "modern Europe" had emerged in the context of a global political economy, the framework that Emmanuel Wallerstein eventually filled out with his sociology (not history) of a "modern world system" (which, alas, explicitly excluded Africa).

Economists have refuted a distortingly narrow calculation of the "profits" needed to finance industrialization, as the decontextualized accounting returns from the ships that carried the slaves. But other historians have begun to trace out the multiple spheres of context from which slaving drew particular kinds of resources that produced a catalytic combination in Europe – and not elsewhere. *Way of Death* worked out a set of contexts, of course, but in a way that focused on the slaves who died rather than on financial returns to Europe. Rather than review the large and lively field of further and fuller calculations, let me just restate the argument in the book in a way that focuses on how Europe grew and others lagged, particularly Africa.

The argument starts by recognizing that monetized liberal "market" economics are not universal in world history. Africans, for

example, did not circulate currencies but instead circulated people, and claims on them. Slaves were one, relatively irregular, form of moving and aggregating or obligating people. “Dependency” does no analytical work at all, since everyone sought as many patrons as they could serve, in return for protection and other favors. In fact, most parts of the world before the eighteenth century – including Europe – did not have the commercial (banking, and other) resources to invest in the very expensive project of monetizing their economies. Like Africans, they mobilized people– family first, also clients – rather than drawing on monetary wealth when they needed to get something done. Modern “economics” wasn’t a discovery of long-standing human “market” behaviors when it was invented at the end of the 1600s but rather a timely recognition of entirely new kinds of human interactions that needed fresh explanation. The new societies consisted of the autonomous, self-interested, rationally optimizing “individuals” formulated philosophically by Des Cartes, and these in turn were products of increasingly commercialized relationships among strangers related to one another only through material exchanges. Marx said as much, of course.

Trade had long existed, but it was thought of as marginal to ontologies centered on people. What changed was Europe’s integration of an Atlantic economy and the unprecedentedly large quantities of African gold and American silver that it channeled back to Europe – as well as into the already commercially integrated Islamic and Asian worlds. Enslaved Africanis, as I’ve said, were the collateral that attracted the credit that concentrated the bullion in Europe. The English financial revolution at the end of seventeenth century multiplied this growing liquidity many times over, in effect financing people’s abilities to support themselves through market – not family – transactions, as “self-supporting “individuals. A historical accounting for this process would follow the key moments of specific incremental changes, in terms of margins rather than aggregates

and averages, as Robin Blackburn does elegantly in *A construção do escravismo no Novo Mundo*.

10. While in *Way of Death* the arguments seem inspired by Braudel's and Wallerstein's structuralism, in *The Problem of Slavery*, you criticized the structuralist approach on slavery and suggest changing the perspective toward a history of the strategies of slaving, taking into consideration the individual's agency in the process of slaving. Is it possible join the structuralist approach to a kind of history which recovers the individual's role?

Again, you very alertly identify exactly what I've been saying in all my answers to your previous questions. And highlighting the abstractions I've contrasted with historical processes in ways that may make them seem incompatible may be a good way to being to conclude. Abstractions are risky because their internal coherence lures the reader into them and seems to exclude alternatives.

But contrasts can be complementary rather than contradictory. A historian should contextualize both processes and abstractions, or "institutions", each in the other, in a dialectical kind of synergy, or a synergistic kind of dialectic. In fact, neither can exist without the other, since "structures" are composites and outcomes of events and people act (historically) in contexts that include "structures", at least in the modern world, where they ease the strains of dealing constantly with strangers. So long as one remembers that structures are historical creations, and constantly being modified in the longer term, even as they seem immutable at any given moment.

To give a historical definition of this process, everyone – as social beings – acts in frameworks of collective consensus, or "consciousness", or *Zeitgeist*, or *habitus*, or "culture", or any of many other formulations of the aspects of our contexts that we take for granted. We

think *with* them rather than *about* them. They are products of collective experiences, merged into consensus through countless debates arising from people's differing positions in ongoing circumstances. They range from vaguely "felt" to highly articulated and elaborated, as political struggles force the parties to them to rationalize, to justify, their positions.

But this process is always retrospective, making sense of the past more than the emergent present, in moments when events have already moved beyond the issues that forced their formulation; they are a generation out of date by the time they are have been worked out, or they are political rationalizations aimed at creating an idealized sort of context that does yet exist (and probably never will). So they are vital elements of history, in that they motivate people to act or people act in terms of their logic, but they do not describe what people are doing, which is the premise of history's epistemology. They may be said to be products of history, things that historians ought to write about, but not what historians should use to write.

"Slavery", an abstraction that is formulated in terms of abstract laws, is a prime example. "Nation-states" – or any political institution – are another. So is the Atlantic "plantation" idealized in a mid-eighteenth-century Jamaican form only after it came under abolitionist attack, leaving an ideological legacy that historians subsequently abstracted and generalized as an "institution". They could document its expected features, though only by using the evidence selectively.

"Slavery" is idealized as a structure, almost always phrased explicitly "as an institution", and the same highly politicized context rigidifies it to an ideology. Once reified in this way, it became a problem for abolitionists to attack, or rather abolitionists had to reify it in order to create some "thing" to eliminate. A major thread in the historiography laments the failures of "freedom", in Brazil as well as in the U.S., treating it as a tragic anomaly, when in fact the change was never more than ideological. Most – 90% – of the historiography of the "institution" also consists of discovering that "slavery" didn't exist as imagined, but then attempting to explain the evidence away as "exceptions". It's a classic example of a Kuhnian paradigm, persis-

ting in the face of evidence to the contrary, but one of reality-defying resilience owing to its intense politicization in modern culture. Its image is, in fact, a creation of abolitionists, who were seldom burdened by personal familiarity with the human realities of the West Indies. I do not intend to minimize the grotesque abuses it enabled, but the abusers did not define life for everyone.

As a highly politicized “structure”, for historians, slavery is another sort of “problem”, in that it conceals more actual human experiences and motivated actions than it reveals. So my respectful adaptation of David Brion Davis’ classic title (*O problema da escravidão na cultura ocidental* in Portuguese), which historicized European attitudes to slavery but made slavery a static institution in order to highlight changed attitudes as a paradox that he went on to write five brilliant volumes to explain. Davis acknowledged that Atlantic “slavery” might have been different, but only in the secondary aspect of its racialization there. I argue that practices of “slaving” in the commercialized Atlantic changed fatefully. It is a historical argument that the outcomes of “slaving”, as a recurring strategy of introducing outsiders to local historical contexts, changed with the changing contexts throughout world history, and radically so in the Atlantic.

To answer your question directly: for historians, structures/institutions/ideological constructions explain only individuals’ specific motivations and actions, in historical ways, and historians have an obligation to explain how individuals consolidate them and deploy them. The choice is not either/or; the opportunity is both/and.

There’s a “world history of slaving”, epistemologically a true history, still to be written. *The Problem of Slavery as History* was a preliminary statement of the analytical schema for such a true history, that is truly historical more than historical truth. I don’t think that I’ll live long enough to write one, and elements of these processes appear in the regionally-specific chapters of the now all-but-complete four-volume *Cambridge World History of Slavery*, but the framing of these volumes presumes the modern purpose of enslaving to assemble productive labor forces in a market economy. Little could be less historical than projecting the unique features of Atlantic modernity into earlier

times and other places. That is ideological reification that suppresses history rather than taking that ideology, and structures and institutions, as products of their respective, unique, times and places.

11. Given your wide knowledge on the modern Portuguese Empire's historical sources, particularly on Portuguese activities on the South Atlantic, and also the wide array of your works, what sources and subjects do you deem that the researchers should delve into yet?

You flatter a historian of Angola who is only following out as much of its historical context as he can. And I would not presume to prescribe a research agenda for others to follow. Nor am I capable of doing so, since I can see no farther in front of my nose than anyone else. The spontaneity and unpredictability of historical research, and thus also historiography, is its excitement. I don't want to spoil anyone's fun.

That sort of question is another sort of problem for a historian, since "should" is deterministic from some (inevitably ideologically driven) abstract ideal. Historians only defeat themselves when they devise research projects around answers, when the real opportunity is to discover the question. So I can frame guidance in terms of process but not in terms of topics – the best, most productive in terms of new and relevant insight, are those that we couldn't imagine when we started. We have to stumble over them. I didn't set out to write *Way of Death*. It emerged from the research, to my repeated surprise (and delight!), as I let it speak to me.

That is, historians can work only with their documentation, and they have to let it lead them into the worlds of the people who created it; that maxim applies to oral traditions and linguistic evidence as well as written records. And the only way to "feel" it is to contextualize it as richly as one can – what Clifford Geertz called "thick des-

cription” – until you can sense the writer’s – or speakers’ – motivation as she or he wrote. So you read everything, until it starts to make intuitive sense. History is not a science; it is deeply humanistic, the humans about whom we write, and also ourselves, whom we also do well to understand sympathetically. *Way of Death* would not have become a business history if I hadn’t thought I was fleeing the world of business. And because we are all different, we will all find different kinds of coherence, and our delightfully divergent conclusions will be complementing and enriching rather than competitive and contradicting. That’s why even seemingly familiar documents can yield new insights to inspired researchers. And by “inspired” I don’t mean somehow inherently brilliant, but rather anyone who allows him- or herself to sink imaginatively into their materials, to lose themselves in the past visible in them.

So the first thing is the evidence. The second thing is to let it rule, listen to it, don’t try to ask questions of it that it can’t answer, but sense the concerns of the past that generated it, and thus what it can illuminate. I don’t mean literally or mechanistically, but rather sensing implications at several removes.

The last thing, and this observation returns to the relationship between structure and history, is that you sensitize yourself to insights of this authentic sort by knowing the entire range of realistic possibilities. The social sciences (not to mention literature and other arts) have compiled a usefully comprehensive (I didn’t write “complete”) array of general modelings of human tendencies. You should familiarize yourself with as many of their insights into general possibilities as you can manage, though you should resist their temptations to draw you away from a usable acquaintance with them to try to engage their finer nuances. That’s the job of the social scientists, and it’s work they can do better than we historians can. So leave the phrasing of theory to them, but pick the insights in their work that highlight aspects of your historical time/place/potential to enrich, or fill out, the context that you must build. Few of them will be irrelevant, but none will be comprehensive; they are all partial glimpses of existence, and compatibly, even complementarily, so, if you see them

as highlighting differing aspects of infinitely multi-faceted and eternally changing contexts of human experience and action. They are all on the 10% side of the “90/10 rule”.

To conclude on a less abstract plane: Angolanist that I am, my personal preference would emphasize the still barely appreciated worlds of the Africans, on their own terms, and actively engaged with their contexts, thus positioning them also as contexts for the more accessible actions of the Portuguese,, in which lurk the shadows of the Africans. That is the opposite of the all-to-common tendency to project Europeans’ ideas inappropriately on to the Africans.

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