INSCRIPTIONS ARE TEXTS TOO[1]

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I

Epigraphic evidence is a virtual *terra incognita* for Africanists; few of our sources have come down to us from the past quite so directly. This is in contrast to many other parts of the world, where dealing with inscriptions falls squarely within historians' purview. Where such evidence exists, it tends to exist in very large quantities. For example, for Ur III dynasty, of circumscribed length and extent (2112-2004 BCE, southern Mesopotamia) at least 50,000 texts have been published and tens of thousands more are known to exist.[2] Even larger numbers exists for what is now India, although admittedly covering both a much larger area and a much longer period of time. One estimate is that more than 90,000 have been discovered.[3] Nearly everything we think we know about the Maya civilization is derived from the numerous stelae that have been discovered there. The same applies to the pre-Islamic political entities in south Arabia. And so on. In contrast, the materials included in the work under review represent almost the entire corpus for sub-Saharan Africa.[4]

[1]A review of P.F. de Moraes Farias. *Arabic Medieval Inscriptions from the Republic of Mali: Epigraphy, Chronicles, and Songhay-Tuareg History* (London: British Academy, 2003), hereafter *AMI*.
[2]Magnus Widell, *The Administrative and Economic Ur III Texts from the City of Ur* (Piscataway NJ, 2003), 1-4.
[3]Manabendu Benerjee, "Some Problems of Editing Sanskrit Inscriptions" in *Problems of Editing Ancient Texts*, ed. V.N. Jha (New Delhi, 1993), 53-73. For a recent example of the tendency to make much of very little in Indian epigraphic historiography see Mahesh Sharma, "State Formation and Cultural Complex in Western Himalaya: Chamba Genealogy and Epigraphs—700-1650 C.E.," *Indian Economic and Social History Review* 41(2004), 387-432.
[4]However the geography might be interpreted, for this review Aksum is not considered south of the Sahara.

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This embarrassment of riches outside Africa involves another embarrassment as well. Despite heroic efforts, many of these inscriptions—a majority for some areas—are attracting dust rather than scrutiny; as a result many of the interpretations built on the edited and published ones are potential prey to the evidence in those as yet unexamined.

The so-called epitaphs of Gao have not wanted for study—study carried out largely by French orientalists looking for sources more congenial to their first fields of study, but harking back to Heinrich Barth, who at least had the excuse of being unaware of the inscriptions. The present work escapes this *faute de mieux* aspect; its author has been at work on them for nearly forty years and did not come to them from a sense of misplaced desperation.

II

Epigraphic evidence presents a range of issues that are both similar to, and different from, other kinds of historical evidence, in particular, written and oral data. Unlike much written evidence, the testimony of inscriptions is sketchy and disconnected, and the materials carrying it often in parlous physical condition. Unlike oral evidence, inscriptions—usually—derive from pretty much the same period as the events they purport to describe. Unlike most written and oral evidence, they sometimes come in languages that, at least initially, are undecipherable, a condition that often yields to the passing of time, as with Egyptian hieroglyphs, Linear B, and the Mayan inscriptions, but sometimes does not, or at least has not yet, as with Linear A, the Indus Valley script, and the so-called *rongorongo* script of Rapa Nu'i.

On the other hand, the contents of inscriptions, whatever else, are texts trying to tell some kind of story—a story that might or might not be true. As such, their study is subject to the same conditions and practices as those concerning written and oral texts, with the proviso that in most cases epigraphic evidence has the advantage of being demonstrably original, or at the least that it has survived unchanged—if not unscathed—over several centuries. Thus it is seldom possible to compare recensions of texts for signs of change, one of the more interesting and significant chores of the textual historian.

A distinctive feature of inscriptions is that they can be studied at four different levels of 'primariness.' Most ideally (probably) is the inscription itself, which once was to be studied by carting it back to home base, but which nowadays will probably require considerable effort since it is more

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likely to remain *in situ*.[5] These recent exigencies have led to ways to study inscriptions vicariously. Latex impressions are often made, which serve as a kind of mirror of the original inscriptions but lack certain of the original's qualities and are certainly not clones, though perhaps fraternal twins.[6] Photographs represent the next generation of formats, probably the most common, as they are produced and reproduced time and again, with their very multiplicity serving in lieu of site visits. Finally, there is the line drawing, which, if not the most studied, is certainly the most arrayed, appearing in books and articles as simulacra for more expensive photographs. It need hardly be said that such drawings, no matter how carefully executed, can claim the least legitimacy, despite their widespread use. They might best be compared to translations, which also, despite best efforts, contain more of the translator and less of the author than can be strictly desirable.

So much for the world of the ideal. In practice, if these modifications were not available, very few inscriptions could be studied by enough people to render their evidence credible. Little thought is required to bring forth examples where the results of idiosyncratic study of the original epigraph has given way to the collective wisdom of those who have less immediate access and therefore also less self-interest.[7]

Since only the archeologist-historian is likely to have access to the original source in these cases, or even to the copies made for remote study, his or her credibility depends on the ways the third and fourth options mentioned above are brought into play. Needless to say, for various reasons the choice is most often to offer line drawing for readers to wonder about. Not in this case, however; *AMI* includes a complement of more than 250 photographs of various aspects of these inscriptions, by far the most I have ever seen in such a work, and a most welcome aid to colloquy.[8]

[5]This set of circumstances has especially marked the study of ancient south Arabian epigraphs. Excessive heat, strenuous travel conditions, and civil war interfere with the ability to study these in anything resembling favorable conditions.

[6]In fact, as Farias (xxvii-xxix) points out, Georges-Reynard de Gironcourt made over 800 such squeezes ("estampages") in 1912, which had since lain dormant in the Institut de France, providing an example of an opportunity wasted. He notes as well that some inscriptions in the *corpus* have no estampages while some estampages exist for inscriptions that do not appear to have survived.

[7]E.g., the Qumran texts, the Ebla texts after the in/famous Sodom and Gomorrah reading was overthrown. In the first case a cartel was established and granted a long-term—some thought indefinite—license to study the texts. The results were disastrous in every respect.

[8]Some of these are line drawings, which complement, but do not replace, the photographs that they render.

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III

AMI can be addressed in at least three ways—as a *catalogue raisonnée* of the inscriptions as they were known in *ca.* 2000; as a contribution to the colloquy/debate over the character of political rule in the Gao area in early medieval times; and as an example of the ways in which evidence is unearthed, preserved, interpreted, and disseminated. Here I concentrate on the last, since I see this incarnation as much the most important in the long term.[9]

Farias presents what might be called a just-in-case edition; that is, his method is broadly inclusive in all its aspects, and effectively anticipates just about any question a reader might bring to it. This is evident from the moment one opens the work, to find a 25-page-plus discussion of the author's *modus operandi*, followed by a "historical introduction" that runs to more than 200 pages. A further 220 pages are devoted to the edition proper, and this is followed by a sixty-page bibliography, several maps, a substantial index, and *mirabile dictu*, over 250 high-quality photographs. This is an unlikely scenario in today's world of attenuated scholarly production and is explained by an even more unlikely circumstance—the author provided camera-ready copy. Even at that, the publisher is to be congratulated for allowing all this to come to pass.

An advantage to a comprehensive text edition like this—and one that Farias constantly emphasizes in his apparatus—is that all parts of the texts can be treated equally, whether or not they might seem to be as important or as 'historical' as other parts. Previously, in the absence of such a full-bore plan of attack, certain parts of these inscriptions—we can all imagine which—were analyzed and scrutinized and force-fed onto hypotheses, while others were virtually ignored, all but forcing later scholars along the same paths as the French orientalists had chosen to take—as much from ignorance as predisposition. This pattern might well continue, but the wherewithal is now available to scholars who are not interested solely, or even primarily, in the politico-chronological data contained in these inscriptions, to pick and choose on their terms and not imposed by others. In short, *AMI* offers a kind of belated manumission.

[9]Already four unusually long reviews have addressed some of the issues I do not: *Annales: Histoire, Sciences Sociales* 59(2004), 1206-10 (Jean-Louis Triaud); *Journal of African Archaeology* 2(2004), 285-86 (Richard Kuba); *Journal of African History* 46(2005), 148-50 (Abdel Wedoud Ould Cheikh); *Libyan Studies* 35(2004), 207-09 (H.T. Norris).

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IV

In his introduction Farias provides a useful and enlightening—and necessary—account of the vicissitudes of the Gao inscriptions in the historiography of the central and western Sudan. Unfortunately for themselves, these were discovered long after the so-called *Ta'rikhs* has become the object of scholarly attention As a result, the latter had the double advantage of priority of incumbency and a comforting resemblance to the kinds of sources most congenial to European—written narratives. With only the chronicles to work with, it was irresistibly tempting merely to take their evidence, their kinglists, at face value and construct cultural and political genealogies that mimicked those from better-documented regions—not all of them by any means reliable. In the process, not much attention was paid to how this information could have been preserved and systematized so many centuries after the fact.

Early in the last century the unearthing of the first set of inscriptions prompted some short-lived enthusiasm, but this was quashed by the dubious, but dominant, authority of Maurice Delafosse, who, in his monumental and influential *Haut-Sénégal-Niger* airily dismissed them as inconsequential and unhistorical curiosities. Or to put it another way, he found it impossible to fit their evidence with the existing written evidence, which was far too convenient to be jeopardized by new and unwelcome sources. This was and is unsurprising, and mirrors work in other fields where new evidence endangers standing orthodoxies.

In any case, the discovery of further inscriptions in the late 1930s forced scholars to take the increasing body of evidence more seriously—perhaps too seriously, as if in compensation. Jean Sauvaget, fresh from work in the Near East, was perhaps the most influential of the new cohort of interested scholars. Sauvaget's opinion was far removed from that of Delafosse; Farias quotes (xli) him as affirming that epigraphic evidence is "authentic and first-hand." Sauvaget also added that it is "contemporary [and] impartial," so presumably Sauvaget meant to regard them as "reliable" as well.[10]

But this predisposition was to be tempered at all costs—the results could not be allowed to undermine the chronicles' testimony in any substantial ways lest the fullness and serenity of the past be compromised. Sauvaget decided that the evidence in the inscriptions could be salvaged by aggregating it into genealogies that would—eventually—prove to be

[10]Jean Sauvaget, *Introduction à l'histoire de l'Orient musulman: éléments de bibliographie, corrections et supplément* (Paris, 1946), 49.

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compatible with the evidence in the chronicles. It was a matter of keeping at it until something intervened. Once this was done the chronicles would be vindicated by the inscriptions rather than jeopardized by them. Sauvaget's hopeful *schema* became the orthodox view of this rogue evidence, which became evidence-in-waiting rather than being judged on its own terms with the proviso that the inscriptions might—just might—be evidence for a political landscape not treated in the chronicles. In this way historians of Islamic west Africa convinced themselves that these nuisome loose ends would eventually prove to corroborate rather than challenge the narrative framework that they—following the chronicles—had imposed. If the inscriptions were to be a nuisance, it was only until some kind of confirming and confluent evidence materialized to save the day. Matters would be out of control only briefly.

V

Most recently—say in the past twenty years or so—the principal questions asked about these epigraphs have been: were they the products of an 'intrusive' Berber dynasty, an 'indigenous' Songhay ruling group, or a Mande-based ethnicity? The answers proffered to this question have been part of a scholarly debate of even longer duration, carrying with them certain overtones of the endogamous-exogamous polarity that has—first implicitly and later more explicitly—dogged discussion of the rise of political entities throughout Africa.

In contrast, although he naturally compares the evidence of the inscriptions with that of the much later chronicles, Farias resists the temptation to graft the former onto the Procrustean bed of the latter. Rather, he sees the Timbuktu chronicles as examples of counter-hegemonic discourse, widely practiced throughout history, from the work of Manetho for the Egyptians, Pausanias for the Greeks, Josephus for the Jews, the old Norse sagas, the Bayeux Tapestry (some think), the Aztecs, Maya, and Incas, the Princely States of India, the Maori, the Buganda and other interlacustrine states—in short, by just about every corporate group after it had been subjected by another. Recognizing this is the first step in treating these sources as something less—and something more—than objective history.

Farias observes (lxiii) that, when faced with the refractory evidence of the Gao epitaphs, Sauvaget "hypothesized a hidden correspondence between the two," which someday would presumably be revealed. He will have none of this casuistry, arguing instead that "[e]fforts to relate the inscriptions to the kinglists are largely an exercise in art for art's sake," not

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least because they are "virtually unfalsifiable." Farias suspects that the later chroniclers had no access to these inscriptions—or chose not to exercise it—which would help to explain the discrepancies.[11] While probably true, there is no particular reason to posit the first in order to explain the second. Like more modern historians, these chroniclers also abhorred a vacuum.[12]

The Gao inscriptions thus constitute a case of almost universal application. In the nature of the beast, data in inscriptions do not often correlate with data from other sources, making it difficult—often impossible—to posit relationships between the two *corpora*.[13] Intuitively we can appreciate why, even if viscerally we would prefer not too. As Farias notes on several occasions (e.g., xxxvii-viii), the viscera has usually won this struggle with the brain, and the relatively fragmented and decontextualized epigraphic data are routinely sacrificed on the altar of Coherence by those who prefer to trust in the chronicles for this early period because they are so much better at dotting i's and crossing t's.[14]

Farias begs to differ with this thoroughly panglossian and self-serving view of things, seeing the Gao inscriptions instead as "rhetorical constructs," susceptible to earlier and later misunderstandings. Inscriptions are not without problems as texts. They tend to be formulaic, disembodied, and laconic. While often intended only for the moment—for example, economic transactions or ritual activities—they are also sometimes intended specifically to impress the present and the future and thus can be expected to be replete with rodomontade and inaccuracies. Even the apparently objective epigraphic testimony suffers from its necessarily telegraphic presentation, requiring their students to bring a full leaven of interpolation and interpretation to their work.

The names, relationships, and dates provided by the Gao epitaphs ineluctably throw previous lapidary versions, both early modern (the chronicles) and twentieth-century (the Sauvagets) of the early historical experience of the area into disarray. Several expedients are possible that could—

[11]Or chose to modify them to suit later imperatives; for one such case see Farias' discussion of "the erasure of early rulers" in *AMI*, clxvii-clxx.

[12]On this particular attitude, writ larger, see David Henige, "Epigraphic Evidence and the Abhorrence of a Vacuum: Some Phantom Dynasties of Early and Medieval India," *Bulletin of the School of Oriental and African Studies* 38(1975), 325-49.

[13]For a case study from the ancient Near East see David Henige, *Historical Evidence and Argument* (Madison, 2005), 135-47.

[14]One pretty salient discrepancy is that the inscriptions imply matriarchy and proclaim matriliny, quite in contrast to the mainstream Islamic attitudes of the later chronicles, which have nothing to say about either.

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in whole or part—retrieve the situation: if the royal names are few, and seemingly cognate, then they could be rulers later omitted by collateral descent. Or they could (all??) be alternative names of those mentioned in the chronicles.[15] Or they could represent the rulers of a different polity entirely, or perhaps just different sets of office-holders.[16] The last will probably prove the most alluring since it would safeguard the testimony of the chronicles, although at the expense of idle speculation. On the other hand, the first two would discredit certain parts of the chronicles and by extension would render many other parts suspect, since neither one is especially plausible.

Of course our understanding of any past can only be fragmentary to one degree or another, no matter the occasional plethora of sources. When the sources pretend to continuity, it makes it easier for us to forget this. One of the potential advantages of inscriptions is that they clearly have both physical and epistemological limits that force us to recognize this exiguous condition as fragments both in space and inherently (like sources on papyrus). Farias declines to play the popular game of conjectural emendation and—paradoxically to some, perhaps—one of the most attractive qualities of *AMI* are the numerous lacunae that festoon Farias' translations, cases where a less scrupulous editor might have filled the gaps with all species of unsignaled guesswork that could ultimately become ensconced as canonical, even original.[17]

[15]In fact, this particular god from the machine has been applied in a closely parallel case unfolding in China, where a cemetery thought to contain the remains of the rulers of Jin during the Zhou dynasty (*ca.* 1050-221 BCE) has revealed a series of apparently royal names that do not match up with the data in the later annals. As with Gao, opinion oscillates between accepting the epigraphic evidence as accurate but decontextualized to finding ways and means to force the two bodies of evidence into matrimony. On this see Jay Xu, "The Cemetery of the Western Zhou Lords of Jin," *Artibus Asiae* 56(1996), 193-231; Li Boqian, "The Sumptuary System Governing Western Zhou Rulers' Cemeteries, Viewed from a Jin Rulers' Cemetery," *Journal of East Asian Archaeology* 1(1999), 251-76; D.S. Nivison and Edward L. Shaughnessy, "The Jin Hou Bells Inscription and Its Implications for the Chronology of Early China," *Early China* 25(2000), 29-48.

[16]This would be analogous to the case of late New Kingdom and Third Intermediate Period Egypt, where lines of high priests co-existed with pharaonic dynasties, leading to much head-scratching in the early days of ancient Egyptian historiography, but it took a lot more evidence to determine this concurrency than is likely ever to be available for Gao.

[17]Such a lack of completeness owing to breakage and loss of part of the inscription accounts for much of the controversy over what is probably the most debated epigraph of recent times—the so-called Tel Dan inscription, which purportedly contains the phrase "house of David," leading many biblicists to pronounce the inscription as

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VI

The appearance of *AMI* encourages us to ask questions that might be—and might remain—unanswerable, and that certainly should not be answered in the present state of the evidence. Does the concentration of these texts in time (1014-1489 CE in several non-continuous clusters) and place suggest that an equal number of congruences of socio-political circumstances occurred then and there that made produced them? Or is it simply a—possibly temporary—result of historical contingency. None of can can know how much—or how little—of the past has survived for our edification, but simple intuition might suggest that the odds are very long against us. The area in question is auspicious for preserving materials but inimical to locating them.[18] Who knows what other caches of such texts remain to be discovered? This means that any conclusions about the significance of just these texts can only be regarded as provisional—the degree of doubt contingent on our own ideas about what else might have been created and that might be discovered at some later date—and also by the ways in which these texts refute the long-accepted testimony of the *Ta'rikhs*.

This state of suspense is also characteristic of the epigraphic remains from the ancient Near East, which mention thousands of individuals' names who never came close to making it to the annals or chronicles, being largely tradesmen, merchants, and low-level functionaries. As a result, we know more about, for instance, the man in the street in nineteenth-century BCE Kani than we know about their contemporary heads of state in neighboring Mesopotamia. Similarly, these inscriptions tell us about long-term and intensive commercial transactions and political, economic, and even linguistic acculturation that we might well have intuited, but could never have demonstrated to the point of legitimating practicable hypothesis-building.

the long-awaited exteral evidence for the historicity of the bibical narrative. The state of affairs has allowed one scholar to re-assemble the pieces to create a substantially different message. Whether this author is correct must await the unlikely discovery of further pieces in this particular puzzle

[18]Such ambient circumstances have resulted in certain areas of the epigraphic world being less favored than others. For instance, commenting on a recent work on the archeology of early Korea, the reviewer notes this: "[i]t is understandable that this book deals mostly with archaeological data from the southern half of Korea . . . I suppose the archaeology of Koguryo is discussed only briefly, because of the relative inaccessibility to archaeological data, which are scattered in northeast China and North Korea." Yangjin Pak, "Review of Gina L. Barnes, *State Formation in Korea: Historical and Archaeological Perspectives*," *Asian Perspectives* 43(2004), 152.

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Of course, a great diversity of arguments emerge from *AMI*. One of the more interesting, if not most novel, is that these inscriptions offer further, and probably incontrovertible, evidence of a high degree of virtually continuous ebb and flow, flux and reflux, acculturation and reculturation across the purportedly impenetrable Sahara. This supports the notion that the Sahara, like the Atlantic or the Gobi or the Hindu Kush, was as much let as it was hindrance, and reminds us yet again, this time with the best kind of primary evidence, that concepts such as "sub-Saharan" Africa might have outlived their utility in all but a few fields.

VII

Text editions must be measured by the highest scholarly standards, not only because sacrosanct materials are at the heart of the enterprise, but because it is work that—at least for most historical texts—once done is not likely to get a second chance. Others will have to judge the accuracy of Farias' transcriptions and translations, but even an outsider can pass judgment on other aspects of the work. Firstly, he has made it easy for others to test his work—not a common habitude—by rendering all texts clearly in script, in transcription, and in translation. Moreover, as noticed above, he (and, again, all credit to the publisher as well) includes photographs of the texts treated here, allowing those able to do so to join him in quarrying these texts.

Besides this fundamental activity, Farias has placed these sources in both historical and historiographical contexts. For some, historiography is boring—today is better, so what does it matter what our scholarly ancestors thought and did anyway?—but Farias' extensive introduction shows why this attitude is fatuous and self-serving. Much of the previous work he discusses, however valuable, was carried out in circumstances and with mindsets that need to be appreciated if it is to be evaluated properly. If the present really *is* better, it got that way by building on the good and the bad that preceded it.

All things considered then, it is no surprise that the scholarly world is not overrun with text editions, in some part because their labor-intensiveness, production costs, and problematic market appeal mean that very few texts get a second chance and remain at the mercy of their first editor indefinitely, creating an impression of surpassing quality that can be belied on virtually every page. *AMI* will probably monopolize its market as well; in this case it deserves the right to do just that. More importantly, it is likely as well to serve to stimulate more productive and more focused discussion by putting all the evidence—with an authoritative interpreta-

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tion—on display. In this way it will, like all good text editions, elevate the level of discourse, interdict the casual appearance of inadequately supported theories, and perhaps even lead to more defensible explanations for these precious yet troublesome sources.

One final point can be made in this respect. Despite the fact that this is a text edition *ne plus ultra*, certain distinct strategic—i.e., long-term and beyond the texts themselves—advantages result from it. This unquestioned exemplification of the author/editor's longstanding and intimate relationship with these texts renders his opinion on matters of controversy the default position. Those who would disagree with the positions he espouses are now obliged to tackle this work and its conclusions, however tentative, on grounds that surpass its own scholarship. This will be no easy task. It now becomes very hard to imagine under what circumstances the testimony of the later chronicles can be rehabilitated, and even harder to justify the bother.

VIII

In a central way, *AMI* is a throwback to antediluvian historiographical times. Paulo Farias has been able to give a large portion of a lengthy scholarly life solely to achieving this edition, to a point where it seems almost surreal in today's scholarly publishing environment. He not only carried out the fieldwork and the translation and interpretation of the text, but also, as noted, provided camera-ready copy. Any reader will find it easy to imagine the magnitude of this operation, with narrative in several Roman and non-Roman scripts. It seems reasonable to assume that the present edition would never have appeared had Farias not undertaken this extraordinarily onerous task. This is undoubtedly symptomatic of scholarly publishing today, where notes are still consigned to the end of a work, where including significant content in these notes is discouraged, where indexes are considered optional, where illustrations are anathema, where words are limited yet jammed onto the page.

At a time when scholarship tends to be measured by pages rather than by ideas, by appeasement rather than by critical thinking, by adherence to the ovine, and by valuing the moment more highly than the scholarly *longue durée*, *AMI* is an exemplary accomplishment in the true sense of that word. It is too much to much to expect that academic departments will allow this kind of life-long commitment to play itself out very frequently, or that publishers in this day and age will deal fully with the results. But if this should happen to change, this work can serve as the measure.

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Despite its success as a text edition, *AMI* is more than a text edition. It is, as noted, a testament to what can be accomplished given a scholarly lifetime of assiduous application. In its own way, it can be thought of as a smaller version of the great compilations *Corpus Inscriptionum Graecarum* and *Corpus Inscriptionum Latinarum*, without which the study of classical antiquity could not have advanced beyond antiquarianism for lack of sufficient contemporary evidence. In this sense, alas, *AMI* is probably also an artifact, for the rules of the academy no longer permit this kind of thing to happen. Nowadays, scholars are constantly expected to be on the brink of "new initiatives," which are deemed to show their versatility and ambition. The new work can be in the same restricted field, but it is expected to "advance" on earlier work, which must be abandoned to make way for it.

Typically, published scholarship is intended to convince others to agree with the author about whatever the matter happens to be, whether by force of argument, superficial plausibility, or congeniality. The degree of market success will vary—from no one to almost (but never quite) everyone—and it is the scholar's perception of this that makes him/her happy or unhappy. But joy is not immortal; even a high degree of agreement has a life expectancy—looking to the past amply testifies to that. We can hope to be timely, but not often timeless. Works like *AMI*, however, escape this mortality. It will not be superseded; its evidence will not disappear into its own discourse; its arguments will convince because that are yoked with the evidence to support them. Long after synthetic and interpretive works on the medieval central Sudan become antiquarian curiosities, *AMI* will retain its authoritative status.

It cannot realistically be expected that Farias' conclusions will command universal agreement, but, as noted, those who would disagree will now be faced with the plain fact that these conclusions are based on a much closer acquaintance with this body of sources than anyone has ever had—or probably ever will have. *AMI* will now be the indisputable cynosure for studies on medieval Gao nad its neighborhood, and perhaps more broadly as the implications of the epigraphs' evidence become more clear. This is one of the fair rewards that accrue to those willing to exert the rigorous intellectual and physical labor and sacrifice required to produce such an edition.

The incredible onslaught of new and often unprocessed information and the technologically enhanced means of gaining access are some sense legions of Loreleis along the academic waterways. In the new environment, intellectual curiosity is presumed to have many foci, and projects are expected to be designed, fulfilled, and forgotten in a constant cycle,

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each with a strict *terminus ad quem*. It is all too reasonable then to expect not to expect many more such paradigms as *AMI*, whatever the field. For African historiography, *AMI* might not only be the very best of its kind but the very last. We might never see its like again.[19]

[19]A (very) few things struck me as falling short of the impeccable standards of the whole. The "Republic of Mali" part of the title jars somehow, as if modern political boundaries also had significance in medieval times—maybe they have little significance even in our own! Farias has divided his very extensive bibliography (221-68) into several sections, and this creates an unnecessary burden on the part of readers, who must check too many places to track a citation (e.g., Hunwick 3/224; 1/229; 3/238; 25/255-56: Farias 1/230; 6/235; 4/241; 14/262-63). While Farias provides a Christian equivalent for every A.H. date as it appears, for those of us have never learned to think instinctively in other than "A.D." terms, a Christian-*hijra* calendar for the relevant period would have been useful for explicating such matters as lunar vs. solar reckoning.

The first is not unlike the correlative situation in contemporary India, when many epigraphical editions are published by state governments and the contents and titles often reflect this. Unfortunately, in India, as in Nigeria, the trend has been to more and more smaller and smaller states, so that a work devoted specifically to the inscriptions in Tamil Nadu, for instance, might find its title rendered obsolete by some kind of retroactive geopolitical legerdemain.

On the other hand, and concerning an equally trivial matter, Farias rightly uses "data" as a plural throughout, in the scientific tradition rather than ape the typical modern usage in the humanities and social sciences, where practitioners fail to remember the word's inescapably plural origins.

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