On Myth as Science Fiction

Walter E. A. Van Beek


Stable URL:
http://links.jstor.org/sici?sici=0011-3204%28199204%2933%3A2%3C214%3AOMASF%3E2.0.CO%3B2-H

Current Anthropology is currently published by The University of Chicago Press.
On Myth as Science Fiction

WALTER E. A. VAN BEEK
Department of Anthropology, University of Utrecht, K. 1222, P.O. Box 80.140, 3508 TC Utrecht, The Netherlands. 11 x 91

De Heusch’s [CA 32:434–37] reaction to my article on the restudy of Griaule [CA 32:139–65] brings out some interesting points that deserve further attention and discussion. It also shows an intensity of emotional involvement on his part that blurs some relevant arguments. Given the deep loyalties to Griaule of his close collaborators [Dieterlen], his relatives [Calame-Griaule], and his disciples [such as de Heusch] and in the light of Meillasoux’s comment on French academic culture, such a reaction is to be expected. Still, a few corrections are called for. De Heusch treats this academic discussion as a trial in which I am claiming to be judge, jury, and hangman; all this judicial terminology is out of order. My aim was to open up discussion on a topic that has too long been closed: the ethnographic validity of the Griaule publications. I waited some time before doing so because of the emotion that would be generated. Still, science is a discourse, even when it is sensitive; I felt I could not remain silent till a less hurtful date. Throughout the article I expressed my respect for the pioneering contributions of Griaule. While criticizing him on ethnographic grounds, I think I did him more justice in the role he really excelled in [and wished to excel in]: as an adventurer and above all as a writer. So, if Griaule’s intellectual progeny find my analysis uncomfortable, it may be because adventure has seeped out of the discipline and good writers are scarce. For de Heusch the literary style of Dieu d’eau [DE] detracts from its ethnographic value. Though I concur with him in this critique, I think that Griaule’s style is more than a device; it is at the heart of his ethnographic endeavour. His aim was to write a story, and so he did, very well. When the difference between it and his subsequent books [and of course Le renard pâle [RP] is in considerable part the work of Dieterlen] had to be explained, the notion of initiatory stages, unrecognizable to the Dogon, was created. De Heusch’s main argument hinges on the notion of hidden myth and the possibility of an immanent cosmology. The Dogon are, for de Heusch, “exceptional” only in that their system of classification is “so meticulous.” He compares it to Pierre Smith’s description of Rwanda proscriptions covering all aspects of social life. Many other examples could be given of intricate classifications tied to a variety of social parameters; Victor Turner’s and Mary Douglas’s works are full of them. There are, however, some crucial differences between these systems and that described by Griaule in DE and RP. First, the latter do not in fact correspond with social parameters, and if correspondences are indicated, mainly in DE, they have no empirical foundation. Secondly, in their detailed and seamless coverage, the DE and RP classifications are still unmatched in the ethnographic literature. Even within the field of symbolism they are an anomaly.

The issue of immanence is pertinent. Turner’s analysis of hidden myth is important and often relevant in Africa, but not for the Dogon material; Griaule does not use anything other than myth itself as a primal source. He definitely does not start with a description and analysis of ritual, nor do ritual elements feature in his analysis or his production of the myth. In fact, if Griaule had deduced the myths from the rituals [this is what I tried to do in my analysis, as does any anthropologist who

1. In my response to Mary Douglas [CA 32:161, citing my article p. 141] a confusion of the names of [Meyer] Fortes and [Daryll] Forde resulted in some misunderstanding. The discussant [and friend] of Dieterlen was Fortes, who by the way has always been critical of Griaule’s mythopoetic Dogon interpretations.
interprets ritual] he would have wound up with a totally different—and in my view definitely non-cosmological—set of interpretations. The bits and pieces that Griaule (and Dieterlen) worked with are not rituals, nor are they the “notes, sketches, or fragments” that Turner mentions; on the contrary, the building blocks of Griaule’s analysis were tailor-made for him during his interviews. The clearest example is the majority of the drawings which were produced for the occasion (and therefore later forgotten by informants in their explanation). So the absence of a story line in the Griaule mythology is indeed a problem, and Griaule and Dieterlen should have not only noted it but recognized it as a major obstacle.

I do not belittle the importance of the symbolic dimension (or its role in the future of anthropology), but I do claim that that dimension should be studied (as it in fact has long been in anthropology) in relation to historical, political, and sociological parameters. This is exactly what Griaule did not do; for him the myth served as the explanans, not the explanandum. In Dieterlen’s (1989) analysis of masks the same strategy surfaces: the myths find expression in the details of the masquerade, without any sociological, ecological, or political reference. In fact, de Heusch concurs on this, as he writes [p. 437] that “myths are not the keystone of Dogon social structure as Griaule and Dieterlen sometimes imprudently implied.”

This, of course, bears on the concept of myth itself. Somehow de Heusch seems to have gained the impression that for me myths must be unchanging, time-honoured tales, fully authentic and untouched by foreign hands. Not at all so, as I made clear. The search for an authentic myth is like the quest for the Holy Grail: an unattainable goal leading to an unproductive pursuit. Myths reflect social changes, political influences, individual goals, and sometimes history, and I fully recognize—and relish—their creative and adaptive dimensions. The very first article I wrote on the Dogon treats this aspect [van Beek 1981]. The shoe is on the other foot, in fact. First of all, as with many oral traditions, the Dogon in their crucial notion of tém (tradition, “found”) imply just such an authentic, unchangeable base. Even in the case of evident adaptations, “tradition” invokes authenticity and absence of change. This, as I said, is no exception at all. However, Griaule and Dieterlen identify with this emic Dogon definition of myth and tradition and implicitly as well as explicitly define the myths as authentic, fully and wholly Dogon, disregarding any influences from outside. In effect, if any similarities between, say, Dogon and Bambara are in evidence, they explain them by referring to a general old Sudanese culture of which the Dogon are one of the more faithful representatives. So it is the French Dogon ethnographers who were looking for the Holy Grail; otherwise they would readily have recognized many influences from, for example, biblical sources. For me there is no “true Dogon” myth or religion, and my opening agreement with the tenets of heuristic anthropology should have been clear enough. Therefore, I stated the problem of ethnographic validity in terms of recognizability, and I still think that that is where it resides. Griaule’s tales are unrecognizable to Dogon informants, even in Sanga. Therefore, even if there are no “true Dogon tales,” some tales are definitely not Dogon.

De Heusch mentions the complexity of Dogon rituals and tries to reduce my remarks on them to some ethnographic simplicities. This is irrelevant: I nowhere underestimated the complexities of, say, the mask rituals; the bush-village distinction, though important, does not at all exhaust the polysemy of the dama. But, again, that polysemy cannot be honoured in an article, especially not in a discussion article; it has to be the subject of a monograph, as Douglas has rightly insisted.

De Heusch accuses me of rejecting a great mass of ethnographic material in order to make sense of a chaos of contradiction. But it was my informants who rejected the myths and, when acquainted with the Griaule myths, pointed out the contradictions in them, explaining them away as constructions (or simply “lies”)—and not only informants from Tireli but also informants from Sanga and, among them, informants who had worked and were still working with Dieterlen. The latter were particularly keen in pointing out the contradictions in the data. In explaining away the texts, they referred to the creative abilities of the individuals who had instructed or translated for Griaule at the time. Anyway, de Heusch here wants to have his cake and eat it: on the one hand he defends both DE and RP, while on the other hand he dismisses DE as an “enigma,” possibly a “smith’s” version. The latter suggestion, though interesting, cannot be validated. Ogotemméli was not a smith, while there were smiths in the RP informant circle. Moreover, the specific blacksmith traditions that can be traced have no bearing on creation but only on migration histories and on technical and ritual knowledge. Thus de Heusch makes a distinction similar to mine, dismissing one text as “enigmatic, problematic, and troublesome” in order to save another.

How indigenous the inventions of Griaule’s informants are—de Heusch’s next question—is crucial. Delving into the famous dung beetles, de Heusch suggests that Griaule’s informants followed the criteria of their own classificatory system. In a way, this is evident indeed. Their reference to certain animals and to specific colours is definitely Dogon: they used the Dogon terms at their disposal. The point, however, is that this did not stem—and need not have stemmed—from any “classificatory mania.” Other Dogon, when confronted with this classification, simply roared with laughter—which is all the more convincing in such a polite society. Though the people who had told Griaule this were old, thus commanding respect, this was too much—this was completely ridiculous. Insofar as it is Dogon, it is a Dogon joke.

2. De Heusch’s challenge to name an example of structural comparison of unconnected myths is easy to meet: Lévi-Strauss’s (1958) analysis of the Cinderella/Ashboy myths will serve.
In his next sections de Heusch, without stating as much, investigates what can be salvaged from the ethnography. He agrees that Griaule erred in presenting a flawless system and goes into some ethnographic detail. On the whole I have problems with his analysis only when he mixes sources. For example, Lébé is important in the hogon complex, but the identification of Lébé as a first ancestor is neither needed for explanation nor recognized as such by informants (even Sanga ones). Lébé is not merely an underworld counterpart of Amma (I never said that) but a supernatural being in his own right. And the ecological relevance of the buró ritual is important in its symbolism and quite straightforward. It is when cosmology is brought in that problems arise. The same holds for Nommo, also an important Dogon deity, even more than Lébé he is drawn by Griaule into a grandiose non-Dogon scheme in which Christian elements gradually emerge. That Dogon elements (menstruation, twins) continue to figure is just as evident and does not at all detract from the bricolage aspect of the myths. Of course the informants used Dogon elements in their constructions. But the informants were not the only ones introducing foreign aspects, Griaule in his analysis removed the data even farther from their sociocultural milieu. And in reanalysing the myths de Heusch removes the Griaule myths from their social and ecological context. In invoking the Brahmanic model, for example, one is clearly out of Africa. Again, de Heusch’s interpretation of Nommo’s blood in comparison with Christ’s blood, though a nice piece of theology, takes us very far away from Dogon country, beyond Dogon recognition. The etymology he cites of yuguru (serpent) and yurugu (fox) is not recognized at all by informants, even Dieterlen’s.

Finally, the question of genius: First, enough time had elapsed between 1931 and 1954 to allow for creative integration of new elements, Christian or other. Furthermore, the reworking of models—in RP disguised as levels of knowledge—is a common human way of thinking, in fact the basis of structuralism. Semifictional (or pseudo-empirical) creations such as DE and RP are not beyond human reach. For instance, many science-fiction or fantasy writers have produced similar and even more elaborate tales of the past and the future: Tolkien’s work, from the hobbits to Silmarillion, and Heinlein’s “future-history” have all the Griaule features: a progressive unfolding of an ever more grandiose scheme, a widening of the creative horizon, the use of more remote additional sources, and a gradual integration of previously unconnected players. In the best sense of the word, DE and RP are science fiction and stand among the world’s great pseudo-empirical fictions. These paroles étonnantes [a nice French translation for science fiction, by the way] should indeed not be rejected; I am adamant on that in the article. They are works of intercultural art and should be read as such.

References Cited


**On Hieroglyphic Literacy in Ancient Mayaland: An Alternative Interpretation**

**DENNIS TEDLOCK**

Poetics Program, 306 Clemens Hall, State University of New York, Buffalo, N.Y. 14260, U.S.A. 11 xi 91

Brown’s ([CA 32:489–96] treatment of Mayan writing calls for corrections and comments on a number of points. Near the beginning, he states that “nearly all” Mayan writing on paper “was destroyed by the Spanish during the early phases of the conquest” (p. 489), citing as his source a passage in which I actually mention both missionary bonfires and “the perils of decay” as factors that have limited the number of surviving hieroglyphic texts on paper ([Tedlock 1985:27]). Bonfires or no, the use of Mayan writing continued well after the conquest. Andrés de Avendaño saw books containing calendars and katun prophecies in Yucatán as late as the very end of the 17th century, and he even acquired a degree of literacy in the Mayan script ([Roys 1967:184]). During the first years of the 18th century, in highland Guatemala, Francisco Ximénez saw books he described as containing divinatory calendars “with signs corresponding to each day,” and he acquired such a book for his personal library ([Ximénez 1967:11]). These two sightings occurred nearly seven generations after Diego de Landa’s fabled 1562 book burning in Yucatán.

Surveying the words for “write” and “read” in Mayan languages, Brown concludes that “while words for ‘write’ are virtually the same across all Mayan languages, words for ‘read’ differ” (p. 490). He argues that the heterogeneity of the terms for “read” could indicate that they are relatively recent and that literacy was not widespread until missionaries spread the alphabet after the conquest (p. 494). It would seem obvious that a separation between colonial and modern vocabulary sources might shed some light on this matter, with the former reflecting older usages than the latter, but he lumps them together. A second problem arises from the fact that he implicitly treats the act of reading as if it were the same for a Mayan hieroglyphic text and for a Roman alphabetic one, for an almanac (the typical Mayan book) and for a catechism (the typical missionary text).

The terms for “read” assembled by Brown (p. 491) can be divided into three semantic categories, one of which has to do with speaking (usually calling out or shouting),

1. There is one exception, a Tojolabal term glossed as “learn paper” (p. 491).