

ISLAMIC FEEDBACK
OR ANCIENT NEAR EASTERN SURVIVALS?
A reply to David Henige

Dierk Lange

Has West Africa inherited the state and other important institutions from the ancient Near Eastern world or not? Some reviewers of my book, “Ancient kingdoms of West Africa” (Lange 2004a), are prepared to consider this idea fruitful and innovative, while others reject it as unfounded. David Henige (2007) belongs rather to the second group. He considers the methodology employed too unsafe to produce valid results and therefore remains very sceptical.¹

Nearly a century ago, a similar controversy occurred between the anthropologist Leo Frobenius and the Islamic scholar Carl Becker. While the former identified ancient Mediterranean, Byzantine and Persian influences on West African societies, the latter acknowledged Byzantine Christian influences and further suggested that apparently Hellenistic and Persian culture traits had reached West Africa through the spread of Islam.² Though unaware of the earlier dispute, in his writings Henige took a similar position to that of the Islamic scholar, extended his approach considerably with respect to oral traditions and made it a key issue of his critical approach. Distinguishing sharply between pre-Islamic diffusion and feedback from outside textual material, he argued strongly in favour of recognising the great impact of Islamic and Christian borrowings on seemingly indigenous traditions (Henige 1974, 1982).

Henige’s review of my book raises first the important question of how long cultural practices may survive. This is indeed a fundamental issue for the identification of ancient Near Eastern remnants in Africa. At present there is a tendency to believe in recent inventions and constant far-reaching changes. However, it should be carefully noted that the cultural practices at issue here are not those of private or family affairs but of the great state celebrations like New Year festivals involving the king and other great functionaries of the traditional states. In fact, my book deals with the ancient New Year festivals of Daura and Ile-Ife, the traditional centres of the Hausa and Yoruba states in present-day Nigeria, on the basis of new field research. It tries to provide some evidence – in both cases for the first time – suggesting derivation from ancient Near Eastern forerunners. On this basis, it places the beginning of the state-building pro-

¹ Details about the response to my book are found on <http://dierklange.com>. I am grateful to Kathrin Mitzinger for corrections and discussion.

² Becker (1913:303–312), Frobenius (1912/13c:xx–xxiv)

cess in West Africa in the pre-Christian era (Lange 2004a:277–287). The comparative approach corresponds to a new orientation in African historical research. To the best of my knowledge, none of the colonial authors had the idea or indeed the ability to conduct in-depth comparisons between African and non-African cultures. Today, however, the opportunity exists to carry out long and painstaking research along these lines owing to university positions or independent funding, but again I am not aware of many scholars having seized it. The neglect of cross-continental cultural comparisons is not due to accidental oversight but to the predominance of the post-colonial paradigm that insists on the acknowledgement of African cultural independence. Hence, it is not surprising that the burden of providing evidence is supposed to be on those who go against the *Zeitgeist*, not on those who follow it, while actually it should be shouldered equally by both. Trying to free ourselves from conventional prejudice, we should question not only the validity of the evidence for early state festivals in West Africa but also the contrary evidence for the late emergence of these festivals. The plea for an early dating can be based on some general links with corresponding ancient Near Eastern phenomena, backed up by circumstantial archaeological evidence pointing to the emergence of social complexity in the Lake Chad region towards the middle of the first millennium BC,³ while the arguments against this that favour a later dating are very uncertain about the rise of the phenomena concerned. In fact, their advocates may opt for quite different periods with respect to the rise of state festivals, i.e. the states of West Africa: the epoch of the beginning of trans-Saharan trade, the era of Islamisation or even the time of the first textual evidence. Looking at the debate from this perspective, the advocates of late datings clearly also face a great problem of evidence.

Closely related to the possible longevity of the ancient Near Eastern state in West Africa is the connection between legend and state ritual. Henige recognizes the importance of the issue when he mentions the embedding of ancient traditions in ritual. However, by singling out my statement concerning the disruption of the line of transmission from father to son in Daura, he inadvertently reveals his own uncertainty about the distinction between verbal knowledge and ritual (Henige 2007:286). Indeed, I made it quite clear that in this instance only knowledge of the Hausa legend was meant, not the practice of any ritual (Lange 2004a:287). Further, by pointing out the importance of the cult-dramatic re-enactment of the tradition of origin during the yearly festival by the main state officials, I tried to indicate the ways in which an oral tradition may be transmitted faithfully over centuries. Since such a close connection between the legend of origin, state drama and state officials only seems conceivable within the framework of sacred kingship, it would appear to have originated from a very ancient period. Obviously it is indicative of a pre-Islamic society in which myth was the overruling state

³ See in this respect Lange (2007), based on recent archaeological research in the region of Lake Chad which dates the earliest proto-urban structures of West Africa to the middle of the first millennium BC.

ideology and in which the main officials represented or acted on behalf of different deities of the pantheon. While myth has progressively been transformed into legend and state officials have become secularised, the state drama itself survived to a large extent even within Islamic society. Hence then we are faced with important changes having affected various cultural traditions without entirely obliterating them. Oral tradition alone is only one aspect of them, its embedding in the cult-drama of the state being itself important evidence for its age and durability.

Henige pleads strongly in favour of a highly critical approach to the available sources. However, the critical method in historical research has been developed mainly on the basis of written texts. To look at oral traditions from the same angle is debatable, since their nature is very different. Probing them by taking account of their social embedding in state institutions seems to be an important supplementary procedure to test their validity with respect to the period of state formation. Nevertheless there is a wide gap between the supposed date of the emergence of social complexity towards the middle of first millennium BC and the first available textual evidence provided by Arabic sources in the ninth century AD. The results of the cultural comparative approach, which should be tested one by one, offer important evidence to bridge it. Comparing precise phenomena and their designations may produce valid links which resemble those which oral historians call tie-ins.⁴

Next there is the issue of etymology. Henige raises this with respect to the deities involved in the New Year festival of Ile-Ifẹ by turning here to the second new essay entitled “The dying and rising God in the New Year festival of Ifẹ”, which among others suggests that the six important deities of the festival can be traced back to Canaanite parallels (Lange 2004a:343–376, 569). Criticizing explicitly the proposed derivation of the name of one of them, he disregards five others, all of them characterized by their functions and the cult-dramatic participation of their supposed descendants in the festival. Thus, no attention is paid to the fact that the book deals more with the nature and the characteristics of the deities and their cult groups than with the phonetic form of their names. Comparative evidence is adduced from ancient Near Eastern texts pertaining to the nature of the apparent forerunners of these deities and their priestly representatives within the context of the reconstructed New Year festivals, in particular that of Ugarit. The focus is hence on the structural complex of the festival, not on individual deities or cult groups. This allows the king and other functionaries of the state to be included in the analysis and to compare them with the *suffet* or chief magistrates of Punic North Africa as attested by a number of inscriptions (2004a:366–369). The comparisons proposed are therefore never based primarily on names but on the associated meanings and functions. Therefore any judgement on the validity of these comparisons needs to

⁴ With respect to the comparison of precise phenomena among the Yoruba and societies of the ancient Near East such as the Egungun masquerades, the Igbo cult-groups and Levite priests, see Lange (2003:18–26, 2005:265–285, 2006a:307–322).

consider the whole of the structural pattern, not individual items, and even less so the phonetic forms of particular names. This methodology dealing with words and things is a far cry from the rightly rejected purely etymological approach.

An important method employed throughout the book is philology based on a knowledge of Semitic languages. The main arguments put forward in the third new essay of my book, called “From Ghana and Mali to Songhay: the Mande factor in Gao history”, is precisely based on the philological analysis of royal names attested in the earliest contemporary internal documents available for West African history (2004a:495–544). Although the interpretation of Arabic texts is not part of the evidence Henige is used to dealing with, he might have given the reader an idea as to why, in its subtitle, the book also claims to deal with “Africa-centred perspectives”. More to the point of my disagreement with the review, it should be noted that in some cases philology also allows recent feedbacks to be distinguished from ancient loans. This seems to be the case with the biblical names provided by the *Dīwān* of Kanem-Bornu. Among the 21 patriarchal names from Adam to Ishmael mentioned, with only one exception, four – Qenan (4), Methuselah Matusalim (8), the commander Eber (14) and Re‘u (16) – can be shown, against all expectations, not to have been derived from any known Arabic text, let alone the Qur‘an, but from an older biblical source (2004a:244–245). Henige must have noticed these elements since there is also an explanatory graphic, but he skips what does not agree with his favourite theory of feedback. Philological analysis therefore tends to indicate that in some cases of suspected Islamic feedback this possibility can largely be ruled out.

Ancient Israelite names, not reducible to recent feedbacks, are also provided by the “Kano Chronicle” for the first kings of the city of Kano – the most important written sources for Hausa history. It was Herbert R. Palmer, whom Henige credits with having ‘purveyed outside information to traditionalists who were eager to put them into use’ (2007:285), who translated and annotated the Chronicle in 1908 and republished it in 1928. Contrary to the expectations concerning feedback, it was certainly not Palmer who suggested these names to the Chronicle’s final author. Although he was indeed searching for such evidence, he overlooked these early Israelite onomastic elements because they were so strangely disfigured (1928 III:92–97). Moreover, in both Kanem and Kano, the Israelite elements perceptible in historical written traditions are also recognizable in oral traditions – fossilised names like Bremmi/Abraham and Biram/Abraham – and, even more so, on the cultic level in the local Ark of the Covenant called *munē/sakīna* and *dirki/cūkana* (= *sakīna*).⁵

With respect to feedback, Henige is more outspoken in his reference to S.J. Hogben’s work on the Northern Nigerian Emirates (1930). He notes that the 1966 edition of

⁵ Lange (2004a:236, 246, 556–557). For the historical identification of the *munē* by local scholars with the Ark of the Covenant of the Israelite King Sau, the koranic *sakīna* see now Lange (2006b).

the book includes king lists which were much longer than those of the first edition of 1930, thus clearly insinuating the addition of worthless later inventions. However, this supposition can easily be discarded. The first book has 204 pages and one author, the second has 638 pages and was composed by two authors – S.J. Hogben and Anthony Kirk-Greene (1966). The preface explains that after independence there was a growing need for history in the country (1966:xiii). Was that the reason for inventing long king lists? Certainly not, because most of them can be shown to have been available even before the first edition of the book. Indeed, Hogben summarises the history of Kano on the basis of the “Kano Chronicle” but his genealogy at the end of the subchapter only lists the Fulani Emirs of Kano (1930:79). For Daura he disposes of Palmer’s “Sudanese memoirs” but only provides the genealogy of the last five generations of the kingdoms, which has 11 names instead of 58.⁶ For Zaria he cites the article by Arnett of 1909 giving the names of 58 pre-*jihād* kings, all of which he omits, publishing only the list of the Fulani Emirs (Hogben 1930:89). Similarly for Katsina, where Palmer’s article of 1926/27 has the names of 40 pre-*jihād* kings, and so on.⁷ Thus Henige largely overestimates the importance of feedback and the spurious invention of historical source material and underestimates the arguments in favour of ancient and fairly stable traditions and the validity of the comparative approach underlying my book and other recent studies dealing with ancient Near Eastern remnants in West Africa.⁸

Still concerning king lists, Henige credits me with working on those of Gobir. In a minor footnote I specifically stated that I could not make sense of the names on that list (2004a:242, fn. 96). On the basis of Kühme’s work, he mentions that the list has over 372 rulers covering over 7 000 years, showing reigns in years and months. What does that have to do with my work? As a matter of fact, I specifically suggested to my student Walter Kühme that he should not include the king list in the appendix of his published thesis since he did not use it in his valid analysis (Kühme 2003:223–234). It is not my work, and neither Kühme nor myself has ever made any statement as to its historical validity. When I began to probe it, the test failed. Does that mean that ‘I desperately want to take the Gobir king list seriously’? Should we dismiss Central Sudanic king lists off-hand as being historically useless, without previously testing them?

Henige’s dismissal of my work on the Kebbi king list deserves more serious consideration. In this case I definitely claimed that among the 33 early names on the list we find 14 Babylonian, Kassite and Assyrian royal names. This surprising conclusion is backed up by some arguments none of which is given any consideration. Suffice it to say that according to John Brinkman – one of the foremost specialists on matters of Assyrian and Babylonian king lists and chronology – Burnaburiaš I. was ‘the first Kassite monarch reasonably well attested as king of Babylonia’ (1976:467). The Babylonian

⁶ Hogben (1930:81), Palmer (1930 III:142–143)

⁷ Palmer (1926/27:221–225), Hogben (1930:99)

⁸ Jäger (2001:169–184), Kühme (2003), Lange (2004a, 2005:265–282, 2006a:303–345), Weisser (2004)

king list, known since the end of the nineteenth century, places his name in the tenth position of the line of Kassite kings. Where did I say that he was ‘the first Kassite hegemon’ (Henige 2007:285)? His rulership over parts or the whole of Babylonia makes him a possible candidate for the onomastic ancestry of Burumburm, the first king of the Kebbi list. Among the other Kassite kings included in the Kebbi list is Burumburum II., corresponding to Burnaburiaš II., in both cases with the duplication of the same name – other Kassite royal names appear in the Babylonian king list three or even four times. Altogether there are five Kassite kings included in the Kebbi list. The indeed unexpected presence of Kassite royal names in West African traditions is confirmed by other lists like the *Dīwān* of Kanem-Bornu, where Kudur Enlil, Kututuru in Kebbi, is remembered as Katūr ‘King of the world’ (Lange 1977:66). A particularly significant onomastic relict of the ancient Near East in the Kebbi list is Dundun-Fāni, a double name composed of Didānu and Ḥanū, designating the ninth and the tenth ruler of the Assyrian king list. However, these names should not be looked at in isolation. Their ancient Near Eastern origin is supported by the royal Kanta legend of Kebbi, which by name and content goes back to the national Assyrian legend of the Mesopotamian foundation hero, Sargon of Akkad.⁹ Here, then, we have a number of disturbing resemblances and coincidences which are far from being purely accidental. Undoubtedly, the occurrence of these names in West African king lists has to be tackled in a new and audacious way.¹⁰

Ancient Near Eastern onomastic elements in the written and oral king lists of the Central Sudan should be seen in conjunction with certain figures remembered by oral traditions and their names. Most significant in this respect is Shango, a figure occupying a particularly predominant role in Songhay and Yoruba mythology and legend. Like certain political terms, the name can be considered to derive from Sumero-Accadian designations of high political and priestly offices.¹¹ Asking questions about their spread into West Africa is not a matter of textual criticism but of historical reconstruction based on probability. Suffice it to say here that criticism based on the rejection of the phonetic similarity of names without the consideration of parallel contexts is as unfounded for the pre-Islamic period as the overstressing of the feedback theory for the Islamic period. The similarity or identity of a dynastic name in Africa and the ancient Near East should not only raise doubts and suspicions but also intellectual curiosity as to their possible connection.¹²

⁹ Lange (2004a:252–254, 323–330, 560–561)

¹⁰ More details about the surprising occurrence of Kassite, Babylonian, Urartian and Assyrian royal names and their relation to the fall of Assyrian empire will be provided in Lange (in press) and other forthcoming publications.

¹¹ Lange (2004a:560–561). See also Lange (1994:217–236, 2004a:239–242).

¹² New insights pertaining to the repercussions of the fall of the Assyrian empire in West Africa were presented in a conference paper read on 12 July 2007 at the University of Bayreuth (Lange in press).

A strange aspect of Henige's criticism concerns my supposed unawareness of trends in biblical studies and hence my closeness to fundamentalist circles (Henige 2007:285). The first point is not totally unfounded since I did not use the results based entirely on the current text-critical tendencies in biblical research because they do not help much in revealing the cultural realities of the different periods concerned. However, it must be noted that there are far more critical approaches towards the books of the Bible than the different contemporary tendencies in textual criticism might suggest. The 'myth and ritual school' mentioned specifically is only one of them (Lange 2004a:559). They all go beyond the surface of the text to try and uncover the hidden social, political and religious realities alluded to by comparative analysis. Is it ignoring the biblical sceptics to make use of the work of such radical critics as Flemming Hvidberg, Sigmund Mowinckel and Tryggve Mettinger? That Abraham, Moses and David are remembered in West Africa does not confer on them the status of historical beings but rather points to their artificial legendary character based on earlier myth. Thus, evidence derived from the Hausa tradition suggests that Abraham was once considered a dragon fighter and hence the mythological figure behind him must have been a Baal-like deity (2004a:235–236). Also, the cult-mythological pattern pertaining to the dying and rising God discovered in Ile-Ife presents formidable challenges to the uniqueness of the phenomena of resurrection (2004a:367). Is there more to enjoy biblical sceptics and to upset Christian fundamentalists? And what about my attempts to use Ugaritic textual material from the thirteenth century BC in numerous developments of the book and documented in some additional articles?¹³ Should they not also be considered as examples of a critical approach towards the multifaceted Canaanite-Israelite society? It is absurd to see any similarity between my supercritical approach of the books of the Bible and that of the proponents of strict textual analysis, whether with respect to the Hebrew or the Greek Bible.¹⁴

Henige also asks why I did not explain biblical or koranic elements in West African cultures by considering 'reciprocal and recurrent influences and assimilation along shifting and porous Islamic/non-Islamic frontiers in West Africa for over a millennium' (2007:285). The answer, of course, is that this approach, being an enlarged version of the feedback theory, is by definition only applicable for the periods of Islamic influence. It disregards a vast field of African history predating these periods and overemphasises the importance of Islam. Of course in certain instances feedback has changed the original message of oral traditions. But more often than creating entirely new narratives, it

¹³ Lange (1999, 2005, 2006a)

¹⁴ My critic may be interested to know that this and other studies have been favourably reviewed in the "Zeitschrift für Alttestamentliche Wissenschaft", the flagship of German Old Testament research, which presently favours the text critical approach (M. Köckert, "Neujahrsfest der Hausa", 2000:477; E.J. Waschke, "Königin von Saba", 2001:120; J. Ch. Gertz, "Das Böse in der Geschichte", 2006:130; O. Kaiser, "Ancient kingdoms", 2007:145; J. Ch. Gertz, "Gründung der Hausstaaten", 2007:286).

influenced earlier accounts by adapting and interpreting ancient messages according to new contexts, thus preserving their basic meaning in spite of a changing world. Such a positive approach to oral traditions recognizes their intrinsic value as historical sources, while the feedback-invention theory nullifies their pre-Islamic – in other regions pre-Christian – historical meaning by giving it the minor role of elucidating only the impact of monotheistic religions. Who then ‘denies the possibilities of producing autonomous early Africa history’ (Henige 2007:286) the feedback theorist, or the proponent of ancient Near Eastern influences who pleads for early state foundations at a precise period of time by means of an outside stimulus and leaves open the possibilities of local contextualisation and later restructuring? No doubt, in the latter case, the scope of African history allowing for the precise reconstruction of origins is enlarged by many centuries.

One word is needed about the longest quotation in the review, which concerns the difference between the Hausa and the Azna as compared to the distinction between the Israelites and the Arabs, and attempts to demonstrate the durability of oral traditions by pointing out their connection to the main social cleavages in two related societies. If the national oral saga of two societies indeed goes back to the times of initial common residence – thus implying in Henige’s words ‘social mitosis’ – it would not be surprising if the distinction between their main figures reflects the difference between the two existing social moieties, in one case the Hausa and the Azna, in the other the Israelites and the Arabs, or rather the Israelites and the Levites.¹⁵ On the Hausa side of the comparison, the long remembrance of the link between a particular moiety and a legendary ancestor of the Abraham-Sara-Hagar narrative is evidenced by independent oral traditions. Indeed, here the national saga is transmitted only in Daura, the cultural centre of Hausaland. But in some Hausa states, the leading clan elder of the oppositional Azna moiety refers his origin back to Karbagari/Ishmael and hence likewise to a figure in the Israelite classification scheme. On the Israelite-Arab side of the comparison, a similar repercussion of the national saga in individual clan traditions can only be hypothesised. If this African evidence of early Israelite and Arab history can be helpful in solving the Near East crisis, why not use it?

There is one minor concession I am prepared to make to my critic: abbreviations like ‘FN97, 16–32’ are indeed rather cryptic and meaningless for present-day readers. Even the more often used indications of the name of the informant and his group or locality – for instance, ‘Lokore/Obameri FN01, 108–109’ – must remain allusive as long as the main results of my field research have not been published. Indeed, I should have mentioned, as I did elsewhere, that my notebooks will be accessible at the University Library of Bayreuth after the end of my active career (Lange 2006a:303n*). In my opin-

¹⁵ The matter has to do with the distinction between Kohanim and Levites (Lange 2003:13, 18–27). For the Levites among the Yoruba, see Lange (2004a:368–369).

ion, authors who have gone to the trouble of assembling new material in long and often painstaking field research should have the privilege of using their own material at leisure for as long as they decide to do so without being expected to make it immediately available to the scientific community, even though technically that might now be possible. To my satisfaction, this insistence on my field notes would seem to imply that Henige admits that field research on cult-dramatic performances during state festivals may be of great interest for historical research.

Last but not least, my comparative approach is a far cry from the box of simplistic diffusionism into which Henige wants to put me. There were, of course, earlier attempts to trace outside influences on African cultures. On account of their racial undertones, they are sometimes branded the Hamitic hypothesis. However, these early suppositions were restricted in scope and limited in elaboration. Leo Frobenius distinguished three streams of cultural influences, as he calls them: first, an early pre-Phoenician Mediterranean input via the Atlantic Ocean that gave rise to the Yoruba culture; secondly, an ancient Libyan-Berber impact from the Sahara and the Niger bend that left behind, among others, the dragon legends; and thirdly a Christian-Byzantine inroad mixed with Sassanid Persian elements (Kisra) coming from the Nile valley and leading to the foundation of Nupe and the Borgu states.¹⁶ Hermann Baumann and others suggested, without going into much detail, pre-Greek and pre-Roman Mediterranean influences generating the neo-Sudanic state tradition (1940:56–71). In early post-colonial times, the idea of the spread of African states from a single centre was revived by Roland Oliver and John Fage within the framework of their Sudanic state theory, which stipulated a common origin for African states in the region of the upper Nile and a diffusion of the ideas of the state in the period following the fall of Meroe (1988:31–38). As a consequence of the prevailing post-colonial paradigm, no attempt has been made in recent times to verify, assert or reject any of these or any other suppositions concerning the ancient Mediterranean, post-Roman Near Eastern or Egyptian origin of the states in sub-Saharan Africa on the basis of original research.

The argument put forward now insists on pre-Islamic Semitic and ancient Near Eastern connections, thus implying quite different orientations. It takes account of precise textual and other evidence from both sides of the picture in a fairly complex manner. This is not just a *réchauffé* of outmoded ideas (2007:286), but a new way of interpreting similar phenomena on the basis of precise long-distance anthropological-textual comparisons undertaken for the first time. Additional research strengthens the idea of the Phoenician-Canaanite factor in ancient West African history and highlights the importance of the Assyrian contribution dating from the fall of the neo-Assyrian empire

¹⁶ Frobenius (1912/13a:347–375; 1912/13b:122–151, 335–344). Becker rightly criticizes the coastal orientation of the Yoruba culture and the idea of a mixed Byzantine-Persian culture (1913:47–58) but overemphasises the importance of Islam on the neglect of the possibility of pre-Arabic Semitic linguistic influences (1913:58–59).

in the second half of the seventh century BC. Many of the ethnic groups the Assyrians deported would appear to have been trying to escape the retaliation of the formerly oppressed local population.¹⁷ This thesis, postulating the migration of certain groups within the general framework of rather limited Punic trans-Saharan trade, is supported by a considerable number of precise parallel phenomena pertaining to different cultures of the ancient Near East.¹⁸ It locates the point of origin of numerous institutions and culture traits on the Levantine coast of the Near East and explains a number of key aspects of the state traditions of the central Sudan in a fairly coherent way. Therefore it should not just be dismissed as wildly diffusionist but considered an attempt to account for many cultural features connecting the West African with the ancient Semitic world which would otherwise remain inexplicable.

In his final verdict, Henige admits that none of the main irritating results presented in the book, as he sees them, is entirely implausible, namely the truth of the biblical image of the Patriarchal Age, the great durability of oral traditions and the survival of the culture of the ancient Near East in West Africa. However, he totally rejects the combination of all three. As we have seen, the book in no way supports a purely textual and conservative interpretation of the Bible. Therefore only two of the three postulates remain. Would that not be a good reason for a great sceptic like Henige, basing himself on the laws of logic and evidence, to revise his initial judgement?

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¹⁷ See Lange (in press, submitted).

¹⁸ For corresponding studies later than my book, see Lange (2004b, 2005, 2006a). I may be forgiven for referring in a reply to a review to more recent publications than my book. Henige, after all, also deals exclusively with the problem of diffusion and survival without considering my research concerning the medieval history of the Middle Niger.

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