Sources, with Particular Reference to the Southern Sudan

Having taken my degree at Oxford in modern history, after having specialized in the same subject at school, I have, since I began to read anthropological literature, been astonished at the very uncritical use of sources displayed in it. There is little, often no, attempt made, such as any good historian would attempt to make, at a critical assessment of the sources from which his information is derived and on which his conclusions are based. This paper is devoted to some observations on this topic.

One is really horrified at the way in which Frazer in The Golden Bough quotes authorities on page after page through volume after volume without telling us anything about them or why he accepts their testimony. It is often very difficult to track down Herbert Spencer's authorities in his many volumes of Descriptive Sociology, e.g. African Races (2nd ed., 1930). Tylor does not even give the sources for his famous essay "On a Method of Investigating the Development of Institutions: Applied to Laws of Marriage and Descent" (1889). Nor did Hubert and Mauss for their essay "Esquisse d'une théorie générale de la magie" (1904) ever publish their sources. Hobhouse, Wheeler and Ginsberg in their The Material Culture and Social Institutions of the Simpler Peoples (1915) were quite unable to control their many sources, and good, bad, and indifferent were cited as though they were all evidence of the same value (it may be said that Murdock—Social Structure, 1949—is a more recent and even more disastrous example). It is the same with all other writers in the earlier days of social anthropology, e.g. in this country (England),

* It is based on a lecture given at the Frobenius Institut at Frankfurt a. M. in 1968.
Crawley, Marett, Lang and all the other scissors-and-paste compilers. Industry and learning they may have displayed, but generally little critical scholarship. It was the same in Germany: Wundt, Bastian, Ratzel, up to people like Ankermann (e.g. his “Verbreitung und Formen des Totemismus in Afrika”, 1915), or Frobenius; what reliability can we place on the sources for some of the maps in his *Atlas Africanus*, e.g. “Der König ein Gott?”, or on those for e.g. Baumann’s essay “Likundu: die Sektion der Zauberkraft” (1928)? But I have said all this, or something like it, before. How rightly Van Gennep wrote in 1914: “Même de nos jours, la critique du document est loin d’être aussi rigoureuse que celle que font subir à leurs documents les historiens” [42, p. 106].

I would now like to give some examples of what I mean by the need for critical assessment of sources, and for this purpose I take the Southern Sudan as my area of discussion. I do this because it is necessary for my purpose that I should take my illustrations from a region of which I have had first-hand experience and about which I can speak with some confidence.

I must say at once that I do so with some reluctance because it means that, as you will see, I shall have later to criticize somewhat severely the writings of my teacher and friend the late C. G. Seligman (together with his wife, B. Z. Seligman). It may then be asked why, since I provided so much of the material which appeared in their book on the peoples of the Southern Sudan [36], I did not make these comments before it was published. The answer is that, except for the parts for which I supplied the bulk of the information, I did not see the book in typescript, and I did not see in their final form the parts for the contents of which I had some responsibility—I would certainly not have accepted some of what was said had I done so. I gave the authors such information as I possessed but they put their own interpretation on it. I myself wrote no part of the book whatsoever. The Seligmans alone must take full responsibility for what they wrote, as indeed they said they were prepared to do. I annotated their book in the margins shortly after its publication but I could not bring myself to making public their errors during their lifetime.

I suppose I am the person on whom this duty should fall as I know better than most the ethnography of the Southern Sudan. It is for me therefore to correct certain errors and to point out certain inconsistencies and confusions in their book and in other writings
before it is too late, for once a statement, however false, remains for long unchallenged it becomes, especially if it appears to have authority behind it, solidified and is then repeated over and over again and seemingly for ever and ever, however overwhelming the evidence against it may be. For example, I later remark how Schweinfurth says that the chief weapons of the Nuer are bows and arrows \[33, I, p. 91\]. This is not, and could never have been, the case. How does one establish the truth when error already holds the field?

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I could take any of the travellers in the Southern Sudan for my first illustrations of the need of caution in the use of writings about its peoples—Piaggia, Marno, Casati, or any other—but I must restrict my comments to a selection from them, chiefly on the eminent Russo-German botanist Georg Schweinfurth (1836-1925), just referred to.

Much of what these early travellers wrote is of little value unless one knows oneself the country they travelled through and the people they met well enough to interpret their information in the light of that knowledge. Take Ferdinand Werne for example \[43\]. He was a vain and vituperative person with a great hatred for the French; and it must be said also that he had a very superficial understanding of what he saw among the native peoples with whom he came into contact on the Nile—as is evident on every page of his volumes—and even less of what he was told (if he spoke and heard Arabic as he transliterated it must have sounded very odd indeed, and I would guess that it was little more than a smattering). He makes such statements as that among the Dinka the moon is an object of veneration \[I, p. 170\], and that the Shilluk worship a tree called by the general name of Nigama \[II, p. 294\], and that they have no knowledge of a Supreme Being \[II, p. 295\]. He did not even understand—at least this is my opinion—that what he calls “watchhouses” and saw everywhere in cultivations and thought were erected against “thieves or beasts” \[II, p. 108\] were for the purpose of scaring durra birds from the crops. I should however add that, though I accept the statement with reserve, according to C. G. Seligman “the Acholi, like the other Nilotes, often erect high platforms near their villages so that a look-out can be kept over their neighbours” \[34, p. 729\]—whatever that may signify!

It must be remembered that these early voyages up the Nile, one of which Werne accompanied, were intended to explore its upper

1. A popular and not very informative article.
regions to discover wealth for Muhammad Ali and that if the explorers
had any ethnographical interest at all it was incidental to their purpose,
and it was in any case difficult to reconcile with their plundering of
the natives whenever they were able to do so without risk to them-
selves. Few of the earlier travellers seem to have understood how
their depredations appeared to the natives, or cared how they appeared
to them. Wilhelm Junker, for example, sometimes gives the impres-
sion that he was insensitive to the sufferings his Arab friends inflicted
on them [23, I, pp. 262-263], a lack of human feeling he seems to have
extended even to his fellow countrymen Borndorf and Kopp. It was
in any case difficult for him to be critical of the Egyptian so-called
government when he was entirely dependent on it for food and
security.

One of the most controversial persons among the first explorers
of the Southern Sudan was the Welsh mining-engineer John Pether-
rick [29]—a not very reliable authority or estimable person. I must
mention again, and not for the last time, knowledge of Arabic, because
I doubt whether his was as considerable as he would have us to suppose
(records of rather involved conversations by several of these early
travellers—Casati was a big offender in this respect—always make
me suspicious; a good example from Petherick's book is the alleged
speech by an old Zande on pp. 458-459. It is clearly fictitious).
I have no doubt that his account of incidents should be treated with
reserve and that his knowledge of native custom was negligible, e.g.
what he says about Hassanyeh marriage arrangements [pp. 142-144]
is difficult to accept, and what he says about Zande cannibalism
[p. 469] must be rejected. On the whole, this first account of his
travels is ethnographically of little worth.

In 1861 Petherick, having recently married, returned to the Sudan
accompanied by his wife to meet there the Speke and Grant expedi-
tion. Their disastrous journey with its enormous caravan and its
unhappy and embittered end has little to do with this essay. He
and his wife published in two volumes their diaries and notebooks [30].
Ethnographically-speaking it is a very superficial book. A pretence
is kept up that Petherick was a competent scholar in Arabic, though
it is obvious, to me anyhow, that he was not. There are clues: aboo
meea (the golden-crested crane) is translated by “father of a thousand”
[I, p. 201]. I am quite sure that when his wife claims that he trans-
lated for her a most elegant (as given in English) Muslim prayer the
statement cannot be accepted [I, p. 104]. So erroneous and vague
are so many statements in these volumes that one indeed sometimes
wonders whether Petherick knew among which people he was, e.g. he
appears to mistake Dinka for Shilluk [I, p. 102]. Apart from other
evidences the word benj, the Dinka for “chief” might have alerted
him. Then, while telling us that the Nuer "believe in no after life, nor in God, neither in good nor evil spirits", he adds that "they believe that the spirits of the deceased visit them and dreams are their converse. Thus the fear of the dead prevents them from robbing orphans" [I, p. 321]. Apart from the evident contradictions in this passage, it does not at all correspond to the facts. Then we are told that the Shilluk pay their king taxes: "An annual tax, consisting of one-tenth of the yearly produce of grain and cattle, is scrupulously imposed and levied" [II, p. 3]. No one else says so, and I can only suppose that some Arab Muslim put the idea of a tithe into his head. Then again, we are told that the king of the Shilluk "acknowledges his fealty to Jockdeng, Sultan of the Bonjack [Anuak], by the payment of an annual tribute, amounting to one-tenth of his revenue" [II, p. 4]. This also makes no sense; and Jockdeng could not, I think, be an Anuak name either. I give two final examples. "The aborigines [the Beli-Sofi group] speak the same language as the Djour [Luo] of the Bahr il Gazal, and are part of the same tribe" [I, p. 247]. This is quite wrong. They do not speak the same language or even languages remotely like each other and they are in no way politically in association. Then again, about the Moro people: "After much cross-questioning through the medium of an interpreter, I found this tribe had neither a belief in the existence of God, nor in punishment or reward after death; neither was any kind of religious worship or ceremony in any form, whatsoever, practised by them" [I, p. 274]. To say the least, this statement is somewhat wide of the mark.

With regard to the earlier mentioned Hassanyeh, Brun-Rollet had said much the same as Petherick: that custom allows the young girls of this Bedouin tribe when they marry "le droit de prélever sur les droits de l'hymen la part des étrangers, qu'elles appellent leur quart franc" [6, p. 43]. There is a sort of ethnographic grapevine along which passes what someone has asserted until it becomes accepted as an established truth and repeated as such. This particular canard seems to have been started by the Frenchman Dr. Peney, at one time medical inspector of the Sudan, who died at Gondokoro in 1861, who told Bayard Taylor, a naïve American, when he was in Khartoum in 1852, about the Hassanyeh:

"The rights of women, it appears, are recognized among them more thoroughly than among any other savage people in the world. When a woman is married, her father states that one fourth of her life thenceforth is reserved for her own use, and the husband is obliged to respect this reservation. Every fourth day she is released from the marriage vow, and if she loves someone better than her husband, he can dwell in her tent that day, obliging the husband himself to retire. Their hospitality is such, moreover, that if a stranger visits one of their settlements they furnish him, for four days, with a tent and a wife. They should add a family of children, and then their hospitality would
be complete. No reproach whatever attaches to the woman, on account of
this temporary connection. The Hassaniyeh, in other respects, are not more
immoral than other tribes” [39, p. 325]. (Whatever that may mean)!

The fact that a statement is made by several writers one after another
need not by any means imply independent witness.

I cite another example. The sententious Sir Samuel W. Baker
(1821-1893) was, I suppose, the most famous of the earliest travellers
in the Southern Sudan, as well as being one of the most disagreeable
and stupid of them. I have had occasion elsewhere [11, pp. 6-7] to
quote what he said in 1866 about the Northern Nilotes: “Without
any exception, they are without a belief in a supreme Being, neither
have they any form of worship or idolatry; nor is the darkness of their
minds enlightened by even a ray of superstition. The mind is as
stagnant as the morass which forms its puny world” [2, p. 231]. In
view of the very extensive literature on the religions of these same
Nilotes which has now been published, one can only wonder at such
confident ignorance and bigotry, which are also evident through the
pages of his other books [3 and 4]. As for his knowledge of Arabic,
it is interesting that in a letter written in 1861, when Baker was in
the Sudan, he asks Petherick to find him a man who could speak
English, French or German, and Arabic since he required him “as
Arabic interpreter and servant” [30, II, pp. 107 and 109].

Speaking once more of religion, I will bring on to the stage again
the successful Savoyard trader Brun-Rollet (1810-1858) who made
expeditions up the White Nile in the early forties and founded a station
in Bari country in 1844. He claimed to understand languages of
the South [6, p. 13], but he obviously did not. With a considerable
knowledge of the peoples and topography about which he wrote some
sense can doubtless be made out of what he said and of the fantastic
illustrations in his book, and even some use can be made of the infor-
mation he records; but what are we to make of his account of the
Nuer? He says that they have a king (“roi”), prostitutes (“prosti-
tuées”), whatever we are to understand by that, and furthermore,
believe it or not, a pope [p. 241]. He tells us: “Les Nouer ne recon-
naissent qu’un seul Dieu, qu’ils appellent Néar. Le chef du culte,
appelé Dowa, est une sorte de pape pour lequel on professe une véné-
ration extrême, voisine de l’adoration” [p. 223]. They believe him
not only to be above human needs, such as that of sustenance, but
also to be immortal. And so on; of course all rubbish. We are also
told that the Nuer fast during the month of Ouich, which corresponds
to the winter solstice: they eat apparently only fish and wild fruits
during this lenten fast (“ce carême”), but they are allowed to drink
[p. 224]. To this, one of the best informed of these travellers, the
Frenchman Jules Poncet, has added that it is true that the Nuer
have no more religious belief than the other Negroes: "Ils ont néan-
moins une idée très-vague d'un être divin", and they do not fast at
all as Brun-Rollet would have it [31]. Poncet's Arabic must also
have been slight.

I am not of course saying that nothing can be gleaned from these
accounts but that the gleaning must be with discretion. It is essential
to decide in the light of our knowledge of the persons and the circum-
stances what observations are likely to be accurate and what state-
ments are likely to be of doubtful validity. The same applies to
what Schweinfurth—and indeed everybody—records, and I should
say that in his own field of botany Schweinfurth was without peer.

I have already placed on record some comments on Schweinfurth's
testimony [12, pp. 137-141]. In the circumstances in which he travel-
led his ethnographical observations could hardly have been other than
superficial. He states, as I have already said, that bows and arrows
were the chief weapons of the Nuer, whereas they have never used
the bow nor have a word for it. Schweinfurth, who could have had,
as we can see from his itinerary, little opportunity to make any
observations in the matter, probably relied, without acknowledgement,
on what Werne said. (In the frontispiece-illustration of his volume 2,
Werne [43] has a drawing of what he describes as a Nuer bow and
arrow. Where the arrow might have come from I cannot say. The
bows are referred to on p. 206 as "artfully-constructed bows" [see
also pp. 210, 219, 284].) This so-called bow is the Nuer dang used
in making orations at wedding ceremonies. It is in fact not a weapon
at all and one could not shoot arrows with it however hard one might
try. The Petherericks make the same obvious error when they write
that the Nuer "carry a bow turned like a shepherd's crook at both
ends, a club, and a large lance; the club is thrown at an enemy, but
the lance is never parted with" [30, I, p. 319]. This is of course
absurd. It is the lance (spear) which is thrown, not the club, which,
if carried at all, is retained in the hand. We are not told what use
is supposed to have been made of the "bow", the dang just referred to.

It may be seen from Werne's account how haphazardly native
specimens were collected by his crew as curious, no doubt then sold
to European collectors in Khartoum without much regard to prove-
nance. Junker's account is perhaps even more revealing. He con-
tantly speaks of his ethnographical collections, but it is clear that
often he had little, or no, idea of what he was collecting. It appeared
to him to be sufficient that specimens were described as belonging
to the peoples of the Southern Sudan, without specifying which people.
A typical entry, referring to his residence in Khartoum: "I lost no
opportunity of securing samples of natives arms, dress, ornaments,
and other industrial products. For this purpose I engaged agents
among the Greek and other European residents" [23, I, p. 195]. Elsewhere in the same volume he says: "Besides the numerous articles I had brought from Makaraka and Ladó, there was another collection of ethnographical specimens made by someone else, which I had acquired by purchase" [I, p. 515]. Again on p. 515, we hear that Gordon gave him a collection from Darfur and an unpacked collection of ethnographical specimens from Shaqqa. So heaven only knows what went into the museums of Saint Petersburg, Berlin and Vienna! Furthermore no clear, if any, provenance at all is given for many of the specimens illustrated by drawings in his volumes, e.g. the drawing on p. 264 described as "Bows and arrows of the White Nile Negroes" (cf. Sir Samuel Baker [2], plate opposite p. 1, "Arms and instruments of various tribes"). Some of Petherick's drawings have no titles. Indeed I feel I must say here once and for all that many specimens in museums are wrongly or inadequately labelled and that the alleged location, if any is given, and use, if any is mentioned, of any specimen and the authority, if one is known, for any statement about it should be most rigorously scrutinized before it should be cited as evidence of anything, especially as historical evidence.

The matter of the dang just referred to is further complicated by Schweinfurth giving a drawing of it and describing it as a Dinka instrument "for parrying club blows" [33, I, pp. 155-156]. I do not think it could be at all effective for such a purpose or is ever used for it. The question is raised again later with regard to what the Seligmans say about the Shilluk dang. It may be added here that Schweinfurth also says that the Dinka do not use the bow and arrow [I, pp. 90-91]; and though he does not say so he might well be thought in what he does say to imply that the other Nilotic peoples do not use the bow and arrow also. However Poncet tells us in his book, which is mostly about the slaughter and wounding of elephants, even greater sufferers than the natives from these plunderers, that the Bor, Kitch and Toudj Dinka do use them. We know also that the Aliab Dinka use them, though Poncet does not say so. The same author tells us that the related Atwot use them [31, pp. 53 and 54].

Then, in spite of the many days he claims to have spent in Dinka kraals, Schweinfurth never noticed the most striking feature of their herds, the training of the horns of the oxen; nor, I believe, did any other traveller of the period. He says that the only mutilations of the body practised by the Mangbetu were boring of the ears and circumcision. The Mangbetu, at the court of whose king he stayed, are of course famous for their artificial deformation of the skull (which was noted by Junker [23, p. 242]). Schweinfurth almost certainly lifted information from Piaggia's account, and without acknowledgements (I gave some examples in my earlier cited essay).
What he says about Zande cannibalism cannot in any circumstances be accepted and reveals much ignorance and dogmatism. It may indeed be doubted whether he saw much of the true Azande at all, as distinct from Amadi and Abarambo, of whose occupation of the country he passed through he seems to have been unaware. Nor does he adequately explain how he made up for his loss of his notebooks in a fire. I need scarcely add that he spoke none of the languages of the Southern Sudan; he does not even provide evidence of having had (at that time) much proficiency in Arabic. As I have said before, such involved conversations [e.g. 33, I, p. 371] always make me sceptical. In one place he gives the impression, though I do not think intentionally, that he understood one of the Nubian dialects [II, p. 122]. I am sure he did not. He was at the mercy of a Nubian rogue, though an open-handed one, whose enormous caravan he accompanied, and so was cut off from more than the slightest acquaintance with the native peoples he wrote about. It must be said that according to Schweinfurth, who obtained a German award for him, this rogue was not to be accounted among the adventurers who came from Darfur and Kordofan, what he calls "incarnations of human depravity" and "cattle-stealers and men-hunters" [I, pp. 190 and 236]. But in the conditions prevailing, no one could avoid being in some degree contaminated, tainted with murder, theft, and every form of cruelty, and traffic in what was called black ivory. So while it has never been determined whether the rumours and accusations that Petherick was engaged in slaving [30, II, pp. 69, 125-126, 139] were true, the Foreign Office took the view that the traffic was incidental to and arose out of the ivory trade and cited the fact that slaves were conveyed in one of Petherick's own boats, though without his knowledge, in support of this opinion [I, p. 137; II, p. 150]. Like other Europeans, Petherick certainly raided Negro herds for cattle [I, pp. 228-229]. Even Gordon and Emin could not escape collusion, to use a mild term. Schweinfurth is candid enough to record that on his arrival at Khartoum after his explorations his servants were arrested for trading in slaves, an accusation he could not deny. Junker frankly admits that he had slaves [23, p. 307].

Our cross-examination must continue for a little while. Schweinfurth has clearly got matrimonial arrangements among the Bongo quite wrong. If, he says, when a man divorces his wife "her husband retains the children, her father is bound to refund the entire wedding-gift that he received" [33, I, p. 302]. This cannot be. We are also told about a minstrel, who is also figured, that he is called *nzangah*,

1. The words are even stronger in German (cf. *Im Herzen von Afrika*, pp. 100 and 125).
a word also, says Schweinfurth, used for a prostitute (whatever he may mean by that) [I, pp. 445-446; II, pp. 30-31]. In point of fact the word means neither "minstrel" nor "prostitute" but "madness" (a woman playing a Zande harp like that in Schweinfurth's illustration is figured by a drawing by R. Buchta in Junker [23, p. 129], where it is described as a "Nubian female minstrel"). Then we are told that the Azande "only use their drums and horns for the purpose of signals" [33, I, p. 446]. This is an incorrect statement (as he himself more or less admits in [II, p. 19]); it is certainly untrue about more recent periods. Schweinfurth did not always know what he was talking about. He says that Piaggia did not witness a single act of cannibalism [I, p. 434]. In fact he describes an occasion on which human flesh was devoured (this is admitted by Schweinfurth in [II, p. 17]). He keeps on talking about "slaves" when the word makes no sense at all; and I may say that writers of the time tended to use the word very loosely and even without meaning: e.g. Ponchet tells us that the Nuer are rich in agriculture (which is not the case) and that they leave the cultivation to their "eslaves" whom they capture from neighbouring peoples and even among themselves [31, p. 37]. Slaves have never been known among the Nuer and would be incompatible with their ideas and way of life. Then Schweinfurth did not understand the difference between a Zande commoner governor (his behnky) and a prince [33, I, p. 479; II, p. 22]—rather an essential difference if one is to understand the political organization of the Azande.

We are told that the Azande do not pay bridewealth for their wives [II, p. 27]. This is not true, either today or in Junker's time, as he (Junker) tells us. There seems to have been a considerable muddle about the Zande word for "divinity". Schweinfurth has two words, gumbah (thunder) and bongbottumu (possibly the Zande word for "medicine of vengeance", bagbudumo) [II, p. 31]. Neither word could mean "divinity". He says also that "none of the natives of the Gazelle district may be credited with the faintest conception of true religion." (Whatever that may mean!) The account of Zande oracles leaves much to be desired. I find it surprising that Schweinfurth in discussing Zande artifacts does not tell us which sex made what, and in particular does not mention that the Azande are the only people through whose lands he passed among whom it is the men and not the women who are the potters [II, p. 25]. I find it surprising also that he does not mention the mabingo oracle of the Mangbetu [II, p. 121], considering, as we learn from Junker, how important and public a part it plays in their lives [23, pp. 224 and 246-247]. One need hardly say that when Schweinfurth attempts to explain the meaning of Zande words he finds himself in difficulties, partly I think because he did
not hear the sounds very well. For instance, if the Azande really called him *Mbarik-pah*, as he says they did, the name could scarcely mean, as he supposes, “leaf-eater” [33, I, p. 513; II, p. 202]—that would be *barikpe*. Nor do I believe that his protector Muhammad was called *Mbahly* “the little one” [I, p. 481]. The Zande word for small is *umba*; I think the name they gave him may have been *ngbari*, the “(warrior) termite” (Schweinfurth often heard an *m* sound when he should have heard an *ng* sound).

I have given some examples from Schweinfurth’s volumes, taken from his account of peoples of whom I am able to speak with some assurance, and I could furnish others. It will be noted that I have criticized him only on grounds of ethnographic fact. Had I cited omissions a more severe judgement could have been passed; for example, though he spent over two years in the Bahr al-Ghazal he tells us nothing about family life, kinship, clans, totemism and so forth among the Azande or any other people, matters with which the modern anthropologist would acquaint himself with soon after he took up residence among a people. It would not be unjust to assume that he made mistakes similar to those I have mentioned in what he wrote among peoples of whom I have no personal knowledge. But more than enough has already been said to show that the author’s account must be used with discretion. In some matters reliance may be placed on his statements and in other matters. It is not suggested that he fabricated evidence, though he may sometimes have embellished his account, but rather that he was not always in the position of being able to make correct observations and sound judgments from them. It is perhaps unfortunate that in their study of the institutions of the simpler peoples L. T. Hobhouse, G. C. Wheeler and M. Ginsberg, took him for their main authority on the Azande [19]. They might have been wiser to have taken Junker.

Perhaps I should add, however, that it is possible that what may appear to be a mistatement may sometimes be a misunderstanding, that the meaning of the German word is not that of the word by which it has been translated. When, for example, he says that none of the peoples of the Bahr al-Ghazal are acquainted with the use of tan in the dressing of their hides [33, II, p. 117], the German “Rindenextrakten” may have meant something different to him to what I understand by “tan”, though I doubt it; the Rev. Canon and Mrs. E. C. Gore give the names of no less than six different kinds of bark used by the Azande in tanning [15, p. 272]. So do C. R. Lagae and V. H. Vanden Plas [24, p. 237]. Likewise, when he says that cheese is a product utterly unknown among the pastoral peoples of North and East Africa [33, II, p. 327], what “cheese” means to me may be different to what “Käse” meant to Schweinfurth. All the same,
what is the jibaa of the Arab lands of North Africa if it be not cheese? Moreover, Junker speaks of the whey-cheese of the Sudanese Bedouin [23, p. 15]. Further, Professor Nicolaisen tells me in a personal communication that the Arabs and Berbers of the Atlas make butter-milk cheese and cheese from fresh milk, and this is also generally the case in the western part of the Sahara, whereas the Tuareg in the South near Ayr and Niger generally make cheese from fresh milk only. Also the Nuer certainly make what I call cheese, and whatever it refers to, the word is given in translation in various vocabularies of East African languages, e.g. for the Dinka annul [28, p. 144] and for the Acholi okulu cak [26, p. 31].

Schweinfurth lived to a considerable age and was able to bring out a revised edition of his book in 1922 in the light of the discoveries, mainly geographical, of later explorers, among whom was Wilhelm Junker (1840-1892) who, like so many of them, died young. He is a more reliable authority than Schweinfurth, having spent a far longer period in the Southern Sudan, especially among the Azande, and in closer contact with its peoples. However, when he is not recording his own observations but what those most bigoted and ignorant people of the Northern Sudan, mostly Nubians, told him what he says must be received with great caution (it should be said also that he admits that, at any rate on his first journeys, his Arabic was pretty poor [23, I, pp. 306 and 351]). I give a few examples.

He refers to the Nuer on the Sobat “who withdrew to the interior after harvesting their durra crops” [I, pp. 216-217]. I think he may have meant the Balak Dinka, for the Nuer, at any rate today, come to this part (the left bank) of the Sobat in the dry season only and cultivate in the interior. He tells us: “Among the Makarakas [Azande] the body of a dead chief is kept for a year seated on an angareb and smoked over a slow fire kept constantly burning, food and a pot of beer are placed before him, and at the end in the year he is interred” [I, p. 297]. No one else has recorded such a custom and it is highly unlikely to have been one, especially as the Makarakas Azande have no chiefs of high status. He says about Mundu, Abukaya and Abaka chiefs: “Five, ten, and even as many as fifteen female slaves are buried alive with the departed potentate” [I, p. 296]. This information, it would appear, was obtained from an Egyptian officer, and is palpably false, for these people have no, and never had any, such

1. The real scholar among these travellers would seem to have been the Austrian photographer Richard Buchta (1845-1894), many of whose fine illustrations adorn Junker’s volumes, to which he contributed also notes (he got his Arabic right in them). He travelled widely in the Southern Sudan and was companion, among others, to Emin and Gessi. Emin was of course a fine scholar.
chiefs or such slaves. We are later told of the Kaliká people on the authority of a so-called “Turk”, one Atrush, that the remains of the dead are deposited in a grave “together with the nearest relatives of the deceased, his cattle and all his effects. It also happens he [Atrush] told me, that a person receiving a deadly wound, with a poisoned arrow, for instance, has his own grave dug, and while still alive gets buried with his *nearest and dearest* and all his worldly property” [I, pp. 358 and 361].

Then he tells us that Ngettua was Ndoruma’s uncle [II, pp. 139 and 151]. He was Ndoruma’s brother. I cannot understand how Junker, who spent so long at Ndoruma’s court, could have made so elementary a mistake.² We are told: “But wherever cattle are bred in Negro land they are always tended and even milked by the men, never by the women” [II, p. 90]. Among adult Nuer only women may milk. Junker tells us that his musical boxes “usually struck the listeners dumb with amazement” and that by their performance he was “doubtless the means of shaking the belief of many natives in that delusion” [II, pp. 158-159]. The delusion was witchcraft. Junker’s comment was stupid. To Junker the native was always childish, unreliable and a liar; and he could not compare with the white man. But Junker was so often in error. For instance, he did not at all understand how Azande use the poison oracle, against which he railed in spleen [II, pp. 220-221]. He wrote: “The consultation is effected by means of the shrub *baenge*, which yields a red powder, and this powder when mixed with water is given to a fowl. If the bird survives the dose, the omen is good; if not, bad. Ours survived, as did a second supplied by me, whereupon Bani coolly suggested that the poison was bad; obviously he had mentally consigned us to a hard fate” [II, p. 206]. This is rubbish. The meaning of the death of the fowl could have depended on the way the question was put to the oracle. That the second fowl also survived was a sure sign to a Zande that the oracle was not working properly [I, p. 437].

Indeed, though Junker spent a longer time in the regions we are discussing (he was in Zandeland or on its borders from May 1880 to January 1884) than most of the other explorers and although he wrote three large volumes about his travels, he tells us really very little about the peoples he met. He was often at sea in political matters. He admits to lack of patience and humour. He does not appear to have spoken a word of Zande if one may judge from his attempts at spelling such Zande words as he records. This is clear also from the vocabularies he records [III, p. 279].

¹ Emphasis is his.
² I should say that I have not had an opportunity to consult the original German.
I have said before that long speeches alleged by travellers to have been delivered by them make me very suspicious of their veracity. A certain inventiveness may be allowed but the more flowery the speech the more certain I become that it could not have been made. Here is Junker trying to impress the Zande king Ndoruma on his first meeting with him, using his Mundu servant Farag Allah as interpreter:

"I enlarged on my proposed visit to his country, my object and friendly intentions, also pointing out that I would be attended by a small Government escort only as far as the frontier of his territory, and merely as a protection for my numerous carriers. After that I should expect, as a harmless private traveller, to be aided and safeguarded by the native rulers, of whom he, Ndoruma, was the first; in him I placed every confidence; to his loyalty, after entering his territory with the few foreigners whom he here saw, I wished unreservedly to confide my safety, as well as that of my people and property. All this I dwelt upon in one of those lengthy palavers which are so dear to the heart of the Negro, employing all kinds of figurative language, such as during the course of years I had picked up from the natives themselves. In conclusion, I assured him of my satisfaction at personally greeting him here, instead of through his envoys, hoping thus to learn from his own lips that he was willing to receive me as his guest, for on no other condition should I venture to enter his country" [II, pp. 103-105].

Can anyone believe that Junker could really have said all this in his slight Arabic or that, even if he could, his Mundu boy would have understood it, and even had he been able to understand it, would have been able to put it into Zande, which was not his language?

One more example:

"As the pilferings still went on, I now threatened in a loud voice to protect myself with my gun, Mambanga [a Mangbetu prince] being evidently powerless to prevent the depredations. He replied that he had no objection to my shooting the offenders; but at a private interview I explained that I should never think of firing on his subjects, and had spoken thus only to frighten them. At the same time, if things did not mend, I should certainly shoot myself, as was customary in our country when people were driven mad by their tormentors. This I said in a highly theatrical manner, and with all the suitable gesticulations, at which I had already become quite an adept. I even went so far as to thrust the muzzle of the revolver into my mouth, adding that this had all been previously arranged between myself and my brother, the Pasha [Gessi], who would thus understand the kind of treatment I had met with wherever I happened to blow my brains out" [II, p. 248].

Can anybody believe that Junker really talked such highfalutin nonsense? Often he tells us of the long speeches he made!

The last of these early travellers whose writings I shall touch on is the Italian soldier and cartographer Gaetano Casati (1838-1902) [7]. Between the years 1880 and 1886 he was in the Southern Sudan and the Northern Congo, and then till the end of 1889 he was in Uganda
or making his way with Emin and Stanley to the East African coast. It is therefore only the first volume which concerns us.

Though he ends his volumes with "Amicus Plato, sed magis amica veritas," I think it must be accepted that only the gist (and not the exact words) of this involved conversations with various native peoples—presumably in some sort of Arabic—is recorded; and that his account and interpretation of events are often confused, which is not surprising in the confused situation in which often enough they took place. The matter is further complicated by the fact that when he ran into trouble in Bunyoro he appears to have lost his possessions, including, one may suppose, his notebooks.

It must be said, however, that some of what Casati wrote, if the translation is faithful, is not only nonsense but would appear to be invention. Speaking of the Baggara country on the Nile he says: "Amongst the reptiles are great pythons, coiled round the trunks of trees, that by thrusting out their heads add the contrast of horror to the magnificence of the picture" [7, I, p. 33]. Can one believe that he saw this? Then we are told that the Dinka "have an especial regard for serpents, for in nearly every house some of them are to be met, generally pythons, boldly reposing and coiled up; the tameness of the reptiles, which are fed upon milk, even reaches the point of answering the calls and signs of the housewife" [I, p. 45]. This has never been seen by any trustworthy writer about the Dinka. On the same page there is a very unconvincing drawing of what is supposed to be a Dinka woman feeding one of these snakes with a bowl of milk. Casati seems to have had a mild obsession about snakes. Writing about the Mangbetu he says: "Karbado, the son of the chief of Bellima, threw his spear at thirty paces, and stuck it firmly into a target marked on a tree; the confidence with which they destroy rats, flying serpents, and birds by means of arrows is altogether incredible" [I, p. 124]. But not so incredible as the "flying serpents"! (If that is a correct translation.)

We may therefore feel somewhat sceptical about what, to give a few examples, he says about the Zande king Kipa's corpse and grave [I, p. 210] and coffin [I, p. 219]. And what he says about Zande oracles is probably hearsay and untrue [I, p. 219]. The plate purports to show "Weapons of the Sandeh" [I, p. 208] but I feel sure that not one of the specimens figured belongs to Zande culture. There are the usual fanciful conversations which could not, I think, have taken place; and if they did could not have been as presented [e.g. I, pp. 110-111 and 176]:

"One day the king [Azanga of the Mangbetu] said to me:
— Does the sun shine at Khartoum?
— Certainly it does, I replied."
— That cannot be; it must be another sun, said he.
— Why should it be so? I inquired.
— Because this is the sun of my abul pate. You inhabit another world.
— As you will; but the sun is so large and is so situated that it can shine both over your country and mine.
— I cannot believe you. My kingdom is very large, and its confines are far apart, and beyond my realm there are others as extensive, so that I cannot believe that I am mistaken in my opinion.
— Be it as you will, I said; but I advise you not to interfere with the prerogatives of the sun, as it might take offence and punish you.

He became silent, and looked at me with eyes wide open. I burst out laughing, and he followed my example, but his mirth was rather forced, and the conversation was turned to other subjects” [I, p. 176].

If this exchange really took place one is not surprised that the king’s “mirth was rather forced”, but can one seriously believe that it took place at all?

One more example: at Lesi Casati met a Turk who had worked as a hunter for Emin Pasha and with whom he alleges he had a conversation which began:

“— Ah, you are a hunter? said I.
— First-rate! Without me the Mudir [Emin Pasha] would be unable to continue his zoological collection.
— What! Is there no one in the province capable of firing a shot?
— Yes, a number indeed, but all they can do is to burn powder and destroy an animal; for instance, a bird must be hit in the breast, in order that the stuffer may show its appearance in the most favourable manner; carrying it hanging by the beak, cleansing the feathers from blood, and preserving it from every damage, are things requiring a certain delicacy of touch, which the Donagla and blacks will never possess.
— After all, I said, it seems to me that what is required is not so difficult to learn; goodwill is generally accompanied by industry and love of improvement.
— It ought to be, but it is not so here amongst us; and then I must remark that the greatest difficulty of the business is selecting the victim, in which I am not afraid of being surpassed by any one. Several specimens of one bird in the same collection means waste of time and gunpowder; the skill I am speaking of is not a common quality, I assure you; the Mudir knows it: I am expecting him to ask me to return to him” [I, pp. 77-78].

Can anyone believe that so flowery a conversation really took place?

The other travellers require critical evaluation if their testimony is to pass the test of credibility. Piaggia’s Memorie is sensational and suspect. Heuglin and Marno tell us little and what they tell us is often wrong. Even the best of our authorities, such as Poncet and, as we have seen, Junker, make major errors of one sort or another. One could almost have drawn up a recipe for early travellers in the Southern Sudan: a reference to cannibalism, a description of Pygmies (by preference with a passing reference to Herodotus), a denunciation of the iniquities of the slave trade, the need for the civilizing influence of commerce, something about rain-makers and other superstitions,
some sex (suggestive but discrete), add snakes and elephants to taste; bring slowly to the boil and serve. But I must delay no further among these earlier writings. The travel-literature on the Southern Sudan has dribbled on till that region was recently closed to Europeans by the Arabs of the North.

So it may be said that if most of what we receive from the early travellers in their reports was hearsay rather than eye-witness they were for the most part guiltless of plain fabrication. I say for the most part because two of the books which have been listed among the sources for the peoples of the area may be held not to stand up to probative inquiry, those by the Frenchman du Courret and the Greek Potagos, and they have been challenged as forgeries [17 and 18].

But if not just liars, the travellers were often not innocent in respect of naivety, ignorance, self-justification, and a tendency to sensationalism and to say what their nineteenth-century readers expected them to say and what sentiments to express in doing so.

We might well ask what sort of account could be written about the peoples of the Southern Sudan had we only these travel records to go upon and no subsequent field-research to guide us. Certainly in many respects the account we would have to write would be quite out of perspective and also inadequate, superficial and largely inaccurate as to fact.

It is perhaps the best way to view the progress of anthropology to compare what was recorded, even by men of culture and scientific training, such as Schweinfurth and Junker, with what was recorded by the earliest professional fieldworkers in the same region, the Seligmans, and then to evaluate the contributions of those pioneers by the standards of modern research.

4

I now turn to a consideration of *Pagan Tribes of the Nilotic Sudan* by C. G. and B. Z. Seligman [36]. The inadequacies of the Seligmans’ account arise primarily from the kind of investigation they were conducting for it could not have given them the answers to the questions they asked. I speak of an ethnographical survey, as it used to be called, as contrasted with intensive fieldwork, and as distinct also from surveys based on, and an extension of intensive research. And I can talk from experience, having conducted such a survey for many months in the Southern Sudan in order to cover on their behalf those

1. In defence of Potagos and with due respect to my friend R. L. Hill, I would call Junker to witness: [23, III, pp. 173, 188, 208, 247].
areas the Seligmans had not been able to visit. It is true that previous field research experience, even only of a survey kind, such as the Seligmans had had, is an aid, but it is never a substitute for long sojourn, knowledge of language, and repeated observations in the centre of native life, in so far as that is possible. It has to be recognized, if one is honest with oneself, that the product of such survey research is largely superficial and dubious. It presents the culture of a people as a lifeless patchwork put together from paper notes about what is said to be custom, lacking altogether the vitality of flesh and blood. This was accentuated in the case of the Seligmans by a singular lack of interest in the Africans themselves, only in them as specimens to be studied to be put into a book.

Although this book is under the names of C. G. and B. Z. Seligman some two-thirds of it are the researches of other people. I provided the material for the chapters on the Azande, the Nuer, and most of those on Darfung and the tribes of the south-western Bahr al-Ghazal. The late Mr. Whitehead contributed the most reliable part of the chapter on the Bari. Although they do not say so, I am pretty sure that most of what is valuable that the authors say about the Dinka they got from Archdeacon Shaw. They also owe much to many others who helped them, some of the helpers—not all of whom were very well informed or even very intelligent—having been recruited by myself. On the whole the Seligmans are generous in their acknowledgements for assistance, but it has to be said, in evaluating their book, that it is often difficult, sometimes quite impossible, to determine whether a statement is based on their own observations or on those of others. Also we have no means of knowing what reliance, if any, should be placed on information supplied by the others—certainly some of it is unacceptable.

The worst deficiency of the Seligmans was that they not only could not understand a word of any of the languages of the people about whom they wrote, but they also could understand very little, if any, of the lingua franca of the Sudan, Arabic. Consequently their inquiries had to go through English into Arabic (probably of the pure northern Sudanese variety first and then into the southern variety which is almost a different language) and then from Arabic into a native tongue (let us say Shilluk); and the response had to come back along the same chain. We are not once told how this took place. On their last expedition they were accompanied by Mr. Hillelson who certainly acted to some extent as interpreter (he was a fine classical Arabic scholar). Presumably on their other expeditions—and possibly also on the last one—some English-speaking northern Sudanese acted in this capacity, aided possibly by some missionaries. Failure to inform us how their oral investigations were carried out does not inspire confidence.
About these question-and-answer methods of field research I must again quote Van Gennep—this time his bitter *Les demi-savants*:

"— Have you a number of legitimate wives?
— Yes.
— Does polygamy exist in your tribes?
— No.
— Are sexual relations free before marriage?
— Yes.
— Do all your women preserve their virginity?
— Yes.
— Have you a ruler?
— No.
— Are you governed by kings?
— Yes.
— Have you any weapons?
— No.
— Do you use spears and arrows in battle?
— Yes" [42, p. 31].

If this is thought to be a piece of irony too exaggerated to be even amusing compare it, though perhaps it is not quite a fair comparison, with the conversation Schweinfurth had with the first Pygmy he questioned—a conversation carried on from Arabic into Zande into Mangbetu into Pygmy and back along the same chain of languages:

"In reply to my question put to Adimokoo as to where his country was situated—he says—pointing towards the S.S.E., he said:
— Two days journey and you come to the village of Mummery; on the third day you will reach the River Nalobe; the fourth day you arrive at the first of the villages of the Akka [Pygmies].
— What do you call the rivers of your country?
— They are the Nalobe, the Namerikoo, and the Eddoopa.
— Have you any river as large as the Welle?
— No; ours are small rivers, and they all flow into the Welle.
— Are you all one people, or are you divided into separate tribes?

To this inquiry Adimokoo replied by a sudden gesture, as if to indicate the vastness of their extent, and commenced enumerating the tribes one after another.
— There are the Navapuka, the Navatipeh, the Vabingisso, the Avadzubeh, the Avagowumba, the Bandoa, the Mamomoo, and the Agabundah.
— How many kings? I asked.
— Nine, he said; but I could only make out the names of Galeema, Beddeh, Tindaga, and Mazembe" [33, II, p. 128].

And so on.

A fine example is an idiotic conversation reported by the blustering Sir Samuel Baker between himself and the Latuka chief he calls Commoro. It appears to have been conducted through Arabic (a knowledge in which, as I have earlier remarked, Baker displays little proficiency) into Bari into Latuka:

"— Have you no belief in a future existence after death? Is not some
idea expressed in the act of exhuming the bones after the flesh is decayed?
— Existence after death! How can that be? Can a dead man get out of his grave, unless we dig him out?
— Do you think man is like a beast, that dies and is ended?
— Certainly; an ox is stronger than a man; but he dies, and his bones last longer; they are bigger. A man's bones break quickly—he is weak.
— Is not a man superior in a sense to an ox? Has he not a mind to direct his actions?
— Some men are not so clever as an ox. Men must sow corn to obtain food, but the ox and wild animals can procure it without sowing.
— Do you not know that there is a spirit within you more than flesh? Do you not dream and wander in thought to distant places in your sleep? Nevertheless, your body rests in one spot. How do you account for this? (Here Commoro laughs.)
— Well, how do you account for it? It is a thing I cannot understand; it occurs to me every night.
— The mind is independent of the body: the actual body can be fettered, but the mind is uncontrollable; the body will die and become dust, or be eaten by vultures, but the spirit will exist for ever.
— Where will the spirit live?” [3, I, pp. 230-235].
And so on with all this rubbish.

I regard myself as an Arabic scholar but I am sure that I could not have spoken such flowery sentences and that even if I could neither a Bari nor a Latuka would have understood them. It is obvious that they were not spoken and also that, had they been, they would merely have shown how far the more intelligent of the two men was the Latuka chief.

Whatever one may think of Petherick, one cannot but have respect for the courage and endurance of his wife. She admits that she knew almost no Arabic when she records this incident. Halima, a Zande girl who had been given this Arabic name, was taken by Mrs. Petherick to a requiem mass for Dr. Peney at the Catholic Mission church at Khartoum. Afterwards she asked Mrs. Petherick:

“— Who was that man nailed to a piece of wood?
I told her it was the image of the Son of God, and that wicked people had crucified him. She passionately argued,
— Why did God let them? You say he can do all things. Your God is no good.
It was in vain to attempt explanation: she would not understand” [30, I, p. 74].

It seems to me that Halima had got the right end to a theological problem that dear Mrs. Petherick had not understood.

And here is another little gem from Brun-Rollet. It purports to be a fragment of catechism of some Chir and Bary on the origin of things:

1. Emphasis is his.
Sources — Southern Sudan

"D. — Qui a créé le ciel, le soleil, la lune?
R. — Nous les avons toujours vus ainsi, nous ne savons pas autre chose.
D. — Qui a créé l'homme?
R. — L'éléphant, le plus grand des animaux.
D. — Qui a fait le fleuve le Nil?
R. — Un chien" [6, pp. 232-233].

5

Apart from the fact that we are not told the means of their communication with the peoples of the Southern Sudan whom the Seligmans visited, we are also not told for how long or in what circumstances they were visited. I have the impression that they went down the Nile on a Government steamer and dropped off here and there for a few days to ask questions of natives summoned by police to meet them. It seems to have been only on their last expedition (to the Latuka, etc.) that they went inland—to get to the Nuba mountains in the north they had of course to go inland. Clearly it makes some difference to one's assessment of the validity of evidence whether a witness spent a few days or a few years among a people about whom he is writing, and also if one knows how he spent them.

A further grave defect of the Seligmans' book is that they made almost no use of accounts written by early travellers, vulnerable to criticism though they may be. One looks in vain, for example, in their chapter on the Azande and other peoples near to them for any reference to Piaggio, Schweinfurth, Junker, Casati, Marno and others. This may have been partly due to their ignorance of foreign languages (other than a little French) but it is mainly due, I think, to their almost complete lack of historical sense. For instance, a passable account of the Dinka could be reconstructed from the writings of early travellers, especially Beltrame (1824-1906). They are totally ignored.

Perhaps another reason for the Seligmans not having made use of the early literature, which can be valuable if one knows how to use it, was that they did not know how to use it, for one can only determine what is useful in it if one has a much more extensive ethnographical knowledge of the peoples to whom it refers than they had. One cannot fit bricks into a building which is not there. So, to be a good anthropologist, one must not only have a sound knowledge of the history of the peoples with whom one is concerned but also, if one is to make use of historical knowledge, whether gleaned by oneself or others, one must have a sound knowledge of their institutions, which alone permits us to determine its significance.

It must also be said that in the very little the Seligmans saw and
heard they had no standard by which to determine what was significant and what was not. The officiant in a ceremony is described as doing this or that but there is no means of knowing what he does or is said to do is routine and obligatory or whether he just happens to do it on some one occasion and it would not matter whether he did it or not. This defect gives the impression not merely that the descriptions are superficial (which they often are) but also unconvincing. It were as though a blind man were painting a picture or a man were trying to put together a jigsaw puzzle with pieces which do not fit.

Then, instead of describing simple things about which bare observation would have informed them, such as distribution on the ground, mode of livelihood and technology, they would inquire into such topics as kinship terms and religion (the Seligmans had an atheist's obsession about religion) which require, among other things, a good knowledge of languages, which they lacked. And now to some details.

There are so many errors and confusions in *Pagan Tribes of the Nilotic Sudan* [36] that I must be content to give just a sample by way of illustration. The sample follows no logical order. The examples are taken from the book seriatim in page sequence.

P. xxii: We are given as an example of the *dh* the Nuer word *dhun* (boy). There is no such word in Nuer. We are given as an exemple of the *nh* sound the Dinka word *manh* (son). This word is listed as "mother" on p. 152. It is true that J. C. Mitterrutzner [25, p. 263] has "Man, Sohn, Kind", but I think this cannot be the same word.

Prolegomena: I suppose that no one today would uphold the Hamitic theory as it was held by Seligman. It is going far beyond the evidence cited to say of the Nilotes that "it is certain that [...] they have in their veins a considerable amount of the blood of those light-coloured early Hamitic invaders and civilizers of Africa of whom the Somali and Beja peoples of the Eastern Desert are probably the purest modern representatives" [34, p. 708]. Seligman would always muddle up the categories of race and language, an error which can only lead to confusion (see also [35]). He was also a firm believer in Nordic superiority (as his student I had to read a lot of literature in support of the belief).

P. 2, Map: It should be noted that Schweinfurth made the same classificatory division of the peoples of the Southern Sudan into dolichocephals (e.g. Dinka) and brachycephals (e.g. Azande, Mittu, Kredj
and Bongo) [33, I, p. 263]. This was based on a very large number of measurements the records of which were destroyed by fire [1, pp. 263, 312, 318, 328, 473].

P. 15: The Seligmans speak of “the low swampy grounds whose lush grass in the rainy season guarantees the welfare of their [the Nilotes'] beloved cattle.” Unless this is a slip it shows that they did not understand the oecological situation. It is the dry season when the rest of the country is parched that they value “the low swampy grounds.” During the wet season the coarseness of the grass and the flooding in the swampy grounds are a menace not a boon.

P. 19: There seems to be no real evidence that the Nilotic peoples moved consistently from south to north as the diagram on this page would have us accept. It would appear to be little more than conjecture, and on such evidence as we have they might just as well have moved from north to south; indeed Schweinfurth claims that the Luo and other Shilluk-speaking fragments in the Bahr al-Ghazal have migrated south-west from the north, that is southwards from present-day Shilluk territory and are emigrants from there [33, I, p. 86], which would seem to be a very reasonable supposition.

P. 21: The authors state (see also [36, p. 105]) that the word Jur was applied indiscriminately by the earlier explorers for both the Shilluk-speaking Luo and various other peoples who have nothing in common with the Luo. It might appear from their footnote that Schweinfurth [33, I, p. 200] was responsible for this confusion; but Schweinfurth states quite clearly:

“The Nubians apply the general term of Dyoor to all the tribes on the Rohl to the south of the Dinka territory, although the tribes themselves, having nothing in common either in language, origin, or customs with the Dyoor of the west (a Shilluk tribe), repudiate the definition. The designation was adopted from the Dinka, who thus distinguish all tribes that do not devote themselves to cattle-breeding” [33, I, p. 378].

In an earlier passage Schweinfurth wrote: “The Nubians have adopted the Dinka appellation of Dyoor for Lwoh, Niam-niam for Zandey, and Dohr for Bongo.” In the passage cited in the Seligmans’ footnote he says: “Dyoor is a name assigned by the Dinka. They speak of themselves as Lwoh” [33, I, p. 261].

P. 22: The authors suppose that the figures of Nyakang and Dag of the Shilluk are carved effigies in human form. This is not the case. (They are “made of bamboo and ambatch surmounted by huge plumes of ostrich features” [21, p. 192].)

P. 23: With regard to what is said about “cult figures”,¹ I have

¹. Four of these figures in the Vienna Museum, for which it would appear Emin and Hansal are probably responsible, are illustrated in C. G. Seligman's
to say that when I looked into the matter many years ago I found that whilst those in the Vienna Museum were described in the exhibition case as Ahnenfiguren (ancestor-figures), they were described in the original entry in the acquisitions book as Puppen (dolls). This I reported to Seligman but he did not take it into consideration. On the previous page, referring to these carved wooden figures, he says: "Anyone familiar with the older writers, especially Schweinfurth and Junker, will be surprised to find no mention of Bari ancestor figures." Schweinfurth never went near Bari country and does not mention them. As for Junker, he indeed gives a drawing of what he calls (in the English translation) "wood carvings of the Bari Negroes" [23, I, p. 252]. What we are to understand by that and what they are supposed to represent we cannot tell: they were stolen from a people Junker believed to be Bari during a typical plundering raid by Egyptian government troops from whom Junker acquired them. He could not have been clear about where precisely they came from and who made them and for what purpose. There is no suggestion that they were "cult figures". Moreover, Werne, whom presumably the Seligmans had not read, is quite clear on the matter. He says that a Bari chief "Làkono and his queen had brought a doll, representing a woman, hewn roughly from wood, as a present, which turned out, contrary to our expectation, to be only a doll; for they laughed immensely when we asked whether they worshipped or adored it as a deity" [43, p. 59]. It may further be remarked that there is in the British Museum a specimen (not mentioned by the Seligmans) described as "Wooden figure of Bari type, probably a fetish"—whatever that may mean!

What Junker further says is worth quoting in full:

"I used the unwelcome delay in Ladô to add in every possible way to my collection, which was already very extensive. I procured for it many fine specimens from Latâka, the country of the Southern Mádis, the Shulis, from soldiers or officials returning from ghazwehs [plundering raids on the native peoples]. Through personal acquaintance with several Bari dragomans I was able to acquire objects belonging to their tribe which had hitherto been wanting, and some new specimens were added to the curious carved wooden figures already in my possession, and which I only met with among the Baris and tribes related to them. These might be mistaken for fetishes, symbols of religious worship, in accordance with the customs of some tribes on the west coast of Africa, who set up carved images in sacred places. It appears to me doubtful whether the many carvings from Africa, marked as fetishes in various museums have really any connection with religious ceremonies or customs. I am inclined to think that many of these figures are endowed with this meaning by the ignorance or credulity of the collectors. The extent of the fetish worship and sects

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is very much exaggerated. The right interpretation of the religious views of savages is the traveller's most difficult problem. The wooden figures of the Baris, which are barely twelve to sixteen inches high, are undoubtedly representations of deceased persons, 'penates' hung under the roof in memory of those who have passed away" [23, I, p. 496].

Shall we add to the confusion by quoting Brun-Rollet, who had fairly considerable dealings with the Bari, that they had wood-carvers ("menuisiers") who made little stools ("chaîses") and crude statuettes ("statuettes grossières")—whatever we are to understand by that.

P. 23: The rain-makers among "the better-known Nilotes, the Shilluk and the Dinka" are "'divine kings', of Frazerian type, in whom is immanent an ancestral spirit." I consider it going beyond the evidence to call them "divine kings." In fact, rain-making, so-called, is a minor function of both the Shilluk king and the Dinka priests, while among most of the other Nilotes rain-making is of no great importance; among the Nuer and Anuak, for example, there is no one who can properly be described as a rain-maker.

P. 29: We are told that the Nilotic kinship terms "conform in what we regard as the most important feature of the descriptive system, viz. the absence of a specific word for brother and the differentiation of brothers and sisters according as they are children of the father or of the mother." I consider this to be incorrect. B. Z. Seligman, who was responsible for the kinship part of the book, did not, I am afraid, understand the meaning of the terms she used: "classificatory", "descriptive", and "European". We may here note just two points: a) the Zande system of kinship nomenclature, which she calls "classificatory", has the feature which she regards as distinctive of a "descriptive" system; b) although the Nuer have separate descriptive terms for mother's son and father's son they, in my experience, always address the father's son in a "classificatory" manner as "mother's son".

P. 31: "About an hour after sunrise the cattle and goats are let out (we are speaking of the dry season)." What about the sheep? And let out of what? Surely the peoples referred to are in camps: among the Nuer and Dinka the cattle sleep out in the dry season (for the Shilluk, see p. 39).

P. 32, footnote: "In Chapter VI we cite the milking routine of the Nuer in the dry season, and this obviously refers to beasts that have not recently calved." How can this be obvious when in fact all cows are treated alike whether they have recently calved or not? The implied suggestion is that there is a midday milking. There is not.

P. 33: "The homesteads are surrounded by plots of beans, hibiscus, tobacco, various pot herbs, and even fish poisons." What are "pot herbs"?
P. 33: Can anyone make intelligible this sentence: "Here, too, there are two millet crops, one sown in the rainy season, remaining in the ground during the dry season and reaped at the beginning of the following year, the other sown in April and reaped early in the new year"?

P. 39: We are told that the Shilluk now, that is in recent times, obtain iron by trade from "the white man." Does this mean from the European or the Arab of the Northern Sudan?

P. 39: "The Shilluk king is absolute head—temporal and spiritual—of a state whose territory is divided into a number of provinces, each administered by a chief directly responsible to the sovereign and acting as his proxy." The evidence does not at all support this statement [9]. Poncet was nearer the mark where he says of the king "qu'il n'aït pas un grand pouvoir [. . .] il n'en a pas moins beaucoup d'influence" [31, p. 19].

In their account of the Shilluk it must in general be said, especially with regard to their political institutions, that the Seligmans show a lack of historical perspective; and I would therefore here quote Schweinfurth who, speaking of the Shilluk forty years before the Seligmans saw them, says that their "perfectly regulated government" was a thing of the past, all now being changed, everything having disappeared "which gave this independent and primitive people their most striking characteristics" [33, I, p. 93].

P. 40: We are told that in the Shilluk settlement "one hut is the sleeping apartment of the householder and his wife—if there is more than one wife each woman will have her own hut—one is used for cooking, and others may be assigned to servants, or slaves if any are attached to the family." Who are these servants or slaves? Only a brief further reference is made to them (see p. 77). And what is meant by slave?

P. 40: With reference to the word gol they write: "Actually, as Mr. Heasty informs us, this is a smaller unit formed within the kwa [clan], arising through some influential man starting a settlement of his own, when his descendants (living there) form his gol until they become a large and powerful unit, take a new name and become a kwa." I would suggest that this is incorrect and that gol means, in reference to a social group, "household" or "family", as Westermann says [44, p. 258]. This what I believe to be confusion between a domestic and a descent group would be a serious error: the gol cannot be a smaller unit within the kwa. Mr. Heasty told me in 1935 that he withdrew this information and that it is impossible to form a new clan (as distinct I presume from a new lineage).

P. 41: The Seligmans did not have a clue to the lineage system of the Shilluk or of any other Nilotic people, which is surprising as
W. H. R. Rivers came near to guessing at it in a very brief encounter with some Shilluk [32]. Westermann was quite right in speaking of *kwa* Nyadwai, etc., though wrong in speaking of them as clans. They are lineages, which is of course a relative term. The Seligmans have not, it follows, understood that one can discover a Shilluk’s lineage by asking him what is the entrance to his enclosure (*kal*).

P. 41: It must be repeated that they entirely missed the lineage structure of Shilluk society, and this was because they would not believe what they were told. The word by reference to which we would translate “lineage”, *kal*, did not have for them any reference to a social group but only to a domestic enclosure. One can ask, I say again, a Shilluk “what is the entrance to your enclosure?” And he will then tell you what is his lineage. It happens that in Sudan Arabic (but not, I think, in any other dialect of Arabic) the expression for “lineage” is *khasm bait* “entrance to tent (or house).” The Seligmans thought that when their interpreter asked in Arabic for the Shilluk expression equivalent to *khasm bait* and were given the words for “entrance to enclosure” their informant was giving a literal translation of the Arabic words, and they therefore ignored the significance of what they were being told. An understandable but most unfortunate confusion.

P. 45: The authors say: “There seem very good reasons why the sons of the twenty-seventh and twenty-eighth kings have not come to the throne, and apart from Government influence it may be that their lines will have less claim in the future, since Kur, the twenty-seventh king, did not meet his death as a Shilluk king should but was deposed by the Government and died in confinement in the northern Sudan.” So did the twenty-third king die in captivity, but two of his sons became kings.

P. 45: It is not made clear here or elsewhere that only the son of a king could become king. Unless this is understood the dynastic succession makes little sense.

P. 46, footnote: for *ago* read *jago*.

P. 47: “The king observes the same marriage prohibitions as his people, even remote relatives on both sides of the family being forbidden.” On the next page we are told that “the king would seldom take a wife from the *ororo* unless she were an unusually attractive woman.” But the *ororo* are his remote relatives!

P. 48: “Nor would a commoner even attempt to recover [from the king] cattle of the bride-wealth that had died.” We are not told how, why, and in what circumstances anybody can claim recovery of bridewealth cattle that have died.

P. 48: “The royal daughters are not allowed to marry, it being alleged that it would not be fitting for a woman of royal birth to
marry a commoner, while she could not marry a nyireth [a king's son], or the descendants of one, as this would be incestuous.” On the next page they quote Mr. Heasty to the effect that they may have lovers among their close relatives, including their half-brothers by the same father. Surely, with regard to the word "incestuous", there is a contradiction here.

P. 49: “We did not hear of the rivalry between princes leading to actual warfare for the throne [. . .]. It is related that Nyakwac killed the surviving children of his great uncle Nyadwai (the twelfth king), though three of them had already reigned before him, but we do not think that this was a common practice.” I should however record that Mr. Heasty told me that he had learnt from the Shilluk that it was a common practice. He mentioned a recent king who had done the same thing.

P. 49: In the footnote to this page we read that, according to Mr. Heasty, “during the reign of king Ocalo there was trouble between the Nuer and Shilluk. The sons of Ocalo, who led the Shilluk against the Nuer, were all killed except Duwad, considered very insignificant by the king, who felt that there would be no chance of one of his offspring reigning since all his important sons had been killed [. . .]. When Duwad became king his half-brothers were degraded to the level of commoners.” One asks whether these statements can be reconciled. Perhaps it is to be understood that Duwad had younger brothers.

P. 49: Clearly the Seligmans have not been able to place the ororo in Shilluk society. By implication they more or less admit this in the footnote to this page.

P. 50: “The outstanding feature of the systems of all these tribes is the absence of a single word for ‘brother’, who is always described as either ‘father’s son’ or ‘mother’s son’.” This is not the case, e.g. the Nuer demar means “brother” simply. In the same paragraph we read: “As among the Dinka and Nuer, the mother is the only person to whom the term for mother is correctly applied.” What can “correctly” mean here? A few sentences further: “The individual father is not confused with the father’s brother.” How could anyone suppose that he could be?

P. 51: On this page and on the following pages I find the confusion so great as to render the whole account of kinship terms to some degree unintelligible, and much the same may be said of kinship terminology throughout the book. The authors seem to have had no idea of the use of terms. How could they? And nowhere—surely this is elementary—do they distinguish between terms of address and terms of reference. A few examples to justify this stricture must be given. It is stated on an insertion slip that “relationship terms
are generally given in the possessive (first person singular).” What are we to make of “generally” and how are we supposed to know when they are so presented and when not so presented? On page 51 we are told that some terms are used out of “courtesy” or “politeness”. What does that mean? On pages 54-55 we are told that

“an unmarried woman uses kinship terms in the same way as a man [how could the Seligmans have known?], but certain terms are used in an anomalous manner by a married woman. She calls her husband’s sister waja [father’s sister] and her husband’s father and mother kwa [grandfather] and wanga [grandmother] respectively. Two points are apparent in the anomalous use of these words. Thereby all persons are addressed by a term which ordinarily is applied to someone of an older generation; further, instead of being addressed as relatives by marriage they are addressed as own relatives. Thus towards three persons a woman uses terms that might have been used by her husband, but in each case these relatives are considered to belong to one generation higher than they really do.”

But what is “anomalous” about a common usage (fit is one)? What is meant by “all persons”? How could a husband address his sister as “waja [father’s sister]”? It is not said in the list of terms that he does so. What is meant by: “Are considered to belong to one generation higher than they really do”? B. Z. Seligman apparently did not grasp the simple fact that a wife and mother is using the terms her children would use—just as we often do: “auntie”, “grandpa”, and “grannie”. B. Z. Seligman tries to explain the use of these terms by saying that widows are taken as wives by a man’s sons and that it is not considered disgraceful for young unmarried sons to sleep with their own father’s wives (a bald statement I would not accept without great reserve): “A young wife may thus see herself as the consort to two generations at once, and address her husband’s sisters and parents by those terms that she would use if she were wife of the younger of the two mates.” I suppose she means the terms the younger man (her husband’s son) would use.

P. 51: In the list of terms no term is given for “daughter”.

P. 55: We are told of the wife’s sister that the husband “does not treat her with any ceremony, and if she visits her married sister the husband is not bound by any taboos, though he cannot have connection with her.” What does “taboos” mean here?

P. 55: “In certain circumstances (the death or barrenness of a wife) he may marry her”, that is, his wife’s sister. On page 61 it is stated that a man may take his wife’s sister as a second wife, nothing being said about death or barrenness. Then on page 68 we read: “There is no objection to a man marrying two or more sisters, but, unless the second sister is taken in place of the first because the latter has proved barren, full bridewealth must pass for each.” Can these statements be reconciled?
P. 56: Rivers was in the Sudan in 1916. He visited the most northern Shilluk villages and he met some Shilluk, Dinka and Nuer in Omdurman.

P. 57: We are told that a man is potentially heir to the wives of any of his *uwa* "father's sons." We are not told whether he is potentially heir to the wives of his uterine brothers, nor what is meant by "heir"—they fail to distinguish between the levirate and widow-inheritance, rather different institutions.

P. 57: "The wide rights of access that the Shilluk have to the wives of certain of their clansmen and the wide sense in which inheritance of a 'brother's' wife may be interpreted is thus intimately connected with their liberal interpretation of avoidance." "May thus be" would be better than "is thus", for how do they know? Other Nilotes have a wide extension of avoidance but no right of access, if there really is a right of access, as the authors assert, and which I doubt.

P. 57: They speak of "avoidance of certain other women", but they do not say who these women are. They add: "To sit upon a skin, which might be the woman's sleeping mat, would be an intolerable breach of etiquette, for such an act might be taken to imply the possibility of sexual relations." Who says so, the Shilluk or B. Z. Seligman?

P. 60: A man must not let certain female in-laws see his nakedness. The comment is: "Among a people like the Shilluk, who when they wear any garment only knot a cloth over the shoulder, leaving the genitals exposed, this objection seems remarkable, and does point to the sexual nature of the avoidance. So, by demonstrating in a ceremonial manner that there can never be any possibility of sexual intercourse between himself and any woman whom he treats as mother-in-law, he leaves open an approach to her daughter." What does that mean? And whatever it may mean, again is it a Shilluk suggestion or B. Z. Seligman's guess? Since the matter was being discussed it could have been noted that in the time of the early travelers the Shilluk men did not wear a cloth or any other garment but were entirely naked [43, I, p. 150; 29, pp. 352 and 390; 31, p. 21; 40, p. 336; 23, I, p. 221].

P. 62: "The breaking of a marriage—divorce—means the return of cattle to the bridegroom, a difficult matter, as though the bride-wealth is paid to one man, the bride's father or brother, it is immediately distributed and may soon play its part in another's marriage." For "bridegroom" I suppose we should read "husband". It is far from clear what we are to understand by "immediately distributed"—how, why, and to whom? Also we are not told, though it may be implied, whether the same cattle have to be returned or only the same number
of cattle; also whether the cattle have to be returned in the event of divorce if there are children of the union. On the following page we read that "should any of the cattle of the bride-wealth die the bride's relatives can claim that they should be replaced." I feel sure that this is not correct and that cattle have to be replaced only in certain circumstances, e.g. if there is a divorce or if the marriage has not yet been completed or if the cattle have not left the bride's parents' home.

P. 66: "The cattle of the bride-wealth provided for another girl, called Bol (twin), were distributed as follows..." The marriage chart given here seems to be a muddle. Anyhow the name of the girl twin would, I suppose, have to be nyabol and not bol. The authors themselves would seem to say this on page 71, though there they give nyabol as the name of "the next child born after twins." They also say of twins that "the first is called Angar, the second Tian." They do not tell us, which I assume to be the case, that these words mean "cob" and "thing". We must suppose—we are not told—that these are male appellations.

P. 66: We are told that the mother's brothers take some of the bridewealth, but it is not said whether the Shilluk regard this as due to the bride's maternal uncles or is regarded as being still owing for the mother's marriage, which is the way the Nuer look at the matter. It is an important distinction but one I think the Seligmans did not have in mind (see what is said on page 62).

P. 67: "In court"—in what court?

P. 67: "If the bride has no full brother they [the sheep] are divided amongst her uwa, her father's brother's sons, and after them her uwa who are clansmen." On page 51 we are told that the primary meaning of uwa is "brother (son of father)". So what about these half-brothers?

P. 68: "Mr. Heasty sent the following information concerning the disposal of children. If the bridewealth is not returned the children will remain with their father, but if it is returned—and this really constitutes divorce—the husband has to give up the children. The new husband who provides the bridewealth will take the children and they will become members of his kwa [lineage or clan]. When such lads grow up the true father will often entice them back; this is becoming a common practice, but was formerly confined to members of the kwareth [royal clan]." Can anybody make out what all this means? In particular, are we to understand that if the bridewealth is not returned there is no divorce? And do the sons have rights in their "true" father's (genitor's) herd—what otherwise would entice them back—or in the herd of their foster father?

P. 68: "The fine for adultery was said to be seven head of cattle—four cows and three bulls." Almost certainly "bullocks" is meant:
throughout the book the Seligmans do not differentiate between bull and bullock.

Pp. 68-69: Hofmayr states that if a man of importance dies between the time of betrothal and marriage a ceremony having as its object the provision of a wife to the deceased will take place at the grave-side. After this the woman is considered as related to the dead man's family. Later she will really marry, but no statement is made as to whether cattle are exchanged at the grave-side marriage. Can anyone say what this means?

P. 73: It is suggested that "the milk avoidance of the Nilotes are dependent on fear of contamination associated with the sexual act." It is difficult to accept this without it being explained why do males only milk among the Shilluk and females (and boys) among the Nuer.

P. 73: "Only small boys herd the cattle." I find this difficult to believe. The sentence continues: "[.] and milk them." A few lines later we are told that "if a man has had sexual relations with his wife or another he is considered unclean and does not drink until the sun has set the following day." I find that difficult to believe also. Does a married man never drink milk in the morning?

P. 77: "Each Shilluk king incarnates the spirit of Nyakang." So it is often said, but the meaning of what is said much depends on the sense one gives to the words "incarnates" and "spirit". One wonders whether a Shilluk would or could so put it in his own language. It may certainly be said that little evidence is adduced in support of the statement; and I must record that F. Crazzolara, who undoubtedly spoke Shilluk well, once told me that saying that the king is Nyakang is for a Shilluk only poetic imagery. On the previous page we are also told that Nyakang is the "ideal" of the Shilluk. I must place it on record however that Mr. Heasty, so often quoted by the Seligmans, told me that the Shilluk say that they killed him because he was such a nuisance.

P. 78: "Before the door of the hut were a number of horns of cattle slain in sacrifice (in 1911 wrongly described as elephant tusks), their broad ends thrust into the ground." They look to me (plate opposite p. 80) like elephant tusks (cf. also [34, p. 16]). How could one mistake the horns of cattle for elephant tusks?

P. 78: With reference to the spears and stool of Nyakang I may record that in my one day's research in Shillukland (largely into cattle-colour figurations at Doleib Hill and aided by Mr. Heasty), I found that the Shilluk headmen also have emblems (stools, spears, and beads) of office, like the Anuak headmen, and also that there is a custom resembling the Anuak agem, village revolution. This, if correct, is important and I hope that if anyone ever gets to Shillukland again
he will pursue the inquiry further because I believe that the Seligmans and others have concentrated their attention on the kingship and paid less to the headmanship of the local communities.

It is perhaps worth noting, and also worthy of further inquiry, that Werne makes a curious statement which neither the Seligmans nor anyone else has taken notice of. He says: "Close above our landing-place lay the tolerably long and wooded island of the deceased sheikh of the Shilluk islands, Abdurachmân [Abd el Rahman], Wooled el Desh [. . .]. Here it was [. . .]. The Turks in the expedition of the year 1839, were not ashamed to open the grave of the sheikh in order to convince themselves that the sworn enemy of themselves and the Bagharas [Baggara] was really dead" [41, II, p. 314]. I do not think it can be ascertained without a firsthand knowledge of the Shilluk who this man was.

P. 78: We are told that the shrine at Fenikang was photographed in 1921. The date is given on the plate opposite as 1922.

P. 80: They speak about "the rain-making ceremony held before the rains at the beginning of the month alabor, at the new moon (the Shilluk calendar is lunar)." It is not clear why they should hold a rainmaking ceremony before the rains. How is the reader supposed to know which is the month of alabor? Then surely if the Shilluk have anything which can seriously be called a lunar "calendar" they would soon be in trouble with their rain-making!

P. 81: "Care is taken not to break the bones, which with all fragments are thrown into the river. The head, one forelimb, and the bowels compose the share of the attendants of the shrine." I find this confusing but I suppose what is meant is that the bones are only severed at the joints.

P. 84: "A number of young men wore leopard and other skins suspended from the waist and hanging behind." I suspect that they were genet and not leopard skins. We are told on page 46 that "anyone killing a leopard must send its skin to the royal household."

P. 84: "Parrying shields." I doubt whether the Shilluk have anything which can be so described. So-called Dinka parrying shields are figured in Casati’s account [7, I, p. 47]. The British Museum more accurately describe one of these Dinka objects as "parrying shield used in mock battle", i.e. in play. On the same page the Seligmans speak of "the ceremonial bow-derivative covered with strips of metal, called dang." I cannot say what is meant by "bow-derivative" but it should be noted that the Shilluk do not use the bow. The object in question is used in marriage ceremonial by the Nuer, by whom it is called dang. That the specimen referred to by the Seligmans may have been introduced from Nuerland is further
suggested by the fact that the word *dang* is not given in Heasty [16] (see also earlier reference on p. 135).

P. 85: “We were told that the spear used to kill the bull was one presented specially by the king; had one of the original spears of Nyakang been used no one could have eaten the flesh of the bull and survived.” On the same page we read: “The large spear referred to above [a spear of Nyakang] was kept sheathed throughout the ceremony until used to stab the bull, thus emphasizing its sacred quality.” The flesh was eaten. There may be a contradiction here, but the account is not very clear.

P. 86: In the footnote we are told that “according to an account, perhaps of Anuak origin or at least showing Anuak influence, collected by the Rev. J. K. Griffin on the Sobat river, Nikaiya is physically part human part crocodile.” I do not see why Anuak influence should be assumed, especially in view of what is said about Nikaiya on page 85. We are not told where on the Sobat and from whom the information was recorded. This is relevant because at one end the Sobat flows through Shilluk country and at the other end through Anuak country.

Pp. 90-92: “There is no doubt that the kings of the Shilluk were killed ceremonially when they began to show signs of old age or ill-health.” The Seligmans were evidently determined to find a clear example in support of Frazer’s theory about the killing of divine kings. Their account is however unsupported by evidence of any weight. As Mr. Heasty pointed out to me, Shilluk kings did not often get the chance to be ritually killed as they were generally killed in one way or another before questions of age or health could have arisen. Hofmayr gives the traditional biographies of each of the Shilluk kings, according to which all of them were either killed in battle or died of old age [20]. None were “killed ceremonially.” (As the Seligmans could not read German I translated most of Hofmayr’s book for them.)

Brun-Rollet, who tells us that the Shilluk king never passes two consecutive nights in the same hut (“appartement”) and that violence and murder are the usual ways by which a claimant obtains the kingship [6, p. 94], nevertheless adds that the present (1844) king “est très âgé, bien qu’on ait tenté par deux fois de l’assassiner” [p. 95]. I add, for what it is worth, that according to Baker King Quat Kare (Kwat Ker) had ruled for more than fifty years before the Egyptian government appointed Jangy (Ajang) in his place on the fiction that he was dead [4, I, pp. 102-103 and 171]. It may here be further remarked that according to the Seligmans the king, in recent times at any rate, was strangled or in another version walled up alive with one or two nubile virgins [36, pp. 91-92]. These statements appear
to be very confused; and I would suggest that the walling up version may be a misunderstanding. This is suggested by the walling up of royal corpses by the related Anuak. What the Seligmans say may also be compared with what Poncet (unquoted by the Seligmans) tells us that the king never dies a natural death, that being unworthy of him, but at the moment of his death agony the Shilluk stab him three or four times with spears [31, p. 21].

P. 94: “The king-elect is escorted to Fashoda, riding a black ox.” I wonder whether anybody saw this. I have never heard of a northern Nilote riding a beast of the herd.

P. 99: A jalyat, “sorcerer”, is said to be typically a monorchid and should be killed at birth. I find it difficult to reconcile this statement with the fact that there are plenty of such people around, or the Shilluk think there are.

P. 104: In the plate opposite this page the men are described as Anuak, but they all have Nuer markings on the forehead, such as the Anuak do not have. Miss Soule had told the Seligmans this (see footnotes to pp. 110 and 223).

P. 105: Of what have been called the Ber people said to be living on the right bank of the Sueh river they say: “We cannot identify this tribe, and suspect that there has been confusion with the Bor of the Belanda complex.” The matter is explained in [8, pp. 20-22] (cf. also [29, p. 481] and [38]).

Pp. 109-113: Most of what is written on these pages about the Anuak is incorrect. The authors had not themselves visited the Anuak. They relied on Bacon (much of whose account is sound) and Heasty (most of whose account is unsound).

P. 136: It is not at all clear—indeed it is very unclear—what distinction, if any, is made between “tribe” and “clan”. On page 149 they write: “Many—perhaps all—the members of the Kiro and Ngong Nyang clans consider the fish recol [. . .]. We may infer that the relationship still acknowledged as existing between the fish and the members of these tribes was once the normal totemic relationship.” Then again on page 151 they speak of the “Thany clans Jakcir and Culil, from whom have strung settlements, which in turn have given rise to other villages whose inhabitants together constitute the Kiro tribe; all the Kiro clan revere Dek.” This is not just a matter of nomenclature. The fact that they did not see the difference between a political and local group (tribe) and a descent group (clan) makes a good deal of what they say about Dinka social organization meaningless.

P. 137: For lwak read lwak.

P. 137: “Hearthers are made, surrounded by dry wood, and uprights to which the cattle are tied at night.” I am not sure that I un-
stand this. What is meant by “uprights?” The photograph on plate XV shows the kraal to be studded with pegs, presumably for tethering cattle.

P. 137: We are told that the word gol means among other things “cattle-enclosure”. I would query this. Then on page 142 we read: “The word for clan varies from tribe to tribe, e.g. Bor ut, Thany and Aliab gol, and Cic deb.” Not only does the meaning of these words appear to be identical, but among the cattle-owning tribes they are all used for the byres and cattle hearths of their clans or local groups, and also to signify “homestead”: I doubt whether the meaning of these words is identical or whether among the cattle-owning tribes they are all used for byres or cattle hearths and homesteads. “Clans or local groups” conveys no meaning at all, at any rate to me.

P. 138: “The Atwot Dinka”—if one wishes to be precise, the Atwot are not Dinka.

P. 138: On the plate opposite: “Dinka huts with Lwak”—but where?

P. 138: “Iron ore is collected from the hills to the west”—what hills?

P. 138: “The Adjong (Dinka) do not make pots.” On page 141 we read: “Pottery is made by the coil process by women, but not every woman has this art.” This apparently refers to the same people. We are also told that the Adjong get pots as well as other things “in exchange for spear-heads and other iron objects”—with whom?

P. 139: The diagram on this page seems to be almost meaningless since the Seligmans had no idea of what was, and what was not, a clan. Also what are (J), etc.—huts? I suppose so since we are told on page 141 that the Cic Dinka have no cattle byres. Where do they tether the cattle? Why do some sections have pens, and not others? How can there be only one “cooking-house” (whatever that may mean)? The diagram on page 140 also seems to have little meaning. Where are the villages located? What is the relationship between the persons living together? What in a dry season camp is a hut?

P. 141: “Each tribe is divided into a number of exogamous totemic clans with descent in the male line.” “Divided into” is meaningless. “There are in each tribe a number” would make a little sense, but not much.

P. 144: “Gol e Mariak has as totem the snake, niel. Long ago one of these snakes came into the hut of one Mariak and there gave birth to its young.” Niel must I think be the python. I am not convinced that “gol” here means clan as the Seligmans supposed (they were very muddled about what constitutes a clan among the Nilotes).
P. 144: “The Ramba clan of the Niel clan derives its name from that of an ancestor who was born as one of twins, his father being a snake called gor. Gor was placed in a large pot with water, but he soon died; so a bullock was killed and Gor prepared for burial by smearing with the stomach contents and wrapping in the skin of the sacrifice. This was at Anako, where there is still a shrine to which sick people go in order to sacrifice.” I do not understand this at all. It would be in line with the general discussion of Dinka totemism if the Seligmans meant that his brother was a snake called gor.

Pp. 151-153: It is said that the list of kinship terms is composed from the Bor, Niel, and Cic Dinka. Only one term is marked Niel. A number of terms are not marked with provenance. Are we to understand by this that they are common to all three tribes? The table seems to be very confused.

P. 153: “Wanmath is the term used between children born of one mother, without regard to the identity of the father.” On the next page we read: “A woman calls her brother’s wife tingawanmath (lit. ‘wife of father’s son’).” Surely this is a contradiction.

P. 154: Tingawenanyankai is given as “wife of my wife’s son”, though it must on the contrary mean “wife of my sister’s son”. On the same page it is said that a woman calls her brother’s wife tingawanmath, “wife of father’s son”, but in their list of terms “father’s son” is given as wenawa and it is the mother’s son who is wanmath (tinga being “wife”). On the same page: “The anomalous use of certain terms by a woman among the Shilluk is not found among the Dinka.” This is possibly because they have not recorded them, e.g. the term a woman uses for her husband’s sister? Then on page 156 we are told that “the wife’s brother is not avoided or treated ceremonially, though he stands in the relationship of thundia.” On the previous page we are told that a man “practises ceremonial avoidance towards those relatives of his wife whom he terms thundia and dhundia.” There are other ambiguities on these pages.

P. 154: “The children of the walen (father’s brother) may be called ‘father’s sons’, or uwalen, thus recognizing clan brotherhood as among the Shilluk.” What does this mean? Uwalen is not given in the list of kinship terms on pages 152-153.
P. 155: "This [marriage] does not separate her [the wife] from her own clan, as may be seen clearly in the example (p. 192) of a child's illness being due to the jok of the mother's family." Far from being clear this is most ambiguous, for on pages 191-192 we are told that the illness was caused by the jok of the father's mother.

P. 156: "There is no avoidance between a man and his wife's sister." On page 155 we are told that "a man, however, practises ceremonial avoidance towards those relatives of his wife whom he calls thundia and dhundia." Then page 153 we learn that dhundia is the term for, among others, the wife's sister.

P. 156: "Though a man cannot marry any of his female cousins, he does not avoid them ceremonially. Thus Tir stated that he did not avoid Pot (see table, p. 161) because she happened to be his wenamanlen, but had Manya, his wife's brother, married anyone unrelated to himself Tir must have treated her with ceremonial respect." It does not seem to me that in the table Pot is Tir's cousin. Then as far as I can ascertain from the list of kinship terms wenamanlen would mean mother's sister's son, i.e. be a male. Can anyone make head or tail of the diagram on page 161? It seems to me to be a complete muddle, made a bit more so by the difficulty of distinguishing between men and women.

P. 162: "Fines are imposed for adultery; according to Capt. O'Sullivan from five to eight head of cattle are paid to the husband, but if the woman is a widow the fine goes to or is held in trust for the husband's heirs." I do not understand this. What does the word "adultery" mean here?

P. 163: Capt. O'Sullivan is quoted to the effect that division of bridewealth among the kin of the bride is just a matter of the father of the girl giving some of the cattle to "his friendly male relations." Surely this is not so—relatives have claims to portions of the bridewealth.

P. 163: "Unless the cattle be returned to him [the husband] (so that he can get another wife) the children borne by these women [his wives], irrespective of true paternity, are his for ever; death does not break the marriage contract, the only thing that can break it is the return of the bride-wealth cattle (divorce)." The word "paternity" is perhaps unfortunate here when what is meant is "irrespective of who is the genitor."

P. 164: After a sacrifice the woman's "clan" are said to take one hind leg and the "clan" of the deceased the rest. What is here meant by "clan"? I think the Seligmans were not able to distinguish between clanship and kinship.

P. 165: "Purchased a girl"—what they want to say is that the girl was married with cattle. In the next paragraph: "A wife was bought for him"—the same comment.
P. 165: The afterbirth is buried "outside the house near the wall of the homestead." Dinka homesteads have no wall.

P. 168: "Male twins we were told would be called Did and Lual, both names of birds." Did is presumably the Dinka word for "bird", not the name of a bird. What lual means is anybody's guess.

Pp. 168-169: There is a muddle here about ox-names. "Boys may be called after a maceng if one formed part of the mother's bridewealth, or even after a cow from that herd." What herd? The names of oxen are "names which they share with all other bulls and oxen of the same class, the class being commonly determined by colour and skin pattern, often considered to resemble that of some kind of vertebrate, a likeness which to the white man frequently appears fantastic and far-fetched." It never struck me that there was anything "fantastic" about Nilotic ox-names. The maceng of page 168 is presumably the muor cien of page 169. Is a majok ox really only one with "a particularly coloured (red with white belly) muor cien"? I doubt it. And what does mayon (a black ox with white face) mean?

P. 170: "Lith being the word for a grey hawk"—I suppose the kestrel.

P. 170: "It is probably correct to say that no boy would be initiated until his forehead had been scarred." But surely this is the initiation! (Cf. also [34, pp. 707-710].)

P. 171: How did the Seligmans know what the names of the age-sets meant, knowing no Dinka and no Arabic? Doubtless they got translations from Archdeacon Shaw. Here again I must insist that it is often—even usually—impossible to say whether they are making statements on their own responsibility or are recording what somebody told them: we then have to decide to what extent we are prepared to accept that testimony. This seems to me to be a grave defect in their book.

P. 174: Has the following quotation any meaning at all? "It must be noted that the kun [the youngest son of the first wife] only inherits from his mother the cattle given to her by his father, not cattle from her brother or her father, so that it is not an inheritance with female descent, nor can it be regarded as ultimogeniture simply. It would appear to be a compromise between patrilineal and matrilineal descent, and to be associated with the Nilotic relationship system in which brothers and sisters, children of the father and of the mother, are distinguished from one another."

P. 178: "The Dinka, and the kindred Nuer, are intensely religious, in our experience by far the most religious peoples in the Sudan." Did they ever meet a Nuer? And so much for the Muslims of the North!

P. 178: A Cic Dinka noticed an unusually large pumpkin in his
garden and sacrificed a goat. The authors do not indicate that this particular man probably belonged to a particular clan that had the gourd for totem; they therefore do not refer back to what they had already written (p. 146): "Among the Cic there is a clan, or perhaps a family, claiming descent from a girl who was twin to a gourd plant; its members do not care to drink from a newly made gourd vessel and apparently do not grow gourds, or if they grow them at all do so sparingly." It may be noted that the Gaagtankir clan of the Jikany Nuer, who are by all said to be of Dinka descent, respect gourds.

P. 178: "Mr. Richards tells us that some fifty bulls were slaughtered" when aeroplanes first appeared near Tonj. Apart from the fact that they would certainly not all, if any, have been bulls I find it difficult to accept this testimony.

Pp. 178 sq.: I find it odd that under the heading of Dinka religion nothing is said about ghostly vengeance (cien) considering how important a part the concept plays among other Nilotic peoples. I feel that this is a most serious omission.

P. 180: It is only by inference that one knows that this hymn must have been given to the Seligmans by Archdeacon Shaw. Also, what does it mean?

P. 185: A Thany Dinka man is said to have said: "The atiup of my animal (ancestor) is a jok." Also on page 186: "It was clear that the spirit of the ancestor of a clan was considered to be embodied in the totem animal; further, ancestral spirits would often take the form of the totem animal [. . .]. All this was definite." This is meaningless to me.

P. 188: Though the Seligmans do not say so, Archdeacon Shaw was their escort and interpreter on this occasion, as no doubt on others. Shaw told me that Wal was most disturbed by Seligman peering at him through high-powered lenses.

P. 196: "Medicine fire"—what is this?

P. 196: "In theory, and no doubt generally in practice, the authority of the [Dinka] rain-maker is absolute." Surely this statement requires some evidence in support of it. I find it difficult to accept.

Pp. 196-200: Without holding that rain-makers may not in some way be killed when near to death anyhow it should be pointed out that, as is the case with the killing of the Shilluk king, the evidence is hearsay. No European has witnessed the killing or has been able to cite incontrovertible evidence that it ever took place. Also, when the Seligmans write that "if it was thought that the banj [rain-maker] was seriously ill he would be killed, even though he was quite young [. . .]. Actually this had never happened", we might well ask whether no young banj ever gets seriously ill.

P. 198: "It seemed that when rain is wanted, i.e. when the
country is at its driest, but also (from the white man's standpoint) when the rainy season is approaching, the rain-maker is besought to seek rain from the particular spirit (Lerpio, in the case of the Bor Dinka of Gwala) that is locally regarded as specially concerned with rain." Whatever "that is locally regarded as specially concerned with rain" may mean I cannot say; but "from the white man's standpoint" seems to me to be a strange thing to write. Is it suggested that the Dinka, who after all are the ones who have to survive in their environment, do not know when the rainy season is approaching? Also I think the Seligmans have not at all understood this business of so-called "rain-making". It is not really a matter of the rainy season approaching. The trouble arises when rain has fallen and then ceases after the grain is sown and has sprouted.

P. 199: "Presently the bullocks are killed by the rain-maker, who spears them and cuts their throats." This may be correct, but to me it conveys no picture of what happens.

Pp. 206-209: These pages are a good example—there are many others in the book—of the Seligmans' misuse of information. Clearly they knew almost nothing at first hand about the Nuer and they do not claim otherwise. They were mainly dependent on my notebooks, but unfortunately they would interpolate information derived from other sources and sometimes, worse still, their own inferences. Consequently it is often impossible for the reader to know on whose responsibility a statement is made.

P. 207: We are told that Gau had two sons, Gaa and Kwook, and a number of daughters and that "as there was no one with whom these could marry, Gaa assigned several daughters to each of his sons, and to avert the calamities that follow incest he performed the ceremony of splitting a bullock longitudinally decreeing that the two groups might intermarry but that neither might marry within itself." To make sense out of this account we would have to accept that it was Gau, and not Gaa, who assigned the daughters to the sons. The account continues: "Gaa being the elder son took the right side of the bullock, i.e. he became the 'Land Chief', kwor muon, the most important man in the tribe, and also, according to the story kwor twac, the 'Chief of the Leopard Skin'." What the Seligmans call the "Land Chief" and the "Chief of the Leopard Skin" are the same person and office. What is meant by "the most important man in the tribe"? And what tribe?

P. 208: "At about 9 a.m. the cattle are loosened." They are generally taken to pasture between 7.30 and 8.00 a.m. in the Lou country, but where there are rich pastures near the camp, which is largely the case among the eastern Jikany, they may not be loosened till between 9.00 and 10.00 a.m.
P. 208: "After the cows have been driven out." "Driven out" does not very well describe what happens—the cattle leave quietly of their own accord.

P. 208: In the footnote to this page we read: "The word gol is commonly used to signify 'village' or 'homestead', and probably has these meanings. Miss Soule tells us that it only means 'clan' in combination with the name of the head or founder of the clan." Naturally were the word so used it would certainly (not "probably") have these meanings; but in point of fact gol is never used to denote a village, nor does it ever, in my experience, refer to a clan. Miss Soule knew the Nuer language well and I think that the Seligmans may have misunderstood what she told them.

P. 209: "Cattle are eaten both when killed for ceremonial purposes and in times of severe hunger when blood may be taken from the animal's neck. Apart from these occasions young men at times kill a beast and eat it." I find the first sentence unintelligible—it is not required that it should be a time of severe hunger that blood be taken from animals of the herd. Nor do I understand what is meant by the second sentence: possibly they refer to the nak, an organized slaughter of beasts in connection with the age-set system; but this is something very different from the statement that "young men at times kill a beast and eat it."

Pp. 209-210: "Although the solidarity of a Nuer tribe is ultimately based on its clans..." This has no meaning. The sentence continues: "...the structure of the tribes and their relations inter se appear more complicated than among the Dinka, our impression being that there has been far more segmentation and reintegration than among the latter." I do not know what this means.

P. 210: In the footnote to this page and passim they have missed the whole point. A cieng can never be a clan. This makes nonsense of the whole account of Nuer political structure and of the descent or lineage structure also.

P. 210: "Each clan generally has attached to it fractions of other clans, local groups from other areas." For "generally" read "invariably". "Local groups from other areas" makes no sense to me.

P. 210: "To such an extent have foreign elements been assimilated in some clans that perhaps 80 per cent of a tribe may be ultimately of foreign origin." It is not at all clear from the context what is meant by "foreign"—also what is meant by "clans".

P. 211: Rul are "folk who are not members of the dyel, often husbands of the women of the clan who have not left the home of their fathers on marriage." "Home" is a very ambiguous word here. The text continues: "Since marriage with a clansman is not permitted, girls must marry rul or Dinka who will probably come and live in
their father-in-law's *wec.*" It would have been better to have used the word *cieng* (village) instead of *wec* (camp). A significant difference between the Nuer and Dinka (at any rate the western Dinka) is that the Nuer tends to speak of his local community in terms of wet season residence (*cieng*) whereas the Dinka tends to speak of his in terms of dry season residence (*wut*). This relates to their different forms of transhumance.

P. 212: "... does not seem to us necessarily to imply Dinka origin, for we consider that the similarity [in totemic beliefs] is best explained by the origin of the two peoples from a common stem or stock." The authors did not, I think, realize how complex is this question. I would say that on the whole the evidence points the other way.

P. 212: "A man of this clan killed a lion; afterwards a lion took some twenty head from his herd. It was decided not to hunt the beast, since the latter was within its rights, but twenty cattle were exacted from the man who had killed the lion, a sacrifice was made, and the affair ended." Exacted by whom for whom? According to the passage cited the man who killed the lion was the same as the man whose cattle the second lion killed, so there is no point in the story as it is here related. What really is said to have happened is told in [10, p. 64].

P. 213: "A sheep was quickly sacrificed to the lions and all was well." However, it is not to the lions that a sacrifice is made but to the lion-spirit (*kwoth*). I do not think that Nuer totemism can be understood without the distinction being made.

P. 213: A *yang ma reng* is definitely not a speckled beast, as they say it is. It is a beast with a coloured back and white belly and legs.

P. 214: For *Win* read *Wiu*.

P. 215: Speaking of the leopard-skin priest they say: "He is indeed endowed with personal sanctity and authority so great that it is improbable that his decisions were ever seriously disputed in the old days." His influence "was probably never greatly affected until Government action led to a greater diminution of his power." In my opinion, as I have made plain in my books on the Nuer, this is a complete misconception of his position in Nuer society, and I know of no evidence whatsoever in support of what the Seligman say. I would further say that the *Sudan Intelligence Reports* (not consulted by the authors, it must be supposed) make it clear that when Administration was imposed on the Nuer every effort was made by its officers to buttress such authority as he was wrongly supposed to possess, it later becoming obvious to these officers that it had never been very substantial.
P. 215: He does not address the cow. He addresses Spirit in one or other of its forms.

P. 217: Lam should not be translated "imploring". Pal has that sense but not lam.

P. 218: The table of kinship terms hardly conforms to what is said on the insertion-slip, that "relationship terms are generally given in the possessive (first person singular)." Unless the reader knows the language how is he to know when the terms are so given and when not?

P. 219: For "commonly employed" read "always employed".

P. 219: "On both sides of the family this usage is the same, certain relatives being generally addressed by terms which are a degree more intimate than the correct terms of reference." I suppose that what they want to say is "than the correct terms of address", though how the use of a term can be said to be correct or incorrect might well be questioned. (I may emphasize here that after I had seen some of what was supposed to have been a final version this chapter was much altered, and not for the better.)

Pp. 219-220: "Society is organized on a patrilineal basis. The father is head of the family, and is in charge of the family herd of cattle. Sons are married with cattle from the herd in order of seniority, and in the same way the cattle that come into the family from the marriage of daughters go to the family herd and are not specially reserved for the full brothers of their sisters. In spite of this the tie to the mother's family is very close indeed; their share in the bride-wealth of a girl is equal to the share of the father's family." I do not know what is meant here by "in spite of". There does not seem to be any logic in the argument. Furthermore there would appear to be confusion between family relations and kin categories, and also between "paternal" or "patrilineal" and "patrilineal".

P. 220: I find the paragraph beginning "A child's mother..." a muddle. "Widows whose bridewealth has been returned..." Who are these? It is clear that the Seligmans did not understand the intricacies and alternatives in the situation they spoke of.

P. 221: "Considering the wide conception of the incest barrier among the Nuer it is not surprising that the rual ceremony takes place fairly frequently." But not, as the context shows the Seligmans supposed, in animal sacrifice but by the cutting in two of the fruit of the sausage tree (also called rual in Nuer).

P. 223: "Forehead scarring in one form or another probably exists among all Nilotes." However, they add in a footnote on Miss Soule's authority that it does not occur among the Anuak. They have themselves already said in the footnote on page 170 that it does not occur among the Atwot. As far as I know it is not practised by any of the southern Nilotes.
P. 224: For koth kel read kwonth kel. We are not told what kel is supposed to mean.

P. 224: "When all is well the wud ok is informed, and he gives the order for the release of the boys." I do not believe that he has anything to do with their "release".

P. 225: "It seems that a number of successive initiation ceremonies constitute a single gar [initiation] period, which may extend over from one to four years." This is quite wrong. It would also appear to be contradicted by the next paragraph: "An age class covers about seven years..." The same sentence continues: "Men are often called by their ric [age-set] name to determine their age." Who would want to determine their age?

P. 226: "The children of age-fellows must not intermarry." Whatever may have been the case in the past they can now marry if one of the fathers is dead. A man can however make love to the daughters of his father's age-mates whether one of the fathers is dead or not.

P. 226: "The bridegroom must practise avoidance of his father-in-law's age fellows." I have no knowledge of this. He would certainly find it very difficult to do.

P. 226: "A brother knows all about his sister's love affairs and only objects if he has a personal dislike to the lover." I do not know where they got this from. He will certainly object, whether he likes the man or not, if he thinks his sister is having intercourse with a person who would not be able to find cattle enough to marry her should she become pregnant.

P. 226: "An unmarried girl should never have a child." This is very much an overstatement. A youth able to obtain cattle may get a girl in child and then marry her without either of them incurring censure.

P. 229: For gat read gaat.

P. 232: "In other parts of the country cattle plague is called jok." All epidemics are called jok. Diu is a particular type of jok.

P. 233: "Many sent to Wanding for the prophet to come and make rain, so that they might sow." On the next page we read: "It is to be noted, however, that no steps are ever taken to make rain before the crops are sown."

P. 235: "He [Evans-Pritchard] gathered that the spirits of the dead were thought to return to the place of creation; moreover, the path of the sun was involved in the argument (and for this reason no one could be buried facing north or south), the sun, moon, and sky, all being kwolith (in this sense God)." I do not know what all this means. In particular, what does "in this sense God" mean?

P. 235: "Professor Evans-Pritchard's informants said that an
important ceremony, at which mourning is put off, takes place some six months after the death of a man and three months after the death of a woman." On the same page: "The following is a shortened account of a ceremony witnessed by Professor Evans-Pritchard at Diini some four months after the death of one Lam. Lam was a man."

P. 235: In the footnote we are told that tobacco is thrown on the ground "for the ancestral spirits." Owning to the rather loose terminology employed one does not know whether this means the ghosts of the ancestors or the spirit or spirits of the lineage.

P. 236: "Professor Evans-Pritchard considers that removing the cuol is analogous to the settling of a debt." It is not a matter of my thinking it to be analogous. I was translating what the Nuer call the ceremony—cuol woc (woc "to wipe out", cuol "a debt").

P. 236: "Prayers to the dead." Strictly-speaking one invokes (lam) the dead. One does not pray (pal) to them.

P. 237: The point of the metaphor in the footnote is that wild rice is soft to the feet.

I have dealt only with a part of this book, and with what is said in it about only one of those peoples of whom I have made studies; but I think I have dealt with enough of the book to have illustrated what I mean when I say that we must not take our authorities at face-value but must subject them to rigorous critical assessment. I regret that we must conclude in this instance that what the Seligmans record should be accepted with a considerable measure of reserve unless it is confirmed by independent witness. What I have said may seem to be something in the nature of a brutal dissection but it has been fair comment, and even kind; and we must all face the same inquest.

Indeed I hold most strongly that anthropology will never reach a high standard of scholarship till it lays upon itself the duty of critically evaluating ethnographical evidence to assess both its accuracy and its significance before it is used in comparative and analytical studies. Every authority should be put in the witness box, if not into the dock, that it may be estimated whether what he alleges will stand the test of probing. It is true that this sifting process presents us with an embarrassing problem. The historian's authorities are usually dead and can be criticized without diffidence and with impunity. Our main authorities are often alive, or their children and friends are;
and we may wish to avoid offending them. If anthropology were a natural science, in the sense that physics and chemistry are natural sciences, the problem would not present itself in quite the same form because observations and experiments are less personal and can moreover be repeated by others. But in anthropology the weight one is prepared to attach to records of field research very often much depends on one's opinion of the man, one's knowledge of his background, and so forth; and also on the information one has about the conditions in which he did his research and his manner of conducting it. And when one can take these matters into consideration without causing offence only too often all can no longer be ascertained.

What makes the situation worse is that it frequently happens that the anthropologist is the only, or almost the only, authority on a people and what he says cannot therefore be confirmed by the independent evidence of other observers. Such authorities have to be assessed by critical examination of their statements in the light of their consistency, of general anthropological knowledge, of ethnological knowledge of the area concerned, as well as of knowledge of the person and the circumstances of his research. Where there are several and independent authorities the operation is simpler, though even in this case it is not entirely simple. Unless observations are made and recorded more or less at the same time it may be difficult to determine the degree of independence, for one observer cannot avoid being in one way or another influenced by what another and earlier observer has recorded. Then, when there appears to be conflicting testimony it can be due, not to faulty observation or recording, but to changes which have taken place between the times of research. Thus if it is found that today the Trobriand islanders do not do what Malinowski said they did it may just be that they have given up doing it and have no memory of ever having done it. Then again, among a widely distributed people there may be cultural differences, as, for instance, dialects, so that what might be taken to be conflicting statements can be due to these differences. Another complication is the fact that it certainly happens sometimes, and probably often, that when an illiterate people become literate they see themselves through the eyes of an anthropologist who has recorded their way of life when they were illiterate; such may be the authority of the written word. So when a Nuer tells me that what I wrote about his people is correct I like to think that what he says is true but, even if he is not just being polite, it could be that his confirmation is derived from the source he is confirming.

It should perhaps be further noted that whereas the historian assesses the evidences accessible to him and others he does not both create them from his observations and then draw conclusions from
what can only be known to himself, the anthropologist may be, and
often is, the sole source and sole interpreter of that source, his conclu-
sions resting on his own selection of evidence, to which none but he
has access.

What I have been saying is elementary, even commonplace, though
it does not seem to have been generally appreciated by anthropologists.
It is simply that we must be able to recreate in our minds the total
situation in which observations were made and recorded. Lacking
any firsthand experience of the conditions in which field research is
carried out would have made it very difficult indeed for writers like
Herbert Spencer, Tylor, Frazer and Durkheim to have cast a suffi-
ciently critical eye on their sources even had they attempted to do so,
which they did not. But even when we come to the first generation
of professional fieldworkers who had some acquaintance with the
circumstances obtaining in inquiries of the kind, we find little aware-
ness of the question we are discussing; and it has also to be said that
we must recognize that the low theoretical development of social
anthropology and a lack of clearness about its technical requirements
militated in their own research against the pursuit of knowledge at
any depth. I am thinking of men like Haddon, Seligman, Thurnwald,
Westermarck, Landtmann, and in America Kroeber, Lowie, Radin
and others. This is why the major writings in social anthropology from
Malinowski onwards have been as valuable for their negative as for
their positive contributions, in that they have shown not only with
greater accuracy what primitive peoples are really like, but also what
they are not like, that is, as they had been thought to be like, and not
only in popular opinion but also by supposedly informed opinion. As
we look back on the history of anthropology we see how one theory
after another has been propounded on false or inadequate evidence
and has had to be abandoned when the facts became better known
and understood; and it is largely on account of uncertainty about
ethnographical evidence that all attempts to reach significant conclu-
sions by use of the comparative method have failed. Let us there-
fore, as we look back, learn and be wise. If we do not exact the
highest standards of critical scholarship we have no right to claim
respect.

And if you think that some of the examples I have given are trivial
and refer to simple matters of no great importance I would reply that
they, the minutiae, furnish the real test of the value of a source
because if an author makes mistakes in simple matters how much
the more carefully must we scrutinize what he says about complex
and abstruse matters. This is why I have paid particular attention
to material objects; if a man says what is wrong about what he can
see, how much more likely that he will be wrong in matters such as
religious beliefs, which require for their understanding a sound knowledge of language.

I would like to conclude by briefly stating for consideration a further question. It has been my purpose to discuss the reliability of ethnographical observations in a region known to me from my own researches, and in doing so I have said little about the personalities of the authors cited. In going into this matter further we would have to consider character, personality, sex, social class, education, ethnic and cultural origin, and other factors which could have influenced observations and the use to which they have been put. We would also, and this is the broad question I would wish to put before you, have to consider the climate of thought at the various periods in which observations were made—what were the interests and categories of thought which directed the attention to certain things and not to others and how the interests and sentiments of those who were to read ethnographical accounts influenced the manner and content of them. It is of course true to say that the stage of development of anthropology as a science may to a large extent be the cause of the failure to note what might have been noted or of the noting of some things and not others, but this still leaves the question unanswered—why has anthropology come into existence and developed in the direction it has? Such a question can only be answered, if at all, by a study of the history of thought.

**BIBLIOGRAPHY**


