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But Maine's view was that the levirate was only one form of the *niyoga*: "Early Law and Custom," p. 100. To be quite accurate it was an usage which ought to be very sharply distinguished from the levirate. In the *niyoga* there was no remarriage of the widow. Her function was to raise up a son and heir to her husband under the strictest conditions: Manu, IX, §§ 59, 145 and 146; even the exception in § 149 "proves" the rule. But in the levirate she had precisely what she had not in the *niyoga*. She was permitted to *remarry* a brother-in-law, and probably had no option but to do so. And on his part the brother-in-law was under a strong social obligation to espouse her. But the remarriage, if it took place, had one imperative result, in that the first-born son of it was affiliated to the deceased husband, and could be in nowise appropriated by his *de facto* father. Manu, on the contrary, simply bastardises the son of a remarried woman as a general rule, or, if he recognises his legitimacy, makes him the lawful son of his mother's second husband. No Hindu lawgiver ever contemplated the affiliation of such a son to her first husband. Hence Jolly's rendering of *niyoga* by *Leviratsehe* is doubly unfortunate, because in the *niyoga* there was no *Ehe*, or remarriage, at all, and if there was remarriage of a widow there was no affiliation of her son by it to her first husband. It is not enough so say, as Mr. Chattopadhyay does in MAN, 1922, § 25, that:—"Niyoga, strictly speaking, differs from the leviration in the limits imposed on "connection with the widow after the birth of one or two sons." It differs from it fundamentally in affiliating the son (and Manu says that a second son would be illegitimate: IX, § 143) to the first husband. Leviration in its strict sense, as connoting the surrender of the first-born or only son of the remarried widow to the deceased husband, is entirely foreign to modern Indian custom, at least in the Panjab. Only once, in 29 years' service, did the present writer come across an apparent case of it. In that case a grandfather had three sons and some grandsons, but one of the sons died sonless. His widow remarried one of the other sons, and the grandfather then divided his land into three portions, allotting one to her son by her second husband, so that the name of his dead son might not be put out. But this disposal of the land was probably set aside by the courts as contrary to Punjab customary law.

(To be continued).

REVIEWS.

Africa: Linguistics.

Johnston.

A Comparative Study of the Bantu and Semi-Bantu Languages. Vol. II. 88
By Sir Harry H. Johnston. Oxford: Clarendon Press.

Here we have the comparative and synthetic work needed to complete the task of which the vast mass of material accumulated in Vol. I. formed the first half. Of the value of this material to philologists there can be no question. With some of the conclusions deduced, it is possible to disagree. The discussion of disputed points thus (one hopes) to be evoked can only further the progress of knowledge.

The chapter on Phonetics—one of the most controversial—may be left for treatment in other quarters, only remarking, apropos of the statement that the voiced *th* sound (ḍ) "is much less frequently met with" than the voiceless (in this notation *t*), that precisely the reverse is said by Mr. Ruffell Barlow to be the case in Kikuyu; and this coincides with my own very limited observations, as far as they go. In Herero, too, the voiced sound seems as common as the voiceless—if not more so. In Makua, one of the few other languages in which this sound is recorded, it is at least a question whether it is not really a strongly aspirated dental *t*.

We may, perhaps, permit ourselves some desultory comments on the remaining chapters, recognising that a full discussion of the questions involved would require

a volume at least. First, with regard to the prefixes and concords to which Chapter IX is devoted : in suggesting “ *zizi-* or *titi-* ” as the original form of the tenth prefix, the author ignores the *n*, which is certainly one of its constituents (where this is absent, as in Ronga *ti-homu* = *izin-komo*, it has dropped out in accordance with well-understood phonetic laws). Meinhof’s suggestion that the original form of this prefix (without the “ preprefix ”) was *lini-* receives some support from the case of Sesuto and Sechwana, which appear to stand alone in this respect (*n-ku* 9 “ sheep,” pl. *li-n-ku* 10. This prefix is written *di-* by Sir Harry Johnston; but, though so pronounced, it is clearly, by origin, *l*).

We should doubt whether *ti-*, *ci-*, *ji-* (p. 231) are really forms of the 12th (*tu-*) prefix. *Ti-* has taken the place of *tu-* in Chinyanja, but the presence of such words as *tulo*, *tubsi* in the same language (with others which seem to be merged in Class 5 those mentioned keep their own concord : *tulo tache*, etc.) suggests that it may be a distinct class, just as we have both a *lu-* and a *li-* class. Whether it is identical with Sir Harry Johnston’s No. 22 (*ti-*, *te-*) is another question. It seems to me far from certain, moreover, that *tulo* and *tubsi* are diminutives : why should not *tu-* have more than one function, as well as *ka-* and *ki-*? In the case of *ka-*, it surely seems more probable that it—or a different prefix afterwards assimilated to it—should have denoted a distinct class, than that it should have become, first an honorific and afterwards an augmentative.

The diminutive suffix *-ana* raises a very interesting question. Sir Harry shows its distribution to be much wider than was generally thought to be the case; while it seemed to be confined to the Zulu, Chwana and Thonga groups of dialects, it was possible to attribute it to a cross-current of outside influence, affecting the general prefix-tendency of the Bantu languages. If, however, it “ is very characteristic “ of the Bantu generally, and penetrates north-westward to . . . Fernandian,” the matter takes on a different complexion, and the stray traces of this suffix in Swahili become less anomalous than seemed to be the case. For we apparently have this suffix in *kitw-ana*, “ a slave-lad,” (literally “ a little head ”—one thinks of the so-called “ tribute of heads,” really of slaves, once exacted by the chiefs of Shaka from the Pokomo tribes)—and *mngwana*, “ free man ”—a word with a curious history. It would seem to have been originally used of the coast people by the inland tribes whom they reached on their trading journeys, and who were so impressed by their superior civilisation as to call them “ little gods ” (Mulungwana, from Mulungu). It is a little difficult to grasp the connection between this and the reciprocal verbal suffix *-ana*, “ to which it is obviously allied.” Where is the warrant for saying that this suffix indicates “ reciprocal or *diminishing* action ”? There is no hint of this in the section of Chapter XV (14 f) to which we are referred.

In this same Chapter XV one is surprised to find the reflexive particle included among “ suffixial terminations to the verb-roots—extending or modifying the meaning “ of the verb ”—or, in other words, among what are usually called the Derivative Forms of the verb. It surely stands on quite a different footing, being an object-infix, like any of the objective personal pronouns. “ Reflexive . . . suffixes added “ generally to the verb-root ” can hardly be accepted as a description including the termination *-ata*. The reference to 75 (Xosa, Zulu, etc.) indicates that verbs like *ambata* “ put on ” (clothes) are meant. This, if it stood alone, might possibly be taken as reflexive in the sense of “ clothe oneself ”; but we have, in Nyanja, *fumbata*, “ grasp;” *kumata*, “ sit with the arms folded;” *tangata*, “ help;” and in Swahili, *kamata*, “ catch.” The function of the suffix is not very clear; but perhaps, as Meinhof suggests, it implies some notion of gathering together. *-ata* is also included under the “ stative ” suffixes (14 j) (where it may be meant to cover the verbs just mentioned), as though it were identical in function with *-ama*—which we may be permitted to question. Another somewhat doubtful point is the inclusion among

these "Derivative Forms" of "Adverbial" suffixes (such as *-po*, *-ko*, *-mo*, etc.) which do not form verbs capable of being conjugated, as do the true derivative suffixes.

However, these strictures are apt to seem ungracious in face of the enormous services rendered by a work like the present, which brings to the notice of students some two hundred languages and dialects previously quite unknown. The author, no more than his predecessors, supposes his work to be final—but the provision of such a mass of material for other workers is indeed an achievement worthy to crown the labours of a lifetime.

A. WERNER.

PROCEEDINGS OF SOCIETIES

Anthropology.

British Association.

British Association for the Advancement of Science. Hull Meeting, 89
September 6-13. *Proceedings of Section H (Anthropology).*

Section H (Anthropology) met in the Royal Institution under the presidency of MR. H. J. E. PEAKE. In his address to the Section, with which the proceedings opened, the President dealt with the "Study of Man." He reviewed briefly the advances in the study of anthropology and in anthropological method during the last eleven years, and, after defining the scope of the science, urged that the time had now come when anthropologists might pay greater attention to the study of the more civilised peoples, particularly in China and the Far East, India and the European region, urging the establishment of a School of Research in India similar to the existing schools of archæology at Athens and Rome.

With some notable exceptions, the communications presented to the Section did not reach that level of interest which is customary, while one or two were undoubtedly too technical for a general audience. It is also to be noted that there was a serious falling off in the number of ethnographical papers which have hitherto contributed largely to the success of the proceedings.

In *Prehistoric Archaeology* first place must be given to the important discussion on "The Relation of Early Man to Phases of the Ice Age in Britain," which was held in a joint session with Sections C (Geology) and E (Geography). The discussion was opened by MR. PEAKE, who gave a summary of his views on the question which have already appeared in MAN.* He was followed by PROFESSOR W. J. SOLLAS who, in an admirable summary of the French and German views on the Glacial question, maintained that the Ice Age in Britain could not be studied apart from the Continental evidence. Penck's conclusions, which, as he showed, could be brought into harmony with the views of French Geologists, made it clear that there were four periods of maximum glaciation and that in relation to these, while Mousterian times straddled the Würm, Aurignacian and subsequent cultures must be regarded as post-Würm. On the other hand, PROFESSOR KENDALL and PROFESSOR P. H. G. BOSWELL maintained the view that the British evidence must be considered by itself. The former, indeed, held strongly that East Anglia must be treated as a unit, and that it held the solution of the problem. Only when the situation had been made clear there would it be possible to bring it into relation with the evidence from the Thames Valley. PROFESSOR BOSWELL indicated how the evidence from East Anglia had narrowed the issue down to the question whether the gravels containing Chellean and Acheulean implements were to be regarded as pre- or post-boulder clay, a question which an attempt was being made to answer by excavation; but the results were unfortunately not available in time for this

* MAN 1922, 5.