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BORNEO DEATH

"When beggars die there are no comets seen;
The heavens themselves blaze forth the death of princes."

Shakespeare, *Julius Caesar*, II, 2.

"He believed in nothing, she in everything. She believed in divine providence, the omnipotence of evil spirits, fate, incantation and eternal justice. She believed in Jesus Christ, and in the good goddess of the Syrians. She believed, too, that dogs barked when black Hecate passed the crossways, and that a woman could inspire love by pouring a philter into a cup containing a sheep's bleeding fleece. She thirsted for the unknown, she called nameless beings, and lived in perpetual expectancy. The future frightened her and she desired knowledge of it. She surrounded herself with priests of Isis, Chaldean magicians, with doctors and sorcerers, who always deceived but never left her. She feared death, and saw it everywhere."

Anatole France. "Thais".

"Nothing in Indonesia is simple or unilinear. Everyone's ideas combine two strains, or three unconnected layers, or four conflicting interests, or five discordant principles."

President Soekarno, 1961.

The Gesellschaft für Völkerkunde (Verein zur Förderung des Rautenstrauch-Joest-Museums der Stadt Köln) makes a welcome reappearance with the first volume (new series) of *Ethnologica* (founded in 1909), edited now by Professor Dr. M. Heydrich. My wife and I know at first hand the small but important Borneo materials at the Rautenstrauch Museum, Cologne; and how alert they are there for outside contact from the field. It is therefore of special interest, here in the Sarawak Museum at Kuching, to find this revived first volume devoting all its 245 pages to a monograph by Dr. Waldemar Stöhr, the substance of his doctoral thesis "Das Totenritual der Dajak" (Dayak Death Rituals) — the word "Dayak" used here legitimately to cover all the major pagan peoples of Borneo (p. 19).¹

¹ I am indebted to my wife Barbara Harrison (herself a German) for help in the present study. Indoors she has specialised in the non-English literature of Borneo, outdoors in the archaeology of our later neolithic and early metal Western Borneo sites, whence we now have hundreds of primary and secondary burials. Where needed, she has provided the English translations used in this paper; and she has also prepared an English summary translation (175 typed p.p.) of Dr. Stöhr's text for local use.

I at once will mention that Dr. Stöhr has not been east of the Danube; this is a theme to which I wish to return presently — in terms of the advantages and disadvantages implicit in any chairborne operation concerning remote and complicated areas. Bearing this limitation of approach in mind, let me hasten to add that Dr. Stöhr has done a wonderful job in its kind: he has put fieldworkers in his debt by at long last bringing together, in an intelligently analytical yet consistent and easily intelligible way, all that is published on his important theme.

The only comparable work is the earlier, looser, wide-scale but abidingly valuable general compilation by Ling-Roth ("The Natives of Sarawak and British North Borneo", London 1896). It is faintly terrifying to see how little we have advanced our knowledge and particularly our *depth of understanding* in more than half a century since then. In particular, since 1947, protracted studies by a good series of anthropologists in British Borneo, have barely touched upon the present theme which to me would seem fundamental to the understanding of a Dayak-type society.

The special quality of Dr. Stöhr's contribution is its clarity and coherence. He has digested what is no longer "raw material" (Chapters IV—XII), presented in such a way that we have in effect a solid encyclopaedia of mortuary practices and a certain amount of related materials for the whole of Borneo. His own generalisations and theories are kept separate, in shorter chapters. These, the hors d'oeuvre (Chapter III), the sweet (XIII) and savoury (XIV) will be discussed later; meanwhile the main meat course. In this, "Das Totenritual der Dajak" provides an object lesson of good service and table manners which badly needs to be learned by some others lately active among these fieldstuffs.

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The main work — the material Stöhr has assembled and supported by nearly 500 references, comprehensive up to 1958 — is opened with an examination of death rituals in general (Chapter I). He considers the approach to burial typologies by *location* of actual burial, as often favoured by others; and he rejects this approach. Had he done otherwise, some of the Borneo data would have presented serious difficulties. He adopts as his central postulate that there are two fundamental attitudes towards the corpse which may be summarised thus:

- (a) *Simple or Primary Burial (einfach)*; this involves (physical) "severing of all relations with the corpse" as such (p. 6).
- (b) *Complicated or Secondary Burial (mehrstufig*, lit. "in several stages"); preservation of the corpse until a ceremony is performed when the living communicate with the dead (p. 6).

Although this over-emphasises the severance aspect in some Borneo primary burial, and simplifies actual thinking over much of Borneo, yet faced with the type of information mainly available Dr. Stöhr has been wise to emphasise that "the distinctive characteristic of either burial form is the use" (or non-use) "of parts of the corpse in ritual performances", — the former being "secondary", the latter and simple "primary", in most English terminologies (p. 7). As a framework within which to arrange the literary data, this is sufficiently direct and specific, involving a minimum of additional subjective interpolation or possible theoretical argument (e.g. about attitudes to souls, supposed status transformation, the spirit role in cosmology, etc., etc.).

Stöhr then considers (Chapter II) the general literature on death rituals and the far from easy problem of the best way to present a very mixed bag of sources. Again adopting a common-sense rather than "academic" line, he elects to take the whole death rites by cultural groups rather than by burial forms, classified as such. In this, he follows Carla van Wylick's 1940 study of death and afterlife in the Celebes. He indicates mild distrust of ambitious interpretative studies at this stage, referring particularly for Borneo to Hertz (1906; spelled "Herz" without the "t" throughout; to him we will return later).

Next our author must face a tricky set of decisions: how to arrange the human groupings within an acceptable classification (pp. 17—20). Again, he adopts a common-sense, workable classification, which, although open to individual argument, is easy to understand and free from personal bias. He adopts "as a guide", the preliminary group structure put forward by Mallinckrodt in 1928 (p. 20) — having rightly rejected later attempts to do better (*no* satisfactory final classification can be made until more fieldwork is done, particularly in central Kalimantan).

Doing the best he can with the existing confusion — which stems, of course, from virtually no wide coverage of Borneo by competent workers in the field — Stöhr divides the main body of text into nine chapters, crammed with human fact (pp. 22—172). He has taken a lot of trouble to present the source material clearly and evenly, by keeping strictly to those groups which he accepts and discusses in the opening

section of each chapter. Thus anyone can evaluate and use (or abuse) his presentation, item by item, even where the actual classification or particular source may be unacceptable to the reader.

Since there is no agreed grouping system for Borneo, it is only too easy to air one's own or niggle at another's — and appear to get away with it! Encouraged by Cologne's self-control I will try and exercise some from Sarawak, and only refer to what could appreciably confuse his text at a few (but significant) points. Two such points arise from his grouping of the Dusun with the Murut-Kelabit (Chapter VII), and the placing of the Bisaya there rather than in his "Klemantan" group (Chapter VIII). So far as I know, nobody *familiar* with the Dusun of North Borneo has ever suggested that they are closely related to the Kelabit-Murut peoples of Sarawak, north-west Kalimantan and the extreme south-west corner of North Borneo (Padas headwaters). This has been flogged out in planning the last two census studies for British Borneo (1947, 1960). But, as is unfortunately so often the case, general agreement has not been adequately summarized in publications available *outside* the countries concerned. The confusion is heightened because even the term "Murut" has been used quite differently in North Borneo, to describe there a people about as unlike the Sarawak Muruts as an Iban is unlike a Dusun. Similarly, in the intermediate State of Brunei, "Dusun" is used in another sense; and so it is again in the Barito basin of south-east Kalimantan — where Dr. Stöhr appreciates the distinction (dealt with extensively in Chapter V under "Maanjan-Lawangan-Group"). But he goes astray in misunderstanding how such common words ("Dusun" basically means "a garden owner"; "Murut" "a hillman", and "Kelabit" nothing at all) are applied in life.

In the State of Brunei, "Dusun" is interchangeable with Tutong, Belait, Bukit, and, more broadly, Bisaya. Dr. Stöhr has unfortunately placed the last of these — always so called in North Borneo and in the north of Sarawak — in with Kelabit and Murut, whereas the Bisaya certainly belong (in his system) with the "Klemantan" types in the following chapter. Thus the important 1915 observations by P. C. B. Newington are mixed up with Sarawak Murut material (pp. 111—119). Similar difficulties arise from linking the North Borneo Muruts (called "Tagals" from Sarawak) with the southern people of the same name, with whom they have only faint linguistic, social or other connections as far as is at present known.²

The Bisaya are closely related to the Melanau of the coastal plain

and the Sekapan, Kajaman, Ba Mali, Seping, and others far in the upper Rejang — all one great linked people until recent times — who elevated secondary burial to be the central rite of living, at a supreme height in modern Western Borneo. Their funerary affairs were at one time comparable, in scale, to the Ngadju and Lawangan in Kalimantan; and there are strong indications that the two are linked in space and time through the Painted Cave death ships and wall paintings, at Niah (*Man*, LIX, 1959, Colour Plate A; and *Artibus Asiae*, monograph in press). Otherwise, Dr. Stöhr has done well with what he truthfully calls a "particularly difficult" Klemantan group (p. 112).

Four other specific points need mentioning:

- (i) The opening paragraphs of Chapter VI give an inadequate idea of the complexity of later-day migrations out of the Bahau uplands by the Kenyahs coming into the Apo Kayan and presently into the Rejang system further south, pushing the Kayans and others in various directions south and west, while a Kelabit (Saban) and Kenya element remained in the Bahau. Similarly, the southward movement of the Kayans has been enormously complicated, — for example the present governor of East Kalimantan is a Kayan of the *Uma Aging* clan, and he lives a month's journey away from the other great *Uma Aging*, the virtual "pope" of the *Bungan Malan* anti-missionary cult in West Borneo, T. K. Baleng of Belaga.
- (ii) Dr. Stöhr is right (p. 124) in rejecting Hose's early generalisations on the supposed confusion of "Klemantan" belief and its borrowings from Kenya and Kayan, which have been rather exaggerated; also in drawing attention to the importance of certain Melanau representations of figures by wood-carving — of which there are some fine specimens in the Museum at Cologne and in the collections of the Sarawak Museum, deserving fuller study (three are handsomely illustrated in this monograph).
- (iii) The apparent conflict Stöhr sees in the reports by W. S. Buck (1933) and G. Jamuh (1945, 1950, 1951) regarding Melanau practices (p. 127) are in fact not due to error but to the very wide degrees both of local and of individual variation inside even that small sector of this widespread group, termed (in literature only) „Melanau". My friend Buck, who recently retired from the Sarawak Civil Service, helped Jamuh (who is now Assistant Curator) to prepare his later papers (all *S.M.J.*). This Borneo variation factor, here and in other chapters, is perhaps insufficiently appreciated. A full-scale study of the Melanau funerary practices would make the existing published information available to Dr. Stöhr seem

² Mr. Newington's paper is in *Sarawak Museum Journal (S.M.J.)* 6 (pre-war), 1915: 188-190. He served in the Sarawak Administration of the Brooke Regime, is still happily alive and well in Malaya. He has lately sent us some further old fieldnotes for publication in our *Journal*, shortly. Dr. Roger Peranio, an American, has lately worked among the Sarawak Bisayas and has published one preliminary paper to date (*S.M.J.* 13, 1959: 613); for a general account of the Brunei Bisaya and Dusun types and their wider relationships see my notes in *S.M.J.* 11, 1958: 293-321; for a full study of the origin and meaning of Bisaya in Borneo and the Philippines see J. Carroll in *S.M.J.* 15, 1960: 459-541.

- woefully inadequate, I am sure; the range of procedure was enormous.
- (iv) Similarly, the sources on Land Dayak burial practices are of such mixed quality and generally so superficial — the Land Dayaks are particularly reticent — that in the result Stöhr tends to put too much stress on the rigidity of their procedure (Chapter IX). Fuller study would again provide a wealth of additional material, though in their case it is perhaps too late. Certainly there has been much misunderstanding about the character of Land Dayak *cremation*. Much of it is — or was until lately — rather a form of what Dr. Stöhr recognises with less certainty as late-stage „lustration” (the same word in German; p. 169). Ways of heating and charring bone played an important part in the end phases of funerary treatment widely in Borneo — including the Ngadju and Lawangan to the south-east and stone age men at Niah to the west (see below). Full-scale cremation, the total destruction into ash, is rather rare — and in my own direct experience only found in T'ang/Sung trade sites with strong Chinese affiliations or traditions.

It is necessary to emphasise the variability and complexity of death rituals in Borneo. The material Dr. Stöhr has so patiently assembled and systematically published is impressive enough. It becomes more so when it is recognised as representing something under 5 % of the total picture for several major groups and better than 50 % only with the Ngadju.

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As, at this point, we have completed a brief review of Stöhr's raw material and expressed our sense of obligation thereto, before coming on to his final and more general chapters (XIII and XIV) — which in my view are open to considerable criticism, — it should make the position factually more clear if I venture some direct observations of my own which become relevant here. I will confine these to only two aspects of Borneo death, simply.

These two personal approaches may serve to indicate the aforesaid complexity of the Borneo material, plus the present patchiness of reported evidence upon which any analysis can draw — despite two centuries of extensive literature and nearly half a century of “professional anthropology” (Professor W. McDougall, F.R.S., often called the founder of social psychology and Professor A. C. Haddon, the father of anthropology at Cambridge, were the first “anthropologists” to work in West Borneo on their way back from the Torres Straits Expedition in 1899; of course, neither were professionals or trained men, in the 1961 sense).

Most of the information in these two following notes is previously unpublished; none of it has been brought together before.

(i) *The Past at Niah.*

This commentary is being written in July 1961, at the Niah Caves (cf. Stöhr pp. 169; 186). Here the Sarawak Museum has a permanent camp and extensive archaeological commitments. During the past fortnight we have been excavating in a newly discovered cave called *Lobang Jeragan*, perched high in the limestone cliffs of the Subis Mountain, very difficult indeed to get at. This particular cave — one of a series we have selected for detailed study — proves to be an almost pure “neolithic” burial place, not used for habitation or long frequentation; and showing no sign of any human visitor after the late stone age until recent times. The following are the sorts of secondary burial so far proved (and also drawn, photographed etc.) in *Lobang Jeragan's* few square feet.

- N (Niah) 1: Complete skull, separated but placed back with main skeletal remains; haematited.
- N 2: Skull and/or other bones associated with double-spout, burnished earthenware water-vessel, specially for the purpose.
- N 3: Bones (including skull and jaw) fragmented, in specially made barrel-shaped earthenware urns with paddle beater decoration — sometimes of more than one individual per urn (mostly small babies).
- N 4: Few fragments of skull only, in ordinary earthenware pot.
- N 5: Bones painted with haematite placed below mosaic of broken potsherds (mostly juveniles).
- N 6: A similar arrangement of bone mixed with “cremated” (usually adult; and in some cases this is done *subsequently* to 5, as a tertiary phase for adults only?)
- N 7: Modification of Nos 2, 4, 5, 6 placed *under* a rock slab, deliberately.
- N 8: Burial of limb bones only — no trace of head, ribs, fingers etc.; with or without associated earthenware (one body apparently tied in a bundle, headless and pushed downward).

Most of these secondary burials are in the upper layer of the Jeragan deposit, but forms of N 1, with special emphasis on haematite of the skull, merge into extended primary burial.

- N 9: Extended primary burial, painted with haematite, but only the simplest or no grave furniture; arms usually flexed, including one or both hands on chest or one under chin, one over pubis; no specific orientation of head, and often face turned from axis.
- N 10: Extended primary burial in small log coffin subsequently painted with haematite.
- N 11: Extended burial of child, arms *crossed* (thickly haematited, as usual with young); including “twins”, bodies overlapped, head to toe (age c. 2 years).
- N 12: Extended burial of a new born baby with *no* haematite, placed on or beside adult's chest (? mother dying in childbirth).
- N 13: Semi-flexed burial, upper trunk curved, legs crossed.

There are no primary burials overlaying secondary burials in this cave and only a few secondary burials (all urn type) as deep as the primary burials. The word “primary” is, however, not here being used in quite the simple sense of Stöhr (p. 8). To find a burial with no after care, “all relations have been severed”, we have to look at the West Mouth of the Great Cave (3 km. away) and Gan Kira (4 km.). Here we find extended and other burials showing no signs of *subsequent* care or re-treatment, including:

- N 14: Extended burial, well laid out on rush matting and with associated objects (and netting), earthenware, clay beads, shell and mica ornaments, stone

artifacts etc. — chin on chest, pointing inward to darkness (East) and not-often haematited.

- N 15: Double-burial, perhaps man and wife, so that one head points outward.
- N 16: Extended burial in hollow tree-trunk or crude bulky coffin; C-14 date for one of these gives 497 B. C. ($\pm$ 85 years; Groningen No. 1907 of 1958).
- N 17: Flexed burial, knees bent up to chin (probably mesolithic; cf. N 12, but more aged and "deliberate").
- N 18: Contorted burial, including arms spread out sideways, legs twisted, trunk headless (cf. *Man* 1957, Pl. P).

On the "secondary" side in the Great Cave we have also the following, not represented in *Jeragan*, but there interspersed among the numerous primary burials:

- N 19: Masses of mixed-up haematited bone, laid out along a strip roughly equivalent to the size of a human body (but representing several).
- N 20: "Haematite Babies" — clusters of similar remains from infants, and arranged in rough circular form (these relate to burials in large coloured earthenware urns in the Great Cave; cf. also Colour Plate in *Asian Perspectives*, III, 1959, 2).
- N 21: A handful only of bone inside two fine earthenware bowls overlapped and placed far back in the cave dark (found twice; once 40 yards from nearest other burial, perhaps a token offering — there are also signs of this in *Jeragan*, not listed above).

In addition to these, we have in the West Mouth and in several small rock grottos along the river separate skull treatments of two main kinds:

- N 22: Whole skull without other remains or associated objects; two of these are certainly in a full palaeolithic context, one giving a C-14 date of between 37,000 and 40,000 B. C. (Groningen 1338 & 1339 — a very early *Homo sapiens* youth fully illustrated and analysed in *S.M.J.* 15, 1960: 323-349).
- N 23: Skulls on rocks or shelves at the surface; these include individual skulls heavily deformed and elongated, which are probably of later date (the Melanaus in this part of Borneo have practised skull deformation until recent times).

The above methods were all practised during some part of the stone age, though sometimes later also. But within these same limestone labyrinths — 5 km x 4 km in area — there occur a great many later burials, including the following believed to date between about 500 and 1500 A. D. (mainly 700-1400 A. D.).

- N 24: A massive conglomeration of fragmented (not charred or haematited) bone scattered over a wide area of cave floor and associated with imported Chinese ceramics and much other furniture.
- N 25: "Ships-of-the-dead", lidded boat coffins, so far found in three caves and associated with wall paintings (cf. *Man*, 1959, Colour plate A).
- N 26: Cremated remains as pure ash in Chinese stoneware jars (full "cremation"; the only examples so far at Niah).
- N 27: Ash mixed with uncremated bone in jars (semi-cremation).
- N 28: Scraped and/or bleached bone placed in cave grottos (limb bones mainly).
- N 29: Skull and bones thrown into crevasses and fissures (? deliberately — cf. below 60).
- N 30: Remains under a plank on an arrangement of leaves with T'ang/Sung period sherds and beads (see *S. M. J.* 12, 1958: 618).

There are other Niah variants of a later date and mostly of the last 200 years (in addition to the deformed skull placings already mentioned above):

N 31: Small wooden plank coffins containing bones of one or more individuals, including babies.

N 32: Jar burials, with *whole* corpse (? forced in) and/or bones put in later.

Not represented in our archaeological results (as yet) but used in the area within living memory or currently, are these further burial practices:

N 33: Inside a "living tree trunk" (cf. Stöhr, 166; *S.M.J.* 8, 1956: 459).

N 34: Burial in cut trunk, drained through bamboo tube for subsequent secondary treatment.

N 35: Secondary burial into hollow tree.

N 36: Coffin burial on hill top (for the poor; bones sometimes recovered individually in basket by "a greatly bereaved one").

N 37: Ground burial marked with *nisan* posts (Islamic influence).

N 38: Jar with primary body, or bones direct, placed in or on a post of Melanau *keliring* type (cf. Stöhr, Chapter VIII).

N 39: Body placed against a sloping board at first; then in coffin for a week inside house, before going on to cemetery (where board is often placed *over* grave again).

N 40: Coffin placed in *salong* type hut near house; left there for years before secondary burial in various forms — including first into jar (7 nights) and then into similar hut in cemetery (cf. G. Barnes, *S. M. J.* 12, 1959: 643-5, with 4 plates).

Here, then, within 20 square km. of what is still only a thinly populated part of Borneo we have forty forms of burial demonstrably practised, no doubt we shall find others as the work continues. It is likely that those recorded represent only a fraction of *all* that have existed in this particular speck of Borneo's nearly half a million square kilometres.

Now there are about 100 caves already known to us at Niah, and many more which remain to be explored — but few of the "obvious" caves and some of the most difficult have been favoured for funerary use.

Thus, the *Lobang Tulang* cliff grottos can only be reached by an agile and slender adult, and involve crossing a formidable crevasse and squeezing up a narrow rock funnel (I have been unable to get through since 1957) into a dark chamber inside the mountain. Here are secondary burials of early metal age overlying a few of late stone age with quadrangular adzes. Ashes and bones of the former are associated with masses of Chinese objects, including thousands of ceramic sherds, over a thousand glass beads, a hundred metal items used as secondary burial furniture, along with Chinese coins, two of which are datable (762 & 961 A.D.).

The Jeragan cave, which I began by instancing, is the highest of several set one above another, and to reach it at all you must climb up, inside the others and through 200 yards of difficult darkness, to emerge superbly high in the cliff side (this one I can just manage, with the help of a portable aluminium ladder). Yet bodies were brought up to these difficult places — and beautifully treated in death.

Another and even more difficult cave, *Lobang Tikus*, involves three quarters of an hour of rock-climbing in darkness. Yet initial exploration of this high cave has immediately revealed cremated secondary burials with neolithic pottery. Those remains had to be taken there without the benefit of areal survey and

electric torches, which alone led us to "discover" this cave after seven years work at Niah, in July 1961.³

(ii) *The Present; Inland "Megalithic" Culture.*

The upland Kelabits who live far inland where Sarawak, North Borneo and Kalimantan meet, have on the whole been subject to less outside influence, directly and physically, than any other people in Borneo except the closely related Sabans in the headwaters of the Bahau. Nearly all the other really remote settled people of the interior have, in the past century, come into easier contact with administration, cash economy and the rest of modern civilisation, with accelerated cultural effects for themselves. I have been fortunate in spending over three years living among the vigorous Kelabit (and Saban) mountain people.⁴

Dr. Stöhr has referred to my published Kelabit work (VII & XIV), but I fear this has remained fragmentary and the following is the first available definitive record. Four main sorts of primary burial have been operative in this century among the Kelabits of my main study area, covering 600 square miles (equals 0.2% of Borneo).

K (Kelabit) 41: Wooden lidded coffin in house; on legs and with drainage bamboo:

- (a) *balang* type ("tiger dragon");
- (b) *payou* type ("antlered stag");
- (c) complicated decoration but not carved;
- (d) simple.

Today, these types relate to socio-economic status, but not necessarily or strictly.

K 42: Direct, tied up, crouched, into a Chinese stoneware jar, cut open along medium joint with red-hot wire.

³ In case Dr. Stöhr and others can — as I hope they will — visit Borneo to reorientate library studies on the ground, part of our Sarawak Museum excavation policy is, where possible, to leave a sample of burial types and other specimen material *in situ*, thus forming a site Museum. Those with specialist interest who have already visited some of our sites at various stages in the last decade include — palaeontologists Sir Wilfrid le Gros Clark and Prof. G. H. R. von Koenigswald; archaeologists M. W. Tweedie (active colleague 1949-'57), Dr. W. Solheim (active colleague 1959-'61), Father Frank Lynch S. J., and Senor Arturo Luz; historians Sir Steven Runciman, Sir Robert Scott and Dr. Juan Francisco; zoologists Lord Medway (active colleague 1957-'61) and Mr. J. Berwick.

⁴ Continuous periods of over 6 months during 1945, 1946, 1949 and 1952, shorter periods regularly since. For comparison in what follows, I should mention that I have also experience of death observances among the Kenya (Leppo Tau, Berawan, Sebu), Punan (Limbang, Niah, Bahau), "Klemantan" (Belait, Bisaya, Sekapan and Ba Mali), Dusun and Tagal ("Murut" of North Borneo), Potok-Murut of North-west Kalimantan, Land Dayak (Bidayuh and Salakau), also Iban and Balau more casually. For a background account of the relationship between burial observances and megalithic activities see *S.M.J.* 12, 1959: 656 where I list 26 forms of the latter. Most megalithic activity in Borneo nowadays is "secondary" burial in character; but this almost certainly was not always so.

- K 43: With or without a few planks as coffin, but direct into ground on a knoll, in grassland or scrub.
- K 44: Buried in ground under the house (still-born child).
- K 45: Body left out in jungle, far away (exceptional; usually "crime" or "outsider").
- K 46: Bodies taken by father in basket and buried in jungle near normal cemetery (twins).

Of these methods, the first is much the commonest at the primary stage, and ordinarily, in recent times, secondary burial does not take place except from this method, or, less often, from the rarer jar *primary* (K42); there is thus no retreatment for K44-46. Secondary burial methods have been recorded as follows in this area:

- K 47: Jar placed on top of high ground — sometimes singly on a mountain pass beside the path, sometimes several in line on a lower knoll. (Jars run from heavy T'ang stoneware to Ming porcelain and later; all Chinese made)
- K 48: Jar with stone menhir erected alongside (or sometimes another form of megalithic monument).
- K 49: Jar placed at one end of a *nabang* ditch, intersecting a path; or as part of an irrigation project, or river diversion scheme.
- K 50: Bones direct into stone cyst or "slab grave".
- K 51: Bones placed (sometimes buried or in small pot) under a stone table.
- K 52: Remains inside stone urn or vat (made of soft sandstone, some 5 ft. long).
- K 53: Remains placed in specifically made small grottos worked into big sandstone boulder.
- K 54: Stone pile on knoll beside stream — relics of the deceased and all his value objects, covered with thousands of stones carried up from riverbeds².
- K 55: Bones taken in basket and hung up in association with somebody else's jar/megalithic or other secondary burial (childless widow, etc.); basket may also be placed among rocks.
- K 56: Bones buried separately under a lean-to roof on a knoll or thrown into a deep crevasse.
- K 57: Whole coffin carried to rock-shelter, without being opened (one record, *payou* type of K 41 b, direct transfer, see *S.M.J.* 12, 1958: plate 19 a).
- K 58: Whole jar carried to rock-shelter or high point (direct transfer of K 42).
- K 59: Jar with bones; jar tied up in tree — only seen at Bawang valley, six instances (see *S.M.J.*, 12, 1958: plate 18 b).

There is also a distinct *tertiary* stage, until recently very important:

- K 60: Hiding skull, finger and some other bones (local variants) in a remote rock crevice or riverbed hollow.

² This stone pile disposal is only used when a rich man is unmarried and without heirs, — so that serious dispute is likely to arise over the division of his property if left intact; see my book „World Within“, London 1959, which also contains a general description of death feasts, *irau*. Dr. Stöhr has mentioned (p. 186) the earlier view of Banks (*S.M.J.*, 15 (prewar), 1937: 415) that only some of the megaliths are connected with burials. But my predecessor as Curator was not told the whole truth — and even after over 15 years contact I am still being let into new secrets about Kelabit secondary burial practices, some details of which are still undoubtedly in reserve.

This last plays an essential part at the end chain of burial rites and is most secret. Apart from any deeper significance, there is a practical cause. Where a group have conspicuous secondary burial sites, like the Kelabit — and their conspicuousness is part of the purpose — whenever other groups exert pressure on the area, there is fear for the "dead". (For instance although the Kelabits successfully defeated several major headhunting raids by the Kayans in the last century, some heads were taken from outlying jars and megalithic secondary burials — these counting as achieved heads for those Kayans).

The above is the briefest account of what is an elaborate pattern. These are methods I know at first hand. The list at least serves to suggest the range of variation, past and present? The Kelabits themselves scarcely recognise such differences. The categories are mine, from outside, reflecting little in their own funerary thinking. For them, death is viable, a fluctuating continuation of uncertain life. Its observances may last for generations, involving the same sorts of individual decision, volatile emotion, shifting economic and topographical circumstances which produce the rich texture and exuberance of Kelabit living. Many factors operate to modify or offset fixed categories, exact rules or prescribed sequences; for instance:

- (i) changes in fashion;
- (ii) local topography, available rock and other materials, drainage, movements of long-houses in relation to burial grounds (in the recent past, most Kelabit long-houses moved at least once a decade);
- (iii) local long-house traditions;
- (iv) family traditions or preferences;
- (v) personal preferences and prejudices — often willed on to a family or community by the deceased;
- (vi) climatic and seasonal influences, specially those affecting rice harvests and the general prosperity level in the particular decade (or district) of the secondary burial cycle through *irau* feast economics;
- (vii) vital statistics, health and sudden concentrations of death (from war and more recently, several major epidemics) disturbing any theoretical "life and death spirit trajectory".

It will be noted that there are few exact parallels between Kelabit and Niah observances. Prehistoric jar burials at Niah are close in method; there is a gap of over a thousand years between the last Niah jar burial and modern Kelabit. Earthenware urns are not used in Kelabit burials or indicated in upland excavations; but miniature ones, thumb size, are commonly placed inside stone-urns and cysts, implying an earlier usage before imported Chinese jars. There is no present trace of bone-washing or painting, burning or any form of cremation in the uplands. The absence of death ships is natural enough, since until very recently there were no boats of any kind on their seldom navigable mountain streams. But their coffin shapes, particularly those of *balang* and *payou*

(K 41 a & b), parallel some lowland forms in North Borneo and can be more remotely related both to Niah death ships and Ngadju coffins in broad conception ⁶.

What now strikes one, when examining Dr. Stöhr's "labour of love" from this equatorial end, is the richness of reference information he has been able to locate, — and at the same time its inadequacy. Apart from the late Dr. Schärer, ("Die Gottesidee der Ngadju Dajak", Leiden 1946) no one has yet published an intimate study of what actually *happens* from the imminence of death towards the last flicker of descendant memory. Schärer, himself as a missionary, was unable to see all clearly. Even so, that part of his work published is only a fraction of the more extensive material he left in notes and which he had planned to use for a further volume on death rites — when he was himself untimely lost to us. (Stöhr had access to this manuscript; we also have many original Ngadju texts on microfilm in the Sarawak Museum, thanks to Professor de Josselin de Jong).

In an "illiterate" place like Borneo, the evolution of current practices cannot be fully understood without basic texts and reference to the past, in which Schärer excelled. We can be greatly helped by the mythology. It can therefore be dangerous to segregate too absolutely any part of funerary observance, primary burial, cremation, feast or teknonomy. The same applies, of course, to most other things in highly pressured, shifting, dynamic groups such as we have — to a marked degree — inside this island. Only by following both a trajectory of time in living procedures for the dead and trajectories of time out of the almost or wholly forgotten past, can we reconstruct an adequate picture of the present. Along these lines, I think, for instance, that it would soon become clear that "giving up secondary burial" (Stöhr) is not necessarily what occurs; and I have even experienced conditions where *secondary* burial (in its psychological total sense) is actually "achieved" *before* death.

⁶ These death-ships and their meaning will be fully treated in a forthcoming *Artibus Asiae* monograph, already mentioned; for Kelabit coffin burial see the picture of our Museum exhibit in *Sarawak Ann. Report*, 1960, facing p. 176. (Kuching 1961):

Moreover, in the interior of Borneo particularly, the tremendous network of customary observance — not all of it based on completely integrated belief — has grown up alongside changes in burial custom; sometimes to such an extent as to submerge the point of origin almost beyond recognition, a familiar enough process in any form of evolution. Thus it is possible, without stretching it, to find surviving elements of the megalithic culture in the most “surprising” places, such as the slabs of rock placed with such difficulty on the very top of Melanau *keliring* burial poles used as bone receptacles as the climax of their burial rites (cf. N 38 above).

Again, the central ritual of the Leppo Tau, senior clan of the Kenya, is the *mamat*, climaxed by the rolling of stones. Dr. Stöhr has noted the *mamat* as comparable in complexity to anything in Borneo; but from the only published data (in Dr. Elshout's “De Kenja-Dayaks uit het Apo-Kajan-gebied”, The Hague 1926) he concluded “it seems clear that the feast has nothing to do with death rituals” (p. 104 footnote). Actually, *mamat* is basically a “death feast” (among other things) parallel to Ngadju *tiwah* or Kelabit *irau*.

Similarly, the central Sea-Dayak communal rites of the great periodic *Gawai Antu* feast derive in the Iban mythology from the “God” Apai Puntang Raga teaching terrestrial Serapoh better methods of dealing with the journey of spirits from corpses and controlling uninvited returns due to inadequate funerary treatment. The apex of *Gawai Antu* is the set of elaborate procedures (*Ngalu Petara*, *Ngerandang Jalai* and *Ngelalau*), which first clear and then enclose a path for the spirits of the departed to return, via the following incantations of the *Nimang Jalong* song-cycle, directly to the longhouse and this actual feast. Only when the dead have, in this way, rejoined the living, can the sacred wines (*ai jalong timang*, etc.) be taken; and only after this should widows and widowers discard their mourning through the *ngambi tebalu mansau* ceremony. Lustration, the cult of ancient Chinese jars, and the detailed set of mourning prohibitions now lifted, all link in this set of innovations, dating “twenty-seven genealogical generations” back, before the Ibans had moved westward, into Sarawak. Iban *Gawai Antu* also has some intimate parallels in Kenyah *mamat*. Many other parallels may be drawn for Borneo funerary rites and “relics”.

★ ★ ★

I have by now exposed myself to the risk of being criticised for what I criticise in others, — roaming too far off the reasonable factual data. But we must equally try and sort out the background *ideas*, difficult though they are. Death rituals cannot be condensed into sentences and simplicities. We must not take the mosaic of literary references at face value or worthwhile in itself; nor consider we have as yet anything like a true picture of Bornean life-death-living. Once we have accepted this qualification, then the existing data are valuable; alas, often irreplaceable. Dr. Stöhr's contribution is that he places before us the western information in a changing situation; what we have to draw upon, for better or for worse, out of the inadequately literate past. In this, of course, our understanding of death rituals is in no worse plight than anything else. Now at least we have his base of literature from which to start out afresh.⁷

Here we must be very careful not entirely to impose European thinking and its binding routines onto Bornean thinking, which in the deepest sense is "non-classificatory", tends to look at everything as interrelated and part of one vast (even where inexplicable) entity. This is where so much recent Western study has suffered. We must not (for instance) fall into the terminological trap of thinking that what is primary on paper need be primary in life. What is second can follow the intricate spirals of the Leppo Tau Kenya tree-of-life back into primary and out again beyond. There is almost another, non-numerical dimension of thinking involved. Not for a moment am I postulating anything mystical, but rather something which is nearly proto-scientific, — a different line in the development of man's attitude

⁷ Even in the field of linguistics, which has received more general attention than anything else, Drs. A. A. Cense and E. M. Uhlenbeck in their recent Introduction to "Critical Survey of Studies on the Languages of Borneo", (The Hague 1958: 3), a monograph comparable in comprehensive scope and competent presentation to that now under review, remark:

„Published data consist mainly of incidental remarks of explorers and travellers, who in their works occasionally also included wordlists, terms for various institutions and sometimes a few set phrases or isolated words. The most important contribution to the knowledge of many Bornean languages is due to missionary effort. The Bible translations and other translations of christian literature, valuable when no original texts are available, are for many languages often practically the only source of information. It is impossible to provide a reliable picture of the present linguistic situation on the island by means of these data”.

The whole situation in Bornean social science problems has been so negative, piecemeal and often trivial, that one is entitled to feel the wider field requires a proper, overall survey and a new research programme *ab initio*.

to rich environment, well suited to life in equatorial caves or evergreen rain forest; but fatal, perhaps, on the cold plains of Friesland or in the fierce sands of Araby.

Nevertheless, we, the prisoners of our own conditioning, have to make Ngadju or Kelabit prisoners in word. That does not mean, though, that while expressing as best as we can what is convenient in our writing, we should obliterate what is inconvenient or difficult to simplify in their own ideas — and (in the process) oversimplify the living picture so much that it may become simply derivative or crudely “primitive”. This particularly applies to death rites and their associated beliefs, which can be of the gravest concern to the group and not considered suitable for outside ears at all. It is particularly difficult, in Borneo, to penetrate some of the inner attitudes regarding the spirit and the soul. Further, the *incompleteness of death observances is one of their commonest characteristics*, for a multitude of reasons. This is not only so among groups who in recent times have not practised secondary elaborate burial in recognisable form, such as the Bedayuh Land Dayaks. It has therefore to be realised that *talk* of any practice (to an outsider) as common within the group can be seriously misleading from two sources: the privacy of *feeling* and yet the pride in public *demonstration*. The sort of rites the Berawan-Kenya and the Kelabit-Murut tell you they *must* observe, may in fact only be achieved once in a life time. Many short-time observers get the impress of this “top picture” only, which tends (naturally) to be the one the group prefers to stress. Certainly, the aristocrats and leaders — whose views alone can ordinarily be heard by anyone who does not thoroughly percolate the group — are likely to use such emphasis.

The “great family” in several widely separated parts of Borneo is often that which has fulfilled, over more than one generation of death, a series of very publicly performed and supported rites, so extensive and expensive that the standard has not been demonstrably surpassed by any other. This has occurred, within my brief knowledge, both for secondary and non-secondary areas.⁸ In order to achieve such a series,

⁸ Here, as elsewhere, for reasons of space and intelligibility, we have to make such distinctions of area or type of ritual (cf. Stöhr, Chapter I). Otherwise, the material becomes virtually unmanageable. But at this stage I would emphasise that in much that pertains to death and nearly everything that relates to the afterlife, *any* sort of boundary — in thought, usage, or space — is liable to be extremely vague in Borneo, particularly where full secondary burial rites are not now practised; and the obscurities have tended to increase in recent times, partly because of actual changes in custom, but largely also

it is not in itself *enough* to be born to long genealogical status; to maintain and if possible enlarge the family affluence; and to finally perish as one of Borneo's cultural millionaires. These are preliminary essentials. Next comes the basic condition: that you are not only materially but psychologically "high"; accepted as a leader of clarity and decision, of "character" outstanding within the ethos of the community — high above its ordinary standard (even to the extent of eccentricity). To be great from the moment of death in such surroundings, it is vital to be great out of womb, *through life*. Only thus can a "line" normally maintain the socio-economic status and prestige, without which the community will not fully participate in making rich observance (including personal elaboration of post-death) possible. With this, too, ties in the desirability of physical strength, good blood and fine health in the family. Too many early deaths can heavily debit the balance of status. A long and greatly respected life can add wide credit.

To understand these difficulties, long field studies are essential. Rituals of death (or any other occasion) are carried through by living people. Descriptions of the position a corpse is put in, or the style of coffin, are minimally meaningful without adequate descriptions of how the participants *behave* at every stage. No one can claim to interpret the meanings, *to the people concerned in them*, of such rites unless they have either observed and participated therein, or had the fullest published reports by skilled observer-participants. In 1906 there were not more than two accounts of death rituals at this level; in 1961 there are still less than six available; of these only Schärer on the

because of a much heightened awareness of the "Christian" attitude to many Bornean ideas, which have lately often from this source been treated as meaningless, lunatic, obscene etc. An example close to Kuching: just half a century ago Howell defined and vividly documented for the Saribas Sea Dayaks, the distinction between *Nyuran*, the initial dirge and rites immediately after death, with the corpse still in the house, from the much more elaborate *Sabak Nerenkah*, with professional singing from a swing and full projection of the spirit into the afterworld. Recent studies in the same area show a weakening of this distinction and a general extension of the term *Sabak* (*S.M.J.* I, 1911, 1 : 5-73; B. Sandin & W. P. N. L. Ditmas in forthcoming issues). Such changes have, of course, been going on and off since the start. But their later day acceleration makes it more difficult now to sort out "primary", and "secondary" elements which are significant in understanding the evolution of death practices — frequently evolution by reduction — over the past century or so (the period for which alone descriptive material is sufficient in Borneo; Stöhr cites 2 references before 1830, both casual).

Ngadju (since 1938) has tried to give a real *insight* into the *native* meaning of the whole.

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I doubt if anybody could have done better, — faced with a multiplicity of mainly weak reports, — than Dr. Stöhr in his general treatment of the unpublished data. In his final chapters he widens his angle of view. Very few library scholars can resist this temptation — here succumbed to with unfortunate and even serious results. First he discusses, under eleven headings, the distribution of “burial forms” (pp. 168—172). Too much stress is put on the “flight from the corpse” by the Punan (p. 168); and more emphasis could be put on *modern* cave or cliff burial, or which Sarawak Museum records now cover at least eighteen areas (cf. Stöhr’s three, p. 169).

But with his next Chapter (XIII) we enter a wider world of roaming mind, glancing out over the exciting horizon of generalisation, towards the sunset of triumphant conclusion. Presumably, if you don’t reach that rainbow’s end, you won’t get your university doctorate in Germany? Far too widely, writing must end in “a theory” or two. But whatever the academic needs, I could have wished these nine pages had not been *published*.

The first paragraph (p. 173) is nearly enough for me. At once, and in line with most recent writers from outside the area, Stöhr has fallen (hook, line and lingga) for the “Indian approach” with which we are already so familiar throughout the writings of Dr. Quaritch Wales. The theme seldom varies. Indian objects have allegedly been found near Kutei, Sambas and a few places elsewhere in lowland south and east Borneo, and sporadically (so sporadically that the Indians themselves usually ignore them) along the west coast (nothing in North Borneo).

The line of underlying argument runs roughly as follows:

- (1) It is obviously impossible for these “primitive people” to have developed such elaborate ideas and wonderful associated objects “on their own”. They are culturally and intellectually backward human beings. If this is not taken for granted it becomes too uncomfortable for cultured gentlemen in London, Leiden or Leipzig (or Kuching, for that matter), surrounded by religious debris and ritual sterility in a materialist and sophisticated modern tempo, 20th century wise.
- (2) Clearly, therefore, a major influence must have come from somewhere else outside.
- (3) What more natural that it should come from the West?

- (4) Much of western Dutch field work and scholarship has been done in or for parts of Java where there has indeed been a strong "Indian" influence in *recent* times. Recent, that is, as compared to the fundamental conceptions which the Niahans or Ngadjus developed out of stone ages. This Dutch basis further favours a western inclination; albeit superficially. (The best Dutch scholars seldom succumb).
- (5) So few western scholars are competent in Chinese and have any feeling for the Chinese, that there is genuine ignorance about this obvious alternative source of influence — if influence must be adduced.
- (6) Recently, strong political feelings have made it additionally easy to be unsympathetic to Chinese influence — in some countries, it has actually become dangerous to recognise China in public.
- (7) The few Indian objects found scattered about in lowland Borneo prove Indian influence on the whole culture⁹.

This is the climate of generalisation in which Stöhr — and unless we are careful all of us — so easily get mental damp-rot. For example, he equates Buddhism exclusively with Indian influence, apparently being unaware of the enormous power, organisation and elaboration of Buddhism reaching apotheosis in T'ang China. He remarks that "strangely enough there is no way of proving considerable influence of the Chinese high culture" (p. 173). Strange indeed! If we take only the last thousand years, the material evidence for major Chinese influence in Borneo is extremely strong; on the scale at which we find it — whole high-class cemeteries of beautiful T'ang and other early export wares, at Santubong and Niah, 350 miles apart — only by a reversal of common sense can we suppose that material impacts were entirely unconnected with Chinese intellectual influence. There is in scale nothing comparable that can be called "Indian". The highest Kelabit chiefs may still lay their bones in T'ang jars, the most valuable things in the world to these people — as also to the completely unconnected Dusun, a fortnight's walk away across the mountains to the north (and widely again in the Philippines and Celebes).

We will not digress into the rich literature of Chinese burial customs, — both long before and after Buddhism reached China in the first century of the Christian era. Let us simply take a recent survey on one aspect; bone cleaning, "lustration" (in Dr. Stöhr's sense). Professor Ling Shun-Sheng has brought information from all over China to show the antiquity of bone washing there and throughout the area (*Ann. Academia Sinica* 2, 1955: 175—194). The Hon-Han-Shu (25—220 A.D.) of the latter Han has a wonderful description of

⁹ For the West Borneo Indian data see my long paper in *J. Malay. Br., R. As. Soc.* 22, 1949: 33-110, cited passim by Stöhr (p. 176).

enormous family coffins into which these bones were placed after primary burial and secondary removal. In the T'ang there are good Chinese records of bones taken up into cliff grottos for final disposal; and in the Sung secondary burial in cliff or tree (Hunan, Sze Chuan).¹⁰

Secondary burial in urns and jars has long been and remains widespread in China. Moreover, the Chinese have continued to practice these burial rites actively as in the islands. A recent paper on the skeletal cult ("Skelettkult") by our friend (and 1958 guest at Niah) Professor G. H. R. von Koenigswald, gives a first-hand account of Chinese in Java practising secondary burial with elaborate treatment of the skull, and jar burial, conforming closely to methods detailed by Dr. Ling for Taiwan (*Anthrop. Anz.* 24, 1960: 169); similar secondary urn and other burials continue on a massive scale among Chinese refugees and residents in Hongkong (B. D. Wilson, *J. Hongkong Br. Roy. As. Soc.* 1, 1961: 120).

As in Java and Hongkong, the immigrant Chinese in many parts of Borneo today similarly observe burial rites transported from the mainland and of very ancient origin. Dr. Ling is oddly unaware of this. Instead, he has a try at the "Dayaks" (= Ngadju) of south-east Kalimantan in Borneo (Dr. Stöhr's coverage of sources was not then available, of course). Taking his idea of Borneo people on the particular subject of bone-cleaning by fire (which is what most Bornean "cremation" consists of) he reaches this conclusion:

"The most important feature among the natives of Borneo is the burial of bones after their being cleaned, by fire... In the opinion of the aborigines, the burning of the bones is an absolutely necessary act for purification". (p. 184).

This exaggeration of an occasional practice is modelled to support Dr. Ling's argument. Chinese scholars are just as ready as anyone else to plunge off into globe-coverage after giving basic facts within own competence. Thus after an orbit of astronautic scale and speed — bringing into vision not only folk like American Indians, Melanesians

¹⁰ Since this was written, valuable new material, both from previously unavailable Chinese sources and first-hand observation, has come to hand in a long paper by Dr. Inez de Beauclair for Kweichow in South China (*Bull. Inst. Ethnol. Academia Sinica*, 10, 1960: 127-199). This shows the relative simplicity of contemporary burial practices in the area ("only in case of contagious disease do they cremate the corpse"), compared with the complexity of recorded past practice including cave and urn burial, "burial on trees", bone-washing (including with wine, several years after death), cliff exposure (including for the unmarried), and burying alive, both as punishment and as the last wish of the aged (in Shansi people completing the normal limit of life-span cycle, sixty years, were left to die in caves; cf. W. Eberhard, *T'oung Pao* 36, 1942).

and Australian aborigines, but several Amazon types including Arakwaks, some marginal Esquimaux and the Easter Islanders of yore — this Professor pulls out of his power dive into a conclusion which is at least documented for the Chinese mainland:

"The cleaning of bones with fire in South-east Asia is a *primary* trait of this region and bears *no* relationship to the Buddhist practice of India". (p. 185).

If the second part of Professor Ling's thesis barely knows what the first part doeth, so it is with Dr. Stöhr's Chapters XIII cf. I. After dismissing Chinese influence, the German scholar takes his plunge back into the burning *ghats* of Hinduism, ending up in intellectual *suttee* (pp. 176—181). He begins this with candid admission, "the Hindu-Javanese colonisation of Borneo is a historic fact, knowledge of which is only very scanty" (p. 176). But (we must ask) in what sense, then, is it a "fact" — "historic"? Where is trace of *colonisation*, anyway? Dr. Stöhr relies on two sources. First the well-known Kutei inscriptions from south Borneo, uniquely early. Second, the "cremation" practices of the Ngadju and adjacent peoples (same as seen to be aboriginal or perhaps "proto-Chinese" by Dr. Ling) in the south-east. Here again, Stöhr writes:

"Nearly everybody who reports on cremation in south-east Borneo attributes this to Hindu-Javanese influence. This *seems so self-evident* that nobody ever goes into it in detail" (p. 177).

Precisely. Each western authority has supported the last, without good data, because it fits the popular view. Dr. Stöhr follows this procedure faithfully. Because there are superficial similarities between cremation practices in south-east Borneo and Bali; because Bali is unquestionably latterly "Hindu", *therefore* such practices in the Borneo interior are Hindu influences. With his great death knowledge, Stöhr sees difficulties where others might not. Why are the Ot Danum "cremation rites" so scrappy, whereas they are highly organised among the Siung *further inland*? He suggests this is due to later influences. But the whole thing can just as well be explained if we allow that there is a much *earlier* use of cremation as a form of lustration, often subsequent to secondary burial (a form of "tertiary" burial) and that some remoter people kept this up in something like "pure" form. There is direct evidence that this is so at Niah (where no Indian influence is indicated); and such practices (as noted) have been common in China and elsewhere, regardless of Bali or Bombay.

Dr. Stöhr is too good a student to feel fully satisfied by his own thin conclusion. He therefore whirls out into a parabola of unconvincing

support argument. He is led to demonstrate that *tiwa*, the term for the main Nagdju secondary rites, may derive from the Javanese *tumi-wahaken*, meaning "to get rid of", to be free. Malay scholars will have little difficulty in finding alternative solutions and derivations; not that it matters, either way. This is the sort of word game one can play equally well, whether as golf, polo or pocket billiards. C'est magnifique — mais c'est pas la science.

Then, in the next paragraph, he gives the only specific evidence, other than that inevitable Kutei stone. He turns back to the Land Dayaks (upon whom much has been written, too little observed) for their brand of cremation. Here he sees "destruction" of the corpse, rather than lustration; and therefore more of a *Buddhist* parallel. He states:

"Of the Hindu-Javanese states some were primary Buddhistic, as for instance the mighty Brunei on the north-west coast of Borneo. The Raja of Brunei ruled up into the past century over parts of the Land Dayak group" (p. 180).

This quaint statement is solely based on an old supposition by Charles Hose and has never been even faintly substantiated from any source, — including the collection of *sila-sila* texts now in our Museum, and extensive preliminary excavations we have made in the recent capital of Brunei, Kota Batu. No doubt, Buddhists did come, were influential. But Kota Batu was a major entrepot of the China trade for at least seven centuries prior to the destructive arrival of Portuguese and Spaniards in the 16th century. (Our excavations there have produced some 80,000 potsherds, including from China, Indochina and Thailand (Sawankalok); and many Chinese cash coins, 36 datable by reigns: the earliest 723—768 A.D. (2); the latest 1402—1425 A.D. (5); the most 999—1023 A.D. (11)).¹¹

Dr. Stöhr has to use this supposed early Brunei ruler's religion and realm to work up some connection between Land Dayaks and Buddhism. Because (he argues) some Land Dayaks were ruled from Brunei (450 miles away) during the relevant period; so:

"Apparently the introduction of primary cremation came from this (Brunei) side. This supposition is supported by the discovery of Buddhistic inscriptions, in the middle of the Land Dayak area, carved into a rock". (p. 180).

His source citation is No. 547: "CHAHABRA (1935) in BANKS, 1937, p. 411". This is the obvious case, out of Stöhr's 584 source notes,

¹¹ *S.M.J.* 8, 1956: 283-319 for a preliminary report on Kota Batu; no objects have yet been found there which can be classed as "Hindu" or "Buddhist".

where he has clearly not checked with the original. The reference is a casual one in a long paper on the northern Sarawak uplands.¹²

To date we know of no Buddhist or other inscriptions, in truly "Land Dayak" country — let alone "the middle of it". It should be added that even the famed Kutei inscriptions prove, on closer analysis, to be surprisingly unsatisfactory; there are grounds for suspecting that these may have been moved (perhaps even from another area) before they were deposited in the Djakarta Museum. As Chhabra himself wrote: "Their exact find-spot has again become a subject of controversy" (p. 38). Of course, there was connection between the Land Dayaks from West Borneo across into the South. Of course, some Buddhists came to and influenced Borneo. But on the present information, there are no grounds which could conceivably be accepted by any serious scientist for explaining Land Dayak or any other Bornean cremation as having been *introduced by Indian* Buddhists through Brunei (or Kuching, or Kutei) — any more than there are for assuming that Ngadju secondary cremation must have come from Bali or prehistoric Niah lustration via cremation by junk down the Yangtse. It is conceivable some of these things so occurred. But to *start* the argument that way round is unscientific as well as unwise. We should first search for explanations in what can be *demonstrated* on the spot. And surely the most reasonable basic working hypothesis, is that a great deal of Bornean culture stems from slow, elaborate developments of more ancient traditions — *Homo sapiens* dates at least 35,000 years here — which can only be fully understood (if at all) by means of extensive archaeology, the intimate analysis of folk-lore

¹² *S.M.J.* 15 (pre-war), 1937: 411-437; in the opening paragraphs, Banks glances at what was then known of prehistoric stone-work in Borneo. After inevitably identifying the famed Kutei inscriptions of a lost dynasty, he went on to Chhabra (misspelt Chabra, Chabhara, etc. variously by Stöhr and him). It seems doubtful, indeed, if Banks saw the paper himself — since he cites it as from *Bombay Br., Roy. As. Soc.*, 1935, I: 41, instead of *Bengal Branch*. The locality, *Batoe Pahat*, given by Chhabra is on the Tengarak River. The place name means "chiselled stone", used widely in South East Asia (the best known *Batu Pahat* is that south of Malacca on the mainland). Through the good offices of Dr. Alstair Lamb of the University of Malaya and Buchari of the Dinas Purbakala, Djakarta, I have been able to obtain fresh information on this ill-documented find, and a fresh photograph. The site is in the tidal reaches of the Kapuas River. There is nothing, either in the literature or on the stone itself, to associate it with the Land Dayaks (or remote Brunei). One might much better argue that the Sarawak Museum in 1962 is especially influencing Land Dayak belief, as plenty live within less than an hour's drive of it — while within a mile are Anglican, Catholic, Methodist, Salvation Army, Taoist and Sikh temples...

and careful comparisons of earlier clear observation (which is where the comprehensive death compilation by Stöhr provides one beginning at least).¹³

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It must not be thought that I am seeking to belittle Dr. Stöhr's theoretical outlook; my respect is already clear. But we are dealing here with certain principles in scholarship. If principles are not observed, our whole method is liable to be brought into disrepute. The more so since other sciences all around us devise techniques and improve standards at a pace which patently leaves anthropology standing. Let us for a moment use another approach to the same problem, also arising out of death rituals in Borneo. Take first a major discussion of Ngadju burial rituals by Dr. Quaritch Wales (1959).

In a lengthy paper, entitled "The Cosmological Aspect of Indonesian Religion", but based on his own specialised interpretation largely of Bornean Ngadju (*J.R.A.S.*, London, 1959; 99—139), this writer sets about demonstrating — among other things — the great importance of Indian influences (his thesis was published before Stöhr's was available). Since, to his highly educated British mind, it is inconceivable that the primitive Borneans thought up their tremendous ideology for themselves, he must to his own satisfaction demonstrate the only alternative conceivable to him as explaining "the origin of the esoteric religious concepts of the Ngadju Dayaks", viz.:

"I shall now state my reasons for concluding very tentatively in favour of Indian, or more particularly, Indo-Javanese influence" (p. 131).

His *first* reason for this view (which within a few pages is taken as proved) rests on the familiar odds and ends of Indian-style icono-

¹³ Relevant to the comparative literary method are two other studies which should therefore be mentioned here. Dr. W. Münsterberger in his "Ethnologische Studien an Indonesischen Schoepfungsmaythen" (The Hague 1939), seeks to draw (in German) familiar world-wide, sometimes breath-taking, conclusions by comparing origin myths in Indonesia, especially in Borneo (pp. 111-230). Confusion in presentation and the absence of adequate field material for many groups, makes it a much less satisfactory piece of work than Stöhr's, though it is stimulating in places.

The second study (in English) is Arne Martin Klausen's "Basket-work Ornamentation among the Dayaks" (Oslo 1957). This is a comprehensive study, so far as it goes — that is, covering the collections in the Oslo Museum. But the author is confused on Borneo ethnography, and it would have been better to have restricted the text entirely to what could be described from the material itself.

graphy and epigraphy which have been found in lowland Borneo. Unusual is the addition of the curious incense burner "from the Sambas treasure" (now in the British Museum) which he argues "to have been the work of a people marginal to Hinduism".¹⁴

Dr. Wales is on even sketchier ground with his *second* main reason, for Indian influential predominance, here quoted in full (my italics): "The Ngadju are *exceptional* in Borneo as practicing cremation, the bones being burnt at the "tiwah". Wherever cremation is found in Indonesia it is *generally* accepted as a *decisive* sign that there has been Indian influence" (p. 132).

Quite apart from the inaccuracy of this generalisation as regards Borneo, Dr. Wales is clearly unaware of Chinese sources. So he is able to reach exactly the opposite conclusion from that accepted as decisive by Dr. Ling (quoted above). Not that there is anything "decisive" about any of it. Cremation occurs in several forms; we have already seen a type closely akin to that found among the Nagdju in a stone-age cave at the other side of Borneo, Niah.¹⁵

To sustain arguments of this sort, over-simplification — especially when faced with a complex like this huge island Borneo — is almost inevitable. In this, Dr. Wales follows in the footsteps of a notable tradition. One of his announced (and oft-quoted) masters, Professor Mircea Eliade, sets an even stiffer pace for this kind of intellectual canter in his "Images et Symboles" (Paris 1952), where globe-circling arguments include jewels such as this:

"Shells play a no less important part in the funeral ceremonies of India. The conch is sounded and shells are strewn on the road from the dead man's house to the cemetery. In some provinces the mouth of the dead is filled with pearls. The custom is found again in Borneo, where a Hindu influence has probably been grafted on to an autochthonous rite" (p. 136; transl. P. Mairé, London 1961).

¹⁴ This attribution (p. 131) is by no means acceptable to most scholars in the area, few of whom regard it as having to do with any Hindu. Having often handled it myself, I think it likely that if it has any Borneo locale of manufacture, the relationship may be with the metal work of Brunei under strong Chinese influence. In any case, it is now doubtful that this "treasure" was found at Sambas at all; the circumstances (during the Japanese war) grow obscure.

¹⁵ It is only fair to say that Dr. Wales's theories are seldom static. In an earlier work "Prehistory and Religion in South-east Asia" (London 1957), already critically reviewed in this journal by Professor de Jong, he lays more emphasis on indigenous "evolution", but always within the framework of what he calls "Indianization". Indeed, part of his reason for thus promoting "local genius" is a manifest distrust of (or disgust at?) the strength of more subtle Chinese influences. He even infers that Chinese religious culture is itself fundamentally Indian (p. 125) as well as irrelevant to Indonesia (e.g. "we can also safely omit any Chinese element from consideration", (p. 126; and p. 133, 1959). This use of the "also" is vintage Quaritch-Wales.

After this astounding observation (unsupported by a Bornean reference) it seems somehow proper that Eliade's following sentence should be based on and attributed to Ngadju-bug Robert Hertz: "In Africa, a layer of shells is spread over the bottom of the tomb". Africa has a lot of layers, of course; but fourteen words suffice for the continent and the next sentence whisks us back among "ancient American peoples" (p. 136).¹⁶

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How bewildering this dialectic can get may be better demonstrated by a western example from my own fieldwork. Let us look another way at "decisive" (Wales) or "self-evident" (Stöhr) relationship between cremation and Indian influence, whether it be via Ngadju-Bali-Hindu, or Land Dayak-Brunei-Buddhist. I spent some months of 1960 repeating an intimate sociological study made at Bolton ("Worktown") in Lancashire during 1936—40. In the thirties, cremation, embalming or any Bolton treatment of the dead other than ground burial in a coffin, was confined to an tiny fraction considered eccentric (population 150,000).¹⁷ Ground burial was a very elaborate and expensive business. The precise position of the grave, style of tombstone, and elaboration of pre-burial celebrations were not only important in confirming socio-economic status as well as group sorrow. More than that, through death a very fortunate few were able to express a deeper urban urge to improve status, by improving their "class" position in death, in the cemetery.

In 1960, we find a large increase in embalment, including "permanentisation" (one undertaker's term) of the corpse *before* its destruction (*immediately* afterwards) by fire in cremation! Cremation began to receive acceptance as an ordinary Bolton idea in the late forties. In 1954 the first crematorium was opened there; result:

¹⁶ Prof. Eliade, perhaps the greatest living purist in the world-comparison, globe-linkage technique, excels even himself in another book "Le Chamanisme" (Paris 1951), where he treats as one island Nias at the western edge of Indonesia and Niue far out in eastern Polynesia (with remarkable effect!). Noticing this error — and no wonder; the islands are over 10,000 km apart, farther than Sarawak is from the Holy Land — in his culture hero, Dr. Wales loyally brushes it off as "an unfortunate slip" (*op. cit.*, 1957, p. 61).

¹⁷ More than all the Ngadju.

	<i>Number of persons cremated</i> (Bolton)
1954	273
1955	1,433
1956	1,835
1957	1,932
1959	2,161
1960	2,272

By 1960 cremations *outnumbered* earth burials in Bolton by 2:1 (for details see my "Britain Revisited", London 1961; 43—84).

Cremation is defined in the Oxford dictionary: "*burning as method of disposing of corpse*" — which, of course, is not all the purpose over much of Borneo (usually the contrary). In Bolton the intricate traditional nexus of coffin, tombstone, etc. is crumbling away, despite the opposition of religious groups — who are still extremely strong in industrial Lancashire. There are 38 of these groups, distinct sects with separate identities, in Bolton. None of them are Hindu or Buddhist, Indian or Chinese. So far as I know, no cremated Boltonian has ever met a Buddhist except on television.¹⁸

The more we find out about character and rapidity of culture change, the less happy are we likely (unless blinded by our own prejudice) to be about ambitious reconstructions of the supposed past, where these are based on the use of terms, on a few isolated examples of culture traits re-interpreted by the writer, on individual material objects out of context or on etymological word play. What strikes one, so often, about the "outside influence" part of it is the way major, recognised outside influences have *failed*, except in a fairly superficial way, when brought to bear on equatorial groups with adequate living standards, topographical security and a highly developed own ideology with ritual. But at a lower level, it is astonishing also how the most "obvious" influences fail to register even where one would think they were bound to have operated "in theory".

There is a remarkable example of this failure in Dr. Wales' paper.

¹⁸ In July 1961 a third cremator unit was added to the previous pair, in order to cope with a further increase in this usage; 1,449 cremations in the first six months of this year (1961).

He has written at length on the Ngadju *without* giving a sign that some of the fact and much of the thinking in his analysis was predated, by half a century, by Dr. Robert Hertz. It is hardly believable that Dr. Wales has suppressed or ignored Dr. Hertz's pioneer analysis, despite his stated hatred for Durkheim and his "school". Dr. Wales unfailingly spares no pains to deride his harsh version of the Durkheim approach. For own moral support, he brings in items such as:

"Durkheim's theory has been rejected by two of the most distinguished anthropologists of this century, Malinowski and Kroeber"

or again, one line later:

"Durkheim's theory has been found unacceptable by virtually all students of comparative religions" (p. 118).

These remarks arise mainly from his need to discredit those parts of Dr. Schärer's Ngadju studies which do not suit Dr. Wales' latest thesis. Unfortunately for Dr. Wales (and all of us) Durkheim's other Borneo-orientated pupil had a punch up his sleeve, too; and Oxford has now pulled it — the sleeve, I mean: out drops a lustrated fist. We will shake this dead hand shortly....

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The existence of a compilation as comprehensive as Stöhr's now makes it that much more difficult for superficial theory and purely subjective scholarship to "get away" with shallow parallels and over-selective illustrations. But, alas, men as devoted, patient and intellectually alert as Stöhr are very few and far between in the social sciences.

Impatience, an urge to produce something impressive in generalisation, perhaps least often tends to upset solid work among the Germans (or Indians), most often among the French (and Chinese). It would be difficult to demonstrate this more clearly than by the surprise resurrection of Robert Hertz's 1906 paper, in Mrs. and Dr. R. Needham's English translation, now re-introduced in glowing terms by the latter's teacher and mentor, my friend Dr. E. E. Evans-Pritchard, Professor of Social Anthropology in the University of Oxford, Fellow of All Souls, and one of the last survivors of the early field anthropologists. In 1906, (when "E.-P." was aged 4) Hertz's book was, in a juvenile sort of way, pioneer. But what were youthful weaknesses then have become diseases now. Hertz himself, later, *specifically* expressed his misgivings on this score. Read in 1961, alongside Stöhr,

Hertz's study "Death and the Right Hand", (London 1960) is a saddening epitaph for a young Frenchman, almost as bold in body as in mind — (he died gallantly in action in the first World War). In a very full introduction (pp. 9—24), Evans-Pritchard comes close to producing an epitaph on social anthropology as well. He concludes (my italics):

"It is a fact, which none can deny, that the theoretical capital on which anthropologists today live is mainly the writings of people whose research *was entirely literary*, who brought to bear great ability, much learning, and rigorous methods of scholarship on what others had observed and recorded. When that capital is exhausted we are in danger of falling into *mere empiricism*, one field study after another adding to the number of known facts, but uninspired and uninspiring". (p. 24).

Social anthropologists must indeed be in a sad state (in England and France anyway) if we need now rely so heavily on "theoretical capital" banked more than half a century ago — before, for instance, there were applicable techniques of intelligence testing, dream analysis, tape recording, cardiograph and encephalograph data, colour film, blood-grouping, chemical methods of measuring associations and dates, statistical sampling techniques, the deep interview, full understanding of the part diet and health play in society (with accurate methods for measuring these) and other new technical advances, quite apart from *new* conceptions and theories, — presumably the justification for whole new and still growing university departments in anthropology. If we were to take the Professor's view seriously, the situation would be still more depressing than he suggests; half a century of academic grants and government money would have been frittered away in vain and Dr. Needham's several sponsored visits to Indonesia proven unnecessary?

Before we panic, however, let us examine the method by which theoretical capitalist Hertz acquires that halo of immortal wealth. He bases his argument on a supposed "*general rule*, among the Dayak" — by which he means the Ngadju again. From his idea of these Ngadju — or rather what was then known about them (as re-interpreted by himself) all else follows. He supposes general rules for all Bornean Dayaks, based on the early records of this group.

From such a confused and incorrect — but not necessarily in itself disastrous — start Hertz begins to select and emphasise reports that fit his own idea of Bornean (and wider) funerary practice. He stresses the importance of disposing the bodies of non-chiefly and non-wealthy persons "in a deserted place in the depths of the forest", since this

is material to his following argument (p. 30). But if there is such a thing — in this kind of treatment — as a fact, then this fact is that the attitude he here generalises is quite rare in Borneo; and that there is no evidence to suggest that it was once more common than now. In recent times, certainly, with very few exceptions such treatment has been confined to strangers, strays, or those who have greatly offended the group code; or to those who have met death under special or bitter circumstances (e.g. beheaded). All of these would be normally — and by their causation — “primary burials”, without further elaborate observance (i.e. not the sort Hertz thinks broadly common to All Borneo).

For Hertz’s argument it is necessary (and sensible) to assume that all group practices were divided into “provisional” or “temporary” (he varies his terminology), and then “final” burials. After that he can derive what he postulates as “the more ancient form”, namely *exposure* of the corpse in the branches of a tree, “which we find mentioned in *several places*” (p. 30). This becomes a major theme in the following argument. As we are still only at his fourth page, it is perhaps natural to expect that these several places are in Borneo; turning to page 117 for relevant note 12, however, we find that these places are merely two, both outside Borneo with two authors as sources (for Timor and Minahassa, Celebes).¹⁹

¹⁹ Hertz’s note 7 gives another insight into this particular literary technique. Müller is cited as reporting some Ngadju placing several coffins together in a collective sepulture, the *sandong raung*. But Hertz does not accept this report because “the rule is that the coffin is isolated during the periods of waiting” (p. 117) — that is, the rule of his own invention. There are several confusions here, but the existence of a collective *sandong raung* is beyond doubt (cf. Stöhr, p. 41), as one of a *wide variety* of Ngadju funerary structures. Here again, it is clear that Hertz did not understand, and perhaps not actually consult, all the references he quotes. On this point he cites Salomon Müller, 1839-44 (p. 117, 168, in Hertz’s text) in *Verhandelingen van de Natuurgeschiedenis... Afdeeling Land- en Volkenkunde*: 321-446, whom he misunderstands. He omits the relevant S. Müller, 1857, “Reizen en Onderzoekingen in den Indischen Archipel”, Amsterdam, from which source Stöhr’s fig. 4 of another and closely related Ngadju coffin structure is taken (p. 43). Stöhr also reproduces (fig. 3, p. 42) the *sadong raung*, but from Braches, who wrote a paper specifically on the subject in *Berichte der Rheinischen Missions-Gesellschaft*, 1882: 101-106. Although the Braches reference is in Hertz’s bibliography (p. 164), he fails to refer to it at this so relevant point in his argument. As with his handling of Perham (see following footnote), one is left with the uneasy feeling Hertz did *not* really grasp the literature he used. There are also rather curious indications that, although he is said to have done extensive reading in the B.M. from 1904-6 (p. 11 of Evans-Pritchard’s introduction), he may have initially obtained most of his (Borneo) sources at

Hertz ends this phase of his argument by insisting that whatever local variety of custom, the body "while awaiting the second burial . . . is almost invariably isolated" (p. 30, my italics). I wish his brave ghost could have been with me in the second World War, when I had to stay in long-houses, notably in the Bahau, where temporary (primary) burials infested long-houses just decimated by epidemic due to the break-down of medical services during Japanese occupation. This identification of the dead body (not just spirit) *with* the living group in the primary or preliminary stage is vital widely in Borneo, almost wherever elaborate funerary procedures are observed, as expressed for the Maanjan-Siung of the Barito basin:

"The soul of the dead must *not* be left alone in the jungle lest he wanders and cause damage to the relations (of the deceased)". (cf. Stöhr, p. 67).

After several more pages, mainly on the Ngadju, but now bringing in the Sea Dayak also (he is understandably confused by the different usages of "Dayak" in Dutch and English literature), Hertz begins to see "strict similarity between the exposure of the corpse in the branches of a tree" (but now: "as is practised by tribes of Central Australia"), house exposure ("as is found among certain Papuans and among some Bantu tribes" — he being evidently unaware of Borneo occurrences); with corpses on a platform ("as is usually done by the Polynesians and by many Indian tribes"); and "lastly the temporary burial

second hand, notably from Ling Roth's great two-volume "digest" published in 1896; this was compiled from quite another angle, but contains quoted or paraphrased English texts (including 46 pages of Perham) and many useful translations from German and Dutch (including some of S. Müller). Hertz's bibliography with well over 200 titles (pp. 163-171) has only 3 of the many Borneo references dated after 1896 — in which year Hertz was aged 14. Of these Creagh (1897) and Kukenthal ("1896-1902") are, however, both included at the very end of Roth's many appendices ("Received since going to press", II, pp. ccix-ccxi). In the third and only other, Hertz confounds confusion by dating Nieuwenhuis' great two volume "Quer durch Borneo" as both 1907, the year of his own paper, whereas Vol. I came out in 1904 (Brill, Leiden). This Hertzian gap for 1897-1906 is emphasised if we compare; Dr. Stöhr lists over 30 Bornean items for the same period — an active one in the literature — including some important to Hertz's theme and certainly available in the B. M. (e.g. A. C. Haddon, W. H. Furness). It is either ironical coincidence or unconscious distortion that let Hertz also abbreviate Ling Roth's actual title and thus suggest his work to be more limited in scope than it is in fact; Hertz lists it (p. 169) as "The natives of Sarawak". I can find no similar abbreviation of other titles.

I naturally take it that these and other peculiarities are not due to any careless faults of the two experienced editors.

chiefly practised by South American Indians" (p. 41; all this in one sentence).

Hertz is now gathering strength (p. 42) and begins an enormous series of leaps into and out of Borneo. Ignorant of humid tropical conditions, he hurdles mummification as a side product of temporary burial ("due to the desiccating qualities of the soil or air"). By literary sleight of hand we are then brought to "the transition" — as he puts it — from mummification "to a rudimentary embalment" which appears to be "almost imperceptible". Achieving this, and a good deal more, Borneo-based, in one paragraph, involves no mean armchair exercise. It requires, for 28 lines of text, 7 supporting footnotes giving 28 references. These include the "Book of the Dead", Herodotus, Genesis, and 7 other sources before 1850; altogether covering among other places, North-West and East Africa (6 peoples); North America (2 lots of Red Indians); Tahiti and the Solomons; Papua (3 groups); North and South Australian aborigines; and — apart from a Timorese mention — only one Asian people, the remoter Ainu of the northern mainland.

By now, the reader of this review may be as bewildered as any critical reader of Hertz's original. It would be painful to continue.... As a prize pupil of Emile Durkheim, Hertz died too soon. Had he lived he could hardly have wished this mental frolic to be both perpetuated and proclaimed out of relation to its original importance. Reading Professor Evans-Pritchard's lead-in applauding the Hertz translation is a bit like sharing in some "Dayak" (Ngadju?) dream or megaloid (Kelabit) fantasy. The Professor again states:

"Durkheim and his pupils used the comparative method with as much skill and rigour as it is capable of. They were careful to concentrate on a *limited* range of facts in a *limited region* and not, like, for instance, McLennan, Frazer and Westermarck, to take them from all over the world, lifting them in the process out of their social setting". (p. 14).

With all respect to Sir James in fullest spate, did he in any one paragraph eclipse the vertigo of context-lifting achieved either by Durkheimian R. Hertz or (for they use just the same technique) anti-Durkheimian Q. Wales and M. Eliade?

We may well excuse a venerated Oxford Don for renouncing the future in return for the now rosily recalled "glorious past", — those gay days when Radcliffe was plain Brown, a rising comet, and the rest younger rivals. But this is a sad reflection upon the position into which our masters have unwittingly led social anthropology. We can all agree with the uses of literary research, as well as field-work. Both

are interrelated and indispensable. Moreover, people like Hertz can theoretically make major contributions, especially as young men, if they live to develop their ideas with further understanding. Hertz scored bulls' eyes with his trigger-happy young finger. But this is rather like saying that a buddy is a first-class shot because, by spraying the bar-room with submachine-gun fire, he breaks some bottles. Whereas Stöhr has some partly unsound terminal chapters after a body of major, solid contribution to further understanding, there is pathetically little solid and lasting even if we take Hertz in a more spiritual mood or try to probe the soul of the matter as he saw it. It is difficult to feel any capital debt to who can include in his "Conclusion" statements as wild as these:

"But the *most common practice among the Dayak* and the Papuans seems to be to enclose the bodies of small children inside a tree or to hang them in branches. The deaths of children thus provoke only a very weak social reaction which is almost *instantaneously completed*. It is as though, for the collective consciousness, there were no real death in this case." (Hertz, p. 84).

This supposedly Dayak-wide generalisation is characteristically supported by only two references for all Borneo — and the one to Archdeacon Perham *not* citing the original text.²⁰ During the past fifteen years of extensive travel in all four Bornean territories, I have neither seen or heard of such practice (children in trees), or anything like it (either in living memory or folklore). To suggest that the custom is or ever has been *general* is to stretch this method beyond the tolerable extreme, whereby one of a hundred varieties of rite could, for subjective reasons, be postulated as proto-typical. Stöhr has a few references, and I can add one other which he has overlooked. Archdeacon Howell briefly noted that among the Balaus — a small group

²⁰ Hertz's omission is barely excusable, since Perham's paper was published in the widely available *J. Straits Br., Roy. As. Soc.*, one in his important series, 1881-7. This can be the only alibi for Hertz's missing a point most disturbing to his underlying argument: namely that the treatment is given not only to children but also to the extremely important class of *manang* or *lemambang*, the „Shamans" who link the living and the dead among the Balaus, to whom alone Perham here refers. Moreover, he omits Perham's other main point: that the infant is hung up *in a jar*, from the tree. I have elsewhere reported the secondary burial of *adult* Kelabits in treed jars (see K 59 above; also reference to Howell in text to follow now). There are, of course, other tree usages, including for Bintulu Melanaus (sometimes secondary burial of aged), Bisayas of the Padas (fresh branches over grave of a chief, otherwise rare), nomadic Punans in the Baram-Brunei watershed (several adults inside one big jungle tree trunk). Attitudes to and beliefs about trees are particularly complex and variable through Borneo and thus provide a fruitful subject for continuing study.

interstitial between Sea and Land-Dayaks in south-west Sarawak, also identified by Perham for what both record as their peculiar habit in this respect — infant tree-burial (in jars) related to the status of infant spirits. These “inhabit the intermediate region” between this world and the next, and have the important function, therefore, of being able to see and to know what is going on in both. They thus serve as intermediaries and messengers, a vital role. Even so, they are not spiritually static, for as Howell puts it: “These infants as time goes on *grow up to be men and women*” (*S.M.J.* 1 (pre-war), 1911:20; confirmed 1961 by Assistant Curator George Jamuh, himself a Balau).

The Saribas Sea Dayaks, a much larger group, bury tiny infants in jars in the ground. They believe — and sadly regret — that the baby cries in the afterworld, disturbing other spirits; to quote from one of the *Gawai Antu* chants already mentioned:

“This is the country of Prin-Rigai, who wears a hat adorned with the longest tail feather of a champion fighting cock The country of Semelendek, who stands upon a single log The land of the aborted foetus, poorest of all creatures — for when it dies it has not been given a piece of iron, and so has nothing to use.” (B. Sandin transl.).

The Saribas and other Sea-Dayaks consider, much as do Kelabit, Dusun and others, that each sort of principal spirit (of the dead) has a special place, conditioned variously by age, sex, status, manner of death, etc. This position cannot ordinarily be altered by the acts of those still living, but can be insured and perhaps eased (in both directions); and may be affected by the “life” of the dead in question. In the case of an infant, the prospect of its inadequate afterlife can be immediately distressing, specially to those who lose a first-born; but rites like *ulit mekang* offer some relief. A touching instance of the modern dilemma happened a few weeks ago, in Kuching. An ordained Iban priest of a Christian sect came to us for advice on how we could properly mourn for his youngest daughter, who had just died age 3 months, in a Government hospital. The difficulty was that he wished to observe a prohibition in public and otherwise show grief, *both* as Iban and Christian; being a man of principle, he recognised that (as an Iban) death at this age did not automatically involve such prohibitions; the parent must first demonstrate a desire for full responsibility by placing an ivory bangle in the coffin, thus providing a (non-Christian) substitute for the missing dentition and (to simplify the matter here), upgrade the child’s age in death. We hadn’t an ivory bangle to spare, but provided an equivalent Ming dish. Our good friend then felt free to wear a black armband with his priestly habit.

There is an incident to the same point in the giant epic of Balan Nyaring, one of the few Kenya culture heroes who succeeded, after death, in surpassing all the tests of the afterworld and returning to life as a great re-orientated leader of his people. This is recited in the marathon cycle, *nenerok*:

"After some time travelling upriver Balan Nyaring came to a place where there were a lot of rocks. It was full of babies, the "*anak bala*". These were babies who died in infancy. Balan was sorry to see them because they had nothing to eat except the small rock-fish which they dried in the sun as they had no fire. They were without any clothes. To keep themselves warm they stayed in the sun. They were in such a poverty because when they died their parents did not give them anything. When they saw Balan, they all rushed to him and hugged him lovingly, calling him all sorts of endearing names. They entreated him to make a fire for them, so that they could cook the fish they got during the day. Balan got a big log of wood and set it on fire and promised them that it would keep alight for the next three years." (1961, preliminary transl.).

It could be argued, from these and other sources (taken from very different groups within Borneo) that the lack of what Hertz calls "real death" for infants actually works the other way round. It could even be argued that the tragedy of afterlife status for such infants is woundingly one of the more serious scars on what he calls "collective consciousness". For the simple fact is this: Hertz can make nothing better than remote guesses about some Dayaks; and his inspirations are not good enough. The facts of Borneo life and death are so rich, varied, intricate and in several respects refined, that to suppose any man can do what Hertz claimed to do — let alone do better — on the then available data, insults eastern intelligence. Few western minds have the equipment and training to understand those respects in which people like the Ngadju and Kenya are deeper, more subtle, sensuous and imaginative than many a Fellow of All Souls. After fifteen years of trying to understand, one's mind can still boggle listening to an old man placing the different souls he feels under his control, of which one is at present away in a deer, another upon a mountain-top, a third due back in a dream and the seventh right here in particular touch with me! It is through pure ignorance that a scholar can use the words "at most" as Hertz does in this concluding passage:

"The death of a new-born child is, at most, an *infra-social event*; since society has not yet given anything of itself to the child it is not affected by its disappearance and remains *indifferent*" (p. 84; my *ital*).

To anyone who has peered into one of the splendid stone-age "three-colour ware" urns of Niah (filled with the scarlet repainted bones of babies and even baby milk-teeth still unerupted) or been in a Kelabit

long-house with a new born infant dying, this kind of conclusion "seems to be" based on some contemptuous underrating of what so-called primitive people are considered able to feel or devise for themselves. Whatever such stuff may be as literature, in any authentic anthropology we can rate it no higher (55 years later) than rubbish, which should have been decently dumped in science's huge, albeit kindly, oblivion of work to be best forgot.

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All wise men support more than one approach in any study. But whatever the approach, facts about far places must not become pawns in a word game, pieces for the display of personal virtuosity, however prosed. One of the deeper defects of anthropology as a human science is that so much of the *wider* approach has been over-written and too literary, so much of the field work too field and too brief. Moreover, latterly, too much of the fieldwork has been done by younger men without wider scholarship, yet highly qualified (indeed, within certain limits, over-qualified) on paper — the sort of background unknown in the days of the pioneer few, who were not "trained anthropologists" in the modern sense. The Association of Social Anthropologists of the British Commonwealth — this is its strange registered title — is, for instance, a closed elite of 151 members to be entered "by invitation only"; this makes it about the most exclusive and potentially restrictive professional body in the world of science (facts & quotation from *Man*, LXI, 1961: 170).

To augment both the literary and field methods, and to provide a new bank account for anthropological capital theory, steps must now be taken to move on, and to cross the discrepancy between field-fact interpretations of plain field source and higher theory. It does not seem to be yet sufficiently appreciated that changes in communications, very relevant to this methodological problem, are making it practicable to move from central Borneo, stone-age Papua, or the Hindukush, to industrial Lancashire or Soviet Russia in a matter of hours and to make parallel studies, keep up contacts in west and east simultaneously. The long established (and basically pedestrian) anthropological approach to individual communities at one end or individual themes at the other, is already out-dated. These changes, with new approaches adjusted to them, could mean wider attitudes in the one social science which still *separates* field and home experience; and which too often

regards the field spell of a year or two as a postgraduate training preliminary to permanent research position of teaching security in the subject. I have had the doubtful advantage of sharing for 17 years the island which the Ngadju occupied long before Buddha or Confucius first dreamed. During that time, I have seen at first hand the work of 22 field anthropologists and related sociologists visiting this part of the world in the course of their duties. Eight British, three other European, seven American, and four mainland Asian. Most of them have been decent intelligent men. Their methods and disciplines have sometimes seemed painfully second-rate, however. The weakness is often not in their "empiricism" but in technique and persistence; a lack of patience, little depth of penetration. Let us hope that Dr. Stöhr for one may steer a new way. His own final feeling is expressed very fairly in his last paragraph:

"It is also necessary to consider the death ritual in connection with other aspects of culture. It became evident in the course of this investigation that there is often a close link between the village offering place and death rituals, and certain myths to death rituals, so that it was sometimes difficult to refer to the death rituals as such only. This close integration may be able to support important historical conclusions" (p. 197).

Yes: and fuller study must *also* be made of how all this works in living culture terms, intimately. Only then can the full conclusions earn more than Dr. Stöhr's favourite adjective, "historical". This historicism is well reflected in the return to Hertz — which has also been welcomed by non-Oxford anthropologists, including Dr. Edmund Leach, Dr. J. D. Freeman and Chicago's Dr. Fred Eggan (for whose work I have wide admiration); and again, in the wide-spread and quite needless "retreat westward to India" as explanation of phenomena much better understood on the spot.

★ ★ ★

As Professor Evans-Pritchard's introduction to Dr. Needham's Hertz well shows, this approach holds an undertone of defeatism, plus the faint sent of an approaching rout for the pseudo-scientific method itself. If this continues, we may expect still wider theorising of the introspective philosophical type along with a growing devaluation for the "mere fieldworker". Since this paper was completed, exactly that attitude has been fluently expressed in a review of Stöhr by Dr. Rodney Needham (*American Anthropologist*, 63, 1961: 601—602). Perhaps picqued by Stöhr's justifiable rejection of his own attempt to distinguish between "Punan" and "Penan", Dr. Needham rends Dr. Stöhr in lengthy passages like this:

"The classification itself, however, is not satisfactory and the enterprise itself is doubtful in other respects. For example, part of the author's intention is to discover the "original" death-ritual of "the Dayak"; but whatever ethnic classification one adopts, there is no one homogeneous Dayak people, form of society, or culture, and there is thus unlikely to be any original form of burial to be discovered."

Quite apart from the weak premise of the terminal argument, this is not in fact a classification adopted by Stöhr, nor his main intention — as I have already shown. Unless Dr. Needham cannot read German, the suggestion that Stöhr has thus oversimplified is unfair. But even had Stöhr done so, that which Needham condemns in him is exactly what he (a few months before) has actively condoned in both translating and editing Hertz. Almost the opening words of Hertz's argument are based on a conclusion which he never sorted out for himself. This is his opening text (translated by Needham; my italics): "The *general rule, among the Dayak*, seems to have been to keep the bodies of chiefs and of wealthy people inside their own houses..." (p. 29).

Of course, Rodney Needham knows much too much about Borneo not to qualify Hertz's generalisation (Hertz mistakenly thought virtually *all* Dayaks practised elaborate secondary burials). So, wisely foreseeing trouble ahead, he adds his *only* editorial footnote — explaining that by "Dayak" his author means "mainly the Olo Ngaju" (p. 29). Actually this does not cover all Hertz's later uses of the same term, which he made characteristically elastic. But in any case, right or wrong, this is not only an example of "one law for the rich and another for the poor"; it is denial of common justice to poor Stöhr, who has taken more trouble than any other author before him (including Needham) intelligently to relate his material to the actual people. He never uses "Dayak" in the chaotic way that Hertz did.²¹

★ ★ ★

It is time to bring a growing dichotomy out into the open. For here is the tension-line that can crack and wreck a young science. The difference between Hertz at his best and Stöhr at his worst is the difference between a confident, clever writer and *socio-philosopher* (who of course illustrates his ideas with facts that suit) and an

²¹ Another example of Dr. Needham's duo-legal mind comes in his review of "Bali" (The Hague, 1960), where he complains: "It is always inconvenient to have footnotes relegated to a separate section at the back, and in this case there is much in them which a reader exasperated by flipping pages back and forth may well miss." (*Man* 1961: 193). How, then, is it that in Dr. Needham's translation of Hertz he compels us to precisely this inconvenience?

industrious, very intelligent, ethnographer and *social scientist* (but who of course has a life of mind as well as eye). Like it or not, for better or for worse, the subject matter of the social sciences is man, the method for studying him initially and continuously *scientific*, though widely variable.

Social anthropology is in surprisingly low water today over a wide area, largely because many of its later *theoretical* practitioners have been either afraid or incompetent (often both) to widen the scope and advance, or revise, the techniques of systematic, scientific study and of orderly, factually-based theory derived from adequate understanding of *whole* problems seen in perspective. They have long been running a "closed shop"; of course it is mighty cosy brother, once you're safe inside. The continued health of this science depends, however, in the first place, on intensive fieldwork broader in scope, livelier in character, bolder in conception. This fieldwork must be more thoughtfully planned than much that has yet been attempted. There is need for new thinking and execution of far-view, deep-feel, long-term field studies, to augment (and no doubt presently outdate) the narrow, formal, repetitive and at times quite shallow operations which, in recent years, have so regularly come to pass as significant and even definitive (which is largely why E.-P. and other elders are so bored with them?). In fact, the surface of human research has barely been scratched, in Borneo (or Bolton). It is needless defeatism or mere egotism to suppose that the "great days are over". They have not yet begun! And since social anthropology and its related disciplines are, by definition, *fundamental* to the modern human process, failure to think forward weakens not only the future of anthropology, but to some extent humanity itself. Durkheim, Hertz, Schäfer, Malinowski, Haddon, Rivers, Radcliffe-Brown are dead. Those who live and are really alive to the present have, now, to face new levels of responsibility — and new opportunity. Much of what in the past has been thought good enough is no longer so.

Perhaps I may summarise what appears to arise out of initially considering Dr. Waldemar Stöhr's valuable, if limited, death study in this welcome revival of *Ethnologica*; and out of comparing his methods with those of others operating in approximately the same field of human fact. Let us postulate, here, six approximate levels of required research within the proper scope — though not necessarily the present competence — of worthwhile anthropology. Although I apply these to Borneo, they are equally valid over a vastly wider field (including other areas even *less* studied than this):

<i>Level</i>	<i>Character of study</i>	<i>Published results of high standard in past quarter century</i>
(1)	Shortish-term "outsider" field study of special <i>group</i>	J. D. Freeman (Balleh Iban) and W. R. Geddes (Tapuh Land Dayak); both New Zealanders now in their forties.
(2)	Long-term "inside" field study of special <i>group</i>	I. H. N. Evans (Tempassuk Dusun folklore) & H. Schärer (Ngadju religion); the first an English civil servant, the second a Swiss missionary; both dead. ²²
(3)	Depth study of special <i>theme</i> over more than one group	Nil
(4)	Wide-scale field study of several dissimilar groups, comparisons and syntheses by direct study	Nil
(5)	Comparison and synthesis of factual results from several field-studies	W. Stöhr; A. A. Cense and E. M. Uhlenbeck (linguistics; as discussed in footnote 7 above).
(6)	Synthesis and fundamental ideas derived from (and <i>firmly based on</i>) 1—5, with or without other data from elsewhere	Nil

It is at level (6) that those talented and well-meaning students like Hertz have so far failed hopelessly. It is here, also, that lesser men like Quaritch-Wales contribute confusion rather than clarification.

²² Of these only Schärer seriously considers funerary and after-world matters; I am well aware of important serious defects in his work, but it remains the outstanding piece of „anthropology in depth" for Borneo so far.

Having failed to comprehend the range and depth of the problems involved, they then tend not only to over-select (or to become unaware of other data not in agreement) but also — by way of compensation for their inadequacies — to exaggerate personal ideas and elaborate pleasant passages of text remote from anything justified at the level of *scientific* observation or deduction. The depressing thing is that work of both these qualities can command full-length publication where more original fieldwork results in detail often cannot; and that social anthropologists can, almost without dissent, *acclaim the consequent ego-publications as important and even admirable*. Such is, alas, the internal self-satisfaction of most contemporary social anthropology, especially in Western Europe. Or (to be more acute) there is an underlying feeling of gathering internal dissatisfaction and even something approaching alarm at the poor state of the science; this tends for the present to bring practitioners closer together, lest anyone start rocking the boat and wreck the lot. No wrecker, I! But isn't it about time one or two of the crew took a nice fresh swim, even if only by way of practise for the long and hazardous, hornbill-bordered, crocodile-guarded journey along that cold wide river which leads through the afterworld beyond Barito, Saribas, Baram and Kapuas (and maybe Leiden too, for the journey of the spirit is very, very long)?

TOM HARRISSON.