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‘Wild Borneo’: Anthropologists at War in the Rainforest

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'Wild Borneo': Anthropologists at War in the Rainforest

*Victor T. King**

Abstract:

Dispute, disagreement and debate are the very stuff of academic activity. The problem arises when the language of the debate takes on a personal dimension and the authority that is claimed in arguing in favour of a particular position, approach or perspective becomes so entrenched that other voices are assigned to the margins. This paper reviews the origins and development of the exchange of views between competing voices in the interpretation of the importance and 'meaning' of the ritual textiles of the Iban of Borneo and whether or not they embody and express a language of symbols. It also comments on the attempt to explain the social importance of Iban weavers in terms of an evolutionary conceptual framework, based on the principle of sexual selection, which claims that historically the Iban focused their attention on the formation of relations between skilled weavers and renowned Iban head-hunters. This was to gain, so it is argued, a genetic-biological advantage, in Darwinian terms, in the struggle for 'survival', but more particularly for social status and prowess in a competitive and relatively egalitarian Borneo society. The paper then addresses a recent turn in the debate which raises issues about the nature of certain academic engagements, the different styles adopted in these engagements, the language used to establish academic authority, and the constant struggle in anthropology, and, in this case, with reference to Borneo, between those who claim to command the field of studies and those who have alternative views.

Keywords: Iban, Borneo, textiles, authority, 'scientific ethnography', evolution, synthesis, paradigms

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Victor T. King

Prolegomena

Marilyn Strathern (1983:6)

Falsification is grand if you know that it defines scientific enterprise; the popular version is likely to be simple that anthropology gets it wrong.

Alexander Pope (1711), Part II, lines 235, 255.

*A perfect Judge will read each work of Wit
With the same spirit that its author writ:
Survey the Whole, nor seek slight faults to find
Where nature moves, and rapture warms the mind.*

*In every work regard the writer's End,
Since none can compass more than they intend;
And if the means be just, the conduct true,
Applause, in spite of trivial faults, is due.*

Introduction

The focus in this paper is to examine a style of argument in academic debate arising from the desire to confirm intellectual authority in a specific field of studies and whose main purpose, among others, is to question the credibility of those seen as dissident voices. This has been prompted by responses to a paper published on Professor Derek Freeman and his former doctoral student, Dr Michael Heppell, in their critical engagement with those with whom they disagree (King, 2017a). The paper attempted to capture a particular approach to academic

discourse designed to exclude those whose dissident voices are considered less than authoritative. The disputes surround the interpretation of the ‘meaning’ of Iban ritual textiles (*pua kumbu*), including their patterns and motifs, and their role in traditional Iban religion and social organization; the arguments relate directly to the exchanges between Traude Gavin and Michael Heppell (see, for example, Gavin, 1996, 2003[2004], 2008, 2012, 2015a, 2015b, 2016a, 2016b, 2016c; Heppell, 2006a, 2006b, 2010, 2014, 2016; Heppell et al, 2005). In this connection a major issue, and one which is embedded in Western thought about what was referred to as ‘primitive art’ in those cultures without written histories is a preoccupation in explaining the ‘symbolic meanings’ which these material productions embody and express.

Professors Vinson Sutlive and George Appell, senior figures in the Borneo Research Council (BRC), in a recent extended letter in the *Borneo Research Bulletin (BRB)*, now wish to deploy the term ‘untrue’ to some of the arguments in this respect, as well as an assemblage of other characterizations of the author’s work (2018: 18-27; and see King, 2017a). Their paper is an example of a ‘rhetorical performance’ (ibid.: 19; King, 2017a: 84). In the response to their arguments, it is suggested that they are, in certain important respects, ‘economical with the truth’ (Wikipedia, 2019). This was a phrase that dates back to Edmund Burke in the 1790s, but brought into popular discourse in the recorded interview with Sir Robert Armstrong, the then United Kingdom Cabinet Secretary in the investigation of the ‘Spycatcher’ trial in Australia in 1986. In this moderate riposte, it is proposed that Sutlive and Appell are exercising an undue degree of ‘economizing’, though some of their observations and criticisms are accepted in a gesture of collegiality.

It appears that the debate has taken on something of a surreal quality, in that those who claim authority admit to no errors. In their paper Sutlive and Appell criticize, but give no ground, even when, in certain areas of the debate, they have no substantial evidence to support their position. As far as can be determined they argue that there are those who uphold the canon and those who do not; they highlight mainly trivial errors in these exchanges, but they do not address the major issues of contention satisfactorily, and misinterpret dissident voices in regard to Derek Freeman’s work in an effort to claim authority. Their objective appears to be to re-establish the canon (hence the quotation of verses from Alexander Pope [1711] in the Prolegomena). If one departs from the canon in Borneo Studies (a standard of judgement, principle or criterion which is based upon a determined set of authorized texts), then one risks a degree of exclusion from these scholarly circles.

In the support of Derek Freeman and his encounters with others (the four cases that are considered in the 2017 *BKI* paper [2017a]), they state that Freeman was ‘right’, or as they put it ‘on point, and [that he] served to challenge and correct misinformation in each [case]’ (2018: 21). This is a somewhat misleading way of framing the issues. In regard to social science researchers who approach the same set of ‘facts’ (of which Sutlive and Appell argue Derek Freeman was in urgent pursuit), we can arrive at different interpretations of those ‘facts’. It is suggested that Sutlive and Appell adopt a ‘positivist’ perspective on social and cultural life; indeed, they use the term ‘scientific ethnography’ as the desired practice for anthropology (2018: 19). But in their quest to determine what is right, it is argued that their observations require considerable qualification. Furthermore, it is important to recognize that social scientists can get it wrong and that, if they recognize that they are wrong, they should be prepared to confess (Rigg, 2020; see also Strathern, 1983, in the Prolegomena).

Sutlive and Appell establish their authority in these matters in the following terms. George Appell announces that he was a student of Derek Freeman and is President of the Borneo Research Council (2018: 19). He then lists 19 items in the Bibliography under his name, only one of which appears in the text. He justifies this by a footnote that this is ‘relevant work’ (ibid.:19). We cannot evaluate this claim unless he demonstrates it through reasoned argument and evidence. Some of his publications are relevant, but in the specific debate on Iban ritual textiles and Heppell’s and Gavin’s interpretation of them, most of them are not. Sutlive’s credentials are obviously substantial in Iban terms in that he has spent his career studying the Iban; he also knew and worked with Freeman. He then states that he was the former Executive Director of the Borneo Research Council. Nevertheless, the relevance of Sutlive’s publications listed in the Bibliography, none of which are referenced in the text, requires justification. Even more problematical is that they list a whole raft of other publications in their Bibliography that are not referred to in their text, other than sometimes mentioning an author in passing. They also make reference to Edward T. Hall in the text (undated), who does not appear in the Bibliography, and Marshall Sahlins and Elman Service who do appear in the text, again undated, without indicating that this is an edited book, but whose volume is then referenced in the Bibliography, without again recording it as an edited book.

Professor Derek Freeman was one of the outstandingly controversial figures in the recent history of anthropology. His biographer, Peter Hemenstall captures the style of Freeman’s approach to academic debate and dispute, in the title of his book: *Truth’s Fool*:

Derek Freeman and the War over Cultural Anthropology (Hempenstall, 2018; and see King, 2019: 183-197). This current working paper has adopted the imagery of Freeman's engagements with his adversaries and developed his analogy of Iban-Kayan warfare in the Borneo rainforest and the defences which the Iban constructed to counter these incursions. This imaginative expression of conflict was deployed by Freeman in his response to an article written by Professor Jérôme Rousseau on 'Iban Inequality' in which, as a Kayan and not an Iban specialist, he was seen to intrude, as an unwelcome aggressor, into the Iban domain (Rousseau, 1980; and see King 2017a: 95).

Iban ritual textiles

The issues at stake in this debate have already been argued in some detail (King, 2017a, 2018a; Heppell, 2018a, 2018b; and the long exchange between Heppell and Gavin). A brief summary is presented here. We do not know whether or not Sutlive and Appell had access to the later exchanges (Heppell, 2018a, 2018b; King, 2018a). Their 'Letter to the Editor', Clifford Sather, was published in the 2018 edition of the *Borneo Research Bulletin* which did not appear in the public arena until 2019. There should have been ample time for them to have addressed the exchanges in 2018, or at least for the *Bulletin* to have taken them into account.

Let us return to the issues. Heppell adopts an evolutionary framework in his analysis of Iban textiles, drawing on the interactions between biology and culture. This is now confirmed by Sutlive and Appell; 'Heppell does employ the interactionist paradigm' (2018: 22). It suggests there is a paradigm, which Freeman spent much of his energy from the early 1960s developing; they then confirm their approval of it by emphasizing Heppell's 'insistence on "getting it right"... 'in the best tradition of scholarship' (ibid.). Heppell's analysis comprises two main strands: one technological which reconstructs the evolution of Iban weaving; the second is 'socio-biological-genetic'. Heppell has focused on sexual selection (in partnerships forged between skilled Iban [female] weavers and renowned [male] Iban head-hunters), the reproductive success of these relationships, and, in Darwinian terms, the achievement of the ultimate imperative: survival of the fittest. An additional objective has been to interpret patterns and motifs in Iban textiles, or, in his terms, 'pictograms', 'texts', 'stories' and 'symbolic representations' (2006a:182; 2014: 91, 150). According to Heppell, Iban and culturally related peoples in West Kalimantan, which he covers by the term 'Ibanic', assign 'a meaning to every motif' (2014:117).

In the interaction between biology and culture, Heppell proposes that the complex of traditional Iban culture, and particularly head-hunting and weaving, can be explained in terms of ‘the biological imperative of survival’ expressed in two principles: first, that ‘[severed] heads enhanced survival’ and second, that of ‘sexual selection’ in that a woman seeing a man holding a trophy head ‘sees someone to protect her offspring and pass on these characteristics to them’ (Heppell et al. 2005: 36; Heppell 2014:100). He reduces the core of traditional Iban culture to ‘a simple equation: beautiful cloths = heads = primacy for sexual selection’ (Heppell et al. 2005:167), emphasizing that ‘textiles were driven by sexual selection’; weaving was used ‘to attract mates’, ‘titillate the male eye’ and ‘seduce their menfolk’, because ‘men had recognised that weaving provided insights into the reproductive fitness of women’ (2014: i, ii, 1, 5, 100, 101-104).

A major difficulty is that the relationships between weaving skills, ‘reproductive fitness’, and ‘better genetic endowment’ are assumed intuitively on the basis of oral histories and genealogies and not demonstrated with genetic and statistical evidence. Wadley too suggests, in his review of Heppell et al’s book (2005), that ‘the thesis is logically intuitive, but intuition is not proof’ (2006: 262-263). Even Sather, who seems to have taken a recent position in this debate to support Heppell (2018a, 2018b), and Sutlive’s and Appell’s letter to him (2018), has said previously that ‘While Heppell has chosen to couch his arguments in terms of “sexual selection”, history, to my mind, might well have offered a more appropriate framework. The flowering of Iban art was, after all, extremely short-lived in biological terms and depended upon a number of historically-circumscribed processes’ (2006: 269; and see Dentan, 2002).

For Heppell, in regard to weavers, their ‘genetic fitness’ is also associated or combined with physical beauty and symmetry, and ‘a capacity for strenuous work’; Heppell then qualifies this by stating ‘that there has been little research about fitness indicators for women’ (2014:103). We need to rethink these statements; the problem with this line of reasoning is that whilst overt characteristics of success and physical attractiveness are undoubtedly important in choosing a partner, the less overt ‘genetic’ and ‘reproductive’ characteristics are rather more difficult to discern in the choice of ‘mating’ partners. Sutlive’s and Appell’s recent intervention does not solve this dilemma in stating that Iban, ‘from the middle of the 20th century...were keenly aware of the family histories of potential spouses for their children’ and ‘care is taken to ensure that one’s children “marry well”’ (2018: 22). Overt characteristics do not necessarily translate into the less overt matter of the ‘quality of genes a potential partner will bring to a match’ (Heppell et al: 2005: 36).

Moreover, there is a complex of encounters surrounding sexual and family relationships and reproduction: pre-marital bonding, extra-marital liaisons, divorce and remarriage (Heppell refers to ‘anecdotal’ evidence of relatively high divorce rates and casual affairs and adultery among the Iban [2014:107-108]), adoption of children, children’s physical survival and, if they survive, their mental and physical health, and the inability of some ‘successful’ couples to reproduce or to reproduce, but without achieving the success they aspire for their offspring. Heppell wishes to explain this complexity by tracing marriages located in oral traditions and genealogies (*tusut*). Yet he says: ‘For our argument about the sexual desirability of leaders and gifted weavers, statistics on the frequency of casual affairs would be helpful. There are, of course, none. Unfortunately, nor are there statistics about the frequency of divorce of leaders and great weavers’ (Heppell, 2014:108).

Sutlive and Appell address the ‘dismissal’ of Heppell’s argument concerning the biological imperatives of coupling between ‘skilled weavers and bravest warriors for lack of evidence from molecular biology and genetics ... [as] ... reductionist’, and the criticism of Heppell’s evolutionary exegesis as ‘exaggerated’ (2018: 22). For them oral traditions and genealogies are part of the ‘abundant evidence’ that ‘from the middle of the 20th century when Sutlive worked among them (1957-1972, 1987-2010) [the Iban] were keenly aware of the family histories of potential spouses for their children’ (ibid.). But this is not evidence of genetic success, specifically targeting the quality of a potential marriage partner’s genes, and then ensuring that the fittest survive over several generations.

In the *BKI* paper (King, 2017a) which Heppell contested, Sutlive and Appell refer to ‘Heppell’s tracing of social and cultural change [as] consistent with Marshall Sahlins’ and Elman Service’s *Evolution and Culture* [1960] (2018: 22). This is another element in their ‘abundant evidence’. One of the main tasks of Sahlins and Service was to reconcile the different evolutionary approaches of Leslie White and Julian Steward. It is an edited book which Sutlive and Appell fail to mention; so, it remains uncertain which evolutionary track Sutlive and Appell assume Heppell follows, in this collection of edited essays, in his interpretation of Iban ritual textiles.

Raoul Naroll’s review of Sahlins’ and Service’s edited volume (1961) captures precisely the problems raised by evolutionary approaches. Naroll says, ‘This is a book of some pretensions and some interesting speculations’ (1961: 389). Naroll concludes his review: ‘When we present untested concepts or hypotheses to the public, as here in *Evolution and*

Culture let us present them with diffidence and due modesty. Let us go back to the task, determined to keep at it until we can flatly assert laws of cultural evolution scientifically established by rigorously defined and thoroughly controlled tests' (1961: 392). The evidence is problematical when attempting to chart events and processes in what is a speculative historical reconstruction.

In examining 300 years of Iban history, Heppell proposes that their weaving is 'certainly more than 700 years old' (2014:141), and that textile styles point to the development of weaving as far back as the 8th and 11th centuries (2013:19). His 'evidence' is primarily Iban genealogies, oral history and 'texts' (2006b: 264-265; 2014:110-113). Here the deployment of oral materials, which Sutlive and Appell endorse, is problematical. Oral histories are not simply 'histories'. They contain information which relates to real-life events and persons and can be and are used for purposes of historical reconstruction (Vansina 1965). But, ideally, they need support from other areas of scholarly activity: archaeology, prehistory, ethnolinguistics, ethnographic studies, and written historical records. They are also used as religio-mythical charters, which establish moral and cultural priorities; they explain how the social and cultural world came to be; they define identities and origins; depending on the individual(s) carrying these histories, and the contexts in which they do so, they can be manipulated. In oral cultures, genealogies are invented and re-invented, particularly at earlier generational levels; they are often used to legitimize current socio-political circumstances in the service of which they can be changed so that old connections are discarded and new ones established; they provide arenas for debate and dispute. Iban genealogies, traced through both males and females, are particularly flexible in this regard. Then, added to this set of issues, Sutlive and Appell provide a statement without explanation or elaboration that 'Social systems exist to be manipulated' (2018: 22).

In this connection Freeman's comments on examples of Benedict Sandin's extended genealogies (1968), bearing in mind Sutlive's view that Sandin is 'a foremost student of Iban folklore' (1978: 27) and that Sutlive and Appell support the historical value of Heppell's reliance on the accuracy of Iban oral history 'as history', which permits statements to be made about 'genetic success' and biological 'fitness'. Freeman says 'One can only conclude that the particular genealogy extending back to a mythical mountain "near Mecca" is the pious invention of a Saribas Dayak who had become enraptured with Malay values and the Islamic faith...' (1981: 10). In his comments on Sandin's own genealogy, Freeman says that 'we are treated to a genealogy...which begins in "the Holy Land in the Middle East"... to padi mortars

that suddenly fly skywards...to the slaying of stars in human form..., and to numerous other trans-empirical acts... such as ancestors cutting down invisible spirits...and turning other adversaries into boulders'. The genealogy comprises 'a continuous narrative that ends with the descendants of miracle-working ancestors becoming clerks in the Government of Sarawak and the Borneo Company Limited'. Freeman's overall observation on Sandin's genealogies and oral history are that they create 'a flabbergasting ambience, in which the mythical and the real indiscriminately jostle....' (1981: 13). Yet Sutlive and Appell, *contra* Freeman, propose that the claim that oral histories "do not present a historical narrative as such" (King, 2017a: 100) 'exhibits a profoundly uninformed position for a scholar' (2018: 20). They then refer to the oral histories of Australian aborigines which are 'remarkably accurate' and that when Sutlive 'described Iban genealogies of 25 generations in depth to a group of Southeast Asian scholars in 1974, some participants questioned the credibility of the statement' (2018: 20).

In a now classic book on oral traditions and genealogies, Henige examined the processes of telescoping, lengthening, combining and fabricating genealogies, and of creating new ones to meet particular political purposes at particular times; issues of legitimation and the enhancement of status and power are especially significant in these processes (1974). A most respected scholar of Iban culture in West Kalimantan, Reed Wadley, who most decidedly meets the Sutlive-Appell-Heppell credentials for speaking with authority on Iban subjects, has said 'Local [Iban] oral history may be considered as a particular form of indigenous knowledge, but one that is routinely manipulated (in the broad sense of the word) for contemporary interests and purposes' (2002: 322). We need to keep in mind the West Kalimantan dimension in that, for a significant stretch of time recorded in the genealogies which Heppell uses, the people who recorded them were not known as 'Iban' and did not use this term to identify themselves; they were intermingled with the complex of linguistically and culturally related peoples in Indonesian Borneo which are referred to by different ethnic names (for example, Mualang, Kantu', Seberuang and populations of the Ketungau river basin [Gavin, 2012]; see also Collins and Herpanus on the Sekujam [2018]).

Patterns, motifs, and pictograms

Leaving aside the question of whether or not an interpretation of Iban weaving and head-hunting can be reduced to biological imperatives connected to social and cultural relations, the other arena of debate is whether or not the patterns and motifs in Iban woven cloths constitute

a ‘language of symbols’, or ‘symbolic representations’, or ‘pictorial narratives’ (Heppell 2006a:184; 2014: 91-94, 117, 138). Sutlive and Appell suggest, in support of Heppell’s criticisms of Gavin’s analysis of Iban ritual textiles, that she ‘reveals an almost complete ignorance of the nature of “symbols” and their functions in the creation of human cultures’ (2018: 22). The only direct reference in their *BRB* letter is *The Symbol Dictionary* which addresses the ‘secret language of symbols’ ([http://symboldictionary. Net/?p=1914](http://symboldictionary.Net/?p=1914), n.d.). The dictionary presents a bewildering range of visual symbols, though one is not sure who provides the explanations of these symbols and from whence the sources of the information have been accessed. The reader is not reassured when informed that these ‘secrets’ can be accessed and downloaded in a pdf. The website states that ‘Knowing the secret system behind these symbols can provide an incredible amount of insight into even the most inscrutable symbols’.

But these references to what is or is not symbolism, and how we interpret it are again irrelevant in that a popular internet dictionary in which Sutlive and Appell seem to place some trust, casts little light on the issues debated in regard to the Iban. ‘What if the Iban did not embrace the need to assign meaning in every motif? What if they thought that a motif was aesthetically pleasing and met an aesthetic need and did not stand for something else? In other words, in the terms that Western anthropologists would want to address it, could we explain it in strictly symbolic terms? And what if we ask several Iban weavers what a particular motif is, and we get different answers, or a simple ‘I don’t know, it’s just an element in an overall design? I filled a space with this motif and gave it a name’. Or what if the individual elements in a pattern do not matter but it is the overall effect, an overpowering composition which is held in awe and admiration because it is not merely a collection of individual ‘symbols’ telling a story; it is a complete and spiritually powerful creation?’

In *The seductive warp thread* Heppell casts doubt on the quality of the Iban weavers who served as Gavin’s informants, as ‘merely competent’, ‘lesser strand’, and ‘low road’ (2014:144, 145, 150, 158). Sutlive and Appell, as does Sather, now lend their weight to this evaluation in that Heppell corrects ‘erroneous misinformation’ (2018: 22). What is most intriguing, which in turn contributes to the notion that only some individuals have privileged access and knowledge in order to identify and interpret ‘the language’ embodied in Iban textiles is that Heppell has maintained that this symbolic language is ‘secret’ (hence, presumably, Sutlive’s and Appell’s reference to secrets contained in *The Symbol Dictionary*). It resides ‘in a secret domain’, though much of it has now been forgotten or lost; misinformed outsiders cannot gain entry (Heppell, 2014:151-155, 158, 169; and see Haddon, 1895).

Gavin's emphasis was on long-established patterns, which are largely non-representational. She argues that the names of these patterns are important in that they function as titles, indicating the pattern's rank, and that of the weaver authorized to make it and the ritual function for which it is appropriate (2003:79, 166). Often, some motifs function as space-fillers, and the names given to them are 'just names' (*pengalit*; 2003:68, 192-193, 236). Interestingly in their letter Sutlive and Appell make no mention of these issues, and without any substantial evidence presented to demonstrate their opinion, they repeat Heppell's view: 'Gavin is insufficiently trained and culturally unaware' (2018: 22). They make no mention of Haddon's work; nor do they engage with the wide-ranging literature on the interpretation of non-Western material productions.

Haddon and pictographs

Alfred Cort Haddon visited Sarawak in 1898. During his stay he examined and purchased a number of Iban cloths, of which many patterns and motifs were named (Haddon and Start, 1936 [new edition 1982]). Haddon did not speak Iban and never spoke with Iban informants about these names; the information was derived primarily from Charles Hose, the then Resident of the Baram Division in the service of the Brooke Raj (Gavin 2003:197). As Gavin demonstrated, Haddon, guided by Hose, assumed that the names of designs refer to what they 'represent', even if the design shows no direct representation of the object so named (Gavin 2003:198). This difficulty was explained in evolutionary terms, an approach which Heppell adopts, and with which Sutlive and Appell agree. Haddon assumed that a realistic depiction was copied until it 'degenerated' to the point of being unrecognizable. Haddon and his colleagues, in his earlier *Evolution in Art* (1895), explained why local people were ignorant of the 'true' significance of their 'pictographs'. For Haddon they comprised a language, which those who had created it had forgotten (Gavin 2003:198-204). Haddon's assumptions derived from a literate culture, claiming a superior interpretative knowledge of the artistic creations within an oral culture which had come under the scrutiny of Western anthropologists who knew better.

As she records, Gavin visited Derek and Monica Freeman in Canberra in 1993 (2003: viii, x) when they generously gave her access to their field-notes and Monica's drawings of Iban *pua* patterns. Monica indicated that when she and Derek collected data in 1949-1951, they focused on modern patterns (Gavin 2016b, 2016c). Gavin demonstrates the shift over time from

primarily non-representational to increasingly figurative, narrative patterns (Gavin 2003:18-19, 80, 83, 97, 104, 150-3, 165). She points out that Monica, in her diaries, only included figurative examples (see Appell-Warren 2009:182, 192, 415, 419, 481, Figure 57; with the exception of Figure 5). From Gavin's reading of Freeman's field notes on these recent cloths he rightly concluded that they were representational and 'told a story'. Again, contrary to Heppell's claims (2010; 2014: 158), Gavin did not say that the Freemans were wrong with regard to recent representational patterns (2016: 32).

We do not know whether Freeman was told that some names are 'just names' in these precise words, as recorded consistently by Gavin (2003:83, 169-171, 191, 198, 206-207, 273). However, he has many examples of motifs, which he was told were 'only space-fillers' (*pengalit*) (Gavin 2003:199). This might have suggested that this device was not about 'representation' alone (Freeman 1949-51, field notes, from Gavin 2016c; and see Gavin 2003: 68, 192-193, 236). Freeman recorded that even when Iban women knew the name of a pattern, they were 'quite unable to point to the elements in the design to which the name refers' (Freeman 1949-51, field notes; Heppell 2014: 92). Echoing Haddon's evolutionary approach, Freeman concluded that the original patterns were 'lost', eventually becoming 'conventional' through successive copying (Gavin 2016c). Gavin demonstrates that experienced weavers cared more about a pattern's rank and title than they did about the names of individual motifs, in that the former determined the weaver's rank, status and her standing in the community (2003:79, 85, 139, 155, 166, 243, 249, 278-279).

Freeman's perspective of lost knowledge has since been repeated by Michael Heppell who refers, with authority, to Freeman's field notes and the Baleh Iban 'process of amnesia' and the 'dissipation of memory'. He devotes his Chapter 4 to 'memory and its loss' and has adopted Haddon's evolutionary assumptions (2014: 91-94, 144, 146, 150; 2015: 151, 152; and see Gavin, 2015). In countering this proposition there is a substantial comparative ethnographic literature demonstrating that patterns and motifs are often seen locally in decorative and aesthetic terms, and, in some cases are deployed for vitally important social purposes. Yet they do not contain a 'language of symbols' (see, for example, Price 1989 [2001], 2005, 2007; Lamb 1975, Thomas 2013; and Gavin 2003:273-280). Rather, this assumption of a language of symbols tends to be rooted in 'outsiders' stereotypes of "primitive art" (Price 2007: 610; Gavin 2016c).

Subsequently, Heppell has pressed home the evolutionary paradigm and presented an extended criticism of Gavin dismissing her ‘contrary view’ as of ‘little merit’ and a ‘non-core track’ (2014: 5, 150: Appendix 1, 149-169). These claims to authority need to be contextualized. Gavin’s work was located in a British anthropological tradition, informed by an art historical discourse, inspired, among others, by Ernst Gombrich (see, for example, 1979), but far removed from the structural-functionalist and structuralist perspectives from which Freeman departed in the 1960s. In the section below on ‘Self-revelations’ an important interconnection between the four case-studies presented (King, 2017a), in demonstrating particular academic discourses, is that three of them involve Professor Rodney Needham as well as other structuralist anthropologists with whom Freeman subsequently disagreed in his pursuit of an ‘interactionist paradigm’.

Addressing a recent intervention in a decade of academic debate: Sutlive and Appell

Much of what Sutlive and Appell address in their recent contribution (2018), and in Sather’s editorials (2018a, 2018b) has already been debated in rejoinders in the pages of the *BKI* (Heppell, 2018a, 2018b; King, 2018a). Though some issues have been put to rest, there are others which remain contested. It has already been accepted that the concept of ‘authority’ requires clarification and is clearly subject to misinterpretation, as Sutlive and Appell demonstrate. The 2017 *BKI* paper focused on the mode of engagement and the style of argument, of which the recent contribution of Sutlive and Appell is an exemplar. The issue of authority does not entail that Freeman had deliberately excluded other researchers from studying the Iban; nor, in contrast to Heppell’s statements (2018a), that it criticizes Freeman for the ‘abuse’ of his authority, for exerting a ‘malign’ influence on Borneo anthropology and studies of the Iban; nor that Freeman ‘distorted or misrepresented aspects of Iban culture or did harm to other anthropologists’; nor that he was bent on ‘actively discouraging a scholar from working with the Iban’ and that his actions were ‘not in the best interests of the study of the Iban’. Exclusion is at the level of intellectual engagement, presented in a particular style of scholarly encounter (see King, 2018a: 473).

The issue of authority does not question Freeman’s professionalism and constructive engagements with those working on various aspects of Iban studies (King, 2013: 39-40; 2017a: 104), nor his formidable skills as a linguist and ethnographer and the outstanding monographs on the Iban which he gave to the world of anthropology (King, 2017b, 2018b). This author has already stated that ‘it can be argued that of all anthropological monographs on Borneo

communities it has been Freeman's very widely quoted *Report on the Iban* (1970, and see 1953, 1955b) and *Iban Agriculture* (1955a) which have been the most influential and which have provided a baseline and set a standard for the study of cognatic societies and for our understanding of shifting or swidden agricultural economies in the humid tropics' (King, 2017b: 84). Furthermore, that in reflections on field research in Sarawak, there was a statement in these terms: 'What also struck me in my visits to Iban longhouses was how impressively Derek Freeman had captured the major social and cultural characteristics of the Iban...' (King, 2018b: 39). Nevertheless, some of what he wrote can be subject to reflection, qualification, re-interpretation, change of emphasis and elaboration, but this is not necessarily an issue of ethnographic error, distortion or misrepresentation (King, 2018a).

In this connection we have to address the personal engagement of the anthropologist and what other anthropologists who happen to follow them into the field or read their publications will make of their work. In the classic studies in Sarawak that the Colonial Social Science Research Council (CSSRC) bequeathed us in the late 1940s and early 1950s (Leach, 1950), we might contemplate what we would now be working with and debating if Professor Raymond Firth and his London-based committee of the CSSRC, in the fading days of British imperialism, had allocated the Iban or the Melanau to W.G. (Bill) Geddes, the Land Dayak or the Melanau to Freeman or the Iban or the Land Dayak to H.S. (Stephen) Morris (King, 2017c: 17-19, 20-28; 2019: 190-191). Clifford Geertz's application of the methods and techniques of literary criticism to ethnographic writings is still worthy of our contemplation (1988; and see Eriksen, 2001: 34), as well the critiques of colonial anthropology (see, for example Asad [1973] and Zawawi [2008, 2012]). In the early post-war period in Borneo the voices of the indigenous populations were presented through the conceptual filter of Western anthropology, which suggests that considerable rethinking and reflexivity is required on our part.

Sutlive and Appell state that, in engaging with the issue of claiming authority (King, 2017a), it is proposed that 'no ethnography is bias-free' (2018: 19). This statement requires qualification. Freeman did not present a biased description and analysis of Iban society and culture, but he did interpret aspects of Iban social organization and culture in a particular way in both his structuralist and then his 'interactionist' phase. For example, the issue of Iban egalitarianism that Rousseau drew attention to is pertinent (1980). Furthermore, though Rousseau did not express it in a sufficiently careful manner on the basis of the sources he used (and Freeman criticized this), he did draw attention to the problem of the tension between Iban

egalitarianism and inequality, which Sather then pursued and explained more fully and subtly (1996).

A final issue in this section is the use of the concepts of ‘domain’ and ‘terrain’ (King, 2017a). These do not define a physical and material space to be occupied, defended and fought over, but an intellectual field of endeavour, a realm of personal knowledge, a sphere of academic influence, a ‘territory’ of academic debate which give rise to different modes of discourse. For example, Bertrand Pulman gives us a conception of ‘le terrain’, a French concept closer to my understanding; ‘Le concept théorico-pratique de terrain a joué un rôle centrale dans le discours des anthropologues, en particulier pour définir la spécificité de l’anthropologie par rapport à d’autres disciplines’ (1986: 5).

Self-revelations as an arbiter and judge of scholarly exchanges

Sutlive’s and Appell’s ‘Letter to the Editor’ in response to the 2017 *BKI* paper, entitled ‘A Rejoinder: “O what a tangled web we weave’ (2018) was contextualized by the Editor (Sather, 2018a, 2018b). An immediate observation is that Sutlive’s and Appell’s style of encounter and language resonate with those of Heppell (and with some of Derek Freeman’s earlier scholarly engagements) in the interpretation and analysis of Bornean and other cultures. They state, for example, that ‘King does not speak Iban and has done no field research among the Iban’ (2018: 19); and to be certain their message is not lost: ‘Rousseau, as King, has never worked among the Iban, and does not speak the language’ (2018: 21). This echoes Heppell’s pronouncements (2018a; King, 2018a). Then they argue that the 2017 *BKI* paper provides conclusive evidence that the author is ‘mischievous’, ‘remarkably self-revealing’, ‘self-serving’, ‘mean-spirited’, adopts ‘a profoundly uninformed position’, attempts ‘to expose Freeman as inflexibly dogmatic’ and presents ‘an exaggerated criticism of Heppell’; it continues that the paper is ‘logically inarticulate’, and ‘theoretically and factually flawed’, (2018: 20, 21, 22). This mode of engagement, in exerting authority, resonates with Freeman’s response to Robarchek and Rousseau, and with Heppell’s criticisms of Gavin.

Sutlive’s and Appell’s evaluation of the 2017 *BKI* paper starts with two comments in that the author is ‘self-revealing’ and ‘self-serving’. The self-revelation is the author’s ‘assumption of the role of arbiter and judge of scholarly exchanges between major figures in Bornean ethnography’ (2018: 18). Leaving aside, for the moment, the status of those who are engaged in an academic dispute, it would seem to be an acceptable practice to consider

opposing or alternative views in debate and discussion and weigh the relative merits of these. In regard to the issue that Sutlive and Appell raise of arbitration and judgement between ‘major figures in Bornean ethnography’ the reader has to guess at those whom they consider to be ‘major’. Undoubtedly one of the major figures is Derek Freeman. But the case material which Heppell misleadingly refers to as ‘a chain of incidents’ or ‘alleged abuses’ rather than ‘as intellectual engagements that are processual’, in which Freeman engaged, is somewhat diverse (Heppell, 2018a, 2018b; King, 2018a).

The 2017 *BKI* paper first drew attention to an article of Freeman’s which was, in part, a response to Rodney Needham’s paper on the themes of ‘blood, thunder and the mockery of animals’ among the Penan of Borneo and certain Orang Asli populations in peninsular Malaysia. Sutlive and Appell seem not to have grasped the ethnography; Needham did not base his argument only on ‘the impact of natural phenomena on Malayan aborigines’ (2018: 21); his thesis also drew on his own ethnographic data from the Penan of Sarawak. They then suggest that Needham’s paper is a return to 19th century ‘naturism’ (ibid.). Their criticism of Needham’s move to ‘naturism’, which is presumably a reference to his interest in ‘natural symbols’, is an inappropriate use of the term, and is then replaced by their own explanation which is ‘the lively interaction of imagination to lightning and thunder’ of the Penan and Orang Asli (2018: 21). It would seem that, in Freeman’s terms, this would not provide sufficient explanatory advancement of the phenomena under scrutiny (Freeman, 1968: 354).

The principle that Sutlive and Appell establish to claim authority, as in the Iban case, is that the anthropologist must speak the language and have undertaken field research among the people so studied. This is a valid point, but requires qualification. The choice of the Needham-Robarchek-Freeman case was deliberate. Needham, in the case of the Penan, and Robarchek, in the case of the Semai-Senoi, spoke the languages of the peoples whose cultures they were studying, and they had undertaken field research among them; Freeman had not. Yet Sutlive and Appell support Freeman in that he is ‘on point’ against those anthropologists who have linguistic expertise and have undertaken fieldwork among those peoples who are the subject of dispute.

In any event, the case-study was directed not to Needham but to Freeman’s exchange with Clayton A. Robarchek, an anthropologist who had undertaken long-term field research among the Semai-Senoi of peninsular Malaysia (1987a, 1987b) and among the Waorani of Ecuadorian Amazonia. Robarchek is hardly a major figure in Bornean ethnography. Moreover,

had we had the opportunity to question Needham on his status in Bornean ethnography it is even doubtful that he would have seen himself as a ‘major figure’. His reputation as an anthropologist is based on his international stature in British structural anthropology, symbolic classification, cognitive universals, indigenous psychologies, and kinship and alliance theory, among other subjects, but aside from his published papers on Borneo, he styled his doctoral thesis on the Penan of Sarawak as a ‘tyro’ effort which he never contemplated publishing as a monograph and never did.

The second case addresses Freeman’s review article of Erik Jensen’s *The Iban and their Religion* (1974). Sutlive and Appell endorse Freeman’s criticisms of Jensen’s work in their statement that ‘the text is astonishingly incomplete with no treatment of headhunting or of death rituals, or dreams or daily obsequies—a little on sickness, in particular the lengthy dirges performed by soul-guides’ (2019). Apart from this monograph Jensen published very little on Borneo, though his recent semi-autobiographical *Where Hornbills Fly* (2013) gives some contextualization of his field research. Given Sutlive’s and Appell’s opinion of Jensen’s *Iban Religion* there is little possibility that he enjoys the accolade of ‘a major figure in Bornean ethnography’. Much of what Freeman had to say about Jensen’s book is convincing, but what is of interest are Freeman’s references to a certain ‘school’ of British anthropology and the kinds of perspectives which they followed in contrast to his developing ‘interactionist paradigm’. Needham was clearly a target as Jensen’s Oxford supervisor, as he was in the blood, thunder and mockery issue; Needham also happened to advise Traude Gavin in her doctoral work on Iban textiles, about which Heppell has provided sustained criticism. It is not without interest that Freeman refers to Jensen’s external examiner for his DPhil thesis, H. S. (Stephen) Morris with whom Freeman, according to his wife Monica’s diaries, did not enjoy the most harmonious of relationships (see King, 2019). However, in the way in which British structuralism was developing at the time, there was some merit in Jensen’s work, and he made a contribution to the study of Iban religion, in the absence of anything else that was available on Iban religion up to the 1970s (see King, 1978, 1980, 1985).

The third case was the exchange involving John E. Smart, based on his doctoral thesis on the Karagawan Isneg of Northern Luzon presented to the University of Western Australia, which is structuralist in orientation. Smart did not publish his field research as a monograph, and he too was hardly ‘a major figure in Bornean ethnography’. However, his exchange with Freeman demonstrated the utility of the distinctions between people’s ideational categories and

what happens in the phenomenal order of action, behaviour and process, and he addressed critically Freeman's description and analysis of Iban bilateral kinship. These issues needed to be addressed. Sutlive and Appell support Freeman; he is 'on point'. However, they do not refer to any of the details of the exchanges between Smart and Freeman because these remain unpublished, other than some of those excerpts which have been reported (King 2013, 2017).

Finally, Freeman's criticism of Jérôme Rousseau's paper on 'Iban Inequality' is a different matter. Freeman adopts a mode of academic engagement which needs to be addressed. Rousseau is 'a major figure in Bornean ethnography', but yet again, even Rousseau might not qualify as such, given the demanding criteria that Sutlive and Appell set in Bornean ethnography: moving into other terrains, domains or territories in Borneo as an outsider, without speaking the language of the people into whose cultures they intrude and among whom they have not undertaken field research is fraught with danger. Rousseau, as a specialist on the Kayan, but then writing on the Iban, did not fulfil these conditions; it appears to disqualify him from commenting on Iban history, society and culture. It should also be noted that subsequently Rousseau distanced himself from Borneo and moved on to more general issues in anthropology (2006). Some of Freeman's criticisms of Rousseau were, as Sutlive and Appell observe, 'on point', but not all of them, in that Rousseau made a most significant observation in the study of the Iban: 'To understand the specificity of the Baleh Iban, we have to consider the historical circumstances under which they came into being' (1980:60). This in turn requires a wider investigation of those communities which 'came to be Iban' in the context of the wider set of so-called 'Ibanic' communities with which the Iban are culturally and historically related. Gavin's re-examination of the problems occasioned by a too Iban- and Sarawak-centred view of this complex of interrelated peoples and a lack of attention to those related populations in Indonesian Kalimantan raises the kinds of issues, which, in the appreciation of Freeman's work on Iban social organization, and specifically 'egalitarianism', need to be addressed (2012: 98-113).

It would seem that Sutlive and Appell are themselves victims of this Sarawak-centred view of the Iban and related populations. And in the pursuit of weavers, warriors, and symbolic meanings in ritual textiles Heppell, with the endorsement of Sutlive and Appell, constructs histories on the basis of populations which had not yet 'become Iban'. The complexities of the oral traditions and genealogies which they use as evidence were generated by a congeries of Iban-related peoples in what is now West Kalimantan, which, in terms of ethnic identity and nomenclature were and are not 'Iban'.

On self-serving exoneration

A second issue is one of ‘self-serving’, according to Sutlive and Appell, they expose ‘[the author’s] efforts to exonerate his role by validating his student’s work that has been called into question by other authorities on Iban weaving’ (2018: 18) and that the 2017 *BKI* paper is ‘to refute criticisms of his student Traude Gavin made by Michael Heppell’ by means of ‘a series of attacks (sic) on the work of Derek Freeman, the rationale being that if “the master” is discredited, so the student will be’ (ibid.: 23). Leaving aside the issue of ‘authorities on Iban weaving’ which would demand an extended separate paper, there is a need to focus on the misuse of the terms ‘exonerate’ and ‘validating’; Sutlive and Appell have also used the inappropriate term ‘attacks’ which those supporting Freeman tend to warm to in images of warfare. Gavin does not need to be defended nor exonerated. She has defended herself in exchanges with Heppell and continues to do so, but in evaluating Heppell’s criticisms, there has to be a scholarly concern about the language in which these criticisms have been couched.

On credentials in the study of Iban

This section addresses whether or not Sutlive and Appell have been entirely accurate in their criticisms, which repeat what Heppell has already stated (King, 2018a: 474). The charge is that ‘King does not speak Iban and has done no field research among the Iban’ (Sutlive and Appell, 2018: 19). The author so charged accepts that he does not speak Iban, but Sutlive and Appell omit to say that he spent over a year in the first half of the 1980s and a further extended period in the 1990s undertaking team projects, in collaboration with prominent Iban researchers, on development projects and environmental change among Sarawak Iban in the hinterland of Bintulu, and other indigenous communities, and published over 20 papers on these projects, two with a senior Iban academic. He also supervised and externally examined some 12 postgraduate theses either specifically on the Iban or which contain substantial case material on them (four of them written by Iban). His early research among the Maloh in West Kalimantan required him to engage with neighbouring Iban longhouse communities which resulted in published papers on Maloh-Iban relations (see King, nd, ‘publications’ at victortking.org). Appell, who does not speak Iban and has undertaken no research among them, though he has written on them, lists his work in the letter to the *BRB* confirming his authority to speak and write on matters related to the interpretation of Iban ritual textiles

Sutlive’s and Appell’s scientific ethnography needs further exploration. Most fortunate is the anthropologist today who can spend extended periods of time in fieldwork in a relatively

supportive and safe environment. Constraints of funding, the requirements of funding bodies in regard to postgraduate training, the conditions placed on foreign researchers by governments, the increasing pressures on anthropologists to provide practical and useful information which feeds into policy, the development of team research and collaborations between foreign researchers and local scholars, the auditing culture which demands publications in internationally refereed journals, have all acted to change the nature of research for a significant number of anthropologists. So, team research, ‘big data’, applied and policy-oriented research, collaboration with researchers from other disciplines and with local scholars in the region or community studied, have become the way forward. The days when mainly privileged, primarily male Caucasian anthropologists undertook long periods of funded field research under the umbrella of European colonialism among subject populations, are over (but see Morris [1977] on relations between colonial administrators and anthropologists). These changes are accompanied by, on the one hand, the increasing indigenization of anthropological research and the development of locally-generated approaches within Borneo and the wider Southeast Asia, and, on the other, the emergence of ‘world anthropology perspectives’ which increasingly undermine the dominance of Western anthropologies (Thompson and Sinha, 2019: 1-18).

Styles of academic approach and debate

Sutlive and Appell adopt a critical stance against the practice of synthesizing in academic analysis. Though it is not relevant to their argument, they suggest that King has ‘an unusual and useful talent for synthesizing’ and that ‘[s]ummarizing the work of others along with a commentary can be a useful scholarly project’ (2018: 20). Appell has also undertaken general overviews of Borneo peoples and formulated synthetic classifications of them, and provided a survey of literature on the Iban (see, for example, Appell, 1976; and 2001). These have been subject to debate and modification. If we set ourselves against entering the domain and terrain of others in attempting to understand Borneo as a whole, let alone trying to contextualize Borneo in the wider Southeast Asian region, then it is doubtful that we shall make much progress. For example, there is a boldness in Edmund Leach’s social science survey of Sarawak (1950), though, he spoke none of the languages of Borneo and got some things wrong; there is excitement in Bernard Sellato’s distillation of materials on the hunter-gathering populations of Borneo (1989) and Jérôme Rousseau’s engagement with Central Borneo (1990), though they did not speak all the languages of the peoples about whom they were writing; and more recently

Peter Metcalf's comparative approach in understanding ethnic identities in Borneo contributes to our understanding of Borneo cultures as a whole (2010).

For Sutlive and Appell synthesis is 'digesting the work of others', with a commentary (2018: 20). But if we do not 'digest', how do we contextualize individual cases? I suggest that Sutlive and Appell have no clear idea what synthesis entails. They neglect to mention its origins in German philosophy. Though synthesis was attributed to Hegelian dialectics and adopted and popularized by Karl Marx in his analysis of the development of capitalism, it was the German philosopher, Johann Gottlieb Fichte, under the guidance of one of his mentors, Immanuel Kant, who formulated the triad, 'thesis-antithesis-synthesis' (Breazeale and Rockmore, 1994, 2001). Synthesizing was not in Fichte's view, an exercise for the faint-hearted; in opposing a thesis, the antithesis would lead to a 'new proposition' and new ideas (and see Williams, 1992). Sutlive's and Appell's principle that, in 'scientific ethnography', anthropologists should operate within the parameters of a society which they know at first-hand and speak their language (and not intrude into another's space), would rule out much of the exciting work that has been undertaken in Bornean anthropology and beyond.

Moreover, an important implication of not synthesizing involves a potential denial of the importance of comparative studies. It is not that one merely digests the work of others, and summarizes and comments on it (though when you are bringing work together from multiple sources it is hardly adequate to characterize this as a summary and comment), it also entails comparison and contrast, a major principle of anthropology that the founders of the discipline strongly advocated (Radcliffe-Brown, 1951: 15-22). Be that as it may, Sutlive and Appell, to illustrate their point, then refer to an example of what they consider to be synthesis, *The Peoples of Borneo* (King, 1993), and that the term 'Dayak', as an 'exonym', is wrongly attributed to some of the indigenous populations of what is now Sabah (2018: 20-21). Their correction is accepted, but this is hardly an adequate demonstration of the perils of synthesizing, nor is the term 'Dayak' (Daya, Dya, Dyak, Dayeh, Dayuh etc.) strictly an 'exonym'. It is used by various indigenous Bornean peoples to refer to themselves; for example, 'Lun Dayeh' and 'Bidayuh'. Metcalf, referring to Robert Blust's work, notes that, besides orientations according to cardinal points, 'Proto-Malayo-Polynesian has two other orienting features: *lahud, 'downriver,' 'towards the sea, and *daya 'upriver,' 'towards the interior.' These clearly *do* apply in Borneo' (Metcalf, 2008: 40).

Sutlive and Appell do not acknowledge the dynamic relationship between externally and internally generated nomenclatures. Some ‘exonyms’ are accepted and become ‘autonyms’ or ‘endonyms’; some are rejected. It so happens that nineteenth-century Europeans took an indigenous term (‘Dayak’, not an ‘exonym’ in that it has an indigenous origin) and used it to designate the peoples of the interior. Furthermore, the term ‘Iban’, for example, is primarily a post-Second World War creation, becoming firmly embedded in the literature with the publications of Derek Freeman, but even then, the term ‘Sea Dayaks’ continued to be used; Haddon and Start used both terms in the title of their 1936 book. The Iban themselves have adopted the term as an ‘autonym’.

Terms such as ‘Dayak’ have also become highly politicized and ‘indigenized’ in Kalimantan and Sarawak with the formation of political parties and movements, going back to as early as 1919 with the formation of Sarekat Dayak in Banjarmasin, then Partai Persatuan Dayak in West Kalimantan in 1945, and the creation of Dayak Besar (The Great Dayak), in 1946, eventually to re-emerge in the formation of the province of Central Kalimantan in 1957 to give the Dayaks greater autonomy (see, for example, Miles, 1976). The East Kalimantan Dayak Association was inaugurated in 1999 (Schiller, 2007). In Sarawak, Parti Bansa Dayak Sarawak was formed in 1983, then dissolved in 2003 and 2004 to re-emerge in 2013 as Parti Bansa Dayak Sarawak Baru. There are certainly problems in Sabah about the term ‘Dayak’ and whether or not it should be used for the indigenous populations, but there is now a debate in process prompted by the Borneo Dayak Forum in Sabah and in the inaugural Dayak International Justice Conference in Keningau.

Wadley said, many years ago, ‘Dayak is the general term for the non-Muslim indigenous people of Borneo’ (2002: 325). In consulting the global media, Wadley’s statement on ‘Dayak’ is confirmed on the websites of Minority Rights Group International, Wikipedia, Encyclopaedia Britannica, Merriam-Webster, Lexico, Lonely Planet, World Wide Fund for Nature, The Free Dictionary, Your Dictionary, Dictionary.com and many others. ‘Scientific ethnography’ has a major struggle on its hands to introduce Sabah-generated subtleties into a term that is becoming increasingly politicized and generalized in Borneo.

On paradigms, theories and jobbing

The opinion amongst those (Sutlive, Appell, Heppell) who have known Derek Freeman personally is that he did not succeed in producing an ‘interactionist paradigm’ to replace or oppose the paradigm of ‘cultural determinism’. Sutlive and Appell have emphasized that Freeman also wanted ‘to get it right’ (2018: 19, 21). But in the case of the development of an alternative paradigm, did he get it wrong in that such a paradigm was not realized, at least in the view of those who support him? If he did get it wrong, then the ‘war’ with American cultural anthropology was possibly unnecessary, and much academic acrimony could have been avoided. The Mead-Freeman controversy (Freeman, 1983, 1996, 1999) has been well documented by Caton (2000) and Shankman (2009a, 2009b, 2013a, 2013b), and in an interesting piece by Tcherkézoff (2001), among many others, and their suggestion is that, in certain respects, Freeman did get it wrong, in particular in relation to his proposition that Mead was hoaxed by her informants (Freeman, 1999).

In their search for paradigms Sutlive and Appell refer to Edward John Mostyn Bowlby and Edward Twitchell Hall Jr. in their explanation of what Freeman’s paradigmatic conversion in Kuching in the early 1960s comprised and its consequences. Indeed Appell (with Madan), in his development of Freeman’s intellectual biography documents this conversion in considerable detail (Appell and Madan, 2008). Freeman’s objectives and ambitions were presented in a wide range of publications. There is no doubt that he was set on constructing an alternative paradigm (1992, 1997, 2017 [2000/2001]), and his later papers on the Iban express this (1967, 1979). Sutlive and Appell seem to think otherwise (as does Heppell [2018a]) and they provide a rather simple interpretation of what they assume he was doing. Freeman’s paradigm is merely ‘explicating the simple fact that humans are complex bio-psychological as well as social beings, and are programmed to relate and change – potentially at every level – by what they see and what they say, some less, others more profoundly’ (Sutlive and Appell, 2018: 21). I think Freeman would be disappointed by his supporters’ appreciation of his work in this field of endeavour in which he was involved for over 40 years. In this context Sutlive and Appell then refer to Bowlby and Hall.

Bowlby was a significant intellectual presence, a British psychiatrist, psychologist and psychoanalyst. He achieved an international reputation for the development of an evolutionary theory of attachment (1958, 1969, 1973, 1980). Bowlby’s emphasis on the ‘emotionally damaging effects of separating children from their families’ (something which he had

personally experienced in early childhood when his parents sent him to a boarding school at seven years of age), and the importance of the mother-child bond, did not go down well with the feminist movement which was pushing for gender equality, the importance of women developing their own careers, and enjoying a degree of independence from child-caring and the domestic round. Bowlby, neglected by his parents, suggested that in early childhood, there was a need for nurturance, proximity, security, comfort and ‘imprinting’ between children and ‘early carers’ (invariably the mother) in ensuring childhood development and mental functioning. He then developed much-needed programmes to address early delinquency and childhood maladjustment in the UK. Freeman draws on Bowlby’s attachment theory in his chapter on the ‘mother-child bond’ in Jack Goody’s edited volume (1973).

Apparently, Bowlby’s influence was much more significant in the USA than the UK (Byng-Hall, 1991: 9-14). Perhaps this might explain Sutlive’s and Appell’s impulse to refer to him. Bowlby died on the Isle of Skye, his vacation home, on 2 September 1990. Bowlby and his wife Ursula are buried at Trumpan Church, Waternish, a place that this author has frequently visited through family connections there. The inscription on his headstone reads ‘John Bowlby 1907-1990, To be a Pilgrim. And his wife Ursula 1916-2000’. Towards the end of his long and distinguished career Bowlby ‘died twice’; he died once shortly after his 80th birthday in 1987, but was miraculously revived (Byng-Hall, 1991). He then published a major biography of Charles Darwin three years after his first death (1990), demonstrating that there is life after death. There is much in Bowlby’s childhood that resonates with that of Derek Freeman. Both of them had difficult relationships with their parents, though in very different circumstances. Bowlby’s parents did not bother much with him and in the English middle class of the time packed him off to boarding school. On the other hand, Freeman was the son of ‘a strong fervent Presbyterian mother’ who entertained considerable ambitions for her only son, and a ‘feckless’ father of whom Freeman was ‘inordinately ashamed’ (King, 2019: 188). Hempenstall suggests, in his biography, that Freeman engaged in ‘a lifelong struggle against domination by others’ (2017: 18). I think it helps explain his mode of academic engagement.

There is then a reference by Sutlive and Appell, in addition to Bowlby, to the work of Edward T. Hall (who does not appear in their Bibliography), and suggest that King and Gavin ‘seem unfamiliar’ with ‘systems theory’ (2018: 21). They refer to Hall’s *Beyond Culture* (1976). It so happens King has read his book, and much else including *An Anthropology of Everyday Life* (1992) and *The Silent Language* (1959), where Hall came to the startlingly

original conclusion that ‘Culture is communication and communication is culture’ (1959: 186). Perhaps in raising the issue of synthesis, Sutlive and Appell might have realized that the digestive system of one who synthesizes is enormously active and capacious and that there might have been a possibility that both Hall and Bowlby had been digested.

In his early career Hall was much influenced by Franz Boas, Ruth Benedict, and Margaret Mead, among others (Rogers et al.: 2002: 5). It seems somewhat surprising, in this respect, that, in Freeman’s defence, his supporters, refer approvingly to Hall. Hall was also in the employ of the American Foreign Service Institute in the Department of State, Washington, D.C. in the post-war years when the USA had emerged as a major power in the global system. He developed intercultural training programmes in language skills for cross-cultural specialists and in non-verbal communication in the era when the USA was becoming a global power and concerned to engage with territories and cultures with which it had had no prior experience. Hall was therefore an important scholarly figure in the expansion of American post-war imperialism from the early 1950s (Roberts et al.: 2002: 5-9).

Turning to the concept of ‘jobbing’, Sutlive and Appell demonstrate their misunderstanding of what it comprises (2018:20). It is a thoroughly professional approach in addressing social and cultural issues from a multidisciplinary perspective and in explaining the relationship between theory and practice and in coming to terms with policy-oriented, applied research (King, 1994, 2009). It eschews grand theory and deploys concepts which are appropriate for ‘the job in hand’. It is a term that was coined in the early 1990s by other social scientists, engaged, as they were at that time, in applied anthropology (Parker and Baldwin, 1992; Barnett and Blaikie, 1994). It should not be confused with synthesis, though Sutlive and Appell seem to confound the two academic practices (2018: 20).

Conclusion

In the debate on the interpretation of Iban ritual textiles, which is taking on the character of a *longue durée*, several devices have been used to argue that there is one view only, which is that of Michael Heppell, in casting light on the complexity of Iban textile motifs and patterns and the significance of ritual cloths. Sutlive and Appell have recently entered the debate in claiming authority, and, also in suggesting that the 2017 *BKI* paper tries ‘to expose Freeman as inflexibly dogmatic’ (2018: 21). Contrary to this mistaken interpretation the paper considers the

implications of Freeman's move from what he perceived to be the strictures of British anthropology of the 1950s and 1960s to the development of an 'interactionist paradigm', which was a courageous and demanding move in his intellectual transition. In addressing the charge that Freeman was 'inflexibly dogmatic' Sutlive and Appell then decide to wander off into Freeman's early work on cognation and the kindred, expressed among other publications, in his chapter in George P. Murdock's edited book *Social Structure in Southeast Asia* (1960), and the debates which arose from this (2018: 21-22). These publications are not given a date in their narrative, but they appear in the Bibliography, though it is not Murdock's book which is referenced but his introduction to the book. A tangential excursion into issues of bilateral kinship adds no clarity to the debate on Iban ritual textiles.

Heppell's characterizations of the case studies in the 2017 *BKI* paper, examining the style of discourses in which Freeman engaged as 'a chain of incidents' or 'alleged abuses' have been translated by Sutlive and Appell into an 'unguided tour through the ethnography of the Iban' which is 'logically inarticulate' (2018: 21). This view seems to be derived from the perspective of 'scientific ethnography', the dislike of synthesis, the misunderstanding of the concept of 'jobbing', and a failure to acknowledge that there are different modes of intellectual engagement, some more appropriate than others. Primarily, the *BKI* 2017 paper was not about the ethnographic 'facts', about bias, or misrepresentation, but the ways in which Freeman defended his interpretation of the 'facts' and addressed the interpretations of others, and then adopted a style of argument. It was an examination of the language used in discourse and what this said about the modes of defending a domain or terrain (in an intellectual sense). If the argument is misunderstood then the conclusion is that it might be argued to be 'logically inarticulate'. Further, the 'unguided ethnographic tour' is seen to be 'theoretically and factually flawed' (ibid.) Nevertheless, if, as they conclude, 'a jobbing social scientist' is not wedded to 'any particular theory' and operates with low-level concepts, the argument cannot, by definition, be 'theoretically flawed', unless the flaw is that there is no theory. In regard to factual ethnographic inaccuracies, the 'facts' are reported as others conveyed and interpreted them, and judgements have been made about those interpretations in relation to their modes of discourse. The main factual flaw is apparently that Freeman is charged with 'anthropological imperialism', in defending Iban territory and his proprietorial rights over the Iban. In fact, the observation is that he adopts a particular style of language in engaging with those with whom he disagrees (as does Heppell, and now Sutlive and Appell). Freeman is not criticized as territorial in the sense in which Sutlive, Appell and Heppell suppose.

There is a more sombre reflection in the concluding comments. The tone of Sutlive's and Appell's letter strikes a rather disappointing note; a desire for a continued fieldwork, participant-observation-based, long-term, cross-cultural kind of anthropology conducted by the lone anthropologist working in the vernacular; it still exists in anthropological research and is one mode, among others, but it has been increasingly replaced by other modes; a perspective on symbolism which appears essentialist (every motif or symbol must represent or 'mean' something); a harking back to issues of cognation and the kindred which no longer energizes us; a positivist view of oral traditions and genealogies 'as history' which was questioned as far back as the 1960s and 1970s by Vansina and others; a 'scientific ethnography' which appears to take no account of the movement in anthropology towards reflexivity, the contemplation of 'fieldwork and the self', admitting error, and recognizing the problematical position of Western anthropologists studying 'the other'.

Finally, there is a need to address a major issue which preoccupies Sutlive, Appell and Heppell in the notion of a 'lineage of authority'. It is clear that Heppell, in his interpretation of Iban ritual textiles, owed much to his supervisor; he claims Freeman's authority and endorsement in his analysis and his authoritative criticism of dissident voices. In Freeman's engagement with Rousseau's paper, Freeman also asserts that the then Department of Anthropology at the Research School of Pacific Studies, where he was a senior member, is 'a leading centre of Iban studies' (1981: v, 1; King, 2017: 91). But there is recognition that the 'lineage of authority' needs to be replaced in light of the intervention of Sutlive, Appell and Sather, in support of Heppell, in this academic engagement. A more appropriate rendering and one which is in the spirit of Borneo kinship organization (with the acknowledgement that kinship relations can be 'fictive') is that we have now to confront 'a kindred of authority'.

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