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Freeman and the Abuse of Authority

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Abstract

Victor King has written a ‘case study’ about an alleged attempt by Derek Freeman to claim authority over anthropological research among the Iban of Sarawak to the eventual detriment of the study of Iban textiles. This article examines the evidence King marshals to prosecute his case and considers his views about evolutionary explanations and the logical coherence of the conceptualization by Iban women of their textile art.

Keywords

Iban – Freeman – authority – evolutionary anthropology – sexual selection – women's studies – textiles

1 Introduction

In his article ‘Claiming authority’ (King 2017), Professor Victor King sets out to establish a chain of actions that has resulted in the work of a student he supervised, Traude Gavin, being wrongly challenged about her interpretation of a once central occupation of Iban women: their textile art. The links of the concatenated chain started with an imposition by Freeman of an ‘authority [on Iban studies] in his analyses of Iban religion and social organisation’; to the active exclusion by him of students from engaging in Iban studies; to the establishment by him of a lineage of authority among the students he supervised, principally myself, to maintain this authority; to his students applying his ‘interactionist paradigm’ to Iban culture; to my application of the paradigm to studies of Iban textiles; and, finally, to my application of this arrogated authority to attack Gavin and her interpretation of Iban tex-

tile art. Like all such concatenated chains, any weakness in one of the links casts doubts on the strength of the overall chain. This article will examine each link.

King's article focuses wholly on the Iban. A reader with little to no interest in the Iban might wonder how the issues raised might be of any interest whatsoever to themselves or to anthropology in general. Three issues in this article stand out for those with an interest that goes beyond Borneo and the Iban, and in anthropology in general.

The question of authority and its abuses is always pertinent in a discipline like anthropology, where often a scholar is uniquely placed because no one else has a similar experience of a particular group. King starts his article with the observation that 'anthropological authority, or the command of a field of cultural studies, has come into question in recent years, and doubts have been raised about field data based on participation observation' as well as about the accuracy of anthropological findings of 'foreign cultures'. Such comments prepare a reader for failures in Iban ethnography. King's 'case study' of Freeman's purported anthropological authority over the Iban and its continuation implies that this imposition was not in the best interests of the study of the Iban, nor of anthropology in general.

The Iban are well studied. Nevertheless, the challenge for King, who has not studied the Iban, is to present a case that offers incontrovertible evidence that Freeman did undertake such a venture and that it was malign in the sense that it distorted or misrepresented aspects of Iban culture or did harm to other anthropologists. The evidence could be circumstantial, in that Freeman's ethnographic data possessed error; it could be based on the work of other anthropologists who have studied the Iban and exposed Freeman's ethnographic error; or it could demonstrate that identified anthropologists have suffered from this imposition. Given that there are now a number of PhDs by native Iban, all but one of whom were not supervised by Freeman, the question of authority becomes particularly pertinent as his influence would have extended to them and, presumably, they did not study a 'foreign culture'.

King gives no clear indication of what he considers constitutes authority, good or bad. Such an indication would be helpful to guard against the accusation that his 'case study' is an exercise in one authority claiming greater authority than another. In pursuit of his case, King (2017:84) presents a list of attributes a person requires to be able to claim authority, including language fluency and time in the field, which can be measured, and a number of other 'attributes', such as level of commitment, work, intensity, and endurance, which cannot. Freeman meets all the measurable criteria and, in the context of the Iban, King

meets none. Had King established a benchmark for himself in the context of the group he studied, the Maloh, a general reader could assess his credentials in prosecuting his case and gain some idea of what he meant by expressions such as intensity and endurance.

A second question of general interest inadvertently raised by King is whether or not evolutionary theories have any place in modern anthropology. Given there is a journal titled *Evolutionary Anthropology*, they clearly do. What that place might be seems to be questionable in King's view. In his article, King raises questions about the falsification—and, perhaps, verification—of the conclusions of such approaches, and these need to be considered.

A third question concerns a neglected area of woman's studies. For most anthropologists, mention of the word 'textiles' induces a yawn and is of little interest. However, in a great many societies in the world, textiles were associated with marriage. In these societies, textiles were produced by women. Households set out to ensure that the textiles comprising marriage gifts were of the highest quality. Why, then, was there such an emphasis on quality? One answer may be that the quality of the textiles reflected important information about the household or extended family concerned. This naturally leads to the question of in what way was that information important?

The Iban present an excellent case study to address this question. Textiles were a central feature of Iban life. No ceremony or ritual could be held without them. They were the principal medium for initiating communication with the deities. Textiles were essential to success because success could not be achieved without the support of the deities. Thus, women held the key: there was nothing produced by men that had such importance. Men had ritualized the requirement for textiles. That raises questions as to how and why textiles might have come to have such an important role in a society, and how women were able to exploit the situation.

King's article takes the reader straight into controversy, which has always been a feature of anthropology, especially where Freeman is concerned. Controversy is important because it can result in inaccuracy being purged and issues being nuanced so that their presentation is more accurate. Borneo ethnography has had its fair share of controversy. For convenience, these controversies can be divided into two kinds. There are those in which a contentious idea or approach is presented that raises the hackles of other researchers because they consider it fundamentally flawed. The second kind occurs when a generalist researcher reinterprets the findings of an anthropologist who has worked with a particular people or, alternatively, reinterprets the findings of others who have also had no fieldwork experience of the people. King's case study presents examples of both kinds of controversy.

An example of the first kind of controversy is the idea, promulgated by Carl Hoffman, that hunters and gatherers in Borneo were not groups which had practised this adaptation from time immemorial, but were former agriculturalists who had adopted such a practice to take advantage of opportunities for trade in valuable forest products (Hoffman 1986). He raised the ire of many, including Brosius (1988) and Sellato (1988), both of whom had done extensive fieldwork with hunting and gathering groups. Brosius argued that Hoffman's 'misrepresentation of ethnographic reality [was] a serious concern'. Brosius and Sellato presented a wealth of ethnographic data and evidence of Hoffman's deficient methodology to support their case. They reasserted a baseline about hunter and gatherer communities in Borneo and Hoffmann's discordant noise was expurgated.

The Iban have held a particular fascination for people engaged in the second kind of controversy. An example was when Andrew Vayda and Ulla Wagner, neither of whom had done fieldwork with the Iban, associated Iban migrations and warfare with ecological pressures on land. King (1976:326), who had stayed for two weeks at an Iban longhouse just above Lubok Antu before proceeding to the Maloh in West Kalimantan, disputed their hypothesis and, to prosecute his case, introduced fanciful ethnographic facts, such as the idea that Iban unity outside the village was at best temporary and fluctuating (1976:311–2); that Iban migration was 'peaceful into uninhabited lands' (1976:314); and that Iban warfare did not extend to the Maloh because of their 'potential for resistance and defence' (1976:322–3). Individual Iban groups actually inhabited river drainages where unity was stable. Sandin (1967), among others, has shown that Iban migration was far from peaceful. It is surprising the Iban were fearful of the Maloh when the upper Batang Ai' engaged fearlessly with the Brookes for many decades. I collected examples of Iban engaging with the Maloh, defeating them, and gaining territory as a result. Conventional wisdom agrees with King that Vayda's and Wagner's hypothesis warranted challenging. There should be concerns, however, about King's ethnographic data, including what his unnamed sources were.

2 Authority, Freeman, and the Iban

Authority has received considerable attention in anthropology and is a vexed issue. When Freeman completed his work on the Iban in the 1950s, there would have been little debate about him being regarded as an authority on the Iban. Indeed, he was commissioned by the British government to study and write an authoritative report on the Iban, so that the incoming colonial authorit-

ies would have a basis of expert knowledge to guide them in the formulation of policies to govern the indigenous population of Sarawak. Freeman's ethnographic data are freely available to the public in the Australian National Library, Canberra, and the Tun Jugah Foundation, Kuching, a fact that does not seem consistent with the actions of a man bent on excluding others from his so-called domain. As far as I am aware, there have been no calls for a serious revision of Freeman's ethnography. Certainly, studies like Cramb's (2007) of long-settled areas have expanded the data on the Iban and their diversity and adaptability, but Freeman's work—*Iban agriculture* (1955), in Cramb's case—was their starting point.

Having produced his report on Iban agriculture, according to King (2017:107), 'Freeman became identified with the Iban and he defended his close bond with them by attempting to exclude others from his domain and develop a legacy or lineage of authority'.

The claim of a close bond was disputed by Freeman's wife, Monica, in an interview with biographer Peter Hempenstall (2017:248), in which she explained that Freeman had not produced a major work on Iban religion in part because he was never integrated as intimately into Iban society as he was into that of Samoa.

If Freeman attempted to exclude others from his alleged hegemony, one might expect King to produce a list of anthropologists who had suffered such a fate. Instead of disappointed students, King presents four 'incidents' as evidence of Freeman exerting his authority over Iban studies to the exclusion of others.

The first incident was an article Freeman (1968) wrote titled 'Thunder, blood and the nicknaming of God's creatures', in which he challenged an earlier paper written by an old foe, Rodney Needham, about the Penan, a hunting and gathering group in Sarawak and the Semang of Peninsular Malaya. This incident fails to provide any evidence because it had nothing to do with the Iban.

The second incident unfolded over the question of whether or not affines should be included among the Iban kindred. The question arose in a PhD thesis by John Smart (1971), which promoted the analytical importance of the conjugal pair in cognatic societies based on fieldwork with the Karagawan Isneg in northern Luzon. Like Smart, King (1976) contended that affinal kin should be included in a kindred; this was in marked contrast to Freeman, who excluded them in his prize-winning essay 'The concept of the kindred' (1961) and in his *Report on the Iban* (1970). My data show that on marriage, the kin of each partner were joined at that level and formed the kin of their children. The incoming kin were not included in the kin of the parents' generation. In an evolutionary context, the Iban concept is consistent with kin selection theory. Divorce

was high among the Iban, and its frequency would have made the operational inclusion of affines very difficult to manage due to the frequent changes in partnerships.

Freeman privately corresponded with Smart and later, in 1977, with King (2013a:18). Other anthropologists were unaware of this correspondence until King revealed it in 2013 in *Ngingit*, a little read, Iban-centric journal. Perversely, in his monograph on the Maloh, King (1985:24) wrote that he had applied a 'kindred concept', including both consanguines and affines, to his Maloh data. However, he found that doing so confused rather than clarified the characteristics of Maloh kinship and he therefore abandoned the idea.

This vignette is an example of the second kind of controversy alluded to earlier: an anthropologist without direct knowledge of the Iban made pronouncements about Iban ethnographic facts and was challenged (privately in this case). Freeman, not unreasonably, was concerned that accurate ethnographic facts were reported. The private correspondence would not have discouraged other scholars from embarking on studies of the Iban and was as likely to have encouraged Smart to study the Iban for himself as to discourage him from doing so.

The third incident concerned a review Freeman (1975) wrote of Erik Jensen's (1974) *The Iban and their religion*. Iban religion was the primary focus of Freeman's research in the Baleh. Freeman (1975:277) wrote that Jensen failed 'to comprehend the protean realities of the remarkable religion of the pagan Iban of Sarawak'.

For a comparable view of the lack of depth of Jensen's research, another anthropologist, Christine Padoch (1982:10) commented on his (Jensen 1966:7) summary of land use. She remarked: [the phrase] "having exhausted the natural resources of soil, game, and timber of one district [Iban migrate] to fresh jungle' is hardly plausible as a description of an area which has been continuously settled for over three hundred years", adding that the 'cultivation of hill rice among all Iban cannot be done as Freeman described it, nor among *any* as Jensen reported'.

It is doubtful that Jensen carried out any dedicated anthropological research on the Iban. He started working for the Anglican church in Sarawak and was perceived as a priest by the Iban, who called him Apai Ragum—Father Beard. Then followed full-time employment as a community development officer in the Lemanak and, finally, as an administrative officer based in the town of Simanggang. An example of his apparent lack of focus on research occurred early in his period in Sarawak, when Jensen (2013:113) was present at a *gawai kenyalang* (a major festival recognizing the achievements of a successful headhunter) in the Undup. Such a festival had a level of importance for the Iban

equivalent to Easter for a devout Christian. Jensen ignored this *gawai* and all other major ones in his *The Iban and their religion*.

According to King (2017:89–90), Freeman's review asserted that 'only Freeman had captured the "essential" character of traditional Iban culture'. Jensen's book of course was about Iban religion, not about traditional Iban culture, and that was what Freeman was writing about. King (2017:96) finds merit in Jensen's work. Given that Jensen managed to ignore just about every aspect of Iban religion other than the rituals directly associated with the rice cult, he did little to reveal anything of interest about the character of traditional Iban religion, let alone culture. Much has been published since on the various aspects of Iban religion not addressed by Jensen, which only further demonstrates the exiguousness of Jensen's work.¹ Freeman's review drew deserved attention to the complexity of Iban religion, in contrast to the belittling attention it was given by Jensen, whose initial 'mission' was to ensure that the Iban abandoned their religion.

King's fourth situation concerned a claim by Jérôme Rousseau (1980:59–60), whose fieldwork was with the Kayan, that Iban society was divided into three status levels: a 'prestige-seeking category', commoners, and slaves. Not surprisingly, he gave no Iban terms for the first two levels, as they did not exist. The Iban did have two quite distinct kinds of slave, both of which were called *ulun*. In contrast, the Kayan Rousseau studied had four named classes: ruling aristocrats (*maren*), 'lower aristocrats' (*hipuy*), commoners (*panyin*), and slaves (*dipen*). Status with the Iban was achieved; status with the Kayan was ascribed. There could not have been a more pronounced difference.

Freeman called the Iban 'egalitarian' and made that clear in a response to Rousseau (Freeman 1980). Being egalitarian did not mean that in life all Iban enjoyed the same circumstances, as essentially, under the *adat* (customary law), everyone was equal. That was reinforced at law where fines did not vary with status. In life, certain Iban achieved more than others, and such achievement determined his or her status. With men status was established by holding greater festivals in the *gawai burung* series (the series of festivals celebrating the successful headhunter), while women achieved status through their weaving. Opportunities to succeed were certainly greater for those born to high-status parents. But there were no institutional barriers, as there were for the Kayan, which prevented a person born into the poorest household from achieving the highest status. As Christine Helliwell (1995:360) remarked, Iban equality was one of opportunity rather than one of condition.

1 See, for instance, Graham 1987, Masing 1997, Sather 1977 and 2001, Sutlive 2012, and Uchibori 1978.

King (2017:92–3) finds merit in Rousseau's argument. He argues that occasional Iban leaders were ennobled by Malay sultans, and that the Mualang, an Ibanic group living in the Sekadau and Belitang areas of West Kalimantan that split from the main Iban group probably in the early fifteenth century, had classes. On King's first point, an ennoblement was personal and non-inheritable and did not result in a class of nobles being established among the Iban. In the case of the Mualang, Freeman was describing the Iban. The Mualang were as irrelevant to King's claim as was Freeman's earlier joust with Needham. This is a further example of the second kind of controversy.

These four incidents are the sum of King's case that Freeman exerted authority to exclude others from his domain, the first link in the concatenated chain. King's case might have been enhanced if there was error in Freeman's ethnography or interpretation. There was not. King's case depends on a fiction: that students were sufficiently scared that they avoided studying the Iban for fear that if they got anything wrong, they would be subject to robust criticism. Anthropologists generally seem confident in their arguments, as was demonstrated by Freeman's incursion into Margaret Mead's Samoan ethnography. Rather than frightening others off, he elicited equally robust ripostes. In Freeman's case, it led to the American Anthropological Association taking a vote deploring the recommendation of his book *Margaret Mead and Samoa* by the magazine *Science* 83.

With these four incidents, King acts more like a prosecutor in an adversarial judicial process, presenting his case and that alone; the more persuasive approach of *juge d'instruction* is eschewed. In a well-balanced case, Freeman's ethnography would have deserved some consideration.

Next, there is the question of the evidence King presents of Freeman actively discouraging a scholar from working with the Iban. King presents none. He omits any mention of the opposite behaviour by Freeman: encouragement. King himself has recorded that he experienced such encouragement. His first contact with Freeman started at the beginning of 1972, when he 'introduced' himself and explained that he hoped to undertake fieldwork among the West Kalimantan Iban. Freeman was 'extraordinarily encouraging, fulsome and helpful' in his replies, expressed the hope that King would undertake research with the Kapuas Iban, and expressed a wish to keep in touch concerning the progress of his research (King 2013a:15–6). King chose to do fieldwork with the Maloh and did not keep in touch.

There could, of course, be circumstantial evidence that Freeman discouraged others from studying the Iban, the most important of which would be the paucity of scholars who have worked with the Iban. Upwards of twenty doctoral studies of the Iban have now been made. Scholars including Robert

Cramb (2007:xix), Christine Padoch (1982:ix), and Traude Gavin (2003:x) have been effusive in their gratitude for the assistance Freeman gave them, despite their published work not being in accordance with the book of the Iban according to Freeman. The evidence is all on the opposite side of the ledger.

If King's claim about Freeman's determination to keep the Iban free of the unwanted is accurate, Freeman was supremely unsuccessful, as King (2013b:7) remarks:

It is also true that of all Borneo peoples it is the Iban, both in Sarawak and in West Kalimantan who have been the most extensively studied across a wide range of subjects and themes. After all only the Iban have a four-volume encyclopaedia devoted to them and several dictionaries of their language. Students of other Borneo societies most certainly view with enormous envy the considerable level of scholarly work and publications on the Iban.

A second link in the chain has thus proved defective.

King (2017:106), while admitting that many of Freeman's findings have stood the test of time, sums up a job well done by this part of his case study: '[S]ome of [Freeman's] observations and analyses in relation to Iban egalitarianism and cognatic kinship, for example, are subject to revision, qualification, and elaboration.' Certainly both are open to elaboration, as Freeman presented summations, but not to revision and qualification, especially in the directions that King proposes. King forgets he fruitlessly tried the path he now proposes with cognatic kin. Forcing classes on the traditional Iban system would be tantamount to what Hoffman proposed for hunters and gatherers. If indeed King were correct in this observation, it is surprising that no Iban researcher has seen fit to correct the record.

Early in his article, King (2017:85) states that there 'is a need to investigate who has the authority to interpret and present Iban culture, and why the authority to do so is claimed'. This is a question that can be posed for any society. With the Iban, the question is becoming increasingly complex. With the Internet, anyone interested in Iban culture can refer to a number of blogs intent on publishing information about their history, culture, and everyday affairs. Some might be based on sound ethnographic data. For example, there are two sets of typed transcriptions, one by a great Saribas bard called Mujah anak Mambang, and the second, of taped interviews, by Benedict Sandin, the whereabouts of which are no longer known. Given the quality of some of the material of some of these blogs, any number of them could be referencing this material. I was offered this material but declined, believing that it should remain in the

Sarawak Museum—though I did accept some carbon duplicates and therefore am fully aware of its quality. There are now many trained Iban anthropologists working in Iban studies, many with PhDs. Who, then, has the authority to interpret and present a culture like the Iban, as King seeks? There seem to be many possibilities. The crucial condition is that the data they present are accurate. And the problem for King's critique of Freeman's work in the four incidents he selected is that, as with his critique of Vayda and Wagner, in three his data were deficient and the fourth incident had nothing to do with the Iban.

Four years before his *Bijdragen* article, King (2013b:7) expressed a view of Freeman's work which contrasts markedly with the view expressed in his article under discussion:

In this connection, I think it can be argued that of all anthropological monographs on Borneo communities it has been (J.D.) Derek Freeman's very widely quoted *Report on the Iban* (1970) 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.

3 The Interactionist Paradigm

Freeman's interactionist paradigm must be mentioned because, according to King (2017:95), my books on Iban art (Heppell, Limbang and Enyan 2005) and on Ibanic weaving (Heppell 2014) sought to maintain Freeman's anthropological terrain and confirm the analytical value of his interactionist paradigm. In fact, the intention of the first one, at least, was to draw attention to a softer side of the Iban than the popular one of wild and successful headhunters. Neither book mentioned Freeman's interactionist paradigm nor, indeed, referred to it obliquely, which would seem to be a very careless omission if my intention was to confirm its value. That leads to a fundamental question: did Freeman ever formulate an interactionist paradigm rather than simply talk about one?

King (2017:86) explained Freeman's paradigm as 'focused on the complex interrelationships between biological, socio-cultural, and environmental factors, drawing on the fields of ethology, primatology, neuroscience, psychology, psychoanalysis and psychiatry, molecular biology, and evolutionary genetics'.

As Thomas Kuhn (1962) wrote: 'Paradigms provide scientists not only with a map but also with some of the directions essential for mapmaking'. King might

think what he describes constitutes a paradigm, but Kuhn surely would not. King's description is simply a catch-all of a number of disciplines.

Freeman talked a great deal to me about developing a new anthropological paradigm. He spent the last years of his life in vain pursuit of Margaret Mead and died without writing anything to demonstrate how such a paradigm would actually work. To my knowledge, he never completed the paradigm nor demonstrated a working paradigm as it applied to subjects such as Iban religion or Samoan sexuality. Thus, another link in King's chain seems unsound. If there is no paradigm, it can hardly be applied to the interpretation of Iban culture.

In contrast, John Bowlby's trilogy *Attachment and loss* (1969, 1973, 1980) demonstrated an evolutionary framework adapted to the social sciences. In it he addressed some of the 'how come' questions of attachment behaviour and its relationship with separation anxieties that have greatly advanced an understanding of these phenomena. He used an evolutionary/ethological model, which he applied effectively to psychology. More recently, and not long after Freeman's death, the American anthropologist Melvin Konner published his monumental *The evolution of childhood* (2010), which describes numerous paradigms and, as many other anthropologists have done, synthesizes the work of anthropologists and other scientists on the relationship of evolution to childhood development. This is an approach that Freeman would certainly have described as an interactionist paradigm. It demonstrates that such a paradigm is alive and well in anthropology. The idea of such a paradigm therefore hardly deserves to be impliedly targeted as somehow deeply or fundamentally flawed. After all, King (2017:97) professes to have 'some sympathy with elements of the interactionist paradigm', but clearly not with all.

4 Freeman's Lineage of Authority

Freeman's approach to establishing a lineage was somewhat Jesuitical. He targeted young and suggestible minds not yet initiated in fieldwork. Once trained, Freeman did not follow the transparent stratagem of ensuring that his students remained employed in academia, publishing suitable encomia and then, in turn, passing on the paradigm to yet another generation of successors to ensure the continuing success of the lineage. Unlike the Jesuits, he let all but one of the four students he supervised (one on Samoa) leave academia, and the remaining one, Moto Uchibori, published in Japanese.

A second student, James Masing, returned to Sarawak where, initially, he found it difficult to find employment as an anthropologist. He joined an opposition political party, was elected to state parliament, and rose to be the party's

leader. Perceiving that there was no great future in being on the state opposition benches and no doubt guided by the interactionist paradigm, he joined the government benches and has done spectacularly well ever since. Goro Hasegawa (2017:32–33), working on the Mujong, Masing's natal river, found that there 'people worship Dr. Masing as god'. Hasegawa (2017:36) revealed how the interactionist paradigm was projected in Masing's hands. The head of the longhouse in which Hasegawa worked confided to him that 'once he had received an order from Masing in a dream to perform a large *gawai*, and so he obeyed and did it'.

In my case, my reappearance on the Iban scene might appear serendipitous, but now we know it was not. I left academia immediately after submitting my dissertation, nine months before my scholarship terminated, for a job, manufactured by Australian National University academics (possibly including Freeman), managing an NGO. The NGO worked in Aboriginal affairs in Australia and had briefly employed Freeman as a consultant. Unlike Masing, I was inexpert at utilizing the interactionist paradigm. After about four years, I had antagonized the Australian government sufficiently for it to stop all funds to the organization and issue a directive to all Aboriginal organizations that they would lose all their government funding if they employed either the NGO or myself. I unsuccessfully tried to obtain an academic position in anthropology in Australia. After two years of unemployment I went undercover, returning to the safety of chartered accountancy, where dyadic pairs were always in unison and binary oppositions were anathema.

Twenty-two years later, in 1996, still working in accountancy, I was approached by an unscrupulous Dutch publisher who asked me to write a book on Iban textiles. He had approached some people in Kuching, who recommended me. By that time Gavin's book *The women's warpath* (1996) was available. I recommended Gavin as the authority. The Dutch publisher was adamant it should be me.

I did know a little about Iban textiles. When in Sarawak, an Iban divorcee called Enyan had started presenting me with a string of small textile gifts. She was from the Labuyan, so I never asked her if she was in cahoots with Freeman. Her final gift was a narrow, shortish *sungkit* loincloth, which, when I wore it, immediately made me aware of why women paid so much attention to bikini lines. The cloth had two pairs of copulating reptiles along its length. Bowled over, as no woman had ever woven a loincloth for me, I invited her back to Australia, where she spent about six months weaving for about eighteen hours a day and talking incessantly about weaving. Thus started an intensive course on the mysteries of Iban weaving. I suggested to my wife, Marguerite, that she take notes. Those copulating reptiles did mean something. Enyan, though, assured

me that it was not what I had flattered myself into thinking. After all, she was ten years my senior.

Accountancy did not give me access to a university and, therefore, to an academic library and journals. So, armed with the only two things that anthropology had given me: ethnocentric fieldnotes and an interactionist paradigm, I started to put together something on Iban art based in part on Iban oral literature. My data were buttressed by Freeman's and others' fieldnotes. I sent a draft to the Dutch publisher. He liked it and asked me to arrange, at his expense, a suite of photographs. Photographic transparencies of objects were taken, and they and the relevant slides were sent to the Dutch publisher. He then refused to pay the photographer's bill or to return the transparencies and slides to Australia. I was left with an unwanted draft book, no slides, and a financial obligation to a photographer.

The depths of the subterfuge that Freeman was prepared to go to, to get his interactionist paradigm in print, now became apparent. In Singapore, I was approached by a CIA operative interested in Dayak artefacts. I visited his house, and in a general conversation he mentioned the name of another Dutch publisher who might be interested in publishing a book on Dayak material culture. I got in touch with him and he expressed an interest.

The book still needed a conclusion. On a long Singapore Airlines international flight, a flight attendant dropped a copy of the *Economist* on my lap. In it was a glowing review of a book by evolutionary psychologist Geoffrey Miller (2000), which discussed sexual selection in the context of art. The fuse was lit. The dormant interactionist paradigm stirred and, in one of only two references to Miller in the book's conclusion (the other was about the appeal of symmetry), I wrote (2005:166): 'In this regard [the oral literature insisting warriors wed accomplished weavers and vice versa], the Iban strongly illustrate Geoffrey Miller's thesis in his book *The Mating Mind* that the basic mechanism for the evolution of art was sexual selection.'

Just before the book was to be published, the publisher became aware of Gavin's *Iban ritual textiles* and asked me to comment to differentiate *Iban art* from Gavin's work. The body of the text could not be altered at such a late date. Footnotes were the only avenue.

Freeman and I managed my working undercover carefully. In the twenty years between my leaving Canberra and Freeman's death, we corresponded just twice: once with my asking him to comment on a draft of *Iban art* and the other when Freeman wrote to me to tell me that he had not long to live and I replied. I also met him once in 1994. I was in Canberra marking time before a meeting with a Commonwealth government department. I went into a public toilet. Another person was present: it was Freeman. I greeted him and asked how he

was. He replied: 'We are winning Mike. We are winning the war.' I replied how pleased I was and thought about suggesting we pop out and have a cup of coffee and a catch up. But Freeman was finished. He zipped himself up and headed for the exit. On his way, he half turned and repeated: 'We are winning, Mike, we are winning.' The federal police monitoring the public urinal's CCTV probably noted the complicit use of the word 'we'. The consorting males, patriarch and anointed successor, passed like two ships in the night, unaware that their dark secret might now be fatally compromised. That was the last time I saw Freeman.

Hempenstall (2017:258) writes that 'critics of Freeman, especially in Australia, point out that Freeman founded no school of anthropological training, no pool of students to pursue it'. He might agree that another link in King's chain was defective.

5 The Interactionist Paradigm at Work

As mentioned, King's idea of an interactionist paradigm was a catch-all. His critique about my so-called use of this chimera raises the question of whether or not evolutionary theory has any part to play in anthropology. His critique divests a proposed theory of its context. He focuses on successful warriors actually marrying good weavers and demands spurious proofs that are required to demonstrate that they do.

Because of their complexity and the richness of remembered information, Iban textiles offer opportunities to propose answers to 'how come' questions such as how their textiles came to be so ubiquitous (their beginnings were simple and crude) and why and how their production was maintained over centuries, when no other indigenous group in Borneo developed such a range. In fact, very few inland groups wove at all and most contented themselves with bark cloth clothing until about the middle of the twentieth century. That simple covering was all a male Iban wore for his daily activities. Textiles were a luxury. There was absolutely no basic need to make them.

A quick summary of the development of textiles by the Iban is that they progressed from small pieces of slit tapestry not requiring a loom, to *sunokit* (supplementary brocaded weft), and then to *pilih* (supplementary floating weft) and *ikat* (tie and dye). Five principles, ignored by King in his article, governed progression to the next stage. There were two core principles. Each new stage introduced a more efficient way of producing a cloth, but was accompanied by the establishment of an increased degree of difficulty, requiring the retention of the previous method and the introduction of intricacies in the new

method, the most conspicuous of which were ever more complex coils. There were two principles directly consequent on the change. The first two were that each change was incremental and facilitated the production of symmetry (the brain processes symmetry more quickly than asymmetry, which makes symmetry visually more appealing). The final principle, the ritualization of each previous stage, revealed an interest on the part of men in the textiles. Men must have decided that textiles were potentially important, and to ensure their production, they ritualized their use. For example, if an Iban wanted to be sure to communicate with the gods, he must wear a *sungkit* jacket. *Sungkit* cloths were required for receiving severed heads. This example of *sungkit* introduces more dissonance to this article. Gavin (2003:1), in her *Iban ritual textiles*, did not discuss *sungkit* because it was not 'as important as *ikat*'. In fact, historically, it was much more important.

A fundamental question, then, is why did Iban weavers introduce the various levels of difficulty and why did men establish a requirement for textiles in all religious rituals? For the women, who, like Iban men, were hugely competitive, the complexities ensured that the best weavers would shine. The introduction of ever easier technical stages extended the competition to most women. In this way, an agonistic arena was established. But that did not answer the 'how come' question, which required an explanation as to why, in the early origins of textile production, very small, slit-tapestry pieces (a sword belt and a small badge for a twined war jacket) were sufficiently attractive to warrant the interest of men and their intervention by ritualizing the use of textiles and, in so doing, ensure their continued production through the centuries. Not coincidentally, both pieces were designed for warfare. There also remained the question of the purpose of women competing through their weaving. Why would one sex compete amongst themselves if not to attract the attention of the other? The hypothesis, then, was that the driving force behind the continued development of weaving was sexual selection.

The ideal of successful warriors marrying excellent weavers was celebrated in verse. In one, a girl, impatient to be married, is told by her lover (Heppell et al; 2005:92)

I cannot take you back home my darling, Because my cutlass which should have tufts of hair hanging; Is but an empty sheath, with no locks of hair dangling, [...] And, besides, you are the same, my love so longstanding, [...] Nor in a cloth, have you woven a fine blue edging [...] As for a cloth dyed burgundy red, of crocodiles, With feet splayed and long snouts, it is yet to be in the making.

The Iban quite consciously linked head-taking and weaving to sexual selection.

Here we have the ideal of a warrior marrying a weaver, which King tries to make much of. But the ideal is a cultural construct; it has nothing to do with sexual selection. King required a 'proof' to demonstrate that the Iban did implement such marital arrangements. His demand that genealogical evidence reaching back through time must be presented to show such preferences, is sophistical. The proof is unscientific because, even if you could provide the evidence required, how many such marriages would have to be met to satisfy the proof? And then, how do you treat exceptions? The same argument applies. If there are many exceptions, do they falsify the theory?

King misrepresents the evolutionary argument that weaving provided information about a level of skill, possessed by individual women, which could not be faked, just as a man returning from a raid with a severed head could not fake its existence, which revealed information about the man. Sexual selection applied to this information implies that the information is of interest to the opposite sex and is used in mate selection. Human sexual selection is not based on one criterion, like an Argus pheasant's tail. Even with the pheasant there are likely to be many factors acting on the brain influencing a selection. As an Iban woman told Freeman (n.d.): a woman who spent all her time weaving was no good to anyone (*nadai guna sekali*). Human sexual selection will be many times more complex than that of another kind of vertebrate, such as a bird, and will involve a broad variety of factors and senses, including body symmetry and capacity for hard work, as well as other factors that still have to be identified. Weaving demonstrates hard work, but it also demonstrates a kind of intelligence. The latter might be significant.

Anthropologists have long had an interest in sexual selection. Much of their early interest relates to marriage forms. This interest has more recently been expanded by evolutionary anthropologists to include issues such as costly signals. A full understanding of human sexual selection will require the addition of many stones by scientists in many different disciplines to complete the mosaic. Anthropology is but one such discipline. And, in my case, one small fleck in the general mosaic is the potential of textiles. In contrast, sexual selection might provide an insight into the origins of textile art. Textile art's association with marriage provides an impetus. While that association is not universal, its frequency invites enquiry. Hence, there is an incentive for anthropologists to pose 'how come' questions about the frequency of textiles as marriage gifts.

This discussion relates to another link in King's chain: that, tarred with the same brush as Freeman, I applied the interactionist paradigm to the interpreta-

tion of Iban textiles. If there is a paradigm involved, it derives from Bowlby, not Freeman—though Freeman's hand was pronounced in posing the 'how come' questions about Iban behaviour. But, for King's chain, the link of an interactionist paradigm is a chimera.

6 Authority Damned

The final link in King's chain is that I have set out to impose my authority on the interpretation of Iban textiles and, thereby, to exclude Gavin from the subject. Worse, my authority is based on ethnocentric Western assumptions, also held by Freeman, about images in art which, as King (2017:83) remarks, 'can be traced back to the ethnocentric tendencies of writers from literate cultures in their search for meaning' and which are not based on rigorous ethnographic evidence of what the Iban say about these images. Compounding the ethnocentricity, King (2017:105) maintains that 'Heppell, like his mentor Freeman, has adopted Haddon's evolutionary assumptions that Iban woven cloths embody a "language of symbols"'.

King is concerned to promote Gavin's (2015:29–30) view that the symbols I refer to do not exist in the Iban weaver's world view: 'Heppell's main contention with my work seems to be that I do not use the word "symbol" in relation to Iban patterns. This is true and done with full intent, because, in the strict sense of the word, Iban patterns do not contain symbols.' King and Gavin make a lot of the idea of a 'language of symbols'. The phrase was not mentioned in *Iban art* (Heppell, Limbang anak Melaka and Enyan anak Usen 2005) and only mentioned once, *en passant*, in *The seductive warp thread* (Heppell 2014). But they were prominent in patterns or designs. The contrast therefore is great. Gavin's view has serious consequences for an understanding of Iban textile design and the creativity of Iban women, as Sandra Niessen (2009:95) points out: '[Gavin] describes what she calls a chaotic situation: while some patterns are faithfully copied, patterns are also "turned around; parts are incorporated into other patterns and given new names. Dissimilar patterns are ascribed the same name and so on"'.

Let us touch on the ethnocentricity. Freeman's fieldnotes describe two women explaining that a representation of food must always be provided to a water serpent pictogram lest the water serpent attacks and kills the weaver. My fieldnotes include an incident in which Enyan, weaving a *sungkit* figure, refused to go to an appointment because she had not completed the heart of the figure, explaining that the figure would be angry with her if she left it incomplete and might attack her on her return. These fieldnotes also correspond with

what the Indonesian Kobus Foundation (2000) reported about the Kantu' and Iban: 'every motif represents something from the human, the extraterrestrial, the animal and the plant worlds'.

King airbrushes out a number of Iban, who have written that Iban motifs are symbols, from his discussion: Jabu (1991a and b), Linggi (1998 and 2001), Kedit (1994, 2009, 2013, and 2017), Inggai (n.d.), and the Kobus Foundation (2000). No Iban has declared in writing that the motifs are not symbols. For Gavin, these airbrushed out Iban lacked authority. According to her, Jabu and Linggi were both educated, making them ethnocentric and 'raised far from a longhouse setting' (Gavin 1996:12). However, in reality both grew up in a traditional longhouse setting, with Jabu being adopted in the Layar, and Linggi growing up in her birthplace in the Julau. In Kedit's case, according to Gavin, his informants preserved the memory of their family's cloths with such certainty and often with astonishing detail, unmatched by any other Saribas Iban family, and Kedit himself provided improvised interpretations of the patterns (Gavin 2015:31–2). Kedit's informants were the last two living Saribas 'master' weavers who explained the cloths they had woven to him. Both were alive when Gavin was in the field, but were not interviewed by her.

In a recent article, Kedit analyses a number of *sungkit* motifs. One analysis states (2017:220):

[In] the representation of *antu gerasi berayah* [a demon performing a triumphant dance] [...] he is depicted with a small head. The small head shows the Iban belief that all decapitated beings in the afterlife, when invited to a *gawai* (festival), declined because they were embarrassed by their small heads. The sex of the demon is clearly indicated with the exaggerated phallus and a *palang* (penis-pin) protruding at the base. Its enlarged feet are curled to the motions of a dance. In the sagas, an *antu gerasi*, who was actually *Nising* the arch-enemy of *Singalang Burung* [the Iban Mars], was finally decapitated by the latter. This act of decapitation then takes on a profound meaning: a metaphor of the Iban triumphing over their enemies supernatural and natural.

Kedit's description gives lie to the idea of poor downtrodden Iban weavers who are living in a chaotic world in which every weaver is a Humpty Dumpty, giving any name she feels like to a motif or pattern she weaves.

King's last link raises an important question for anthropology: when indigenous people reveal something about their culture that has merit but contradicts what an anthropologist has reported, do other anthropologists rush to support the anthropologist concerned? Is the reputation of a member of the discipline more important than the ethnographic record?

7 Conclusion

King has presented a 'case study' of the alleged abuse of authority by Freeman in particular. The evidence presented by King against Freeman is tenuous. Not one link in the chain of alleged abuses that King presents stands up to scrutiny.

Freeman now carries a lot of baggage as a result of his jousting with American anthropology over Mead and cultural determinism. Freeman's respect for accurate ethnographic data, however, was exemplary. One of the tasks he set me in the field was to falsify his data. The only aspect I found to query was the concept of *kuasa* (authority) in the *bilek* (household), which Freeman had subsumed under his description of the *tuai bilek* (household head) but not elaborated.²

Authority seems to be an unfortunate concept to apply to anthropologists. So complex are the societies that they investigate that, inevitably, the good anthropologist will find that the more he or she discovers, the more the abyss of what they realize they do not know opens up. I ask the Iban questions; rarely is it the other way round. On the few occasions it is, I report what other Iban have told me. The ethnographic facts are ascendant and the anthropologist has a heavy responsibility to report them accurately. They are, after all, with the rapid social changes occurring, the flesh and blood of important social histories.

As a chartered accountant expecting both sides of a ledger to be balanced, King's one-sided account, which ignores the contribution that Freeman has made to Borneo and Iban studies, comes as a surprise. Even on irrelevant points, like, for example, the number of years in the field, the number of long-houses visited, the number of cloths documented to support a claim to authority, he provides the information for Gavin but no one else. What King has failed to do is to give any credence to a number of accounts produced by Iban that run counter to Gavin's findings. These accounts are consistent in their opposition to Gavin. It could be that within them resides the authority King seeks.

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² The Batang Ai' occupied long-settled land, so their agricultural system was quite different from the pioneering situation investigated by Freeman.

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