

**COMMENT ON DENHAM'S
*BEYOND FICTIONS OF CLOSURE IN AUSTRALIAN ABORIGINAL KINSHIP***

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COMMENT ON DENHAM'S
BEYOND FICTIONS OF CLOSURE IN AUSTRALIAN ABORIGINAL KINSHIP

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This is an important paper with far-reaching consequences for the analysis of Australian indigenous kinship and social organisation, and potentially for other small-scale societies. At its core are hypotheses about the relationship between societal exogamy and systematic age difference between marriage partners (husbands significantly older than wives), addressed through systematic network modeling and backed up with demographic evidence of the kind rarely examined for Australia. Based on this core, the paper extends to consideration of the reasons for the configuration found in Australia, which include avoidance of lethal inbreeding depression, which would otherwise afflict these small groups, and mitigation of the extreme ecological conditions found in many areas. This paper gives attention particularly to changes in kinship and allied systems (such as sections and subsections), which are claimed to be mechanisms for extending marriage alliances and thus increasing chances of group survival. It is a ground breaking attempt to unify various social and environmental factors, which unlike other approaches of this kind, is very specific about data and methods, and provides a way forward in terms of testable hypotheses.

The initial part of the paper is dominated by a critique of the notion of boundedness, especially supposed closure of marriage networks (endogamy) within tribes/language groups. I totally agree that this is a baseless notion in Australia, and has had a detrimental effect on some approaches to studies of Australian indigenous society. I also agree that it is an important step to come to grips with the reality of societies linked in wide networks in Australia, and to forge the theoretical tools to start operating in this larger domain – which is where, incidentally, Indigenous people have operated for a very long time. Denham uses earlier studies, along with those from his own work, to prove the point that there is societal exogamy, and to arrive at general levels it reaches (averaging around 15%).

There is also heavy emphasis in the paper, on the bio-genetic motivations of societal exogamy as being the avoidance of ‘inbreeding depression’. Over many years similar motivations have been highlighted by anthropologists, although some socio-cultural anthropologists have been less attracted to this idea. However later in the paper ecological motivations for exogamy are advanced– that pressures towards exogamy arise from a ‘food crisis’ which makes sharing of a range of resources in different territories desirable, even essential. How these two factors are reconciled, and the proposed link between mechanisms to promote exogamy and ‘firestick farming’ remain problematic. I return to this later in these comments.

The genetic question is linked to the variability of the presumptions in writers (and readers) about Aboriginal societies on the question of whether ‘cross-cousin marriage’ (Kariera), and similar regimes, mean marriage between actual first cousins or marriage with classificatory

cousins who may be distant genealogically or 'fictive' kin. In my experience many, particularly non-Australians, assume the former when reading sources like Radcliffe-Brown, while others (like Tindale cited by Denham) the latter. Denham does an admirable job of explaining that in fact cases lie along the continuum between the two. It is also the case that some societies prefer close, and others, distant marriage. This is discussed sketchily in the latter part of the paper but data has not been assembled to show how this correlates with other traits of the societies concerned, and is potentially another avenue for research along the lines Denham is advocating.

One of the departures in this paper from most of Denham's published work is that he is embarking on the task of transforming static network models into dynamic models of change, a move that I consider both necessary and welcome. However there are problems in his presentation of this part of the paper, both in terms of theory and data, which I examine below. I have been focusing on transitions in kinship and social categories in Australia for many years, with the bulk of the evidence drawn from historical linguistics. For me, and it seems for Denham as well, as least for some cases, there is an intimate connection between these two dimensions: the interaction between language groups, on the one hand, and the transition to new kin and skin systems. Beyond that there do seem to be differences in modeling. For instance I lay some stress on expansion and migration of groups, whereas Denham seems to be still in great measure anchored in the familiar 'immobilism' which has characterised Australian indigenous studies for so long. This leads him to downplay Sutton's 'pulsating heart' (based on migration) and propose a different kind of pulsation in which groups expand by extension of marriage alliances and contract, while remaining essentially static. It would be important to find empirical tests of which of these had been in play in different places and periods, and in which kinds of combinations, and to model the circumstances which motivate 'frame breaking' changes from oscillations in networks to migration. There are other studies which show how migration is the most likely explanation of a good number of language group expansions and typologise different types of language spread (McConvell 2010), drawing initial inspiration from Sutton's idea. Abandonment and recolonisation of the arid interior, which mean migration, are key issues, especially for Denham's main example areas the Western Desert and Central Australia, and have been thoroughly canvassed in Australianist archaeology in recent times.

Another aspect of Denham's theorising about change which is not well supported, is the notion that kin and skin systems move up and down a scale of complexity based on other factors, such as environmental change. His view is one of 'phase transition', a notion borrowed from physics, which describes a change from one state to another, such as a liquid to a gas, and is reversible. For instance he surmises that sections (4 skins) not only turn into subsections (8 skins) but the opposite also happens. In fact the former has happened (probably once only, followed by massive diffusions) and strong evidence has been presented for that (McConvell 1985, 1997). While some earlier works raise the possibility of a reverse in directionality, later enquiry does not support this with proven examples to any extent. There is one case (Eastern Kukaj) where 4 subsection terms were taken from the west and converted into a section terminology at the leading edge of the eastward diffusion of subsections (McConvell 1985). This is probably just diffusion of part of a terminology, rather than reversion to an earlier system. While I would not

wish to deny that reversion ever happens, or advocate strict adherence to a unilinear evolutionary sequence, my hypothesis would be that social categories overwhelmingly develop in the sequence of 4>8 (sections>subsections).

Denham's approach also implies oscillation of social categories in Australia over a very long period of many millennia, but there is unlikely to be evidence for this in the distant past. In contrast the evidence for subsection and section origin and spread is strong and detailed. In the case of subsections and the western sections studied by Dousset (2005), this is likely to be a unique and unprecedented series of events occurring over about the last two thousand years at most. For eastern sections the time-scale is likely to be somewhat longer but probably not more than the last three to four thousand years. Rather than searching for a long term oscillation of some other social or environmental parameter to explain the 'phase transitions' in social categories as envisaged by Denham, it would be better to focus on the late Holocene revolutions that we can and do know about, and explain them. Denham criticises me for not explaining why the circulating connubium which led to the origin of subsections occurred. This is a good point and especially important as circulating connubia are likely to be involved in the origin of sections in eastern Australia also (McConvell 2013). Such connubia are precisely a combination of language group exogamy with moiety exogamy with an asymmetric twist in marriage pattern, so follow quite clearly the kinds of changes Denham describes as occurring under demographic stress. The key question is not only why this occurs, but why it so infrequently led to a revolutionary change in the system.

The context of the origin of subsections in the Upper Daly region is one of a multi-society interaction rather than internal evolution in a single society, very much in tune with Denham's striving to move beyond closure. Nevertheless he does not seem to fully embrace the implications of this kind of mechanism of structural change occurring in the open intergroup domain.

The detailed scenario of marriage and filiation occurring around the origin of subsections may be speculative, but the location of the merger of the section systems in a particular way is not. This then has to be taken as a first step in a hypothesis about what happened in the diffusion of subsections into Central Australia, of which Denham attempts a reconstruction. This only partially takes into account my initial hypothesis (1985, 1997) or Koch's earlier paper (1997), which he is currently revising for publication (to appear). These latter papers are heavily indebted to historical linguistics, whereas Denham tackles it from a different, ethnological, angle. As noted also, Denham, unlike the others, entertains the idea of reversibility of the section to subsection transition. He also mentions that the spread of social categories, especially subsections, was stimulated by the genocide perpetrated on Aboriginal people following colonisation, but the chronology largely does not fit (only the latter part of subsection diffusion was in this period). These contributions (also including a hypothesis floated by Harvey 2008) potentially pave the way to understanding these changes better and developing an interdisciplinary methodology of regional social change.

Apart from the development of sections and subsections, another feature attributed by Denham to demographic stress, and the need to promote exogamy, is Omaha skewing (for instance the classification of some cross-cousins like Mother's Brother's Daughter, as 'mother' making them unmarriageable), found in a number of Australian Aboriginal societies. Omaha skewing also plays a catalytic role in the 'phase transition' between symmetrical and matrilineal marriage in Cape York Peninsula and North-East Arnhem Land (McConvell and Alpher 2002, McConvell and Keen, 2011 McConvell 2012). I take the position that Omaha skewing has resulted, in Australia and elsewhere, from, and assisted in, the encroaching spread of groups (also known as 'downstream spread'). Again my interpretation is dynamic and historical whereas that of Denham tends towards stasis and homeostasis, but the underlying properties of the model are similar.

An important aspect of the general analysis of marriage across Australia shown in the case of the Yolngu of North East Arnhem Land is the relationship of polygyny to other aspects of the social organisation, which is mentioned but not fully integrated in Denham's argument. Keen (1982) analyses why Yolngu men are highly polygynous, in contrast to many other groups such as their western neighbours the Gidjingali (Burarra). He links this feature to big 'gerontocratic' age difference between husbands and wives, among the Yolngu, significantly less among the Burarra; the overt asymmetrical marriage system and kin terminology of the Yolngu, and the symmetry of the Burarra system; and, flowing from the marriage asymmetry so heavily embedded in the ideology of the Yolngu as well as their practice, what Denham would call the long 'sibling-in-law chains' of the Yolngu. Keen, in keeping with the practice of most anthropologists ('fictive closure' In Denham's terms) talks about Yolngu marriage (i.e. between Yolngu) and does not mention the fact that there are significant numbers of marriages between Yolngu and neighbouring groups, including the Burarra. Over time the built-in asymmetry of this type of circulating connubium was probably responsible for expansion of the Yolngu block or 'nation', as my hypothesis predicts.

Beyond this is the question of the reason why these clusters of properties exist in certain areas. The groups practicing high levels of polygyny (Yolngu and others) are in areas of relatively rich resources quite close to the coast (Keen 2004); although not all such areas are highly polygynous. Denham in this paper however links high polygyny to high levels of stress on groups due to ecological 'hard times'. This seems to be contradicted by the kinds of evidence mentioned above, as well as being counter-intuitive. It is possible that men sought to contract marriages more widely as a form of insurance against the 'hard times' but the evidence is not presented, and if men did adopt this strategy it is unlikely to have been successful.

This ecological hypothesis sits uneasily together with the other ethno-genetic hypothesis (incest avoidance) in explaining motivations of various strategies for promoting exogamy. The two are not incompatible, but it would be good to hear which has greater weight, and what kind of variation exists. For instance if a geographically distant potential marriage partner (who could provide rights to distant resources) is relatively close genealogically, which condition wins out? There is variation in closeness of genealogical distance tolerated for a spouse in different places:

does this interact with geographical distance? This goes beyond what Denham engages with in this paper but his discussion provides a possible road to further research on such questions.

Despite these problems with the detail of the hypothesis, the idea is important that 'hard times' and a 'food crisis' lead historically to opening of networks including marriage, and on the opposite side of the coin, that abundance can lead to more restricted interaction. This is not exactly a new thought, but it is important that it is stated here clearly and with testable consequences. There are other contributions which jibe with this thought and not just in Australia (e.g. Jane Hill's 1996 work on 'localist' and 'distributed' stances linked to resource distribution in the south-west of North America). In my view an approach which links to work on other hunter-gatherer and small-scale groups may be a counterbalance to Denham's insistence that Australian Aboriginal people are in some (to me mysterious) way unique in their essence.

I find the attempt to link 'firestick farming' directly to changes in marriage regimes and opening of social networks less than convincing. It does not seem to mesh very well with the idea that the social effects are 'phase transitions' toggling back and forth over many millennia. If on the other hand these are major changes in the size of inter-group alliances occurring in the Holocene, as frequently proposed by archaeologists, it is difficult to match any indicator of 'firestick farming' increase with these. In fact the whole issue of prehistoric 'firestick farming' is now contentious, with little charcoal evidence for it at any period (Mooney et. al. 2011). Trying to make environmental degradation in Australia parallel to population pressure elsewhere is a big call and I can't see that much compelling evidence is offered.

In general though, Denham has a strong orientation towards the factual, and scientific approaches, including mathematical and statistical models, and is making links to other scientific disciplines which he sees as moving away from other 'fictions of closure'. We do clearly need an advance in this direction, which can enhance dialogue between socio-cultural and biological anthropology, archaeology and linguistics and other allied fields. The debates that might occur between these different approaches do show signs of generating methods of deciding between solutions based on empirical evidence.

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