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IN AUSTRALIAN ABORIGINAL KINSHIP”**

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Introduction

I thank Dousset, McConvell, and Sutton for their many constructive comments concerning Aboriginal Australia, for addressing important issues that I omitted, and for including valuable unpublished data, previously unknown to me, that significantly enrich this work. I thank Munt for her valuable historical comments on F.G.G. Rose’s important but largely ignored contributions to research on Aboriginal Australia. The scope of the comments is large, and writing a coherent response to them has been challenging. I hope that my responses do not misconstrue or misrepresent anyone’s comments, and that in trying to keep the response to a manageable length, I do not fail to address key issues.

Dousset on public policy

Dousset begins by addressing the public policy implications of openness and closure. He notes that kinship studies were largely abandoned by anthropologists in the 1970s but have re-emerged in Australia in the context and legislation of *Native Title*, where “the durability of boundedness, genealogies and kinship are central elements of the evidence Aboriginal peoples, and their consulting anthropologists, have to present during negotiations with the state or during court hearings.” Unfortunately, consulting anthropologists working in that applied context often take for granted some of the theoretical models, including the ideas of closure, exclusion and boundedness that contributed to the partial abandonment of kinship studies forty years ago. After arguing that “systemic repetition of *tribal inbreeding* was not viable and, furthermore, in fact was not the strategy Aboriginal groups adopted,” Dousset presents data from the Western Desert to support his position that “diversification of alliances and thus marriage partners in social, spatial and genealogical terms seems to have been far more the norm than the exception.”

Challenging the *Native Title* process is indeed one of the largest problems raised here. That was not one of my objectives in writing the paper and I deliberately left it implicit in my discussion of data and theory. But once raised, it cannot be avoided. As an outsider who is out of touch with current Australian politics, I do not know how best to approach the problems Dousset describes. But I am confident that replacing policies based on *terra nullius* (by that name or some other) with policies based on the equally inappropriate concept of closure may not have been the best way to proceed. Both extremes unsuccessfully attempt to force Aboriginal concepts and practices into a colonial European mold. Perhaps arguments presented in the paper - and especially in the comments - will contribute to solving the problems to which Dousset refers.

Dousset, Sutton and McConvell on unpublished data, public policy and theory

Ngaatjatjarra kinship and marriage. Dousset's discussion of kinship terminologies and marriage strategies among the Ngaatjatjarra and other Western Desert groups offers brilliant insights into the intricate combination of a) standard Australian Aboriginal Dravidian-like kinship systems and b) the diverse ways in which people can navigate around the constraints of such systems to achieve generational and societal openness. With Dousset's kind approval, I have reorganized and summarized his material to make it fit better with my own comments.

Ngaatjatjarra kinship terminologies. Dousset identifies a basic set of Dravidian-like kinship terms and rules plus three sets of special rules used optionally to reclassify kin. The universality of Aboriginal kinship means that "kin" may include all Aboriginal people.

- The *default option* is a variation on standard Australian Aboriginal kinship, employing basic Dravidian-like kinship terminology with a bifurcate-merging rule, a cross-parallel distinction, a preference for classificatory cross-cousin marriage, etc. It is always in place but is invoked to classify kin only when discussing actual marriages of actual people. It is compatible with endogamous reciprocal exchange marriages (e.g., bilateral sibling exchanges), but in fact only 8 (3%) of 283 known marriages feature reciprocal exchanges and all of them are exogamous between classificatory kin from different dialectal groups.
- The *same generation option*, precluding marriage between cross-cousins who grow up together, appears to be a classic example of the Westermarck (1891) hypothesis in operation. Cross-cousins who grow up apart are marriageable, but those who grow up together are "made of the same substance and have the same heritage", and are perceived in the same way. They are too close, too identical, to be cross-cousins and potential spouses, so they are reclassified as unmarriageable siblings; i.e., as parallel cousins.
- The *adjacent generation option*, used in some ritual contexts, classifies all members of Ego's parental generation as M and F, and disregards terms for FZ and MB, thus neutralizing the cross-parallel distinction and making classificatory siblings of all members of each level in the generation moieties. The logic used here, if followed consistently, seems to be a global, multi-generational, society-wide implementation of the much narrower logic used in the *same generation option*.

- The *MB option*, invoked to signify that a particular cross-cousin CAN marry ego, may be a significant variation on, or reversal of, the common use of Omaha skewing to signify that a particular woman CANNOT marry ego. This option determines whether ego should refer to a specific classificatory MB by the default term for “uncle” (*kamuru*) or by the special term for “father-in-law” (*waputju*). If ego should refer to that MB as *waputju*, the man is thereby designated as the person who will initiate ego and contribute a proper or classificatory daughter as the cross-cousin that ego will marry. The man designated as both initiator and father-in-law must live in a geographically distant community, if possible outside ego’s dialectal group, and be genealogically unrelated to ego.

Among the Ngaatjatjarra, *same generation*, *adjacent generation* and *MB options* all modify the spacing or distance between cross-cousins. Likewise among the Alyawarra, a male ego can refer to a proper or classificatory female 1st cross-cousin with three different but equally legitimate kinship terms: a) MBD, a close matrilateral 1st cross-cousin and optional spouse, b) MMBDD = W, a more-distant matrilateral 2nd cross-cousin and preferred spouse, or c) M, a most-distant person whom he cannot marry due to the optional use of Omaha skewing. The details are different, but the principles are similar.

Ngaatjatjarra marriage strategies. Dousset says that the explicit objective in acquiring a spouse in the Western Desert is to diversify one’s network of affines as far as possible. In addition to rules built into the kinship system, ancillary rules say, “never marry a person with whom you are already connected; never repeat marriages between people who already stand in a close relationship; never marry actual brothers-in-law and sisters-in-law: they are too close to become spouses (therefore do not engage in direct or reciprocal exchange).” Thus men generally marry women from distant geographical communities and distinct genealogical branches.

Dousset outlines four strategies for acquiring spouses, all of whom should be marriageable cross-cousins. The first two options yield ritually promised wives, the last two yield “actual” wives.

- In *pikarta*, the *waputju* identified in the *MB option* (above) promises a proper or close classificatory daughter to the boy during the initiation ceremony.
- In *pampurlpa*, a classificatory wife of *waputju* stands in the position of mother-in-law to the boy and promises her daughter - born or unborn - to the boy.
- In *karlkurnu*, men obtain wives through bride service or payment of a bride price.
- In *warngirnu*, men obtain wives through elopement.

After initiation, a man has two ritually promised wives whom he may or may not marry, and he may obtain other wives through bride price and elopement. But his relationship with his ritually promised wives and their close families, as well as the rights and obligations that stem from that relationship, remain in effect regardless of whether they become actual wives.

Thus a man's marriage network extends throughout the region as Dousset illustrates with Figures and Maps.

Ngaatjatjarra people have developed an ideology and a complex mechanism of kin term options and marriage rules that promote a diversification of relationships with strong exogamy, together yielding openness rather than endogamy, repetition and boundedness.

Note that Dousset's discussion focuses exclusively on kinship terms and marriage rules, and does not deal with skin terms. That is a separate issue, saved for another time.

I am most grateful to Dousset for taking this opportunity to publish his valuable data and his theoretical and public policy contributions to future discussions of generational and societal openness in Aboriginal Australia. And I am pleased with his discussion of the AustKin and AustKinII projects as works in progress.

Nyikina and Mangarla linguistic exogamy. Sutton's initial comment on my paper dealt broadly with the percentage of exogamous marriages reported by Birdsell and Tindale from their 1953-54 fieldwork. Sutton says, "Tindale's (1953b:182) calculations leading him to a continental percentage of 14% for 'extra-tribal marriages' were based on a list of marriages that contained a very large number of language groups for which only one or a very few marriages had been recorded. In fact, for 36 of Tindale's 102 'tribes' – 35% of the total – only a single marriage was used for his analysis." In other words, their published data was defective, perhaps for legitimate reasons related to field conditions, but nonetheless defective.

Indeed I was fully aware of this statistical issue and eliminated as much problematic data as I could. Specifically, I said, "I omitted societies labeled as having 'incomplete data' or showing a total of fewer than 10 cases, and recomputed all subtotals accordingly."

But Sutton's criticism turns into praise when he a) argues that my mean exogamy rate of about 15%, computed from Birdsell and Tindale's admittedly defective published data on exogamous marriages, probably is too low, and b) in the process introduces valuable unpublished datasets from Birdsell (1954), Sutton (1978) and Tindale (1953a, 1954) to which I had no access. Those data not only support his argument but also support mine. His conclusion, as I understand it, is that my argument would have been even stronger had I used his unpublished data.

Upon reading Dousset's report concerning unpublished Ngaatjatjarra data, Sutton submitted a cross-comment "intended mainly to enter onto the published record some further well-based statistics on rates of marriage inside and outside the linguistic identity group in Aboriginal Australia." Expanding on his earlier comment, he presents and briefly interprets several pages of unpublished tabular data collected by Birdsell and Tindale in 1953-54 from the Nyikina, a Nyul-Nyulan language group, and the adjacent Mangarla, a Pama-Nyungan language group, of the South Kimberley region.

From a respectable sample of 80 marriages, the statistics are remarkable. For example, $23/41=56\%$ of Nyikina marriages and $5/15=33\%$ (or $3/13=24\%$) of Mangarla marriages, all from the late 19th and early 20th centuries and reportedly unaffected by European contact, were language group exogamous. These unpublished data are strikingly different from Birdsell's published data that I used to prepare my paper; viz., Birdsell (1993:15) Table A-5, based on Birdsell and Tindale 1952-1954, which shows "The rate of intertribal precontact marriages" to be: Mangala 5.6% and Njikenka 0.0%. It is interesting that these two language groups come from a cluster of societies near the Kariera whose canonical diagram appears in Figure 1.1 of my paper.

Yolngu kinship and marriage. McConvell's brief but important discussion of Keen's (1982, 2004) reports concerning the Yolngu suggest that my decision to focus primarily on arid and semi-arid zones was perhaps unfortunate. Keen's Yolngu materials make several brief appearances in my recent paper (Denham 2012), where I point out that Keen "almost captures the age biased generational structure" depicted in Figure 3.4 of the present paper. However, McConvell goes further when he notes that "Keen, in keeping with the practice of most anthropologists ('fictive closure' in Denham's terms) talks about [marriage among the] Yolngu ... [but] does not mention the fact that there are significant numbers of marriages between Yolngu and neighbouring groups." In other words, the north coastal Yolngu engage in a circulating connubium that is age biased (i.e., perhaps helical) and engage in extensive societal exogamy "flowing from the marriage asymmetry so heavily embedded in the ideology of the Yolngu as well as their practice."

The data on language group exogamy introduced and discussed above by Dousset, Sutton and McConvell are valuable contributions to a generally under-reported aspect of Australian Aboriginal societies.

Genetic isolates and the pitfalls of jargon. Sutton says, "Denham appears to assign a closed-system view on cousin marriage to A.R. Radcliffe-Brown (1930-31)", then takes me to task for misunderstanding Radcliffe-Brown. I plead not guilty. My problem statement refers to Radcliffe-Brown three times, but it never assigns to him a closed-system view of cousin marriage, even though canonical Kariera and Aranda diagrams can be construed as implying such a view. In my opinion the problem lies with Birdsell, whose references to genetic isolates I misunderstood.

Read (p.c) says, "Denham refers to Birdsell's supposed 'fiction of endogamy'... as if Birdsell considered Australia to be made up of isolated subpopulations", but this "seems to be a misreading of what is meant by a genetic isolate." Read is correct on this important point.

As a cultural anthropologist, not a geneticist or demographer, I misread Birdsell's (1953) comment. He said he modelled Australia as an "idealized distribution of genetic isolates for use in gene flow models", which I read as "... an idealized distribution of genetic isolates that can be used in gene flow models ..." The difference is tiny, but I see now that "genetic-

isolates-for-use-in-gene-flow-models” is a technical term, a label, jargon whose nuances escaped me. *Mea culpa*.

Perhaps I am not unique in having misunderstood the significance of this usage. My suspicion that other non-specialists have made similar mistakes is strengthened by the fact that, among the many who have read my paper, Read alone corrected my error, and did it only in an endnote to his comment. Since Birdsell was an important figure, misreading of his work by non-specialists over the last 60 years may have helped to reinforce the presence of closure as a dominant theme in the study of Australian Aboriginal kinship. Fortunately, my argument does not rest solely upon my misunderstanding of Birdsell’s use of that expression.

McConvell on immobilism and conjectures

Immobilism. McConvell says, “Denham seems to be still in great measure anchored in the familiar ‘immobilism’ which has characterised Australian indigenous studies for so long.” In an earlier draft of my paper, in the subsection entitled “A different pulsating heart”, I contrasted a) Sutton’s “pulsating heart” based on expansion and migration of groups, with b) my proposed “pulsating heart” based on cyclical changes in permeability of boundaries, but mistakenly worded that proposal as if my pulsations were an alternative to Sutton’s. I corrected that error in the published version where I say “Building on Sutton’s ideas with which I fully agree, I suggest an additional kind of pulsation that might occur concurrently with or separately from his”, and expand upon that important notion in the remainder of my discussion of his work. Perhaps McConvell failed to notice the revision. In any event, I do indeed agree with Sutton’s argument (with which McConvell agrees as well) and reject the suggestion that I remain tied to immobilism. In fact, my entire argument advocates openness, not closure of which immobilism is an important symptom.

Conjectures and speculations. Several of McConvell’s comments, which I discuss separately in a moment, deal specifically with some of the more conjectural parts of my paper. As conjectures, they are necessarily vulnerable to criticism. But first an anecdote is in order.

When Atkins and I worked together in 1976-77 on our original “double helix” argument concerning Alyawarra kinship (Denham, McDaniel and Atkins 1979), he urged me to speculate more on factors that might have generated the exceptional geometry. But working at that time in a mode that some called “blind empiricism”, I demurred. So now that I have finally – perhaps belatedly - accepted his challenge and begun to publish my speculations, McConvell suggests that maybe I should speculate less. Perhaps I have gone too far, but at least I think I know more now than I did in 1976.

Reversibility of section to subsection transitions. I accept McConvell’s argument concerning the emergence of subsections using data from the Upper Daly region, but I reach beyond his historical reconstruction of that single event to investigate the general, scientifically analyzable, process of which that one important historical event is an example.

Is it indeed unidirectional and unique, or is the appearance of uniqueness based on data that is insufficient and devilishly difficult to acquire? I believe that McConvell's (2013) current investigation of the origin of sections in Queensland and Dousset's (2005) similar work in the Western Desert have the potential to move beyond particularistic reconstructions. I am eager to encourage that development.

Omaha skewing and the need to promote exogamy. In my opinion, explaining human behavior in terms of "needs" is almost certain to yield a circular argument, so I do not use the word in an explanatory sense.

Denham ... links high polygyny to high levels of stress. McConvell is not quite right on this matter. I do link increases in the frequency of polygyny to increases in levels of stress in the arid zone, and suggest that they may oscillate together as do other demographic and ecological features. But I use "high" in a relative sense, not as an absolute value. For example, in a region where monogamy predominates under some ecological conditions, a small increase in the absolute frequency of polygyny under changing conditions might make a significant difference; likewise, recall that an exogamy rate of 15% seemed to be a low absolute value to Birdsell but appears to be high enough to make a significant difference in a small-world network. As I noted at the very end of my paper, my decision to "focus primarily on arid and semi-arid zones" resulted in neglect of coastal regions where polygyny is more common; also, I was (and am) unwilling to re-open the "Murngin Controversy" at this time.

Gerontocracy. In response to McConvell's reference to "gerontocracy", I make the important point that Rose, by failing to notice the helical structure of age biased generations, portrayed gerontocracy as a disjunctive age difference between members of adjacent generations who "compete" for younger women, when it is better portrayed as a progressive or "rolling" age difference within the same helical generation between older brothers who have become eligible to marry younger women and younger brothers who are not yet eligible to marry anybody. In other words, the age difference, like so many aspects of Aboriginal life, is interpreted better in terms of cooperation rather than competition.

Trying to make environmental degradation in Australia parallel to population pressure elsewhere is a big call. Indeed McConvell is right. It is a big call, and I hope I got it right. Had I seen Gammage's (2011) *The Biggest Estate on Earth* before I submitted my final draft of the paper, or been able to obtain a copy of Smith's (April 2013) new *Archaeology of Australia's Deserts*, I probably would have worded the Ecological History section differently in some places. But that was not possible.

Insistence that Australian Aboriginal people are in some (to me mysterious) way unique in their essence McConvell raises at least two issues here. First is the problem of manageability: some have argued that the paper is too long as it is but it would have been a lot longer had I attempted to broaden its scope, so I did not. Second, I do think that Aboriginal Australia can be viewed as an exceptional "natural experiment" whose

dimensions are becoming clearer with rapid developments in climatology, paleontology, historical genetics and related disciplines, but I see nothing “mysterious” about it.

Munt and Sutton on historical matters

F.G.G. Rose. I am especially pleased that Munt included brief selections from reviews and correspondence concerning Rose’s (1960) Groote Eylandt monograph, and look forward to reading her biography of Rose.

The previously unpublished comments on Groote Eylandt kinship by Lee (1962), Lévi-Strauss (1958), Murdock (1958) and Montagu (1964) are intriguing since all of them are quite positive, but none of them seems to have led to significant innovations by their authors. In other words, Rose’s work did not fall on deaf ears. Perhaps the timing was wrong: important people heard and acknowledged his message but did not use it in their own research. In light of the opportunities lost, Elkin’s (1945) suppression of Rose’s monograph for fifteen years is especially troubling and reprehensible.

Rose’s Groote Eylandt volume has not yet produced the “revolution in our thinking about marriage patterns in primitive societies” that Lee (1962) predicted. Sutton’s suggestion that I should have taken the canonical Kariara and Aranda diagrams semantically - thereby glossing over their disregard of demography and genetics - points directly to one reason for the failure of Lee’s predicted revolution. Dousset says it well for Australia: kinship research that fell into oblivion in the 1970s rebounded by the 1990s, but retained flaws that contributed to its earlier failure, including the idea of social closure, exclusion and boundedness that was one of the reasons for the earlier abandonment of kinship studies.

Finally I am pleased with Munt’s comments as a historian because of the broad importance of her discipline to the study of Australian Aboriginal societies and cultures. In addition to the historical findings contributed to my paper by Flannery, Jones, Lourandos, McConvell, Munt and many others, Gammage’s Appendix 1 (2011:325-342) clearly demonstrates how historical methods and sources used by professional historians - not by scientists untrained in history - are essential for reconstructing the history of Aboriginal Australians.

Historical memory. Sutton says, “The attribution of long historical memory to traditional Aboriginal people is repeatedly contradicted by evidence from various eminent field scholars that traditionally these were a people who were profoundly ahistorical in outlook.”

Certainly I erred in generalizing too far in the offending paragraph, where I was thinking specifically of the Alyawarra but failed to note that limitation.

Nevertheless, on the basis of my own field experiences and the methods I used to elicit deep genealogies and related data, I suggest that there are good reasons to doubt traditional assertions concerning the “profoundly ahistorical outlook” of Aboriginal people. I suggest that nonlinear or cyclical time at the global level does not preclude paying attention to linear

segments of time – yesterday, today, tomorrow – at the local level. Perhaps I generalize excessively again, but the ability of the Alyawarra to reconstruct flawless genealogies 5 to 7 male generations deep, each male generation having a mean depth of 42 years, means the Alyawarra could traverse approximately 250 years with no difficulty. This feat, confirmed insofar as possible by Chalmers' family records at Macdonald Downs and by Northern Territory Administration censuses, is incompatible with their being truly ahistorical.

I agree that Aboriginal views of history are profoundly different from those of White Australians, and I agree that translating their historical knowledge into Standard English has only just begun. But the fact that eminent scholars have not yet “cracked the code” of Aboriginal notions of history does not mean that there is no code to crack. When I fail in that regard, I assume that the problem lies in an ethnocentric filter that prevents me from recognizing history when it does not match my own peculiar model of it. Until late in the 20th century, following Stanner's (1968) discussion of the “great Australian silence”, eminent scholars in diverse disciplines argued that Aboriginal people had no history at all, and certainly had no place in the history of Australia. As much as anything else, this paper is a contribution toward the scientific reconstruction of Aboriginal history, and the potential contributions of historians to doing that job are most important. I urge creative young Aboriginal scholars with open minds and alternative perspectives to share with all of us the indigenous Aboriginal history that eminent scholars of many ethnicities, disciplines and generations have failed to recognize.

Conclusion

In the broadest sense, my paper is about generally accepted theory, evidence from eminent scholars, preconceived notions, assumptions, ethnocentrism, received wisdom and self-evident truths all of which present profound problems in the history of science. On the one hand they are indispensable guides to our research when they are right, and on the other hand they are among its greatest obstacles when they are wrong. Rose's (1958) observation to Günter Gühr, which Munt quotes in her comment, is fitting here: “Are we not coming to conclusions that are logical enough on the basis of our assumptions but which, in fact, do not agree with reality and certainly do not measure up with what Aborigines know?”

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