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I am grateful to Dwight Read for spending many hours in email conversations with me concerning my paper. His probing questions and incisive comments helped me rethink much of my argument. Here I respond to challenges appearing in his initial unpublished Comment, our lengthy subsequent discussions, and his final published Comment. As a result of his misunderstanding of the implicit thought experiment upon which I based much of my argument, I have outlined it in some detail.

Part 1. Kinship

Data models and theory models. Read's probing begins at the very beginning of my paper, with fundamental questions about Figures 1.1, 1.2 and 1.3. In the past I have not used Read's (2008) concepts of "theory models" and "data models", but I use them here as an alternative way to state the problem that my paper addresses. He and I agree that there should be no conflict or contradiction between these two kinds of models; they simply do different kinds of jobs.

It is acceptable to begin a kinship analysis by using Figures 1.1 and 1.2 as representations of Radcliffe-Brown's canonical theory models, for they provide limited information concerning the structure of kinship terminologies. But using them as (even idealized) models of marriage in Aboriginal Australia fails because of several errors embedded in them; viz. a) they imply that marriages would be between spouses of approximately equal ages; b) they imply the presence of both generational and societal closure; c) they imply the use of European ideas of regulative law to determine what a marriage rule should be; and d) they present the marriage rule in a genealogically interpreted form that implies marriage with biological kin.

My Figure 1.3 in an attempt to up-grade Radcliffe-Brown's diagrams by eliminating some of the problems with Figures 1.1 and 1.2. In Figure 1.3, I retain features of his diagrams that are applicable to kinship terminologies, and replace features that are not applicable to marriages. Thus Figure 1.3 is a data model based on field observations rather than a theory model based on kinship terminology, and I construct it with data from the Alyawarra of Central Australia.

First, I begin with the simple expedient of introducing accurately measured parent-child generation intervals to replace the arbitrary and fictitious intervals that were standardized at a value of zero in the 19th century when accurate intervals were unknown and the implications of differences in intervals were unrecognized. Specifically, Figures 1.1 and 1.2 disregard generation intervals, while Figure 1.3 is based on a mean Mother-Child generation interval of

28 years, a mean Father-Child generation interval of 42 years and a resulting mean Wife<Husband age difference of 14 years. Age differences of this magnitude appear to be typical of Australian Aboriginal societies, but the fact that the generation intervals are unequal is more important than either their absolute values or the magnitude of their inequality. In the context of preferential cross-cousin marriage, which I retain, the presence of these unequal parent-child generation intervals means that it is impossible to sustain systematic bilateral sibling exchange marriage, and matrilateral (MBD, MMBDD) cross-cousin marriage replaces it. As a result, horizontally closed generations in endogamous societies as shown in Figures 1.1 and 1.2 are replaced by diagonally open generations in exogamous societies as shown in Figure 1.3.

Second, Read's analysis of the kinship terminology (Leaf and Read 2012) leads away from the traditional understanding of the marriage rule as a European-style regulative law dictating prescriptive cross-cousin marriage, and toward an Aboriginal-style Ancestral constitutive law that specifies what kinds of marriages, using their kin terms, are consistent with the logic of the terminology. Here and below, where I discuss this matter in greater detail, I rely on Searle's (1995, 2007:88) distinction between regulative and constitutive law.[1]

Third, marriages occur at variable rates between biological and close-distant-remote classificatory kin. In Figures 1.1 and 1.2, the genealogically interpreted form of the traditional prescriptive marriage rule is seriously misleading when used in the context of a classificatory kinship terminology (Read 2007) that is applied universally to all Aboriginal Australians, and that co-occurs with skin terms that seem to be the principle medium for establishing inter-societal exogamy.

Although the theory models in Figures 1.1 and 1.2 may accurately depict some kinship terminological aspects of Kariera and Aranda kinship systems, they either omit or misrepresent vital non-terminological aspects that are part of my data model in Figure 1.3; i.e., since the theory models are not isomorphic with my data model, they fail to explain it.

Relations between kin terms, skin terms and marriage rules. To explore further Searle's (2007:88) distinction between constitutive and regulative rules as they may apply to Australian Aboriginal kinship and marriage, I ask whether the distinction is useful for interpreting the Alyawarra usage of the kinship term "anowadya", which is cognate with Northern Aranda "anua" and generally is glossed as "spouse".

[1] Searle (1995, 2007:88) says: " … we need to make a distinction between … "regulative rules" and "constitutive rules". Regulative rules regulate antecedently existing forms of behavior. A rule such as "drive on the right-hand side of the road" regulates driving, for example. But constitutive rules not only regulate, they also create the very possibility of, or define, new forms of behavior. An obvious example is the rules of chess. Chess rules do not just regulate the playing of chess, but rather, playing chess is constituted by acting according to the rules in a certain sort of way. Constitutive rules typically have the form: "X counts as Y", or "X counts as Y in context C". Such and such counts as a legal move of a knight in chess, such and such a position counts as checkmate, … and so on."

First, a strictly regulative interpretation prescribes marriage of a man with a woman to whom he would refer as "anowadya" both before and after their marriage. This woman might be his proper double 2nd cross-cousin (MMBDD and FFZDD), but a more relaxed interpretation accepts someone who stands in an equivalent relationship, such as a classificatory double 2nd cross-cousin or a proper or classificatory single 2nd cross-cousin. (Remember that in the Northern Aranda subsection system, 1st and 2nd cross cousins are in different subsections, whereas in the Alyawarra section system they are in the same section but in different implicit subsections.)

Second, a constitutive interpretation says that a man who marries a woman to whom he properly refers as "anowadya" before their marriage thereby complies with the logic of the kinship terminology, and explicitly depicting the genealogical or classificatory linkages that justify his use of that term is not required. Thus a constitutive interpretation may circumvent the genealogical nature of Figures 1.1 and 1.2.

The first and second options place primary importance on the kinship term that the man uses for the woman *before* their marriage. Thus, since kinship terms generally vary across languages and dialects, these two options implicitly assume that the marriage is language group endogamous.

A third interpretation, still applying to language group endogamous marriages, violates both of the earlier rules. It says that when a man marries a woman who is his proper or classificatory 1st cross cousin, who is an entirely acceptable alternative to a 2nd cross cousin – not a "wrong marriage" in any sense - he refers to her as "anowadya" after their marriage even though that was not the term he used before their marriage. Among the Alyawarra in 1971, this interpretation was a common occurrence, and its effects could ripple outward to the spouse's primary kin.

A fourth interpretation applies to language group exogamous marriages and is linked with skin terms rather than kin terms; viz, a man marries a woman of a different language group on the basis of her membership in a specific section or subsection and generation level even though he may never have actually referred to her, in either her language or his, by any kinship term before marriage; he then refers to her as "anowadya" after their marriage since that is the proper reference term for a proper wife.

In the third and fourth interpretations, "anowadya" is used as an affinal term *after* the marriage but not as a precondition *for* the marriage.

A fifth option applies in the case of a "wrong marriage"; i.e., a marriage between people who before marriage stood in a relationship that violated a rule of moiety, section (and perhaps subsection) exogamy, and for that reason may be treated as incestuous. Using "anowadya" in this context may be tolerated informally, but it does not serve as the basis for systematically reconfiguring a person's universal kin relations.

Thus Searle's distinction may be useful for understanding "anowadya" in the context of theory models of societally endogamous marriages, but may be less useful in other contexts.

Origin and dynamics of kin terms and skin terms. Multiple attempts to account for the emergence and dynamics of these two closely related kinds of kinship terminologies have appeared through the decades. Some efforts assume the primacy of skin terms, others assume the primacy of kin terms. As Read notes, "Unambiguous historical evidence regarding the first appearance of the section and subsection systems is not yet available for helping resolve the matter".

Jolly and Rose's (1943) effort, based on Morgan's (1871) earlier speculations concerning group marriage and the primacy of moieties, is no longer convincing. McConvell's (1985) account of the emergence of a specific 8-subsection system from a specific pair of 4-section systems in Northern Australia is based on historical linguistic evidence for the merger of two societies with circulating connubia. His is a convincing special case but its possible contribution to a general explanation remains unclear. Dousset (2005) offers a convincing reconstruction of the logic of historical changes in 4-section systems in the Western Desert, but says nothing about factors controlling the emergence and dynamics of these systems, or their relations to kin terms. Allen's (2007) effort to understand the emergence of possibly related South Asian (Dravidian) kin terminologies likewise assumes the primacy of moiety and section terms.

On the other hand, as Read shows in his Comment, Leaf and Read (2012) assume the primacy of kin terms. Their demonstration of "the way the structure of the four-section system emerges from marriages consistent with the logic of the [Kariera] kinship terminology" suggests that the "four-section system is not so much an invention as [a matter of] identifying and naming the parts of an emergent structure", but their persuasive general explanation seems not to connect with McConvell's special case. Read and I agree that kin terms and skin terms are "coordinate parsings of the same conceptual universe" and that "structural changes between systems with an even number of sections will be through multiples of two: a two-section system can be changed into a four section system by introducing a generation moiety based on odd versus even generations, and a four section system can be changed into an 8-section system by introducing a criterion that divides each section into two parts." Despite my agreement with Read on some points, my own speculations concerning the conditions that might control the emergence and dynamics of 2-moiety, 4-section and 8-subsection systems do not address relations with kin terms. Furthermore no works cited here deal with Ambrym 6-section systems (Lane and Lane 1958) or Shang Chinese 10-section systems (Cooper, et al. 1983).

This is a large, complex and fascinating set of problems, and solutions remain elusive.

Structural differences between Kariera, Dravidian, Polynesian and Iroquois classificatory kinship terminologies. In my paper, I said "I cannot solve problems [concerning kinship terminologies] ... that lie beyond the scope of this paper", but "examine"

would have been a better word than "solve". Yet my infelicitous word choice elicited Read's excellent 5-page summary of basic issues in the study of a) classificatory terminologies, b) similarities and differences between closely related Kariera, Dravidian and Polynesian terminologies, and c) the structural difference between Kariera and Iroquois terminologies. I have spent years being mystified and baffled by unreadable accounts of all of these topics written in artificial languages by specialists who seem to talk only to each other. I suggest that many people, including other anthropologists who are not experts in leading edge research on kinship terminologies, are reluctant to struggle with a multitude of formal methods and artificial languages (Kronenfeld 2000) to process materials that all too often do not repay the effort. Here Read presents a challenging, short, lucid and fully professional account of contemporary studies of an important family of kinship terminologies, and I thank him for that. There is a great deal of merit in recent efforts to foster the use of Plain Language (Plain Language 2011) and Plain English (Plain English Manual 2003).

Part 2. Thought experiment

My overall argument is based on a thought experiment that I deliberately left implicit in my paper. Since I can answer a number of questions raised about the paper only by reference to the thought experiment, I must make it explicit here.

Thought experiments, or *Gedankenexperimenten*, following in the tradition of Plato, Galileo, Mach, Einstein, Schrödinger and many others, facilitate thinking that is designed to allow us to explain, predict and control events without physically engaging in experimental research. Since cultural and social anthropology are not experimental sciences, and long term experiments in human evolutionary biology are impossible to design and conduct, I chose not to describe my argument in unfamiliar experimental terms even though it is based soundly on experimentation. Here I describe and execute my experiment in a narrative format, as an exercise in reasoning rather than as a numerical simulation, and add some numerical values provided by Read.

Dimensions. Throughout the paper, I seek mechanisms that will build and strengthen hypothetically tiny, intensely inbred populations when they face centuries of slowly declining resources that may lead to their extinction. I am looking for mechanisms that enhance their survivability in hard times rather than suppress their populations in rich times. Thus the thrust of my experiment is diametrically opposed to the long tradition in anthropology of seeking mechanisms that keep populations stable in rich times.

My objective in this experiment is to design an UTTERLY UNREAL situation to serve as the foil, the limiting case, the background against which to develop my argument. Quite simply, I want to determine what would become of an experimental population of 600 isolated societies such as those depicted in Figures 1.1 and 1.2 if they were allowed to function totally on their own, under the following conditions, for 50,000 years.

a. **Habitats**. In my thought experiment, the societies are totally isolated from each other in habitats with finite carrying capacities and absolutely sealed boundaries; i.e., the experiment begins under "test tube" conditions defined by *ceteris paribus*, but each habitat is subject to a full range of environmental stochasticity so we can see what effects *environmental stochasticity* would have on the survival of human societies if anyone chose to interpret Figures 1.1 and 1.2 literally.
b. **Population sizes.** The experiment begins with 600 isolated human populations (societies) of mean size 500 corresponding to Birdsell's "magic number" of 500 which is treated as the upper bound for the population size so we can see what effects *demographic stochasticity* within those tiny populations would have on the survival of human societies if anyone chose to interpret Figures 1.1 and 1.2 literally.
c. **Inbreeding coefficients.** *Inbreeding coefficients* in the experiment are measured against an absolutely literal interpretation of Figures 1.1 and 1.2 so we can see what effects extreme inbreeding depression would have on the survival of human societies if anyone chose to interpret Figures 1.1 and 1.2 literally.
d. **Birth spacing.** This mechanism adjusts population size to match resource availability as recommended by Read (Read + LeBlanc 2003).
e. **Duration.** The duration of my experiment is 50ky, thereby spanning the entire time since humans arrived in Australia.

What will be the pattern of survival of these populations at the end of 50ky?

MVP. Computing MVP on the basis of a great many assumptions may be more art than science, so the following flurry of computations should be treated as suggestive rather than definitive. Gently massaging uncertain numbers with the most benign of intentions can have significant impacts on not-so-robust results.

Primack (1993:291ff) defines Minimum Viable Population (MVP) as, "the smallest isolated population having a 99% chance of remaining extant for 1000 years despite the foreseeable effects of demographic, environmental, and genetic stochasticity, and natural catastrophes". In setting up my experiment, I used 50ky instead of Primack's 1ky, and that makes a difference. Under the conditions of my thought experiment, each society is as totally isolated from all others as are the species in Traill's meta-analysis; i.e., there is NO recruitment across societal boundaries, no fission and no fusion.

Traill et al. (2007:Table 2) conduct a meta-analysis of data spanning 30 years, showing: a) for 95 species of mammals a median standardized MVP of 3876; b) for 182 vertebrate species a median standardized MVP of 4102; and c) for all 212 species "a cross-species frequency distribution of MVP with a median of 4169 individuals (95% CI=3577-5129)". They conclude that "… the MVP for most species will *exceed a few thousand individuals"* (my italics). Since MVP for a single species is a *minimum* not a mean, and the magic number 500 is a *mean* not a minimum, it follows that populations below the larger minimum, not the smaller mean, would be expected to go extinct.

Read, citing sometimes ambiguous ethnographic examples to the contrary, suggests in his Comments and in our exchanges that I should be more conservative in my comparisons. Read uses a 20% trimmed mean rather than a median and obtains for Traill's data a trimmed mean for the standardized MVP of 944 for all primates and 1163 for the great apes. For isolated human populations, Read finds that several hundred persons have survived at least 400 years, but whether they would have survived for 1000 years is unknown. These data suggest that the n = 500 value for my test tube experiment is below the value needed for 99% certainty of surviving at least 1000 years. Read says that a 99% figure for surviving 1000 years pretty much guarantees that the metapopulation would still be intact after 50 ky, but that result drops off rapidly as the probability of surviving 1,000 years decreases. The value of n = 500 is right at the edge of the drop off, thus uncertainty in the value of MVP for humans means uncertainty in estimating the probability of surviving 1000 years by a group of 500, and that translates into substantial uncertainty about what might happen over a period of 50,000 years.

I conclude that, under the conditions imposed by Figures 1.1. and 1.2, it is unlikely but not inconceivable that some societies in my thought experiment would reach the end of the 50 ky period with viable populations.

Inbreeding coefficients. My purely hypothetical concern with Figures 1.1 and 1.2 has nothing to do with 1st cousins that are so popular in studies of Australian Aboriginal societies. Rather I am concerned with the systematic, repetitive nature of mating within the structures of those Figures over a period of 1190 male generations of 42 years each, a total of 50 ky, as implied by the reiterative nature of the diagrams. For illustrative purposes in the thought experiment, consider only 10 generations which is but a tiny fragment of the 1190 generations that span 50 ky. In Sutton's words, if we "take Figures 1.1-1.2 literally", the inbreeding coefficients for the youngest of only ten generations would be as follows: Kariera = 0.5078; Aranda = 0.2451; Alyawarra = 0.2430 (computed by FSpeed). Thus after *only 10 generations*, the hypothetical Kariera kinship coefficient would be more than twice that of parent-child and sibling incest, while Aranda and Alyawarra kinship coefficients would almost equal those of parent-child and sibling incest and would be twice those of double 1st cousins. In my test tube situation, these numbers would continue to increase albeit more slowly for the remaining 1180 generations, assuming that anybody would survive and successfully reproduce after almost 1200 generations under the specified conditions. Unless the stringent inbreeding successfully purged the recessives quickly and completely, the utterly abnormal conditions of the thought experiment would yield inbreeding coefficients lying far beyond those to which Bittles and Neel (1994:120) referred when they said that consanguineous marriages are not undesirable. Read, on the basis of his own computations, reached the same conclusions. The degree of inbreeding generated by Figures 1.1 and 1.2, if interpreted literally, is lethal by any reasonable standards.

On the one hand the *inbreeding depression* generated by those Figures extinguishes the tiny isolated populations. On the other hand the *small population sizes* and *nonporous boundaries* generated by those Figures also work together to extinguish the populations. But it is not an

either-or situation. In fact, in my experiment, both operate simultaneously. It is a situation in which extinction is "overdetermined"; i.e., the single observed effect (viz., extinction) is determined by two or more causes at once (viz., population size below MVP and intense inbreeding depression, both of them operating in the context of extreme environmental stochasticity over a period of 50ky), either one of which alone might account for the effect; i.e., if inbreeding depression does not drive you to extinction, then MVP will do it for you. Thus more evidence is available than is necessary to justify the conclusion. In other words, *all* or virtually all of the societies in my experiment would – and should - be extinct at the end of the experiment as a direct result of the artificial conditions of MVP and inbreeding depression imposed together by Figures 1.1 and 1.2. If Aboriginal people behaved the way those Figures imply, they would have vanished long ago. Since taking these Figures literally leads to the extinction of the societies to which they pertain, it may be worthwhile to think of them as "figures of speech".

Beyond fictions. In Sections 2 and 3 of my paper, I discuss mating systems or reproductive strategies that systematically reduce the kinds of closure generated by Figures 1.1 and 1.2. The extinction of all experimental populations as a result of the absolute closure of Radcliffe-Brown's canonical diagrams sets the stage for me to introduce, one by one, a great many options based on kinship *behavior* that Australian Aboriginal people can and do use to achieve openness. My objective there is to very cautiously peel away the needlessly imposed constraints that make Figure 1.1 and 1.2 nonviable, while at the same time retaining as much as possible of the structure and terminology that characterize Australian Aboriginal kinship based on the constitutive Law of the Dreamtime.

I did not toss out cross-cousin marriage, but rather I "opened" the generations to advance MBD marriage and retard FZD marriage, thus replacing double-cousin marriage that is implied by Figure 1.1 with a less constrictive single-cousin marriage. I did not toss out marriage with kin, but rather I introduced marriage with classificatory kin as an alternative to marriage with "proper" kin that is implied by Figure 1.1. I did not abolish societal endogamy, but I opened the boundaries to permit systematic societal exogamy as well. And so on through several such transformations. The end result is that on the surface the kinship terminologies (i.e., kin terms and skin terms) look exactly the way they always have, but the marriage rules have been adjusted – made vastly more realistic - so that following them will not destroy the societies.

Part 3. Regulation of population size.

At the end of his section entitled Metapopulations and Isolated Populations, Read refers to the 4-part speculative scenario that I introduce on page 64 to demonstrate how Australian Aboriginal societies could respond dynamically to changes in "stress levels" broadly defined. In that scenario I briefly refer to inbreeding depression as one of a large congeries of factors, including all of the reproductive strategies and mating patterns discussed in Section 2 and 3 that might participate in those dynamic responses. However, I do not even vaguely imply

that "inbreeding depression could be the means through which population sizes were stabilized". I have never made this argument, and in fact my extended discussion of inbreeding coefficients and inbreeding depression in the context of my thought experiment run directly counter to this idea. Read acknowledges this fact when he says that, "Though Denham does not make this argument, I will show, however, that inbreeding depression is not sufficiently large, under actual marriage patterns, to stabilize the population size under this feedback loop" (i.e., my 4-part scenario on page 64). In other words, he introduces the idea that inbreeding depression might stabilize population size, then demonstrates that the idea will not work. I welcome his argument, agree with it and fully support it.

After demonstrating that inbreeding depression is not sufficient to stabilize population size he then discusses "an alternative means by which the population size may have been stabilized, as a supplement to [Denham's] discussion (pp. 5-10, 31-51) of factors affecting population size of local groups." This discussion of carrying capacity and birth spacing is another supplementary argument that I welcome and support. I respond very briefly to some of Read's points.

Inbreeding depression. In a separate message to me, Read says, "The idea that increase and decrease in inbreeding due to changing marriage patterns in response to stress and non-stress conditions might act to stabilize population sizes is certainly a possibility; it's just that, as far as I can see, the amount of inbreeding depression under realistic marriage patterns, not the imaginary ones of Figures 1.1 and 1.2, is not sufficient." Since my 4-part scenario on page 64 is, for all practical purposes, an extension of my earlier thought experiment, I will use his point to refine my experiment.

Read says: "The historical marriage pattern measured by genetic pattern has not led, on average, to inbred Australian populations." That finding is in agreement with my prediction that inbreeding would be prevented by the various reproductive strategies and mating patterns I discuss in my paper.

Read says: "Substantially more marriages between close, biological kin would be required before inbreeding depression would act as a deterrent on population growth." The relative frequency of occurrence among the Alyawarra of endogamous marriages among proper kin and close-distant classificatory kin, and exogamous marriages between remote classificatory kin, together preclude significant inbreeding depression.

Read's comments on inbreeding generally focus on inbreeding depression while mine generally focus on inbreeding avoidance. Certainly they are two sides of the same coin, but there are significant differences between them.

Birth spacing. Originally my thought experiment did not contain birth spacing as a mechanism that adjusts population size to match resource availability, and I thank Read for suggesting the addition of it.

Read says: "The birth spacing model (Read and Leblanc 2003) both increases and decreases the population size without deliberateness on anyone's part." All too often, birth spacing has been used unidirectionally, in the sense of increased spacing to reduce the frequency and number of births thereby holding population size down. In my paper, I used birth spacing to denote both increases and decreases in spacing in two contexts: a) with regard to dynamic carrying capacities in Figure 4.2, and b) with regard to dynamic responses to changing environmental conditions on page 64. In retrospect, Read has convinced me that I should have said more about it, but what little I said about it fully agrees with Read's position. I agree that it can keep population down when resources are ample, but I also see it as an important mechanism – operating in the opposite direction – when population expansion is more valuable than population constriction.

Read says: "Denham relates difficulty in finding endogamous spouses to scarcity: 'hard times of scarcity would yield more open boundaries that would facilitate exogamous marriages when endogamous marriage partners might be scarce.' Thus, he assumes that endogamous spouses are rarer, hence harder to find in small, than large, populations. [But] there is no demographic reason to assume that endogamous marriage rates vary with environmental stress." Opening the boundaries to exogamous marriages has no significant effect on the availability of proper kin for marriage, but it may yield a significant increase in the number of classificatory kin in multiple societies who are potential spouses by way of section and generation memberships.

I appreciate Read's offer of birth spacing as "a concept that is more robust than inbreeding avoidance [and] might have informed my argument better" (p. 67). I do not think it replaces inbreeding avoidance, but it greatly enhances it.

In our extended discussions of extreme environmental conditions that might lead to catastrophic failure of food and water, I have been unable to reconcile the following: a) Read's frequent references to competition for land and resources ultimately combined with a more-or-less universal human proclivity for violence when all resources fail, with b) what I perceive as a profound Australian Aboriginal commitment to a traditional Dreamtime ethic expressed as "We take care of them" and manifested in cooperation combined with the virtual absence of competition and conflict over land and resources.

I do not suggest that Read is wrong in his cynicism or skepticism about human nature and its relationship to "nature red in tooth and claw", but I suspect that Aboriginal Australia may constitute the black swan that refutes – or at least softens - this hasty generalization that characterizes European thought. My original failure to cite Read and LeBlanc (2003) extensively was due to the distinct contrast between their emphasis on competition and my emphasis on cooperation as alternative approaches to understanding Australian Aboriginal societies. With Read's help, I have begun to see these approaches as complementary rather than as mutually exclusive.

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