

UNIVERSITY OF WASHINGTON  
SEATTLE, WASHINGTON 98195  
Department of Anthropology DH-05

23 March 1977

Dear Woody

I'm extremely pleased to have your manuscript draft on "Aranda Kinship...", which is a real mind-blower. It's as good as I'd been hoping and then some; you've got a real classic in the making, in my opinion. You've brought your extraordinary data to bear on the "standard" Aranda-system model in a splendidly effective way, showed clearly the need for a better or more refined model, and have made an interesting start (at least) on building that better model.

My keen interest in what you're doing here has several bases in my own history of kinship model-making. In 1962-64, when I was at the third institution at which I did graduate study (Pittsburgh), I (a) designed 3-D ball & stick models of various kinship structures, including a very effective version of the standard conception of the Aranda system, and (b) suggested to Barbara Lane that the Melanesian Ambrym system (on which her husband Bob had just published a paper) could be made intelligible if helical-type of staggering of generations caused by husband-wife age difference were introduced (in this case, the cycling was among "3 lines" instead of the "4 lines" needed for the so-called symmetric second-cross-cousin-marriage systems). Then, more recently, I have worked again on 3-D geometric models and produced a number involving helical cycling, usually a double helix. Stimulated by your manuscript, I am back at the drawing board with the excellent empirical justification you've provided egging me on: I'd like to see if one of my abstract inventions in kinship geometry turns out to be the model you're looking for (one looks quite promising). Please understand, however, that I'm not trying to "horn in" or preempt you in any way. I will simply pass along to you what I'm working on, when it's a little further along, for comparison with your own evolving model.

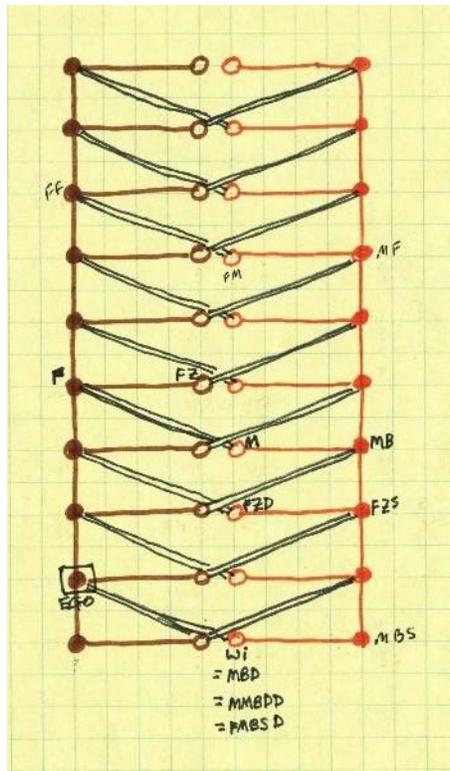
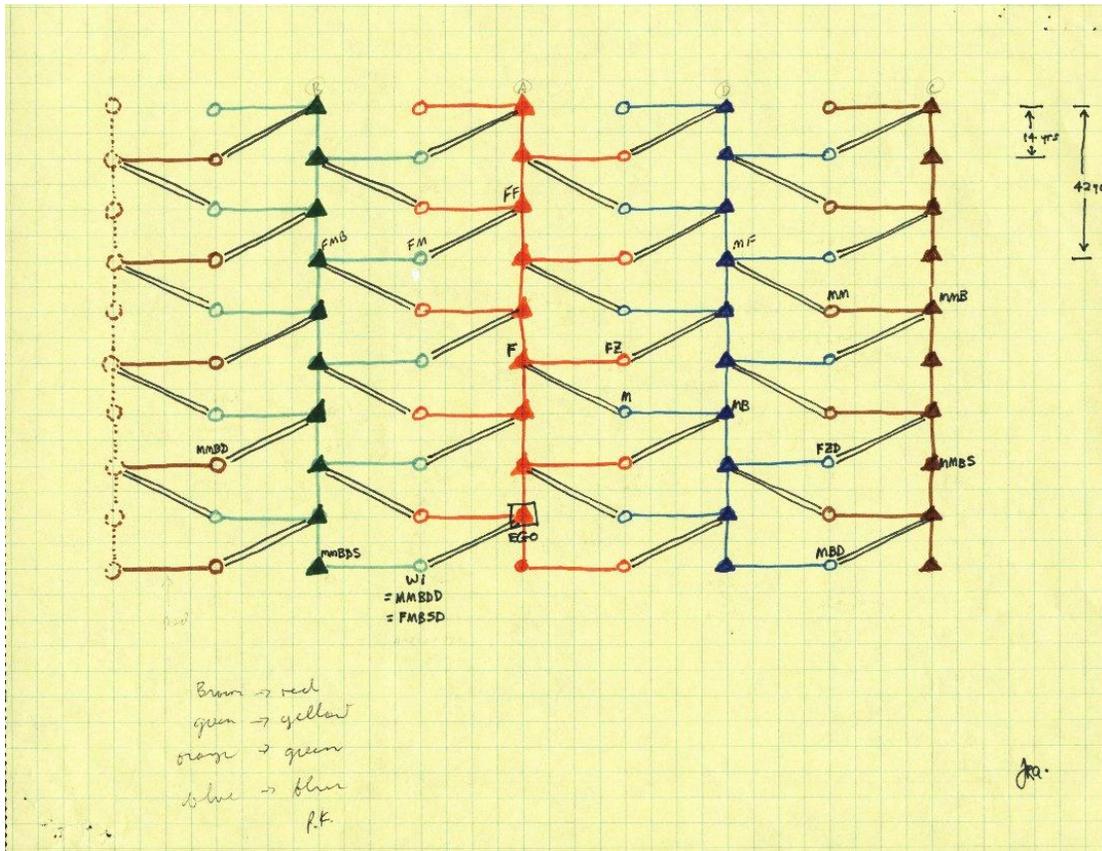
There is one difficulty with the simple cylindrical model of your Fig.12, as you may already realize. If each husband-to-wife link involves, on average, a drop of 1/3 of a father-to-son generation, the helix of {spouse-sibling-spouse...} links starting with me cannot pass through my son's son unless the me-to-him portion of the helix contains 3 or 6 marriage links (or other multiple of 3), which is inconsistent with a 4-patriline model (although it would be consistent with a 12-line model, which is certainly NOT what's wanted!). However, there are several ways out of this difficulty, including a more complete abandoning of the "standard" assumptions I'm still employing in some of the terms of this paragraph.

As for your group-composition data, only one thing has emerged with clarity in dealing with the sizes of sleeping and residence groupings, so far as I've gotten to date. Cohen shows for the primate sleeping-group data he used that the distribution of group sizes over all nights sampled could not be fit by a Poisson but could be fit by a negative binomial (truncated at zero in both cases). The negative binomial will NOT fit either your sleeping or residence group sizes. This in itself is a reportable finding, but I want to understand the situation better before writing up something for co-authorship with you on this. Your sizes do show a nice regularity of distribution, but I'm not sure what function fits the shape best yet, and there's more to be done. I'm looking at composition by age & sex also. Have not yet considered Cohen's LOST models.

The Chairman at McMaster phoned me to ask for a letter on your behalf; I got one right off to him. Hope you hear good news (like a plethora of attractive choices) before long.

Best regards,

JRA



UNIVERSITY OF WASHINGTON  
 SEATTLE, WASHINGTON 98195  
 Department of Anthropology DH-05

4 July 1977

Dear Woody,

Finally I am answering your interesting letter of 22 May (!) concerning the counting of coups, or in this instance sibling-exchange marriages.

For convenience below, I shall use 'sibling' and 'Sb' to mean 'real or classificatory sibling' throughout. Very different numerical results emerge depending on whether the counting is to be of sibling-exchange marriages (SXM's), exchange-married sibling pairs (XMSP's), or sibling-spouse exchanges (SSX's), with the latter interpretable either as the number of pairs of sibling-exchange marriages (PSXM's) or the number of pairs of exchange-married sibling pairs (PXMSP's).

It seems to me that the cleanest, simplest, and very possibly "best" operationalization is SXM, i.e., 'How many marriage relationships are also SbSpSb relationships?' or, more specifically, 'How many pairs of persons who are linked mutually as Sp are also linked mutually by at least one geneapath of the form SbSpSb?' This is an "existential" criterion, under which the existence of two or more distinguishable SbSpSb paths linking a given married pair does NOT increase the "degree" to which the pair is exchange-married. Note also that any given SbSpSb path is only counted once, not twice (not once from  $H_u$  to  $W_i$  and then once again from  $W_i$  to  $H_u$ ).

By this rather straightforward rule, your five diagrammed cases contain 2, 2, 3, 4, and 6 sibling-exchange marriages, respectively-i.e., all of the (yellow-coded) marriages shown qualify, and each is counted once. Of course, the number of SXM's, by this rule, may be either even or odd (as in case 3).

The text of your letter, however, suggests that you are inclined to count SSX's rather than SXM's. I've suggested above two different ways that the SSX idea might be interpreted (and there are more ways possible, not to be discussed here). In any case, resolution of these conceptual questions should take into account the "counting universes" within which the counting is to take on its meaning. For example, if one decides to count SSX's defined (say) as number of exchange-linked pairs of marital pairs (PSXM above), then the universe within which this number gets its meaning is the total number of pairs of marriages in your data: that is,  $(N(N-1))/2$ , given a total of  $N$  marriages. You would be asking, for every possible pair of marriages (in which at least one partner to each marriage is not a partner in the other marriage), whether the pair of couples in question is linked by exchange. By contrast, if one decides to count SXM's, as I am inclined to recommend, the universe is simply the total number of marriages,  $N$ , for each of which you would be asking whether the couple in question is or isn't linked also by at least one (mutual) SbSpSb path, i.e., an "exchange" path.

One factor giving you pause in your diagrams 4 and 5 is the nontransitivity of the sibling relation; i.e., it does not follow from  $aSb$  and  $bSc$  that  $aSc$ . In diagram 5, for example, there are only 22 sibling relationships shown in the zero generation (11 red and 11 black: 3 'own' and 8 'classificatory' in each case). Note also that (in this generation) there are 6 marriages;  $15 = (6 \text{ times } 5)/2$  pairs of distinct marriages; and  $231 = (22 \text{ times } 21)/2$  pairs of distinct sibling pairs (including red-red and black-black pairings as well as red-black).

Suppose now that the 12 zero-generation or "yellow-married" persons of diagram 5 constituted your entire population.

- 1) One could say that there are 6 marriages here, all of which are involved in an exchange (Sp linkage accompanied by a SbSpsb path);  $6/6 = 100\%$ .
- 2) Or that there are 22 distinct sibling pairs here, 16 of which are involved in an exchange (Sb linkage accompanied by a SpSbSp path);  $16/22 = 73\%$ .

- 3) Or that there are 15 distinct pairs of marriages here, 8 of which (8 pairs of pairs) are linked in the closed exchange type of loop (i.e., formed of 2 marital and 2 sibling links in the closed cycle Sb, Sp, Sb, Sp,...);  $8/15 = 53\%$ .
- 4) Or that there are 231 distinct pairs of distinct sibling pairs here, 8 of which (8 pairs of pairs) are involved in the exchange type of loop;  $8/231 = 3\%$ .

These are not the only possible ways to do "the" (?) counting, clearly, but it must be admitted that the results for just these four ways are marvelously varied. I vote for the first and simplest way-but I welcome hearing your views of the matter.

One of my reasons for preferring the simplest way is that there are further potential complexities beyond this particular counting task. E.g., one might well wish to ask: For all married individuals in the population, how many of all the persons they could have married (with the latter defined either WITH or WITHOUT attention to cultural rules) were their SbSpsb either at the time of the marriages) of the former or later? And on the basis of this information, do members of this population marry persons who are or will be SbSpsb with a frequency that is greater, less, or about equal to the frequency that would be expected by chance alone?

Of course all that I've said above would have to be changed if the actual interest were not in sibling-exchange marriages generally, but rather in the subclass of these that are sister-exchange marriages, in which Wi (as against Sp) is also SiHuSi (as against SbSpsb), and reciprocally.

I hope you'll find the above to be a useful beginning, at least, of the needed sorting out process ... it was done a bit hastily, and may contain errors, perhaps even fundamental ones. Let me know how it strikes you, and whether it helps.

Best regards always,

JRA

13 August 1977

Dear John,

Much to my chagrin, I'm still in Berkeley - waiting for my Canadian immigration papers. But I've been using the time reasonably well, I hope. Mainly, I've spent the last two weeks revising the article on Alyawara/Aranda kinship.

You'll notice that the title page shows you and Chad McDaniel as co-authors. Chad put a great deal of time into SOCSIM/KINPATH program development, and the results of his work were essential for the development of the paper. Likewise, the double-helix model that you suggested occupies the most prominent place in the article, and without it, I would not consider publishing the paper. I sincerely hope that you'll agree to appearing as co-author along with Chad and me, but if for any reason you would prefer that your name not appear there, please let me know, and I'll revise the title page and footnotes accordingly. Footnote 1 sets forth the division of labor among you, Chad, and me, and I think it might be reasonable to publish it that way, if that's agreeable with you.

I've been considering the idea of sending it to the *American Ethnologist*. Can you think of a better or more appropriate journal for it? If so, I'd appreciate the suggestion.

And, as usual, I would appreciate receiving any suggestions for revisions in content, organization, or style. I think it would be nice to get it into print as soon as possible, but I definitely want it to be right when it goes to press.

Regards,

Woody Denham

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 SEATTLE, WASHINGTON 98195  
 Department of Anthropology DH-05

27 August 1977

Dear Woody

I'm pleased that you found my model useful, and I accept co-authorship (with division of labor explained in a note, as you suggest) with the feeling of being honored by the invitation, for I think this paper of yours is going to be a little classic. And yes, I think *AE* would be a good place for it. The only other high-visibility placement that occurs to me as a possibility would be *Current Anthropology*.

The new draft is much better. There remain some parts that perhaps could be smoother or clearer, but at the moment I do not have time to be as specific as I'd like about that (and there's certainly nothing disastrously wrong with anything in this version, as I see it-unless there's some fatal flaw in my model none of us has noticed yet!).

I do have some thoughts for your consideration, however, which I'll list in random order.

1. "Figures" 6-11 are really Tables and ought to be numbered and labeled as such; they will look best and read most easily if set in type as Tables I-VI.

2. Legends, in some cases quite extensive, ought to accompany the Figures; in general, it is a good idea to make one's figures (and tables) as self-contained and informative as possible, on the model of standard practice in the journal *Science* (to take one good example). This can cut down on what needs to be said in the text, although of course some redundancy in the text is acceptable and even desirable.

3. Since Fig.12 is turning out to be crucial, I have been asking myself how to improve its perspicuity. First, I fear that the strange convention of going up three links in what appears to be an agnatic descent line to find Ego's father is going to throw some readers and irritate others. Secondly, I think the females should be moved into closer physical proximity with their brothers, not only to improve the helicity of the affinal helices but also to show the patrimoiety memberships of the women more clearly.

Hence I invite you to consider the enclosed rough sketches. Drawing I shows what might be called a "disaggregated" version of the model which I think should be found quite clear visually and conceptually. If this drawing were enlarged considerably and copied onto clear plastic or highly translucent paper, it can be rolled up (in one of two ways, see below) to form a triple-walled cylinder that makes an effective 3-D representation.

One of the ways it can be rolled up brings all of the "A" patrilineal lines into exact coincidence and all "B" lines into coincidence. This, however, recreates the problem mentioned first above, namely the oddity of what looks like Ego's F not in fact being so. The other way of rolling up the cylinder is to bring the "A" lines near each other but not coincident, and similarly with the "B" lines of course. Each patrimoiety is then represented by three distinct agnatic descent lines (within which F-S relationships are perfectly clear, as in the disaggregated or unrolled depiction) on one of two opposite sides of the cylinder.

Drawing IIa is a two-dimensional representation based on the idea of the cylinder rolled up in the second way.

Drawing IIb is based on the other or first way, except that vertical agnatic linkages are simply removed to prevent unwanted confusion in F-S relations. Each patrimoiety is shown simply as a vertical column of B-Z pairs (with the F of each B-Z pair contained in the third pair up but not explicitly shown as such except through kin-type labeling). Drawing III is the same without color but with fuller labeling. (I have added some labeling not in Fig.12, hopefully correctly; I don't know whether the additions are improvements or distractions. I mean the added kintypes.)

If you like these newer representations, the orange-black contrast in my sketches could be rendered as solid penlines that are respectively heavy (thick Leroy pen) and very light (very thin penpoint).

The dashed central vertical line (a) separates "A" moiety people from "B" people and (b) suggests the central axis of the implicit cylinder. The impression of helicity seems to be less without it.

4. Let WB be regarded as 'wife-giver,' 'woman-giver,' or simply 'giver,' and let ZH be regarded as '(woman-)taker.' Then from Ego's point of view ZHZH is grandtaker (and WBWB is grandgiver). This is a convenient terminology. In the model, Ego's great-grandtaker's great-grandtaker or T6 is FF, whereas his great-grandgiver's great-grandgiver or T-6 is SS. His grandtaker's grandtaker (T4) is MMB; his grandgiver's grandgiver (T-4) is ZDS. And so forth. Note particularly that Ego's grandgiver is Ego's FF's grandgiver's SS. The T2/T-2 or "grandaffinal" relations among men seem to be as special structurally as the alternating-generations agnatic or "grandagnatic" relations...in the model, anyway.

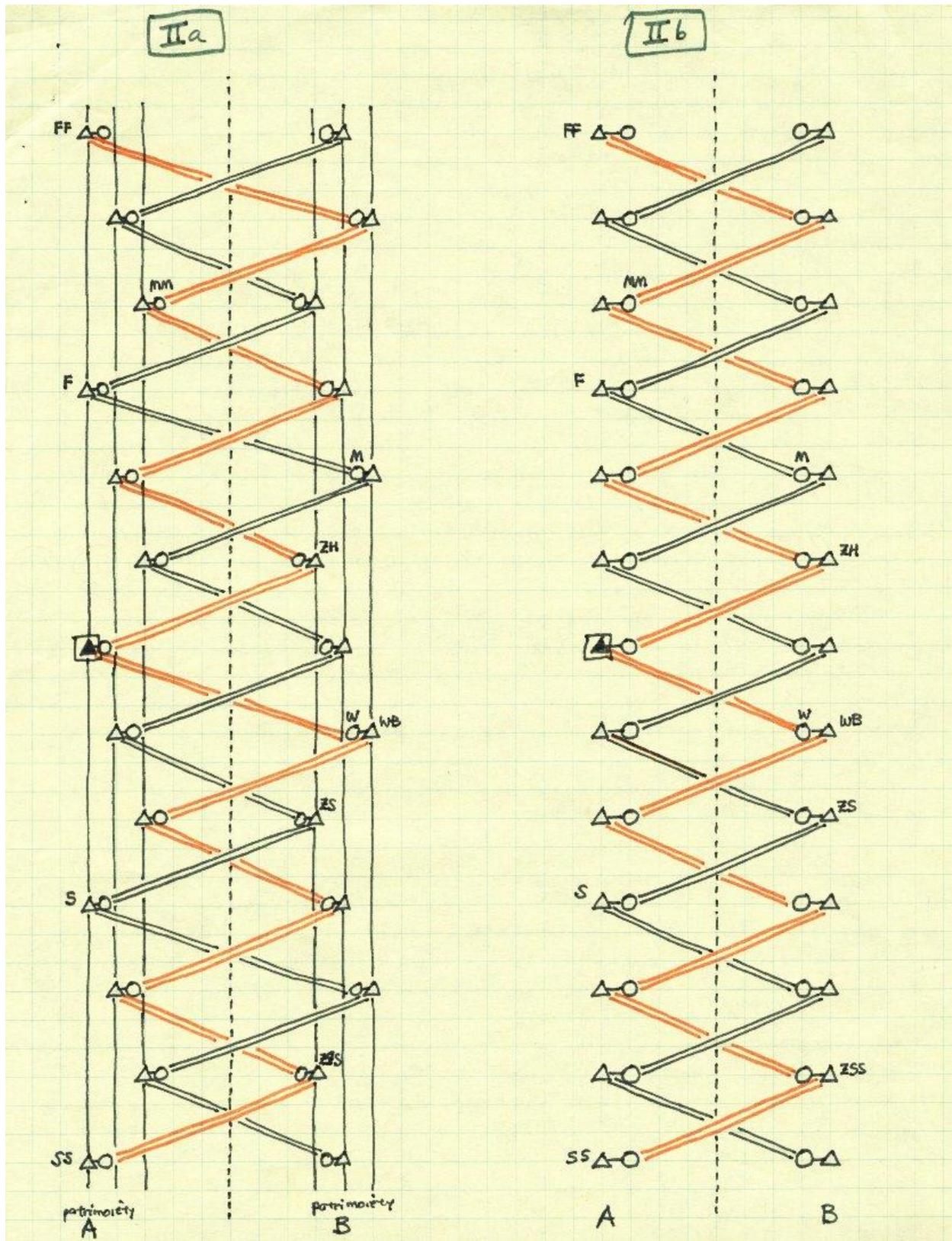
5. Perhaps Fig.13, since it's not really used, could be eliminated? I have no strong opinion either way. If it's retained and the enclosed substitutes for Fig. 12 (I plus IIa, or I plus IIb) are used, then Fig. 13 would need to be redrawn in a manner similar to a laterally-extended IIb.

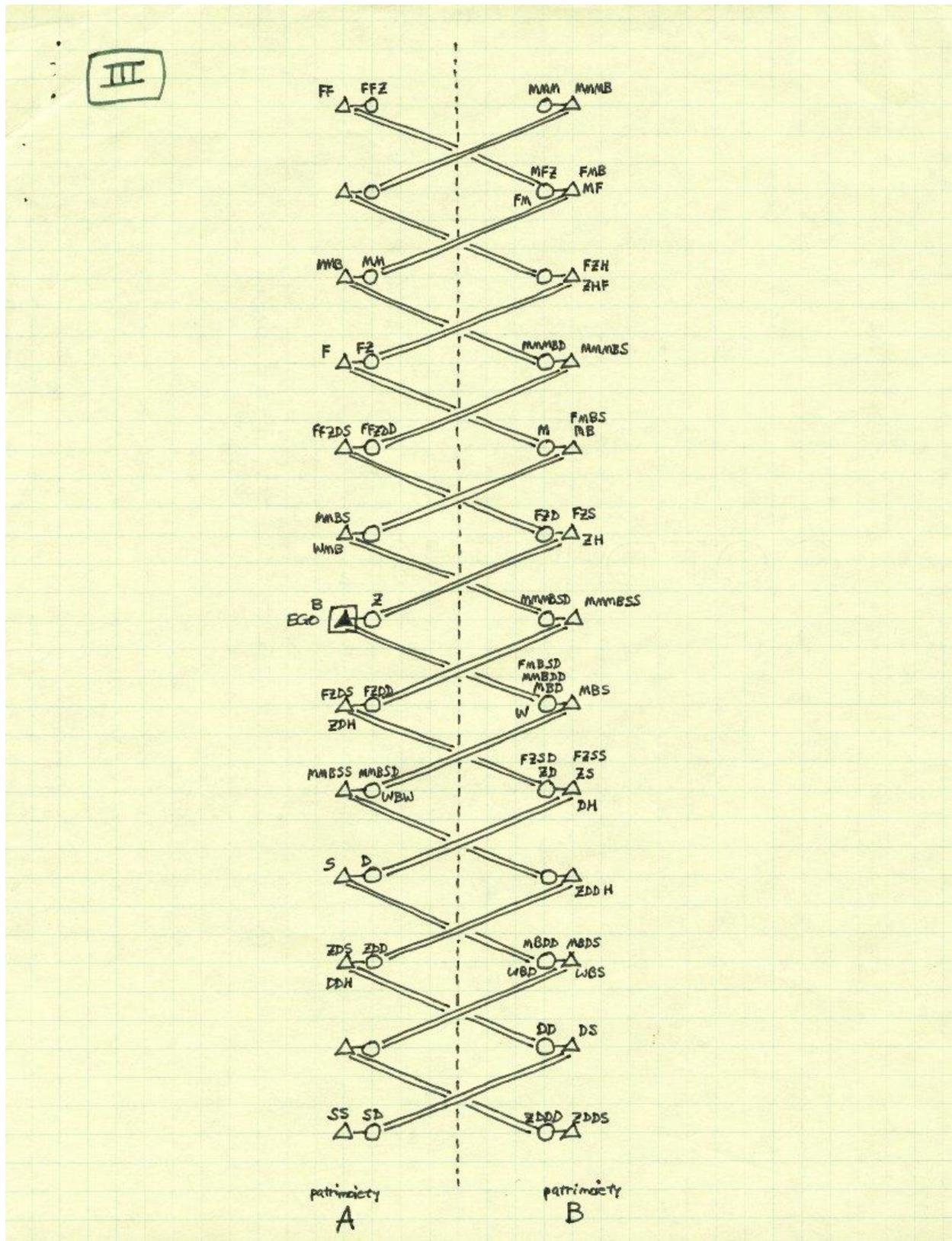
Enough for now. I'm sending this to Berkeley, but wherever I send it you're likely to be in the other place!

Warm regards,

JRA







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31 August 1977

Dear John,

The day after I mailed the manuscript of the kinship paper to you, I received my Canadian papers. So I just got to McMaster.

Your July letter concerning ways to count cross-cousin marriages was waiting for me when I got here, and your latest comments on the revised draft of the kinship paper reached me today. Thanks very much for both of them. Although my decision concerning the counting of cross-cousin marriages was the same as yours, I certainly didn't state it as elegantly as you did. I'll revise the footnote to reflect your thinking on the subject, but I'm still inclined to leave it as a footnote. I think the issue deserves more recognition than it receives as a footnote, but I think it would be somewhat distracting to deal with it in the text of the article. Maybe a short note on the problem should be published separately somewhere?

I'm especially pleased with your comments and suggested revisions in the latest draft of the paper. I agree that the revised Figures are much easier to read, and I'll fix the paper to reflect the improvements. Your comments concerning the grandgiver-grandtaker relationship are intriguing, but I haven't had time to think through them yet. When I receive Chad 's comments on this draft (hopefully in a few days), I'll revise the whole thing once more and send you a copy for final review. Maybe we can get it to the editor of AE in a couple of months. I'm very pleased that you were agreeable with the co-authorship arrangement. I feel just as honored by your acceptance as you did by my invitation.

Regards,

Woody

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4 October 77

Dear John,

Enclosed is the latest revision of the kinship paper.

I've made the distinction between Figures and Tables, and I've used the revisions that you suggested vis-a-vis the age-biased Kariera diagram(s). After spending about four hours in trying to draw a quadruple-helix age-biased Aranda diagram-in the same form as the new Figures 7 & 8, I gave up. I can visualize it perfectly, but there is no way for me to present it in a clear fashion in two dimensions. We'll leave it for the readers to do as an "exercise" ...

I tried to do longer, more descriptive captions for the Figures and Tables, and decided that truly informative captions would lead to extensive re-writing of the article if we were to avoid an excessive amount of redundancy. I've looked through several recent issues of *AE*, and noticed that some articles use brief captions and others use longer ones. In principle, and in retrospect, I agree that more informative captions would be good; in practice, I'd prefer to avoid the amount of rewriting that they would entail at this point in the evolution of the paper. If you feel that longer captions are really important (not optional), I'll change them. Otherwise, I'll leave them brief and hope that doesn't offend too many readers.

Your comments concerning grandgivers and grandtakers led me to discover the relationship that is sketched in Figure 8, and discussed in the text; but I couldn't decide just what to do with grandgivers and grandtakers per se. I would like to include that notion in the paper because it just seems so nice - in some way that I can't put my finger on - but I couldn't quite figure out how to do it. Suggestions?

If you don't find anything else that calls for further rewriting, the enclosed copy is the version I'll send to *AE*. The Figures and Tables will be done professionally by people in the University Audio-Visual Department - I just got a grant that will cover those costs comfortably. As soon as I get your approval (or suggestions for further improvements), I'll put the Figures and Tables into production, make the final revisions in the paper, and get the whole thing in the mail as soon as possible.

Cheers,

Woody

UNIVERSITY OF WASHINGTON  
SEATTLE, WASHINGTON 98195  
Department of Anthropology DH-05

14 October 1977

Dear Woody,

Nice to hear from you again, and to have the new draft. I have only some extremely minor suggestions for further changes, so as far as I'm concerned the ms. can go off to AE as soon as you have the professionally drawn figures in hand and suchlike.

First, I request that the author-order revert to Denham, McDaniel, Atkins\*. This is not out of any "modesty," but rather self-interest. I'd rather not get "lost" (psychologically) in the middle of the list, and besides the last position in the list is a rather traditional one (in other disciplines anyway) for a thesis advisor. OK?

Secondly, do you feel strongly about 'helixes' as the plural for helix? It is true that most dictionaries these days list -xes before -ces as the alternative pluralizations, but I have always rather strongly preferred 'helices.' If you feel equally strongly the other way, fine; no big deal. (The word occurs in the plural on p.33 twice, p.38, and perhaps elsewhere.) However this turns out, the 'milage' in the caption for Table 4 should be 'mileage.' And you might consider that the Tables would be numbered better as I-VI, given arabic numerals for the Figures.

Thirdly, might the phrase "Atkins designed Figure 7..." in Note 1 be replaced by "Atkins invented the model shown in Figures 6 and 7..." (or, if that's too strong, "Atkins constructed the representations in Figures 6 and 7...")?

It's a damned good paper. Thanks again for bringing me in on it; I appreciate that.

Warm regards,



\*Besides, DMA practically announces that the paper will be about double helices, and would do so even more explicitly if Chad would agree to change his name to NcDaniel and subject himself to the McName "Nick."

January 12, 1978

Professor John R. Atkins  
Department of Anthropology  
University of Washington Seattle, WA 98195  
USA

Dear John:

Here it is! After much delay and confusion surrounding preparation of the figures and tables, it is done. I've mailed the original and two copies to the editor of the *American Ethnologist*, but, if there is anything else that I should change, I can do that after (if) it is accepted for publication. Please examine the text--and especially the figures and tables--for errors of any kind, and tell me about them soon.

Regards,

W. Denham

UNIVERSITY OF WASHINGTON  
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Department of Anthropology DH-05

18 January 1978

Dear Woody,

Great! Glad to hear that the ms is off to AE, and I agree that any further changes should be post-acceptance.

I enclose some possible changes of a fairly minor character; evaluate them and do what you like about them, however you want.

It occurs to me that perhaps the most glaring omission from the Conclusion section for many readers will be the absence of any statement about the Alyawara ideology concerning marriage, in view of the conflict between this ideology and (a) their behavior and (b) the Fig. 6/7 model. Even if we have no speculation to offer as to why they say they marry second but not first cross-cousins (or at least "should" so marry), the disparity probably ought to be mentioned explicitly again in the Conclusion. I have no hunches to suggest, and I would trust any hunches you might have on this subject more than my remote armchair ones anyway. If you have even a glimmer of a hypothesis as to why their marriage ideology takes the shape it does, stick it in! (Or at least let's correspond about it, with an eye to eventual solution of this puzzle.)

For the first time in 14 years here I have a "sabbatical" quarter off with pay, Winter only, mainly to work on the Atkins-Kay book, which is going very slowly but due at Academic Press in September. Unfortunately this precious quarter began for me with three weeks of the worst flu I've ever had, plus the news that my diabetes is not under nearly as good control as my doctor and I had thought. My brain is being bathed by about 3 times as much sugar (in the bloodstream) as normal brains have to put up with, which is good neither for my brain nor for me. As yet we have not hit on the right combination of diet and medication; probably I soon will be on insulin. I surely would like to get on the right regime, as the accelerated aging, possible blindness, etc. that too often result are not pleasant to contemplate. In August I'll be 50, and I would prefer that each passing year (and they now seem to pass with lightening speed) add no more than a year's worth of biological aging-not TWO year worth.

Through March, it would be better to use my home address:  
7410-45NE, Seattle, WA, 98115 (206-524-5859).

Best regards,

JRA

**JRA/18 i 1978**

On p.3 you write "Matrimoieties are unnamed and exogamous just as patrimoieties are." But on the preceding page exogamy is attributed to the named patrilineages only, not to the patrimoieties. I suggest that 'exogamous' be inserted twice in the 3rd & 4th lines from the bottom of p.2, just before the word 'patrimoiety' in each case.

p.5: "Almost all of the Alyawara terms are identical with those used by the Aranda," with a similar statement on p.6. This is far from obvious to the general reader of Table I (if I can qualify as a general reader). I assume you do mean the terms and not merely the categories these terms label. Less than a majority of the terms in the two lists look alike. Are two different orthographies being used? Are there regular sound shifts or correspondences between Alyawara and Aranda that, when known, increase the similarities? What are your criteria for "almost all" (more than 50%?) and for "identical" (a very strong word)? I believe we need to help the reader here, either by showing greater similarities than immediately meet the eye or by reducing the claim to something like "Many of the terms [perhaps even giving a number or percentage] are clear cognates..."

p. 11: if you're going to have the ordered series OWN, PATRI, SPOUSE, MATRI in line 15 and in Table III, then the clauses in lines 13 & 14 ought to be reordered to match.

pp.13-14: I believe this would be more nearly instantly clear if rewritten as "For example, the number 321 appears at the intersection of the third row (labeled '12') and first column (labeled '10'). This means..."

p.14: I suggest that "dark square" (line 10) be changed to "dark-bordered quadrangle" for improved clarity. The region is not generally dark, and it's not square.

p.15: I was misled (and other readers might be) by "lower right-hand portion" (line 3), which took me to only a small subarea of the region you intended. I suggest striking out the word 'lower,' leaving just 'right-hand portion.'

p.25: This is minor, but in the last four lines I would prefer "The model itself is internally consistent, and it is compatible with...; however, it does not square well with the actual Alyawara...". (Changes underlined.)

p.29: Beginning in line 5, I would suggest some rewriting, perhaps along the following lines: If we trace out a line of sibling-in-law (or "wife-giver"/"wife-taker") relationships cycling upward from ego and passing through Z, ZH, ZHZ, ZHZH, and so on, we discover that it ends at FF at the top of ego's descent line. The same line continues below ego, cycling down through W, WB, WBW, WBWB, ..., to SS at the bottom of ego's descent line. This line of affinal connections from FF through ego to SS does not include ego's father, mother, or offspring. However, a second such line, passing through ..., FZH, FZ, F, M, MB, MBW, ... encompasses these relatives and all remaining positions on the diagram. The two lines or cycles in question form parallel curves on the imagined cylindrical surface, with their obliqueness due to the 14-year mean age difference between spouses. The first curve accounts for all positions belonging to sections A1 and B2 (from Fig. 2), while the second accommodates all positions in sections A2 and B1.

p.29, bottom line: Could be improved, I think, by changing either to "By rolling Figure 6 more tightly to form a..." or to "By rolling Figure 6 to form a tighter, triple-walled..."