FURTHER THOUGHTS ON OCEANIC SUBGROUPING

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My involvement with Oceanic languages, which began 30 years ago, was associated from the outset with the problem of subgrouping.[1] It came about because some anthropologists who were interested in reconstructing the culture history of the Pacific area wanted an account of the genetic relationships within the Austronesian family and particularly within the Oceanic branch of the family. My remarks here will be limited to the Oceanic languages although the approach which I follow should be applicable to the rest of Austronesian, or for that matter, to any other language family.

I have avoided saying that what these anthropologists wanted was a 'family tree' or a 'subgrouping', although I am sure that they would have found either characterisation acceptable. I have chosen instead the more general statement 'an account of the genetic relationships' to indicate that I think their needs could have been satisfied with some other kind of account.

In fact, work going back as far as Isidore Dyen's lexicostatistical classification of the Austronesian languages (Dyen 1965) has recognised patterns of relationship different from those recognised in the classical formulation of the family tree model. Recognition of such relationships has appeared regularly in recent discussions by Paul Geraghty, Andrew Pawley,[2] and most of the other scholars who have been involved in research on genetic relationships among Oceanic languages. However, most of these discussions have continued to use family tree *terminology*. That is, such terms as "proto-language" and "subgroup" have been used, but in loosened senses. What I want to suggest here is that the *real* lesson in all of this recent work is precisely that the family tree model is not adequate to represent the genetic relationships among the Oceanic languages. And I suggest furthermore that by continuing to use the family tree terminology we inhibit ourselves from realising the full benefits of this lesson.

What we are concerned with are the cases where a given member of the Oceanic group is more closely related to some of its co-members than to others. The established practice has been to conceive of these closer relationships in terms of subgroups in a family tree. But, as I will try to show here, it is possible to separate the concept of closer relationship from the concept of subgrouping and the assumptions of the family tree model.[3] Accordingly, it will be possible to claim that two members, A and B, of a language family are in a closer relationship with each other than with some other member, C, without claiming that there

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is a well-defined subgroup of the family to which those two belong. To say that A and B are in a closer relationship as opposed to C will mean nothing more than that they have undergone one or more innovations in common which C has not undergone.

Let me begin by recalling that the family tree model is not the only recognised model of linguistic differentiation. In fact, it is a standard practice in textbook treatments of linguistic differentiation to present the family tree (Stammbaum) model of August Schleicher and the wave (Wellentheorie) model of Johannes Schmidt as two competing models, neither of which has succeeded in displacing the other.

Now there is in this what seems to me to be a strange contradiction. It seems to be accepted as a truism that the wave model corresponds to the actual process of differentiation more accurately than does the family tree model. On the other hand, when someone wants an account of the genetic relationships within a language family, the automatic expectation seems to be that the answer is to be expressed in terms of the family tree model. It is appropriate to ask why that should be so.

In what follows I will briefly consider some factors which may help explain the popularity of the family tree model. Then I will try to show that the wave model is indeed a more accurate representation of the kinds of events which must have led from the Oceanic proto-language to the contemporary linguistic situation. Finally, I will attempt a very rough account, based on the wave model, of the probable historical relationships among the Oceanic languages.

ATTRACTIONS OF THE FAMILY TREE MODEL

The question is: If the family tree model so seriously fails to reflect the actual historical relationships, why has it persisted in use to the extent that it has? I cannot attempt to give a complete answer, but I can make a few suggestions.

I believe that there are probably two considerations which have tended to encourage its use. One consideration which has surely been significant is that it has a visually attractive product. A family tree offers the great convenience of being two-dimensional, it can be printed on a single sheet of paper, and it can be easily read. This is certainly a very attractive feature of the approach. However, I would suggest that this aesthetic appeal is bought at the cost of accuracy.

A second consideration, I suspect, is that this approach to linguistic relationships heightens the analogy between genetic linguistics and evolutionary biology. The point of analogy which has always seemed to me to be particularly striking is the parallel role attributed to particular key evolutionary units in each. The units of which I am speaking are, respectively, the species in biology (as contrasted with subspecies or races) and the individual language in linguistics (as contrasted with dialects, etc.).

What is of such crucial importance about these units is that they are supposed to represent the point-of-no-return in the process of differentiation. Evolutionary biology (or, respectively, historical linguistics) traditionally makes the following assumptions. Subspecies (or dialects) may arise through natural differentiation, but unless they are physically isolated from the other subspecies (or dialects), gene flow (or the spread of innovations) will continue among them. As long as this gene flow (or spread of innovations) continues, a levelling of the differences remains possible.

However, there is assumed to be a sort of biological (or linguistic) Rubicon which is occasionally crossed. This Rubicon-crossing is supposed to occur when the subspecies boundary is elevated to the status of a species boundary (or when a dialect boundary is elevated to the status of a language boundary). From this point on, the newly established unit - the species or the language - has become irreversibly separate from its relatives. Gene flow (or the spread of linguistic innovations) is no longer possible.[4]

My objection to this biological analogy is that, as far as linguistics is concerned, it is a false picture. I do not believe that there is any such Rubicon-crossing between dialect differences on the one hand and language differences on the other. I would propose a different picture of the process of differentiation.

I would say that any local speech variety will produce linguistic innovations. Some innovations will spread from one variety to its neighbours. Two conditions are necessary if an innovation is to spread from one variety to another. First, there must be some communication between the speakers of the two varieties, and second, the receiving variety must have the necessary linguistic characteristics. The reason why particular linguistic characteristics may be required is that, if the particular innovation consists of a change undergone by some particular feature of a linguistic system, only a system which possesses that feature in the first place will be capable of undergoing the diffused innovation.[5]

In short, each variety may be expected to originate some innovations. Innovation in itself is a differentiating factor. That is, each innovation will at first differentiate the originating variety from the other members of the family. However, some innovations will diffuse between varieties. Diffusion has the effect of partially offsetting the differentiating effect of innovation. The amount of diffusion between two varieties will decline as the amount of communication between them declines (other things being equal; that is to say, provided the sociological circumstances of the relation between the two remains constant).[6] One important influence on the amount of communication will generally be the geographical distance which separates them.

The amount of diffusion may also be expected to decline as the linguistic similarity between the two declines. That is, it will decline in the measure that what we may call their "systemic compatibility" declines.[7] However, this effect is a very gradual one; there is no single Rubicon-crossing step in the process.

THE NATURE OF THE PREHISTORIC EVENTS TO BE RECONSTRUCTED

As I said, I believe that the wave model permits a more accurate representation of the kinds of events which led from the Oceanic proto-language to the contemporary state of affairs. Of course, we have no direct evidence of those events, but I believe that we would find ourselves in approximate agreement as to their general nature. And I believe that our model of linguistic relationships *should* be compatible with the events by which those patterns of relationship were produced.

I would imagine that most students of the field would agree that, starting from the time when Proto-Oceanic was being spoken in its original homeland, the most likely pattern of spread would have been something like this: From very early some dialect differentiation along geographical lines would have been apparent. However, the differentiation, except in rare cases where communication was completely cut off, would have been in part counterbalanced by the spread of innovations. The pattern of spread would have conformed essentially to the classical wave model.

I would suggest that a typical event in the spread of Oceanic would have been for a group of people, usually from somewhere near the periphery of the area then occupied by Oceanic, to move out and establish a new settlement in some place beyond the old periphery.

Let us imagine this situation. Before the migration, our migrants-to-be live in an area in which some linguistic differentiation has already occurred. The nearest village, both geographically and linguistically, to the village of the migrants-to-be speaks what I will call the "neighbour variety". The actual migrants make up only a part of the population of their village and their speech community. Those who remain behind will be called the "stay-at-homes". Thus, at the time of the migration, the migrant and stay-at-home varieties are identical.[8]

Now let us consider the linguistic situation which develops after the departure of the migrants. We may imagine that from this time on the stay-at-homes will share more innovations with the neighbours (because of their relative accessibility) than they do with their departed next of kin, the migrants. The situation which eventually develops is one which is ambiguous in the family tree framework. The stay-at-homes share features (innovations of the period prior to the departure) with the migrants. These shared innovations suggest that the stay-at-homes subgroup with the migrants. But the stay-at-homes also share innovations (those of the period subsequent to the departure) with the varieties which we are calling the "neighbours". *These* sharings appear to constitute evidence for a subgroup consisting of the stay-at-homes and the neighbours as opposed to the migrants. In fact, there is no clear basis in what I have told you for determining which subgrouping should be judged to be correct.

As I understand it, the traditional basis for deciding such questions relied on the old assumption that something of particular importance had occurred when what had been a dialect boundary was elevated to the status of a language boundary. That is, it relied on the "Rubicon-crossing" of which we have spoken. The key question would be which boundary, that separating the migrants from the stay-at-homes or that separating the stay-at-homes from the neighbours, first attained the level of a language boundary, as opposed to a dialect boundary. As I have explained, I believe that that is a misconceived question, and therefore I do not want us to have to be concerned with it.

A STRATEGY BASED ON THE WAVE MODEL

Rather than attempt an extended critique of the family tree model here, I will focus instead on four characteristics that I believe are desirable in a strategy for determining the relationships within a language family, and which I will accordingly incorporate in my proposed wave-based model. They are as follows:

(1) We always tacitly assume that the geographical distribution of the languages should correspond in some measure to their genetic linguistic relations. It seems reasonable to expect geography to provide leads and it seems sensible to attempt to make use of them.

I proposed above that the most likely kind of event in the spread of Oceanic languages would have been for a group of people from the then periphery of the Oceanic-speaking area to establish a new settlement somewhere beyond the old periphery. It seems also to be likely that most of these moves would have been short - that the destination would have been an area which was known beforehand by the migrating group and which was not very difficult of access. I propose therefore what I will call the "principle of shortest moves". This principle is that in the absence of evidence to the contrary, it is to be assumed that each new Oceanic settlement was made from the geographically closest Oceanic-speaking place then in existence.

This principle may be generalised to say that, in the absence of evidence to the contrary, the relative linguistic closeness of any two varieties corresponds to the relative geographical proximity of the locations in which they are spoken. The task of the comparative linguist will then become that of identifying cases where varieties are out of place - where the linguistic relations do not conform to their geographical location - and of providing supporting arguments.

(2) We want our strategy to permit us to escape the assumption that language boundaries - that is, the boundaries which separate different languages - are different in kind from dialect boundaries. Therefore, in the approach which I propose one does not need to talk about languages at all. Instead, I will speak of "local varieties". For present purposes I think it will be sufficient to think of a local variety as that which is spoken in one settlement - e.g. a village.

(3) It is desirable to design our strategy so that the allocation of burdens of proof is symmetrical. What I mean is this: in the framework of the family tree model the conventions of argumentation all seem to concern argumentation for one kind of claim - the claim that some subset of the languages of some language family constitute a subgroup. The most often cited statement on the evidence required is that of Karl Brugmann (1884:253) that the proof of a subgrouping consists of a mass of common innovations, phonological, flectional, syntactic, and lexical - a mass so great as to exclude the thought of accident.

There seems to be no comparable discussion of what is required to prove the opposite claim, i.e. that a particular set of languages do *not* constitute a subgroup. The only strategy available to whoever wants to argue against such a grouping is to try to create doubt about whatever evidence has been adduced or seems likely to be adduced in support of the grouping. We might therefore speak of the kind of evidence appropriate to support a claim that a subgroup exists as *positive* evidence and that appropriate to argue against its existence as *negative* evidence. I call it negative evidence because it can be seen as evidence at all only in relation to, and in reaction against, some adduced or anticipated positive evidence.

Where this difference between the kinds of evidence exists, it is impossible to measure out the burden of proof in equal proportions. That is, it is impossible to design the rules of play in such a way that the requirements of one who would argue for a subgrouping and one who would argue against it are equally rigorous.

In the strategy which I will propose there will be no difference in the kinds of evidence required to support counterclaims as compared to that supporting the original claim.

(4) In Grace 1985 I tried to show that we ordinarily make the tacit assumption that languages which are closely related will be generally similar to one another. As a consequence any perceived lack of similarity between varieties is likely to be construed as prima facie evidence that they are not closely related to one another. I tried also to show there that such a tacit assumption rests on another tacit assumption - that the rate at which languages change is approximately constant. I pointed out that there are serious reasons to doubt that that is true.

In view of these considerations, we do not want our strategy to involve any assumption of gradualism, overt or hidden. That is, we assume that rates of change and the cumulative effects of change may differ widely from one instance to another.

APPLICATION OF THE WAVE-BASED STRATEGY

There are a few points which I would like to mention before presenting the results of the application.

First, my own experience convinces me that it is very useful to recognise groupings which consist of chains of most closely related varieties but which may not be subgroups in the strictest sense. That is, they may not be subgroups in the sense that they may not be descended from a proto-language with all of the necessary qualifications. A proper proto-language according to family tree theory must have been a homogeneous entity in the sense of being characterised throughout all of its dialectal range by the same set of linguistic innovations. In addition, it must have been set off by full-fledged language boundaries from any other varieties.

The kind of groups which I am proposing are what I called "subgroups" in the so-called "subgrouping" which I published in 1955 (Grace 1955). Those were intended to be simply chains of varieties such that each chain was completely set off by boundaries from all outside varieties. Thus, it was required that the closest relative of each member variety was another member but not that each member must be more closely related to *every* other member than to any non-member. I find it very difficult to get along without some such linguistic units, so I will implicitly recognise them here.

Second, we will often encounter gaps in the geographical distribution of the linguistic units (i.e. varieties or chains of varieties) belonging to the family. A gap may be occupied by languages not belonging to the family or it may be unoccupied - that is, it may be uninhabited land or, especially in the ocean area with which we are concerned here, water. Where there are gaps, we may speak of the distribution as being broken up into different linguistic 'islands'. That is, an island will be an area which is occupied throughout by Oceanic linguistic varieties, and which is separated from all other Oceanic varieties by geographical gaps.

The existence of such islands will naturally lead to the question of where each island fits in relation to the others. I expect that in actual practice one of the islands under consideration at a particular time will usually be seen as the matrix island - or 'homeland' - in relation to the others, and that the problem will turn out to be that of placing the other islands with respect to this matrix island. In accordance with the principle of shortest moves which I described above, I will assume that closeness of relationship corresponds to geographical distance even where such gaps intervene. Thus, the assumption in the absence of contrary evidence will be that even if the closest relative of a particular linguistic unit is not within the same island, it will still be the geographically closest unit - i.e. the unit separated from it by the smallest gap.

The most important kind of question which will arise is whether there are any units which are geographically out of place. That is, are there any cases where varieties or subgroups of different geographical proveniences have come to be juxtaposed by secondary migrations? I will discuss the evidence bearing on questions of this sort below, but first, I want to lay out my tentative hypotheses about Oceanic linguistic prehistory.

HYPOTHESES ABOUT OCEANIC LINGUISTIC PREHISTORY

A few of the hypotheses which are suggested by the proposed wave-based strategy are as follows:

(1) It is hypothesised that the Oceanic homeland was in Irian Jaya (Indonesian New Guinea) in the region including the Sarmi coast and probably the Kumamba Islands. This hypothesis is based on the assumption that the homeland of Oceanic must be the Oceanic area which could have been reached by the shortest move from the area in which the most closely related non-Oceanic Austronesian languages are spoken.

(2) Turning to the areas most remote from this homeland and therefore presumably the last reached by Oceanic speakers, we may, first of all, accept the conclusions of various investigators that the Polynesian settlement results from successive migrations from central Vanuatu to Fiji, thence to Rotuman and also to Polynesia, with the subsequent spread throughout Polynesia and back to the Outliers.

However, the genetic relations of most of the other remote groups have been more problematic. Here are the hypotheses which the principle of shortest moves yields for the languages and groups at the far end of the Melanesian chain, namely the languages of New Caledonia, the Loyalty Islands, Southern Vanuatu, Central and Northern Vanuatu, and the Santa Cruz Islands:

(a) New Caledonia was presumably settled from one of the Loyalty Islands.

(b) Each of the Loyalty Islands was presumably settled from southern or central Vanuatu or from another of the Loyalty Islands (if we were to apply the principle of shortest move strictly, we would have to hypothesise that Mare was settled from Aneityum, and that Lifu was settled from Mare, and Uvea from Lifu. The main island of New Caledonia would then have been settled from Uvea or Lifu).

(c) Southern Vanuatu was presumably settled from central Vanuatu.

(d) Vanuatu was presumably settled from the south-eastern Solomon Islands (probably San Cristobal).

(e) The Santa Cruz Islands (at least as far as the Oceanic languages there are concerned) were presumably settled from San Cristobal or thereabouts.

(3) Perhaps the most conspicuous problem concerns the Oceanic but non-Polynesian languages of Micronesia, which I will just call the 'Micronesian' languages. They presumably reflect a movement from somewhere in the Melanesian chain to somewhere in Micronesia. The closest geographical distance between Micronesian and Melanesian (i.e. Oceanic, but not Micronesian or Polynesian) languages is between the Mortlocks and the St Matthias Islands. However, both Micronesia and Melanesia are large areas, and the distances between them at several different points are very roughly equal. Therefore, it is difficult to make any decision on geographical grounds alone.

Micronesian internal relationships suggest, on the basis of linguistic diversity, that the original settlement of Oceanic speakers in Micronesia was in the eastern part of the area - somewhere in the area from Ponape to Kiribati. The closest points in that area to Melanesian languages are Nauru and Ocean Island. The closest Melanesian languages to Nauru and Ocean Island are in the Solomons (Malaita and Isabel) and the Santa Cruz Islands. The very shortest distance would be that separating Nauru, in Micronesia, and Malaita in Melanesia.

If we were to make these assumptions: (1) that the original Oceanic settlement of Micronesia was most likely in eastern Micronesia, and that the most likely point is the closest point to Melanesia in eastern Micronesia - namely Nauru, and (2) that the most likely point of *origin* is the geographically closest point to Nauru in which a Melanesian language is spoken (that is, if we apply the principle of shortest moves), then the hypothesis would be that people from Malaita (or possibly Isabel, Choiseul, Santa Cruz, or thereabouts) settled on Nauru (or perhaps Ocean Island), and that thence they gradually spread throughout Micronesia.

I should point out at once that the principle of shortest moves does not seem to constitute a very weighty argument where all possible distances are quite long as in this case. However, the hypothesis is made more interesting by the fact that Robert Blust (1984) has recently found some, admittedly skimpy, evidence linking Micronesian with the languages of Malaita.

Most of these hypotheses have not been proposed before, as far as I know. That concerning the position of Fijian and Polynesian is the most noteworthy exception. In addition, Blust has, as I mentioned, suggested a possible connection between Malaitan languages and Micronesian. Finally, one might want to count the hypothesis of the settlement of southern Vanuatu as an exception since I personally have always thought that the southern Vanuatu languages would turn out to be most closely related to other Vanuatu languages.

I am not aware of any previous suggestions about where any of the other groups fit in. What the present approach has done is put forward some relatively specific hypotheses. These are not based on linguistic evidence, of course, but then no one seems to have been able to find any linguistic evidence in those cases. What these hypotheses can claim to be, therefore, are the best guesses available in the *absence* of linguistic evidence. But they might possibly have one further merit; they might provide a specific focus for a future *search* for linguistic evidence.

Let me say just a few further words about how this approach would reconstruct the history of the spread of Oceanic throughout Melanesia. It suggests that, from the original homeland on the Sarmi coast, Oceanic languages must have spread eastward along the coast of New Guinea. When they reached the Sepik Province of Papua New Guinea, there was presumably a split. One offshoot would have gone north to Wuvulu-Aua and thence eastward through the Admiralties to Emira-Mussau

(St Matthias) and New Ireland. It was presumably this prong of the advance which reached the Solomons - perhaps via Nissan - and thence, as we saw, moved on to eastern Melanesia and Polynesia.

From the Sepik Province, the other prong would have continued eastward along the coast and offshore islands such as the Schoutens. Again there must have been a split on or around the northern coast of Morobe Province with one branch going to New Britain[9] and the other continuing down the coast to provide the Oceanic languages of Papua.

Some parts of these latter hypotheses have been proposed before, and some supporting evidence for some of them has been published. However, some of the seemingly most significant of them are entirely new, and again, I can only suggest that they represent best guesses in the absence of linguistic evidence and possible foci for a future search for linguistic evidence.

HOW HYPOTHESES OF THIS KIND CAN BE DISPROVED

What kind of argumentation is possible against a claim made in this proposed new game? The possible attacks seem to be of two kinds. One would be to show that I have made mistakes. For example, it is not impossible that I have made mistakes in determining the distances, and that some of the moves which I have proposed are not actually the shortest possible.

The other kind of attack, and this is the most interesting case, would involve disproving one or more of my purported moves by showing that the geographical proximity on which the postulated move was based is the result of a secondary juxtaposition - i.e. that it involves a move other than the minimal sequence of shortest moves needed to account for the present distribution of Oceanic languages. But how may one go about proving this?

First of all, one obvious kind of argument is excluded by one of the assumptions which we have adopted. That assumption is that linguistic change might be greater in some times and places than in others. The main practical consequence of this assumption is that we cannot base a claim against closeness of relationship on the negative grounds of linguistic *dissimilarity* between the linguistic units involved. We are, therefore, left with the following possibilities in a case where a unit A is supposed on the basis of its geographical proximity to have a unit B as its closest relative:

(1) Possibly upon closer examination, A can be shown to share innovations exclusively with B or with B and a few of B's close relatives. If so, that is evidence that the geographically-based hypothesis was right, after all.

(2) Possibly A can be shown to share innovations exclusively with other units geographically and linguistically remote from B. If so, that is evidence that some unforeseen migration has brought A secondarily into this geographical juxtaposition with B. In this case, A's true next-of-kin will have been identified.

(3) Possibly A can be shown to have retained features of an earlier protolanguage which have been *replaced* in some of the innovations which are shared exclusively by B and some of B's closest relatives. If so, that is evidence at least that B's relation to A is more remote than its relation to the languages which share those innovations. This would be at least compatible with A's location being a secondary juxtaposition.

CONCLUDING REMARKS

This scheme is being put before you as a serious proposal. Of course, it will require much further testing before a final evaluation of its usefulness can be made, but its underlying assumptions appear to be intuitively sound and it seems likely to provide valuable research leads. Furthermore, the application of it which I have attempted here has generated a whole series of more or less novel hypotheses.

I believe that it offers two important advantages over our present strategy. The first is that it provides an alternative to the family tree with the latter's reliance on the concept of the subgroup. The difficulty with the subgroup concept is that the kind of discrete and homogeneous entities that are required as proto-languages for the subgroups must surely have occurred only sporadically in the history of any language family. Any strategy for determining the genetic relations among a group of languages which is prevented from recognising any relationships except those which can be expressed in terms of such subgroups seems destined to provide very unsatisfactory results.

The other advantage which it offers is that it makes reasonable hypotheses about genetic relationships available for use even before all of the linguistic evidence has been compiled. These hypotheses should be of value to the culture historian as well as to the linguist. I say that the hypotheses are reasonable because I believe that in most cases it will turn out to be true that the nearest relative of a particular variety will be the variety which is geographically closest to it, as the theory holds. Moreover, I believe that in the majority of cases where the closest relative turns out *not* to be the nearest geographical neighbour, our hypothesis will prove still not to have been wrong by very much. That is, I predict that in most such cases the closest relative will still be relatively near, and that the nearest neighbour will turn out to be a fairly close relative. In short, I predict that the hypotheses will usually be right, and that when they are wrong, the error will most often amount to no more than a minor imprecision.

I believe that it deserves the serious consideration of both culture historians and linguists.

NOTES

1. This is a slightly revised version of the historical linguistics keynote address delivered on August 16, 1984, to the Fourth International Conference on Austronesian Linguistics, Suva, Fiji.
2. In fact, the 'network-breaking' model of linguistic differentiation proposed in Pawley and Green 1984, comes quite close to that proposed here.
3. The idea of defining closer relationship as a relation holding between individual languages independent of claims about subgrouping as such probably was first suggested to me by Isidore Dyen. Dyen (1953:580) wrote:

   > In contradistinction to Brugmann's usage, closer relationship can be used as a broad term to include both membership in the same dialect area of the proto-language as well as membership in the same subgroup, where a subgroup implies a private proto-

language which was one of the languages resulting from the differentiation of the proto-language of the whole family. In this sense a closer relationship is indicated by any exclusively shared innovation.

4. Actually, there is one qualification which must be made in the case of language boundaries. It is generally acknowledged that lexical items may diffuse across language boundaries. However, it is customary to think of the lexicon of a language as more nearly an accidental accumulation than as contributing to the essential character of the language. Therefore, this qualification is generally not regarded as a serious one.

   I think that this sharp distinction between lexicon and grammar (or whatever the other parts of language dealt with in linguistic descriptions are to be called) is a misleading exaggeration. See my discussion of the process of differentiation just below.

5. I presume that this requirement of systemic compatibility is the source of the idea that only lexical items can be borrowed from one language to another.

6. Not nearly enough is known about this, but as John Wolff has reminded me, there are as many different kinds of language contact situations as there are kinds of possible sociological relations between the languages or varieties in contact. Each different kind of contact situation is likely to manifest a different pattern of diffusion. In the model I am describing I assume the sociological relations to remain unchanged.

7. The suggestion that the amount of diffusion will be in some inverse relationship to the degree of "systemic compatibility" again assumes everything else to be equal. Furthermore, it should not be understood as claiming that very great structural differences cannot be levelled given favourable circumstances and sufficient time.

8. The same problem would arise if, instead of being linguistically identical at the beginning, the varieties spoken by the migrants and the stay-at-homes were simply more closely related to each other than to that of the neighbours, as long as there was enough systemic compatibility between stay-at-home and neighbour varieties to permit fairly free diffusion of subsequent innovations.

9. The Tolai area of New Britain was surely settled by the Oceanic prong which passed through New Ireland. Therefore, Oceanic languages entered New Britain from both ends. My guess at this point would be that all present-day Oceanic languages in New Britain except the varieties of Tolai are traceable back to the settlement from the Morobe coast.

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