How hopeless is genealogical linguistics, and how advanced is areal linguistics?

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A review article of:

Alexandra Y. Aikhenvald & R. M. W. Dixon (ed.) 2001. *Areal diffusion and genetic inheritance.* Oxford: Oxford University Press.

1. Introduction

The basic question addressed by the 15 contributions to this volume can be formulated as follows: "To what extent is the family tree model useful in describing relationships between languages, and to what extent are the relationships shaped by areal diffusion rather than genealogical relatedness?" The papers were first presented at a 1998 workshop at Dixon and Aikhenvald's Research Centre for Linguistic Typology, for which Dixon's (1997) provocative essay on this topic served as the basis of the discussion. In this essay, Dixon argues that historical relationships which can be modeled by family trees are not the norm, but arise only as a result of "punctuations" (momentous historical events such as the introduction of agriculture). During "equilibrium" periods (i.e. periods with no punctuations), languages do not split repeatedly, but coexist and influence each other. After a long period of such multilateral areal diffusion, the languages in a particular area may end up being fairly similar to each other, but according to Dixon, this should not be interpreted as genealogical relatedness. His primary example is Australia, whose languages show a great deal of similarities that Dixon argues are largely due to areal diffusion. So how does Dixon's view fare in view of the contributions to this volume? This is one of the issues addressed in some of the papers, but the papers are actually quite diverse in their interests. What unites them is that they were written by linguists

with first-hand experience in a particular area and that they describe the effects of contact-induced language change in a whole group of languages.

In addition to Aikhenvald & Dixon's "Introduction" (pp. 1–26), the volume contains one paper on archeological evidence (Peter Bellwood, "Archaeology and the historical determinants of punctuation in language-family origins", 27–43), one general paper on constraints on borrowing (Timothy Jowan Curnow, "What language features can be 'borrowed'?", 412–436), and twelve papers on areal diffusion vs. genealogical[1] inheritance in different areas of the world:

Africa

Gerrit Dimmendaal, "Areal diffusion versus genetic inheritance: an African perspective"   358–392

Bernd Heine & Tania Kuteva, "Convergence and divergence in the development of African languages"   393–411

Western Eurasia

Geoffrey Haig, "Linguistic diffusion in present-day East Anatolia: from top to bottom"   195–224

Calvert Watkins, "An Indo-European linguistic area and its characteristics: ancient Anatolia; Areal diffusion as a challenge to the comparative method?"   44–63

Eastern Eurasia

Hilary Chappell, "Language contact and areal diffusion in Sinitic languages"   328–357

N. J. Enfield, "On genetic and areal linguistics in Mainland South-East Asia: Parallel polyfunctionality of 'acquire'"255–290

Randy J. LaPolla, "The role of migration and language contact in the development of the Sino-Tibetan language family"   225–254

James A. Matisoff, "Genetic versus contact relationship: Prosodic diffusibility in South-East Asian languages"   291–327

Pacific

Malcolm Ross, "Contact-induced change in Oceanic languages in North-West Melanesia"   134–166

R. M. W. Dixon, "The Australian linguistic area"   64–104

Alan Dench, "Descent and diffusion: The complexity of the Pilbara situation"   105–133

Americas

Alexandra Y. Aikhenvald, "Areal diffusion, genetic inheritance, and problems of subgrouping: A North Arawak case study"   167–194

2.   Equilibria and punctuations

Apart from Dixon's own paper on the Australian linguistic area, there are only four other papers that seriously address Dixon's (1997) "punctuated equilibrium" idea: Two authors fully agree with Dixon's views (Peter Bellwood, the archeologist, and Alan Dench, a former student of Dixon's), and two others are rather critical (Calvert Watkins and Malcolm Ross). Bellwood endorses the view (well-known to linguists from Renfrew (1987)) that many of the major language families (Indo-European, Austronesian, Uto-Aztecan, Bantu, Dravidian, Sinitic) were formed as a result of the initial spread of agriculture, as farmers moved into areas occupied mainly or entirely by hunters and gatherers. In contrast to Nichols (1998), he claims that such widespread language dispersal requires substantial movement of speakers, and that language shift by largely unmoving populations is not sufficient.[2] However, he admits that hunter-gatherer populations have also spread and formed large language families, especially in North America (Athapaskans, some Algonquians, some Uto-Aztecans), and he notes that if Dixon's scenario for Australia is correct, this continent is rather unusual in world terms, and we do not know why.

Dixon, too, explicitly notes the unusual status of Australia in his contribution on the Australian linguistic area: "this is a completely different situation from those reported from anywhere else in the world" (p. 88). But in Dixon (1997), one gets the impression that Australia is the prime example of a long history of equilibrium that has blurred all deeper genealogical boundaries (cf. 1997:93, n. 20). If this example is not representative, then one wonders how general the model can be. In his chapter, Dixon provides a very useful summary of areal features that are common to most or many languages of Australia. Although this is a rich list, this procedure falls short of demonstrating that Australia is a linguistic area, because linguistic areas need not only be internally coherent, but also distinctive with respect to languages outside the area. Thus, one would have to show that the Australianisms are uncommon in the rest of the world, or at least in adjacent areas such as New Guinea. This would not have been difficult, but instead Dixon goes on to discuss two grammatical phenomena (complex verbs, i.e. coverb plus inflected verb, and bound pronouns), showing that their distribution across languages can only be explained by contact-induced change. In an appendix, he argues in some detail against the Pama-Nyungan hypothesis. This is interesting especially for non-Australianists, who are likely to be unaware of the nature of the evidence on which a commonly recognized family such as Pama-Nyungan rests.

Dench examines in greater detail a cluster of 17 languages from the wider Pilbara region in the westernmost corner of Australia. He looks at a number of innovations in phonology, morphophonology and case-marking patterns, and concludes that it is difficult to group the languages in such a way that each innovation can be attributed to a single ancestral variety. However, Dench's conclusions are not as strong as Dixon's, because he only argues that it is not easy to draw family trees, not that genealogical inheritance plays no role at all. In fact, it must play a crucial role, because the similarities among the grammatical morphemes of the languages (including idiosyncratic suppletive allomophy) are overwhelming. It seems clear that the Pilbara languages are all rather closely related genealogically, and the difficulties of subgrouping of closely related groups of languages are well known (recall the difficulties in subgrouping of the Romance languages).

It is my impression that the main difference between Australia and other parts of the world is that "core" vocabulary appears to give very little information about genealogical relatedness, because all kinds of words are very easily borrowed. Dixon claims that in Australia "cognate scores are roughly the same for both core and non-core vocabulary" (p. 90), but he does not say how "core vocabulary" is defined. Perhaps a more sophisticated ranking of lexical stability could be established (along the lines of Dyen et al. 1975), and then even lexical information could be used for establishing genealogical relationships. But since most Australian languages have plenty of grammatical morphemes, which are generally not so easily borrowed, I do not see why deeper genealogical classification in Australia should be as pointless as Dixon suggests it is. But I fully agree that "we must start at the bottom [and] provide proof for those low-level genetic subgroups which can be recognized (p. 98).

Ross's focus is on sociolinguistic (and partially even psycholinguistic) conditions for contact-induced change. His main example is the massive adoption of semantic-syntactic patterns from the Papuan language Waskia into the Oceanic language Takia (Papua New Guinea), a process that he refers to as "metatypy".[3] Ross sees this case study as an example of "equilibrium under the microscope", but the Waskia-Takia contact seems to be different from the Australian situation (as portrayed by Dixon) in that it involves no lexical and phonological diffusion at all, only massive semantic-syntactic influence of one language on another. Moreover, in Australia, all languages were supposedly equal and influenced each other. Ross makes a strong claim that is difficult to reconcile with this:

> "If 'converge' means 'change to become more like each other', then languages do not usually converge. Instead, one language becomes more like a second, while the second may be relatively unaffected by the contact." (Ross, p. 139)

This asymmetry is particularly evident where one of the two languages is clearly associated with economic and military dominance, as in the case of eastern Anatolia, discussed by Haig. Turkish, Kurmanji, Zazaki and Laz have not really "converged", but rather Kurmanji, Zazaki and Laz have changed to become more like Turkish (and to some extent the similarities may be due to Persian influence on all four languages). So Anatolia is not felicitously described as a "linguistic area", anymore than Siberia (where all the indigenous languages have undergone influence from Russian) or Latin America (where all the indigenous languages show influence from Spanish or Portuguese). On the other hand, Ross has difficulties explaining situations like the Balkan Sprachbund, where it is not clear that a single model has influenced all other languages.

But Ross's main criticism of Dixon is that change by diffusion and genealogical divergence are not mutually incompatible (p. 153–4). As Takia adopted many features from Waskia, it simultaneously diverged from its closest Oceanic relatives, and this can be modeled by the family tree. Dixon would probably say that his alternating periods of equilibrium and punctuation represent idealizations, but the question is whether we really need to say more than has traditionally (i.e. before 1997) been said.

Watkins makes a similar point on the basis of data from ancient Anatolian languages, both Indo-European (Hittite, Palaic, Luvian, and a few poorly documented languages) and non-Indo-European (Hattic, Hurrian). In a short period, between 2200 and 1700 BC, these languages converged phonologically and syntactically and formed a linguistic area. Thus the areal diffusion was fairly rapid and associated with population movements, in contrast to Dixon's scenario, where rapid events of population expansion ("punctuations") lead to families, and diffusion happens under equilibrium conditions. Watkins points out that the Balkan and South Asian linguistic areas arose under similar circumstances, and especially in South Asia it is clear that genealogical divergence (the fragmentation of Old Indo-Aryan into the varieties of Middle Indic and the modern languages and dialects) and areal diffusion can occur simultaneously.

Watkins even suggests that Dixon's scenario could be turned around, following a side remark made by Heath (1998:765): "A rough linguistic analogue [of punctuated equilibrium] might be rapid change in a short period of intense language contact, followed by a long era of continuity under monolingual conditions." In their introduction, Aikhenvald & Dixon rightly point

out that before the advent of agriculture, multilingualism would have been the rule, but it is also possible that it is primarily greater population movements and shifting dominance relationships (i.e. "punctuations") that lead to intensive contact-induced change.

So what remains of Dixon's idea of "punctuated equilibrium" in language history? I think that the concepts of equilibrium and punctuation are too general and too vague to be of much use when particular languages and historical situations are examined. Potentially far more useful is Ross's detailed sociolinguistic typology of contact-induced change (see also Ross 1997), which distinguishes open and closed, loose-knit and tight-knit, and inwardly and outwardly associating groups, and tries to correlate these sociolinguistic parameters with types of change. This should also be helpful for linguists' interaction with archeologists, because archeologists are usually in a position to say quite a bit about social structures of ancient populations. In my view, dixon's most important insight is that classical language families usually arise in connection with population expansions, and that these occur only under special historical circumstances. This has been overlooked because since the advent of agriculture and sophisticated technology, human history has seen one expansion after the other, so that expansions now appear to be the norm rather than a special case.

3. Family trees

In the introduction, Aikhenvald & Dixon discuss the problems associated with the family tree model in historical linguistics, pointing out that "a family-tree-like diagram does not adequately demonstrate the many kinds of historical and current relationships between languages" (p. 6). This criticism is not quite fair, because nobody ever claimed that family trees capture all historical relationships — they are intended to show relationships among languages that go back to a common ancestor. And if it is the case, as Thomason & Kaufman (1988) argue (and as Dixon (1997: 11–13) emphasizes even more strongly), that normally a language has a single parent, then it follows that (apart from the truly exceptional cases), constructing a family tree of languages sharing a common ancestor is in principle possible. Of course, there are many practical problems associated with this task, and the problem of separating inherited features from areally diffused features is only one of them, perhaps not even the most serious problem. For the Australian languages, Dixon notes that the isoglosses of various distinguishing features do not bunch, and he concludes

from this that the distribution of such features is the result of separate processes of diffusion (p. 64). But such a conclusion is not necessary. Two changes happening roughly simultaneously in a language could divide the area up in different ways. Take the schematic model of (1), where (1a-2b) represent four villages in which the same language is spoken.

(1) 1a 2a
     1b 2b

Now there could be an innovation that starts out in 1a and only spreads to 1b, and another simultaneous innovation starting out in 2a and spreading only to 1a. If the four local varieties then diverge further and split up into separate languages, one would say that the first innovation provides evidence for the subgrouping 1a–b vs. 2a–b, and that the second innovation provides evidence for the subgrouping 1–2a vs. 1–2b. The evidence is contradictory on the usual assumption that there must be a single correct tree, but this assumption is unwarranted. There are as many correct trees as there are simultaneous innovations. So subgrouping is a tricky business, as every practitioner knows, but I wonder whether Aikhenvald & Dixon's pessimistic attitude is justified ("The family-tree model may well be suitable for some situations, but it is simplistic and misleading for others and should not then be employed", p. 6). Even more worrying are LaPolla's concluding questions:

> "Is it not possible for two languages that were not originally related to become related through intense contact? For example, could we not resolve the question of the relationship between Tai-Hmong-Mien and Chinese by saying they were not originally related but now are?" (LaPolla, p. 246).

This would throw us back to a pre-modern, pre-neogrammarian (or even pre-Schleicherian) era when the single-parent principle had not been established yet, and when language thinkers imagined all kinds of unconstrained language mixtures. In all cases where several languages must have arisen from a common ancestor by unbroken transmission, it is by definition possible (in principle) to draw a hypothetical family tree (or several trees, cf. above), whatever the practical difficulties. Of course, when there is too little information available, we may not be able to provide much evidence for particular hypothetical trees. And when we have evidence for several simultaneous innovations, we may prefer to speak of relatively flat "linkages" rather than several layers of cross-cutting hierarchical "families" (cf. Ross 1997).

But a family tree is "misleading" only if one attributes more to it than it claims. Nobody ever said that contact-induced change should be reflected in

family trees, so when Chappell observes that "[the family-tree model] only accounts for a small part of a much more complex linguistic picture", being "unable to capture the successive waves of Mandarinization of Southern Sinitic languages" (p. 353), this is an accurate but not very surprising statement. For areal diffusion, the obvious mode of representation is the map, and this is illustrated nicely in the volume with Dixon's four isogloss maps of Australia and Dimmendaal's four maps of Africa. Where we know more about the historical situation, we can further add arrows to the isogloss map, showing the direction of spread of a feature, and we can lump the isoglosses and draw isopleth maps (as in van der Auwera (1998) and Haspelmath (2001)).

But maybe this overcautious attitude toward family trees by some leading scholars does serve the useful purpose of reminding us that the actual family trees that linguists are working with are often based on extremely tenuous evidence. And this concerns not only the higher-level groupings, whose controversial status is reasonably well-known, but also and especially the subgroups. If it is true, as LaPolla suggests for Sino-Tibetan, that "[t]hose who do subgrouping often do not give the reasons for their groupings … often subgrouping is affected by the author's subjective "feel" of the language, shared features, or shared vocabulary" (p. 245), then we still have a long way before us, and users of subgrouping proposals should be aware of this. It appears that there is such a strong demand for subgrouping that we are lenient with respect to the robustness of the proposals. Subgrouping is needed by typologists, ethnographers, historians, geneticists, and every introductory paragraph in a description of a language nowadays gives the exact genealogical affiliation. We simply need subgroupings to organize the bewildering variety of languages in some way. So whenever a historical linguist ventures a hypothesis about subgrouping, it may be right away adopted gratefully by a large number of "users" who have no way of assessing its merits. And once a classification has gained wide circulation, it acquires a life of its own and creates the impression that it is based on secure knowledge. It is in this sense that Dixon's warnings may serve a useful purpose, but not, I would say, as fundamental objections to the family-tree model itself.

4. Metatypy

As was mentioned earlier, "metatypy" is Ross's term for a series of semantic and syntactic changes of a language under the influence of a neighboring language

that have the effect of changing the language's type. Ross's primary example is Takia (Oceanic), which has adopted a substantial number of features from Waskia (Papuan), such as SOV order, postposed demonstrative, postpositions, preposed full-NP possessors with a binary alienability distinction, and cosubordinate clause linkage. Moreover, Takia has largely adopted Waskia's semantic patterns and idioms (Ross's clumsy term is "ways of saying things"), e.g. compounds like *bor-gonn* 'animal', literally 'pig-dog'. At the same time, Takia has not borrowed any words or changed its phonology in any way. Ross feels that the semantic reorganization and the syntactic restructuring are part of the same underlying process, so instead of just talking about "syntactic borrowing", he coined the term "metatypy", which is also used by several other authors of this volume. Ross mentions quite a few further cases of metatypy that are described in the literature:

(2)   **metatypic recipient**   **metatypic donor (= model)**
      Kupwar Urdu and Kannada   Kupwar Marathi
      Anêm (Papuan)   Lusi (Oceanic)
      Phan Rong Cham (Austronesian)   Vietnamese
      Tariana (Arawak)   Tucano languages
      Ilwana (Bantu)   Orma (Cushitic)
      Arvanitic (Albanian)   Greek
      Asia Minor Greek   Turkish
      Mixe Basque   Gascon

What all these seem to have in common is that the contact-induced changes are quite massive, and there is a clear asymmetry of power relations in most cases. Ross proposes an interesting quasi-psychological explanation for the changes: Having a different semantic and syntactic organization in two languages that are used often imposes a burden on the bilingual speaker, and by assimilating the ingroup language to the dominant outgroup language, this burden is reduced. The actual forms (what Ross calls the "lexification") of the two languages remain distinct, because the forms of the ingroup language serve as "emblematic" symbols of the speakers' identity. In earlier publications (e.g. Ross 1997), Ross contrasts metatypy (where the agents of change are native speakers) with substrate influence (where the agents of change are non-native speakers) and claims that substrate influence is by and large restricted to phonological restructuring: "[S]hifting speakers are very unlikely to impose [semantic-syntactic] features of their emblematic language onto their inter-community language, as this would run counter to its use as an inter-community

language" (Ross 1997:247). If this turned out to be correct, it would be an extremely interesting result, because we would then have three different and clearly distinguishable types of contact situations:

(3)		lexicon	phonology	syntax
	metatypy	no	no	yes
	substrate	no	yes	no
	adstrate = borrowing	yes	no(?)	no(?)

Unfortunately, things are not so simple, and the other authors of the volume who adopt the term "metatypy" do not do it in Ross's constrained way. LaPolla mentions the use of *then* as a discourse marker in Hong Kong English (as spoken by Cantonese native speakers), modeled on Cantonese *kəm³⁵* (p. 242), and Chappell discusses syntactic influence from Southern Min on Taiwanese Mandarin. Chappell notes that this is at variance with Ross's scenario, because Mandarin is the prestige language, and Ross would expect Southern Min to undergo metatypy, not Mandarin (which should at most show phonological peculiarities, somewhat like standard German in Switzerland, which is mainly phonologically divergent). And there seem to be other good cases of syntactic influence due to non-native (perhaps ultimately shifting) speakers: influence from Finnic on Russian (cf. Thomason & Kaufman 1988), from Cushitic on Ethiopian Semitic (Weninger 2001), from northwestern Afroasiatic on Insular Celtic (Gensler 1993), among others. For the syntactic influence from Franconian on French, which would appear to fall into this category as well, Ross (1997:242) claims metatypic status, which at first sight seems impossible, because there cannot have been widespread Romance-Germanic bilingualism among the Romance population. But maybe the Romance élite used Franconian and adapted their Romance to the Franconian model, and then this Franconianized Romance spread to other social strata? This might be regarded as a hopelessly speculative move to save an overly strong claim, but it seems to me that we need such strong claims about possible and impossible contact-induced changes, because otherwise we are not likely to discover interesting generalizations. The present volume is full of facts and observations about contact phenomena around the world, but there are few attempts to arrive at correlations between types of social and linguistic influences, or other generalizations about possible and impossible changes.

5. Diffusion and borrowability

The editors and authors of this volume are of course quite right to emphasize the role of language contact in determining the distribution of linguistic features. One of the reasons why many 19th and 20th century linguists were reluctant to seriously confront language contact was linguistic nationalism and / or romanticism, and although such attitudes are no longer widespread, the effects of this period are perhaps still with us. But probably another reason why linguists have been more interested in genealogical inheritance is simply that it is better understood than contact-induced change, at least if we go beyond loanwords. How phonological and syntactic patterns spread across language boundaries is still largely unclear. Of course, we will gain a better understanding only by taking more and more data into account, so this volume will undoubtedly contribute to our understanding.

How little we know is brought home by abstract metaphorical formulations such as "changes might have occurred as a result of regional diffusional pressures" (Dench, p. 127), "tone…constitutes an ancient diffusional trait" (Dimmendaal, p. 387), "restructuring of an Arawak language under areal pressure" (Aikhenvald, p. 169). Clearly, all "diffusion" and "areal pressure" will have to be reduced to contact between pairs of languages (and ultimately to language usage by bilingual speakers), but I still see a huge gap between our observations of geographical patterns and individual contact situations. This is particularly clear in the case of large-scale patterns of continental scope (see, e.g., Dryer 1989, Haspelmath 1997:241), which are represented in this volume in the maps in Dixon's and Dimmendaal's chapters. Dimmendaal maps the distribution of cross-height ("ATR") vowel harmony, nasal vowels, noun classes and serial verbs in African languages, and at least the first two seem to show an areal pattern that cannot be explained by genealogical inheritance alone.

In order to make progress in understanding areal patterns, we need to know how easily different linguistic features diffuse (i.e. are adopted from one language to another one). There is no shortage of claims about this in the papers; a selection is given in (4).

(4) a. "Prosodic features are known to be particularly prone to diffusion" (Dimmendaal, p. 376; cf. also Matisoff's chapter)

    b. "The first place to look in grammar for diffusional convergence is in the phonology, as Trubetzkoy noted long ago" (Watkins, p. 52)

    c. "The most pervasive borrowing generally involves categories and constructions [i.e. not forms]" (Aikhenvald & Dixon, p. 2)

    d.   "The category of noun classes is one of the most easily diffusible" (Aikhenvald & Dixon, p. 8)

    e.   "Verbs are replaced at a slower rate than nouns" (Dixon, p. 84)

    f.   "One linguistic domain that appears to be particularly prone to contact-induced change is word order, more precisely the arrangement of main clause constituents" (Heine & Kuteva, p. 395)

    g.   "[P]atterns of clause linkage, and of basic constituent order are features which diffuse quickly across large areas, cross-cutting genetic groupings" (Haig, p. 220)

These claims are typically presented as if they were securely established common knowledge, and in no case do the authors refer to work where solid evidence for these claims has been provided. The only work that has attempted to determine genealogical and areal (in)stability of different features in a systematic fashion, Nichols (1992: ch. 5), is not mentioned at all in the entire volume.[4] But we need more systematic quantitative studies along Nichols's lines before we can make assertions like those in (4) with any confidence. With respect to claims about genealogical grouping, the authors tend to be conservative and cautious; a similar attitude would be healthy with respect to borrowability.

On the other hand, making bold and not fully substantiated claims sometimes serves the useful purpose of instigating others to look for counter-examples or confirmation, so I do not mean to say that such claims are never appropriate. They should not be cited as background assumptions as in (4a-g), but when they are mentioned prominently in the foreground and it is made clear that they are proposals subject to verification, then they may well be helpful in guiding future research. Two such examples are found in the volume, in Haig's and Ross's papers.

Haig discusses a constraint on "linear alignment", i.e. the assimilation of ordering patterns of one language to another one (p. 217–22). He claims that linear alignment proceeds from larger to smaller units, roughly as follows:

(5)   Sequence of linear alignment (= contact-induced order change)
narrative structure > direct speech > topic introduction / tracking >
main / subordinate clause > main-clause constituents >
NP / PP constituents > word constituents

Haig also discusses two possible explanatory factors: Larger units generally allow more constituent-order flexibility and so can more easily undergo change, and they can be matched more easily across languages.

Similarly, Ross proposes the following sequence for the changes associated with metatypy (p. 149–50):

(6)   Sequence of metatypy
      lexical semantic patterns > discourse structure > clause linkage >
      clause-internal structure > phrase structure > word-internal structure

In the final chapter of the volume, Curnow provides a useful general discussion of borrowability and summarizes earlier proposals of borrowability hierarchies and other constraints on borrowing. He considers 15 types of linguistic units and provides examples of borrowing for each of them, generally taken from the other papers of the volume. This is a good starting point for further, more systematic research, but unfortunately, the tone of Curnow's article is somewhat pessimistic: "the probably conclusion is that we may never be able to develop [universal constraints on borrowing]", he writes at the end of his article (p. 434). But this conclusion is not based on any demonstration of repeated failure of proposed borrowability constraints, merely on general considerations regarding the multitude of confounding factors that make it difficult to see patterns. After presenting the earlier proposals, Curnow just notes that "in almost all cases counter-examples have been found" (p. 419), as if a single exception were sufficient to invalidate a claim about a general trend. So it is clear what needs to be done: Existing proposals need to be tested systematically and qualitatively, confounding factors need to be controlled for. Ultimate success is not guaranteed, but there is no reason not to start the work.

6.   Conclusion

When reading this volume, one can get the overall impression that research on areal linguistics is currently still in the hunting and gathering stage. All the articles are rich in data and individual observations, but there is not much systematicity in this research — no sampling or quantitative methods, no evaluation of specific competing models or hypotheses (apart from Dixon's punctuated equilibrium model, cf. §2 above). Perhaps surprisingly for a volume on areal diffusion, only two of the chapters have maps showing the areal distribution of linguistic features.

This does not mean that this is not an excellent book — it is, without any doubt. All the authors are highly competent in their respective areas, all the articles are relevant to the general topic, and having these papers on different

areas of the world together in a single volume makes it possible to try to gain a truly global perspective of the issues. I really enjoyed reading this book, and I recommend it to anyone with an interest in historical linguistics and typology. But the volume does not break any new ground, the way Greenberg (1963) broke new ground for typology when he began to search for universals in an empirical and controlled way, thus propelling typology from its hunting and gathering (and speculating) stage to a stage of systematic "cultivation". This revolution is still ahead of us in areal linguistics, and I see no reason why it could not happen.

Thus, I am optimistic for the future of diffusional linguistics, and the ubiquity of contact influences does not make the enterprise of genealogical linguistics hopeless either. The *Stammbaumtheorie* and the *Wellentheorie* are both right in their own ways, and the challenge for the future is to find better ways of assessing the inherited or diffusional origins of linguistic features. This volume will inspire researchers to take a truly global perspective on these issues.

Notes

1. I use the term "genealogical" instead of "genetic" in order to avoid confusion with biological genetics. A different terminological choice is to differentiate between "linguogenetic" and "biogenetic" (e.g. McConvell 2001), but I doubt that geneticists can be persuaded to rename their science to "biogenetics". Moreover, the term "genealogical" is more transparent than "genetic", because the linguistic relationships in question are indeed family-like, and not merely "related to genesis / origin" in a general sense.

2. The question of whether language dispersals happened through population dispersals can hardly be resolved by purely linguistic or archeological methods. It seems that here the evidence from genetics is highly relevant, and this evidence holds some surprises for linguists (cf., e.g., Sajantila & Pääbo 1995 arguing for language shift from Germanic to Uralic in the prehistory of Finnish).

3. This is pronounced with stress on the second syllable

4. one of Nichols's findings directly contradicts Aikhenvald & Dixon's claim in (4d): according to Nichols (1992: 181), noun classes exhibit high genetic and only moderate areal stability.

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