

REVIEW

REPLY TO REID'S REVIEW OF LLAMZON'S  
HANDBOOK OF PHILIPPINE LANGUAGE GROUPS

It is unfortunate that this reply to Reid's review of my book entitled HANDBOOK OF PHILIPPINE LANGUAGE GROUPS (HPLG), which appeared in the *Philippine Journal of Linguistics* (PJL) in 1979, has been delayed this long. Constraints of time and priority of obligations have prevented me from correcting at once the false impression that his review may have created in the minds of the PJL readers. This is all the more regrettable in light of Reid's stature which could have caused unjustified damage to the credibility of HPLG among the less well-versed readers of the journal. It is, therefore, necessary even at this late date to try and repair whatever dents his review may have caused. I will first comment on the review as a whole, and then answer the specific charges it brought up in the second part of this reply.

Those who have read the review will agree that although Reid tried to be objective in his criticisms, he was unsuccessful in his efforts to maintain this stance of professionalism beyond the first three paragraphs. How else should one characterize an individual who calls the author of HPLG 'amateurish' (p. 117) and someone who 'perpetuates myths' (p. 115), and 'ascribes' terms like *Cordilleran* to an author who never used it (p. 116)? From the fourth paragraph on, therefore, Reid's review turned into a set of unfounded accusations, opinionated statements, contradictions, and nit-picking. Although this is a serious charge, I make it responsibly and will, therefore, substantiate it.

Reid has indulged in overgeneralization in that he has called the entire work into question because of points raised concerning three out of 25 language groups covered by HPLG. He claims that 'because of limitations of space' he could comment on only three groups. Is it merely a coincidence that the three groups happen to be just those he is most familiar with, having studied and written books and articles about them? Why did he not, for example, comment on Tagalog? Is it because this would have given me an advantage? Although the HPLG admittedly has its share of printing errors, oversight and one or two downright mistakes, there was really no justification for the reviewer to declare the whole work 'a failure' (p. 117). This is clearly a case of over-reaction.

Now for some specifics to substantiate the statement made in the second paragraph above. It is false accusation to say that the author of HPLG did not take 'pains-taking care . . . to eliminate errors of fact and interpretation' (p. 109). As regards errors of fact, it seems impossible to eliminate these altogether. I still have to see a perfect collection of published data — and this does not exclude Reid's own handbook (1971). The project HPLG took nearly five years of careful checking of data to complete. The author did actual fieldwork to ensure that the items in the scientific reports used were accurate, and to fill in the gaps in the information for each language group included in the handbook. Moreover, after the descriptions had been written, the draft manuscripts were circulated to the authors of the sources as well as consultants to double check on the contents. The printing process went through several galley proofs. It is, therefore simply false to say that not enough 'pains-taking effort' was exerted to ensure accuracy of fact.

As regards accuracy of interpretation, the reviewer reveals his lack of understanding of the nature of linguistics as a science, when he charges that the interpretations of fact in the HPLG were erroneous (p. 109). In any empirical science, hypotheses and theories are as valid as the evidence presented to support them. Moreover, unless a person is opinionated, he cannot claim that there is one and only one way of interpreting data. I am reminded at this point of Yuen-ren Chao's famous article on 'The Non-uniqueness of Phonemic Solutions of Phonetic Systems' (1934). In this article, Chao calls attention to the fact that there is no one way or unique way of interpreting phonetic data, since the analyst has many possible choices of guiding principles and considerations to adopt and these surely influence his solutions to specific problems. It is, therefore, futile to castigate someone for not viewing data in a certain way and unreasonable to expect everyone to arrive at one and only one solution to a given problem. Reid has done precisely that – taken the author of PHLG to task for not interpreting data his way. We will return to this point later when we discuss Reid's criticisms of the HPLG's account of the probable dates and manner of the early Filipinos' migrations from their original homeland.

It is also false accusation to say that as far as the ethnographic information is concerned, the author likewise has not provided us with a summary of the published material. He has, instead, taken one or two works for each language, and copied sentences verbatim from them (often without appropriate source citations), stringing them together out of context into a *Sunday Magazine* type of popular presentation. (pp. 109-110)

It is simply not true to say that the sources for the ethnographic data used were not mentioned; there were copious references in the introductory essays of the first section, and bibliographical listings in the second section, of the handbook on individual language groups. Reid failed to document this very serious and sweeping statement, in spite of his promise to substantiate all his charges (p. 110). Plagiarism is a grave accusation and failing to prove it is unpardonable. It is also unethical. There is no question about the fact that the ethnographic data presented in HPLG were from published sources, and like all attempts at surveys and syntheses (e.g. Peter S. Bellwood's 1980 account of 'The Peopling of the Pacific'), the ethnographic accounts in HPLG were an effort at putting together the discoveries and interpretations of other scholars. How else can a body of knowledge grow except by accretion and accumulation of sources? The issue is who are the sources, and how reliable/acceptable are their interpretations, and how well does one integrate them for one's purposes? Reid has chosen to ignore these important and obvious considerations.

Reid's contradictory statements in the review are multitudinous, and we will have occasion to point out a few of these as we proceed. Here, it is appropriate to cite just one case on page 109, paragraph 4, where he doubts that the fieldwork undertaken by me was merely 'to fill in the lacunae and to check the accuracy of the data'. He then proceeds to state that all the illustrative statements are 'from fieldwork' (though he does not say how he knew this) and 'suffer from the problems that all fieldworkers face when too little time is spent ascertaining the accuracy of their data' (p. 109). Yet in his very close scrutiny of the Ilokano data, wherein he calls in question almost all the facts presented (including typographical errors, such as *diay kusina* for '*diay kusina* = *idiay kusina*'), on page 112 paragraph 6 he admits that 'the illustrative sentences given

for Ilokano are generally accurate'. In other words, on the one hand Reid doubts that the data from HPLG, which he claims are the result of 'too little fieldwork', are accurate but at the same time he admits that they 'are in general accurate'. This is plainly a contradiction.

It is on this same page (p. 112) that Reid outdoes himself in criticizing the facts presented on the Ilokano language and indulges in nit-picking. In paragraph 4, he points out that *daytay* and *daydi* are demonstratives and yet they do not appear in the paradigm of demonstratives. He is right. They are demonstratives, which are not ordinarily used. If they had been included, a systematic search for counterparts in the other languages included in the HPLG would have been obligatory. Moreover, extensive hours of additional informant work would have, or should have, been undertaken to discover other existing, but not ordinarily used, forms not only in this grammatical category but in the other categories presented as well. This is true, for example of the Ilokano verb particles *panang-*, *pannaka-* and *pannaki-*, which Reid also cites in paragraph 5, page 112. These particles should be included in a reference grammar, but not in a handbook of basic forms such as the HPLG. To hold it against the author of HPLG that these particles were not included is either to show off one's knowledge of Ilokano, or to indulge in silly nit-picking. I prefer to think that, in Reid's case, it was the latter.

There is a rationale to the adoption of a single framework in the presentation of facts concerning linguistic and ethnographic data on a number of languages such as those included in HPLG. The differences and similarities in structure among the languages covered stand out more clearly and it is then easier for the comparativist as well as the non-specialist reader to go about their own tasks. Consider, for example, Trubetzkoy's *Grundzüge der Phonologie* or Hockett's *Manual of Phonology*. Both adopted a single framework for presentation of data. Reid himself adopted a single descriptive framework in his *Philippine Minor Languages: Word Lists and Phonologies*, and so did McFarland in his *Northern Philippine Linguistic Geography*. It is, therefore, unreasonable of Reid to require the author of HPLG to reproduce in their original framework the analyses of data from the published analyses of various authors, as this would require explanations of the models used by the different authors before each set of data is presented. Yet this is what Reid demands when he says 'the analysis of the data likewise is not from published analysis, but is the author's own attempt to fit the linguistic facts . . . into a single framework . . .' (p. 109).

On page 112, paragraph 2, Reid states categorically that the Ilokano particle *iti* 'primarily marks singular common locative NP's. Here is an instance of an opinionated statement, since there are many utterances in which *iti* marks the object, and not locative NP's. The following are some examples from *Bannawag*, a respected and popular Ilokano magazine . . . all taken from page 32 of the January 5, 1981 issue:

Umanay met a pangbiagko *iti pamiliak*, apo.  
'It is enough at any rate to keep my family alive, sir'

Maragsakanak laeng a nakasarak *iti naisangsangayan*.  
'I am happy that I have found someone else.'

Mabalin met a mangilasinka lattan *iti bassit* . . .  
'At any rate it is possible for you to set aside a little something . . .'

Asino dagiti tattao a mangar-aramat laeng *iti uloda iti panagsapulda iti kuarta?*  
 'Who are those people who use only their heads to earn money?'

Reid questions the HPLG's account of the historical development of Philippine languages, which involves not only a reconstruction of the probable date when the early Filipinos left their homeland, but also a discussion of their migration routes and eventual settlement in the Philippines. This is probably one of the most difficult and also most controversial topics in the entire handbook. The truth is that there is no unanimity among scholars on any of these points, and it is typical of Reid's overly-critical approach that he demands that the HPLG present a scenario of Philippine proto-history with which all comparativists would agree (p. 115, paragraph 4).

Concerning the use of glottochronological data, Reid contradicts himself by saying that 'Llamzon's slavish adherence to glottochronological dates goes counter to all the evidence that such dates must be treated with great scepticism' (p. 115, paragraph 5). Yet earlier on (p. 115, paragraph 4) he admits that HPLG 'attempts to date the original dispersal of the Proto-Austronesian speaking peoples by combining an archaeological date for the presence of iron in South China (ca. 1500 to 1000 BC) with a glottochronological figure provided by Thomas and Healey for the separation of the Philippine languages from the Chamic languages of Southeast Asia'. In other words, Reid has denied and at the same time admitted that the glottochronological figure was not slavishly adhered to. There was an attempt after all to find out if the figure could be supported by archaeological evidence.

It is true that in 1971 Dyen did not agree with Kern and Blust that iron was known to the Austronesians in their original homeland, but in 1973 in a paper read at the 29th International Congress of Orientalists in Paris, he admitted that the metal could have been known by them and even become a part of their culture. This is not something startling, since Dyen was satisfied later that there was sufficient evidence for him to accept the hypothesis. What is surprising is that Reid should base his guess on the probable date of the original dispersal of the Proto-Austronesian peoples solely on archaeological evidence. There is no unanimity with regard to the date of dispersal of the Austronesians from their homeland, but there is unanimity that to determine that date all evidence available from anthropology, archaeology, linguistics, and history must be taken into consideration.

Reid rejects the statement in HPLG that there was once an Itneg tribe on the island of Formosa, and in an ironical tone asks a rhetorical question, 'one wonders where this piece of 'scientific' information comes from' (p. 115). The irony is signalled by the word 'scientific' in single quotation marks. The source of the information is Otley Beyer (Reynolds 1959:18) as well as the eminent Filipinologist F. Blumentritt (1882). Like several other Austronesian tribes in Formosa, the Itnegs may have disappeared, but this does not mean that 'the Formosan Itneg is a myth' (p. 115, paragraph 5). Consider, for example, the parallel case of the Sama people. The presence of a Sama group on Capul Island, off Northern Samar, has been reported by several scholars and can be checked by fieldwork, because it is still alive and strong and is maintaining its identity. However, according to Pallesen (Reid 1981), another Sama group established itself in the Cagayan Valley in Northern Luzon, and has since failed as a community either because they returned to their place of origin or was assimilated by other groups in the Valley. This Sama group in Cagayan Valley will soon be a 'myth' in Reid's terms, because not much was documented and not much 'evidence' remains.

The reviewer accuses the author of HPLG with 'perpetuating the myth of sequences of migrations each responsible for a subgroup of Philippine languages' (p. 115, paragraph 6). It seems that the word 'myth' has a special meaning in Reid's usage, or else it is difficult to understand why the copious evidence from the subgroupings presented in HPLG, all of which were from reliable authors (pp. 23 to 27) were rejected by Reid. I should also include my own subgrouping here (Llamzon 1976). The wave theory of migrations has not been totally rejected, as it provides a convenient explanation of hard data. Note, for example, the account of 'Man's Conquest of the Pacific' by Belmont (1978) and his description of the movements of the various groups from the Austronesian homeland to their present locations. What is questioned is the periodic and mechanical implication of the wave metaphor that present groups and traits are unchanged prolongations of the group that represented the first, second, or third wave.

It is quite difficult to see the distinction between 'sequences of migrations' (which Reid rejects) and 'multiple migrations' (which Reid apparently accepts). Moreover, he states that the Philippine languages 'even those which seem to be most aberrant, such as Ivatan, Ilongot, Bilaan and the like, apparently form part of a single language' (p. 116, paragraph 2). This is actually in contradiction to his statements in the paper 'The Demise of Proto-Philippines', which he read in Bali in 1981 at the Third International Conference on Comparative Austronesian Linguistics. In that paper, he maintained that the northern Philippine languages belonged to a different node of the AN language family, while the rest of the Philippine languages belonged to another. Ironically, Reid failed to come up with sufficient evidence for this rather audacious dismemberment of the entire Philippine language family, his argument on the absence of nasal clusters in the northern Philippine languages notwithstanding, because such nasal clusters may well be reflected in geminate consonants, e.g. Ilocano *kiddat* \*kindat 'wink', and Ibanag *kazzing* \*kandij 'goat'. Could not the Central Cordilleran groups subsequently have lost the geminate consonants while Ilocano and the North Cordilleran groups retained the geminates?

This being the case, it is surprising that Reid rejects the interpretation of the Northern Philippine form *sikada* as cognate with the proto-form *sika(n)da*, which is attested to by some Manobo languages among others. One of the characteristics of the Northern Philippine languages, according to Reid himself (1981:67), is that they do not have the (facultative) nasals in words so reconstructed for Central and Southern Philippines. It is also possible that the agreement of Manobo and Northern Philippine pronouns is fortuitous, since it appears that markers like \*si-, \*i-, \*di- and \*da- were analogically attached to pronouns according to dialect-specific innovations and were done so independently.

Reid rejects the account given in HPLG of the development of the Northern Philippine pronouns, especially the form *dida*, which Reid is absolutely sure does not come from *di* 'locative marker' and *da* 'they', both of which have been reconstructed for the Austronesian parent language. Without giving his own morphological analysis of *dida* in the review, he states flatly that no locative marker is involved. In another article, however, Reid (1974:523) explains that the *di* formant is a reduced form of Proto-Central-Cordilleran (PCC) *daqi-*, which is attested to by Bontoc and Kankanay forms. Actually, Reid in the passage quoted above mistakenly says this formant consistently occurs in Kalinga, although it does not appear in his chart of pronouns for Kalinga. At any rate, taking his own evidence as presented in Chart I of the nominative forms in seven Northern Philippine languages, Reid has chosen to ignore the fact that the form unanimously employed by the languages in his chart is *dida* and not *daqi-*, which occurs only in Bontoc and Kankanay. However, since these two languages form a subgroup within the Northern Philippine group, should *daqi-* not be considered an innovation of Bontoc and Kankanay, rather than a retention from PCC? An added problem of assign-

ing *daqi-* to PCC is that its semantic content is uncertain. The formative *di-*, which was originally a locative marker, was redistributed to the nominative and other cases by analogy.

If one follows Reid's methodology strictly and takes the comparative method to its limits, one would have to reconstruct some five nominative pronouns for Proto-Philippine, and hence for even higher nodes up to PAN, namely \**si-aku*, \**si-aken*, \**di-aken*, etc., and this is clearly wrong. What is clear is that there were pronouns, and there were case-marking particles, and the two systems got mixed up throughout 'Austronesia', but this does not justify the reconstruction of numerous pronouns. One needs only to reconstruct a series of case-marking particles, which then got attached to pronouns and also to demonstratives, e.g. \**i-ni*, \**di-ni*, \**a-ni*, \**ya-ni*, \**ha-ni*, etc.

Reid next questions the generalizations made by HPLG about the cultural characteristic of Philippine groups. 'It fails,' he says, 'most noticeably in believing that homogeneity actually exists and is easily describable' (p. 116, paragraph 3). There is no doubt that diversity exists among the language groups, and this is precisely why in the second section of the handbook individual groups were described. This was an attempt to highlight the differences among the various groups, in contrast to the introductory essays that discussed the commonalities among them.

The problem of similarities and differences, of course, is a question of degrees and levels. There are certain traits that unite all the lowlanders together as against the uplanders. While the lowlanders embraced universalistic religions like Christianity and Islam, the uplanders remained unconverted, except for a few. Even in the centuries just preceding Spanish contact, the lowlanders were more exposed to the outside world than the highlanders. See, for example, Colin's accounts in Blair and Robertson (Vol. 40 pp. 38-48), where he contrasts the Negritos with the brown highlanders and with the brown lowlanders. Likewise, Eggan (1954) contrasts the Mountain Province cultures with the coastal Philippine cultures.

Reid accuses the author of HPLG of ascribing the term 'Cordilleran' to Scott (p. 116, paragraph 3). This is simply a misreading of the text of HPLG. The term, as a matter of fact, was used by Reid himself (1974), and before him by Dyen (1965). The term is used to refer to a group of languages and cultures in northern Philippines and is convenient for purposes of the introductory essay in HPLG.

I could continue discussing the specifics brought up regarding the Bontoc and Ivatan materials, but the exercise will only make this reply to Reid's review needlessly lengthy to the general reader and confirm my original statements about the reviewer to those seriously interested in the facts. At any rate, Reid himself provided explanations for the alleged discrepancies between his data and mine on these two languages. There were intrusions by neighboring languages (e.g. Ilocano on the Bontoc forms), and there were instances of dialectal variation. All this should not cause surprise to those who work with linguistic data.

In conclusion, one can only regret that Reid should have failed to recognize the contributions of HPLG towards a better understanding of the Philippine ethnolinguistic scene and chosen instead to call attention to every point in the HPLG that he could not agree with, thus giving an incorrect impression of the book's worth. In effect, what he was saying in his review was 'Llamzon does not agree with Reid's interpretations, and his data differs from Reid's, therefore, Llamzon is wrong'. After this response to Reid's review, I hope that the readers of PJJ will realize that his long list of grievances is, for the most part, unfounded.

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