# Rejoinder to Malcolm Ross's Squib

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# Rejoinder to Malcolm Ross's Squib

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Typological features may be used in an impressionistic bottom-up approach to characterizing and formulating hypotheses on the genetic affiliation of an unknown language in a contact zone—while leaving the proof/disproof of genetic classification and reconstruction to other, generally accepted types of evidence, such as basic cognate sets and cognate paradigms.

The aim of my paper was to present a first typological characterization of the linguistic area of C/E Indonesia, and I suggested three ways in which the overview could be useful: (1) as a synthesis of data that have recently become available, (2) to fill a gap in the (skewed) typological picture of Austronesian languages, and (3) as an instrument in comparative research—synchronic (language contact) or diachronic (364–365). It is this last point that Malcolm Ross takes issue with: typological features cannot be used to make preliminary hypotheses about the genetic affiliation of a yet unclassified language; only morphological forms and lexical data allow us to do this. I cannot but agree with this position, because it is the only type of "individual-identifying" evidence that the comparative method, for very sound reasons, accepts as "probative evidence" to identify a unique individual protolanguage (Nichols 1996:48). The list of features was never intended to act as such evidence, so that the question of the probability of their multiple independent occurrence among the world's languages becomes irrelevant, because—as the title of the paper and the selection of languages under comparison indicates—the features are all taken from languages of the same genealogical group, from one particular geographical area only. Therefore, I cannot have claimed that the typological features can be used to "determine" whether a particular language is Austronesian or Papuan, nor "assign" it "to genealogical groups." Instead, my claim was, much more moderately, that typological features may be used to "characterize languages in contact areas" and "formulate hypotheses" on the genetic affiliation of an unknown language in a contact zone—thereby leaving the proof/disproof of "genetic classification and reconstruction" to other, generally accepted types of evidence, such as "basic cognate sets and cognate paradigms." In fact, I mentioned explicitly that, in most cases, the features are "typical" patterns, quantifying "typical" as patterns found in more than half of the Austronesian languages in my (limited) sample, stating that they reflect tendencies, and are relative rather than categorical statements (366–367). This implies that I did not "report [them

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as] distinctive of the Austronesian languages of C/E Indonesian," or else all the features should have been present in all the languages of my sample.

What I intended (perhaps poorly worded) was to make explicit the impressionistic bottom-up approach a researcher may use when s/he attempts to characterize an unknown language in, for example, C/E Indonesia, an area where two language families with quite distinct typological features are present, where speakers of those languages are known to have been in extensive contact, and where genetic classification is notoriously difficult (e.g., for the members of the CMP subgroup). At the beginning of the investigation the researcher observes certain lexical, morphological, and structural features of the language, compares those with features of other languages that are spoken in the area, and formulates generalizations such as: "This language C is similar to neighboring language A because it has final negation and subject and object prefixes; it is unlike neighboring language B, which has preverbal negation and object suffixes." If the genetic affiliations of languages A and B are known (e.g., A is Papuan, B is Austronesian), then initial hypotheses can be formulated such as: "Language C is Papuan"; or: "Language C has Papuan features"; or: "Language C shows interference with Papuan"; or the like. I have always understood that a similar approach was used in Ross 1996, because this work reports that "Metatypy ['contact-induced changes in structural typology'] and language shift both interfere with a language's correspondence with its genetic kin, and many of the Austronesian languages of Papua New Guinea display this interference to a marked degree" (Ross 1996:182). He presents a table in which features of Austronesian (Proto–Western Oceanic) and Papuan (Trans–New Guinea type) languages are compared, and uses this to describe how Austronesian languages in Papua New Guinea had copied typological features of their Papuan neighbors. My suggestion was that (some of) the typological features in my paper could be used in a similar way.

Having clarified this, many questions of course remain. First, regarding the features chosen: how typical are they for this area? Are they correct? My sample was admittedly small, and more research is needed. The problems Ross points out with respect to using the existence of subject prefixes/proclitics relate to questions like these. Note, however, that the feature as I described it is different from Ross's reproduction of it in the squib, because I refer to subjects expressed as prefixes/proclitics *in conjunction with* object suffixes/enclitics, and *in contrast to* being expressed as full NPs (371–372, 378).

Second: can the features indeed be taken as representatives of the family they are meant to represent? Certainly not all of them, and not in the same way: clearly, there are huge qualitative differences between, for example, the features "a dispreference for homorganic CC clusters," "parallelism," and "head-initial syntax." But recall that the paper was intended as a typological characterization of an area, not as a table of Austronesian structural features. Ross suggests that the non-Austronesian feature of clause-final negation may just as well be Austronesian. This is a legitimate subject of empirical debate. For the empirical motivation (linguistic and historical) of why I assumed this feature to be of non-Austronesian origin, and one that may have spread into Austronesian languages, I referred to various descriptive sources, as well as

unpublished work by Reesink (which has by now appeared as Reesink 2002), Andaya 1993, and so forth. Indeed, Ross is also right in pointing out that my putative Austronesian features could have their origin in a non-Austronesian language, dead or alive. However, all we know at present is that they occur in a bunch of geographically defined languages that are Austronesian. And this information, however limited, could be useful for the study and comparison of languages in this area.

In sum, I agree with Ross that "the idea that one can use typological features to assign languages to genetic groups [is] fundamentally flawed," yet I also believe that such features may be used, among other things, to formulate hypotheses on the affiliation of newly studied languages in complex linguistic areas like C/E Indonesia.

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