Review: PETER MÜHLHÄUSLER
LINACRE COLLEGE, OXFORD UNIVERSITY
AND BOND UNIVERSITY

Of the many issues that this book raises I shall address only two: the question of continuity and the problem of time.

Regarding the question of identity over time and place and what

Keesing refers to as   "spurious discontinuity": Languages are brought into being by all sorts of forces, for instance by acts of identity of members of a speech community, by acts of non-identity (as was the case between the speakers of Chinese Pidgin English), by forced official decree, and so forth. The crystallization (or gelling in Philip Baker's terminology) of a language would seem to be crucially dependent on something like a moderately closed social network. I feel that Keesing misinterprets the nature of the whaling, sandalwood, and bêche-de-mer trades by arguing that they led to "a single early-Pidgin speech community" or "the linguistic community" (pp. 34, 35). Rather, contact between the members of this postulated speech community was tenuous and often only indirect (via visiting Europeans). Structural and lexical identity of a language over time and space depends on reliable and homogeneous patterns of transmission. As Keesing himself argues, the nature of transmission of early Pacific Pidgin differed considerably from place to place and time to time: on board whaling and other vessels (p. 33), adults learning from other adults in the plantations (pp. 56-59), children learning pidgin from returning adults as a second language (p. 55), in the early mixed beach communities (pp. 15-21). That such different modes of transmission and crystallization are signs of a single speech community or likely indices of a shared core grammar seems implausible. My logic leads me to conclusions quite different from those of Keesing.

   I am aware of the fact that Keesing rejects the use of Tok Pisin as   a canonical language, whatever that may mean. Nevertheless, certain observations made during my own fieldwork in this language seem pertinent here. My first observation is that varieties of very different degrees of sophistication can coexist quite happily within a small area. The existence of a highly developed creolized Tok Pisin in Malabang village in Manus Island or Urip village in the West Sepik Province of Papua New Guinea did not influence the second-language Tok Pisin of surrounding areas to any great extent. This suggests that the early presence of centers of creolization in the Pacific (for which Keesing can present no firm evidence) may have had much less influence on the development of Melanesian Pidgin than he suggests. A second observation relates to Aitchison's study of variation in creolized Tok Pisin (1984). She found the speech of small social groups within a larger social network differed significantly from that of other such groups. Moreover, there was evidence of differences even between same-generation members of the same family. Keesing's view that the use of   *we(a)* as a relativizer was common to all Melanesian pidgins in the late 1880s and

his dismissal of my own evidence from Tok Pisin to the contrary (p. 112) contrasts with Aitchison's finding that the "Goroka girls had fully developed relative clauses introduced by the marker *we,* whereas the Lae girls did not" (1984: 17). One needs to add that Tok Pisin was introduced to the Goroka area in the 1950s whereas it has been spoken around Lae since the turn of the century. Continuity of grammatical tradition would seem to have a rather shaky empirical basis.

Let us assume with Keesing, however, that the first occurrence of certain diagnostic constructions was followed by continuous diffusion and transmission. Of the ten constructions that Keesing claims to have been Common to southwestern Pacific pidgins in the late 1880s (listed on p. 112-113), the majority turns out not to have originated among speakers of Oceanic languages. Philip Baker's provisional analysis of the corpus of data collected for the Atlas of Languages of Intercultural Communication in the Pacific Area project (1989) suggests that the majority of them originated in Australia before Melanesian immigration to that country. For instance (using Keesing's numbering):

1. The basic pronouns were first documented as follows:

m e	'I'	New South Wales 1817
yumi	'we' (incl.)	Queensland 1814
yufela	'you' (pl.)	Queensland 1880
alltogether	'they'	Queensland 1858

3. The systematic use of the transitive affix *-im* is first documented for New South Wales in 1826.

7. *-Fela* as a suffix for quantifiers occurs in Queensland in 1848, with attributive adjectives in New South Wales in 1842 and with demonstratives in Queensland in 1842. Contrary to Keesing's assumption (p. 113), *-fela* was not introduced from China Coast Pidgin.

8. Phrasal interrogatives of the "what name" type are first documented in Queensland in 1868.

9. The marking of possession by means of *bilong* first appears in New South Wales in 1826.

I am not suggesting a continuous transmission of these or other features. Rather, I would like to point out that such constructions could arise even where Oceanic substratum is absent. Given the quite considerable typological differences between Australian Aboriginal and Oceanic languages, the similarities of the Pidgin English used by their speakers will have to be explained in terms of linguistic universals or shared superstrate influence and not, as Keesing wants us to believe, substratum languages.

In the above discussion, chronological time features prominently, per-

haps too prominently. The date of first occurrence is not a sufficiently reliable indicator even when culled from a vast corpus such as the one compiled by Baker and myself over many years. What is needed is an approach that distinguishes chronological from relative time. The former concept is needed to answer such questions as: When did Tok Pisin become severed from the Melanesian Pidgin tradition--1880? 1890? 1900? When is a construction first documented for a particular area? When were the first Pacific Islanders repatriated from Queensland?

Separate from these issues is that of relative time. Underlying my entire body of writings on the development of Pidgin English in the Pacific is the implicational or quantum-linguistic model that asks (a) in what order do constructions (rules or rule environments) emerge in   a pidgin language and (b) does the presence of C imply that of B and A for a given lect? Thus, with the third-person plural pronoun, for instance, the question is not so much the chronological one of When is it first documented? but rather queries such as, If speakers use the third-person plural pronoun, will they also use the second-person plural and the first-person plural pronouns? If speakers use plural pronouns, will they also use dual pronouns? Will plural pronouns be used to refer to animates before they refer to inanimates?--and so on.

Along these lines, in my 1981 article quoted by Keesing, I looked at such implicational patterns and found that, for speakers of different ages in the same location, one could establish patterns such as D implies C implies B implies A, but that, at the same time, not all speakers have reached stages D or C (Mühlhäusler 1981:80). The importance of the implicational argument for the universals versus substratum debate is considerable.

The claim is that such implicational patterns as the animacy or accessibility hierarchies provide principled limitations on what can be transferred from another language in what order (not, as Keesing interpreted it, "that substratum models will have an impact on a developing pidgin only at certain crucial points in its development" [p. 171]). They can thus provide an answer to a problem that neither Keesing nor any other substratophile can answer: Why is it that many constructions, rules, or rule environments found in the substratum languages are not borrowed by pidgins, and why do those that are adopted get borrowed in a particular sequence rather than all at once?

Let us briefly return to the pronoun system that Keesing suggests had been established by 1890 (pp. 133-142). Ignoring the observable fact that the distinction between inclusive and exclusive first-person plural pronouns (a typical feature of Oceanic languages) could not be found in

most Melanesian pidgins by that date, it is also quite striking that there appear to be stages in the development of the pidgin pronoun systems prior to 1890. Thus, there is an earlier system consisting of *me* and *yumi,* and a later system where *he, you, youfela,* and *alltogether* were added. These stages were postulated in an article I published long before Baker's data had become available (Mühlhäusler 1986). Inasmuch as pidgins change in complexity over time, any comparison with a static substratum grammar that does not change in complexity must remain unsatisfactory.

I have discussed these matters in much more detail in a forthcoming review article to appear in *Studies in Language.* Baker's analysis, made available subsequent to my writing this review, seems to further confirm the fallibility of Keesing's substratist position.

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