

Comments

Comments on S. R. Fischer's 'Mangarevan doublets: Preliminary Evidence for Proto–Southeastern Polynesian'

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Fischer (2001) has recently published arguments for an early East Polynesian proto-language distinct from Proto–Eastern Polynesian and Proto–Central East Polynesian. Many aspects of the work constitute valuable contributions to fleshing out the early language (pre)history of East Polynesia. Linguists will have many colleagues in archaeology and anthropology who welcome the presentation of some of Fischer's data and conclusions. However, the data do not always mean what Fischer says they do. He deviates from several important conventions in linguistics in his presentation and, in general, the work lacks a well developed sense of language, population, and their interaction in the sociolinguistic sense.

Geologists have a concept called “uniformitarianism,” which involves the general idea that geological formations to be observed today are the result of weathering, sedimentary, glacial, uplifting, meteoric, and other processes that, in the main, can be observed today. While there is no similar term with a similar meaning generally employed in linguistics, some writers (Christy 1983, Labov 1994:21–25) propose that we should simply use the geological terms for our general sociolinguistic concept, which might be stated roughly as “In the main, sociolinguistic processes of prehistory fell within the general range of those to be observed today.”

Fischer violates this general principle in the present work and in an earlier one. Fischer (1992) presents a number of valuable linguistic observations, but then claims that they mean things, in the sociolinguistic sense, that they do not. Paraphrasing criticisms of that work from Marck (1996a and 1996b), the problem with Fischer (1992) is as follows.

He presents a study of the Rapa Nui language (Easter Island) where his grammatical comparisons are a unique addition to our knowledge of East Polynesian internal relationships and are consistent with the view that the Rapa Nui language developed in isolation after an early break from other East Polynesian languages. However, he reaches the unwarranted conclusion that, based on those observations, we can conclude that “one courageous canoeful of East Polynesians were the first and only ones

to arrive on Rapanui until their descendants' historical encounter with Europeans in 1722" (Fischer 1992:187).

Such a conclusion is not recoverable from the comparative method of linguistics. Fischer presents a well developed argument that careful examination of the Rapa Nui language reveals no non-Polynesian substrate, and a similar strong argument that postsettlement (of the "one courageous canoeful") influence from other languages cannot be demonstrated for the period prior to European contact. But we cannot infer from this that there was a single canoeful of initial settlers or that there was no contact with other people from the beginning of settlement to the time Europeans appeared. Neither does it establish that more people over a longer period of time were not involved in Rapa Nui's initial or even continuing settlement through some generations—a century or more.

His data simply show that such potential continuing influxes and later contacts had no perceptible linguistic results. Such lack of impact is actually common in what we know of same-source immigration and of even very meaningful later contacts from elsewhere in terms of world languages, even with borrowing of material and subsistence items and their sociolinguistic results.

It must be confusing, to nonlinguists, to read some of Fischer's work and wonder how a linguist can infer the things he does. We cannot, and the present work (Fischer 2001) contains more of such errors. In addition, there is substantial misuse of historical and comparative linguistic concepts and terminologies.

There are two general kinds of problems with Fischer (2001). The first involves the linguistics. The second involves a kind of circularity, with archaeological and linguistic inference feeding upon each other more than they rightly should. The problems occur in that general order, so I will give my comments by page or section of the article, with a final summary.

The main linguistic problem I see is a lack of clarity about when we can refer to components of a language as substratum, and how that directs or informs us when we then have to talk about inheritance and whether it is direct or indirect. Traditional linguistics, for instance, classifies Mangarevan as a Marquesic language because the preponderance of the phonology, lexicon, and grammar is of Marquesan origin. A simple analogy is the classification of English as Germanic due to the basic history of the language being more intimately tied to the incursion of Germanic speaking peoples than it is tied to Celtic, even though a Celtic substratum is known to exist.

What Fischer (2001) does is similar to calling English Celtic rather than Germanic. He demonstrates more non-Central–East Polynesian substratum in Mangarevan (sections 4.3 and 4.4) than is known for Celtic in English.¹ Still, the convention in linguistics has always been to classify a language according to its obvious main component. The obvious main component of Mangarevan is Central–East Polynesian and Marquesic, and Fischer is not free to invent his own meanings for well established linguistic terms. It is therefore odd and undisciplined for Fischer (2001: figure 2) to chart Mangarevan as something other than Marquesic, implying that the so-called Southeastern Polynesian component is more basic to the language.

1. Other than place names and family names.

Section 4.5 is especially odd in this respect and reveals a disregard for clarity, weaknesses in sociolinguistic vision, or both. In section 4.5, Fischer presents “depleted” doublets” of Mangarevan, words of clearly Marquesic origin for which there is no substratum doublet. To claim that some old doublet has been lost is a little too adventurous. A sociolinguistic scenario in which these “depleted” doublets might have “developed” has not been established. It is more likely that the doublets never occurred in the more or less instantly dominant speech of the intruding Marquesic speakers, and that they then never occurred in the speech that descended from that group into modern Mangarevan. Statistically speaking, there may actually have been a very few doublets that once existed in the dominant Marquesic-based speech that Fischer’s “selected examples” come from. But we are not free to use or introduce such terminology without defining it. Fischer has done so, and it is misleading of him to use the “depleted doublet” term with its implication that doublets once existed on any larger scale. The nonspecialist might assume the linguist has some way of knowing this, and we linguists actually do not. We do not even know if the partners of the presumed doublets occurred in the idiolects of the surviving substratum language, for that speech has died out. To say that the examples in section 4.5, and the main body of Marquesan vocabulary in general, are depleted doublets really goes too far.

It is possible that Stratum 1 pronunciations were more or less known to the Stratum 2 speakers during the years and decades in which they were first in coresidence (the fact of survival of *any* Stratum 1 speech at all suggests that it wasn’t a sudden conquer/slaughter-all situation). While allowing that much of the Stratum 2 speech may have been known to Stratum 1 speakers, and vice versa, the sociolinguistic context was perhaps a bit more like code-switching for Stratum 1 people or passive competence for Stratum 2 people. Actually, we don’t even know *that*, and the assertion of “depleted” doublets (that probably never existed) suggests full merging/mixing of the dialects and, later, magically, reversion to mainly Marquesic pronunciations.

The argument at the end of section 4.5 about the replacement of **niu* seems to discount the possibility that Proto–Nuclear Marquesic² still had **niu* compounds at the time Stratum 2 Mangarevan diverged from South Marquesan. Possibly a better example could be offered.

Fischer (2001:119) begins section 5 with: “MGV’s ‘full’ and ‘depleted’ doublets of ancient provenience provisionally indicate that MGV immediately derived, like Rapa Nui, from a language anteceding the differentiation of Proto–Central Eastern (PCE).” I have already protested Fischer’s use of terminology and his lack of sociolinguistic vision. Here he tangles those problems together with a more basic problem: he really has no argument for his “Southeastern” subgroup, and because he does not, it should not be surprising to learn that, even if such a group could be demonstrated—as Fischer has not done—there would still be no ordering argument that necessarily followed with which to assert that Proto–Southeastern “antecedent” the differentiation of Proto–Central Eastern.

2. As Marck (2000) defines it: the Marquesan dialects plus Mangarevan as in Green (1966 and 1988) and Fischer’s (2001: figure 1) faithful representation of such.

The only thing to be gleaned from his actual evidence is that some of these languages (e.g., Rapa Nui) and substrate (e.g., Stratum 1 of Mangarevan) are non-Central Eastern. This means they have the status of unclassified East Polynesian languages that stand outside Central–East Polynesian. Fischer has shown no common innovations. Subgroups have *shared innovations* and Fischer has shown us none for “Southeastern” Polynesian. Therefore, no such subgroup can be claimed to exist.

The fourth paragraph of section 5 (Fischer 2001:119) asserts that “the Marquesic intruders subjugated the related Mangarevans.” That is, that the Stratum 1 people were socially dominated by the Stratum 2 speakers. In fact, this cannot be concluded at all. The Stratum 1 speakers could just as easily have remained sociologically dominant, while due to larger numbers, the Stratum 2 speech became ultimately more dominant. Fischer’s conclusion is perhaps more likely than my alternate suggestion, in terms of known cases of a similar demographic and sociolinguistic profile, but his assertion is conjecture, and it is inappropriate for him to conclude, as fact, that a particular sociolinguistic relationship obtained, for there are a number of scenarios that are known to have produced the particular linguistic results he observes.

Also unsettling to the linguist is the statement beginning in the fourth paragraph of section 5 (Fischer 2001:119) that “several centuries after the initial PSE settlement of Mangareva, a specifically Marquesic language—that is, one that now contained the PCE innovations and was also distinct from the Tahitic subgroup of languages—engulfed MGv.” We (linguists) wouldn’t know if it was several centuries or a millennium, at least not with the slim evidence available to us at the moment; and we would not be talking about the “PSE settlement” of Mangareva because, in the first instance, PSE has not been demonstrated, and if it had, we would be talking about “settlement of Mangareva by SE speakers,” because “PSE” would be a language rather than its speakers, even if it could be shown to have existed.

Having said that, we can look back on the statement in paragraph three of section 5 (Fischer 2001:119) and reflect upon Fischer’s statement that “PSE differentiated, while Marquesic and Tahitic remained one linguistic community.” If the nonlinguist finds him/herself asking how linguists could know such things, the answer is that we cannot. Again, Fischer has a particular scenario or sequence of events in mind and has assumed that certain things would just naturally follow. It is undisciplined linguistics and cannot be offered up as language science.

Fischer has only, apparently, a geographical intuition that Rapa Nui speech emerged out of the “Southeastern” Polynesian localities. He has no linguistic innovation or ordering argument. By linguistic evidence, Rapa Nui speech could just as easily have come from anywhere else in the geographical center of East Polynesia prior to the innovations of Proto-Central–East Polynesian.

Fischer has not considered the importance of all the many ordering and subgrouping possibilities of his own work: Rapa Nui and “Southeastern” could have emerged more or less simultaneously (but separately—our method cannot tell), “Southeastern” could have emerged before Rapa Nui but before the onset of the Central Eastern innovations, or Rapa Nui could have emerged before “Southeastern” but before the onset of the Central Eastern innovations. Fischer considers only the possibility that

Rapa Nui emerged from “Southeastern” after “Southeastern” emerged from other EP. There is no linguistic ordering argument for that possibility sustainable from his present data.

Fischer would have done better to have given certain conclusions first, such as: “Evidence presented here shows that Mangarevan (a Marquesic language [Green 1966]), a Nuclear Marquesic language (Marck 2000), or even a South Marquesic language (Fischer’s own worthy evidence and conclusions) has significant substratum from a previously unrecognized East Polynesian lineage. That lineage is some sort of non-Central–East Polynesian linguistic tradition that existed in Mangareva and, perhaps, nearby islands prior to the intrusion of Nuclear Marquesic speakers (i.e., *non-CE* traditions extant around Mangareva *after* the divergence of Marquesic from other CE, *after* the divergence of Hawaiian from other Marquesic, and *after* the initial divergences of North from South Marquesan.” Then section 4.5 could be dropped, the depleted “doublets”, in the statistical probability sense, never having existed in a single ideolect or Stratum 1 / Stratum 2 communolects anyway.

One must also dismiss the final paragraph of section 5 (Fischer 2001:120–121), which states that “one cannot stress strongly enough the historical significance of MGv’s first stratum being pre-PCE (thus about A.D. 500) and second stratum PCE (about A.D. 1150). It suggests that the original settlement of Mangareva anteceded those of Hawai’i and Tahiti. By logical inference, it also anteceded those of Pitcairn, Henderson, and Rapanui. In Eastern Polynesia, only the Marquesas and Eastern Tuamotus appear to have been settled before Mangareva.”

Linguists wouldn’t know the general dates of Strata 1 and 2, or at least we wouldn’t be citing dates without telling the reader why. We wouldn’t know if Stratum 1 speakers were the first Polynesian speakers to settle Mangareva, and we wouldn’t know if this happened before the Central–East Polynesian settlements of Hawai’i and Tahiti. As Fischer doesn’t tell us by what “logic” it anteceded the settlements of Pitcairn, Henderson, and Rapanui, we cannot know whether to evaluate the utility of that suggestion by archaeological, linguistic, or other criteria. If he is speaking of the logic of linguistics, we cannot.

In section 6, Fischer gives us his first evidence for the social dominance of Stratum 2 speech over Stratum 1 speech. The data and arguments are not in error, but the data are scant and are presented *after* he has already *assumed* the validity of the general assertion in other sections. Section 6 “concludes” (Fischer 2001:121) that Stratum 1 “is manifestly pre-PCE” while we have seen that it is simply non-Central–East Polynesian and that its *divergence* from other Central–East Polynesian was “pre-PCE”. It is formally incorrect to speak of a living language³ as predating a parallel protolanguage, when our method cannot determine the relative age of the two protolanguages concerned (PCE and “PSE,” if such ever existed).

Section 6 (Fischer 2001:121) also concludes that “Southeastern Polynesian, evidently Proto–Eastern Polynesian’s first differentiation that occurred around A.D. 500, would have been the original language of the settlers of the Eastern Tuamotus, Mangareva, Pitcairn, Henderson, and Rapanui. Southeastern Polynesian would have

3. Stratum 1 as it existed at the time of the arrival of Stratum 2 speakers.

comprised the region's dominant language for several centuries." In fact, it could just as easily have been a general, dispersed language area of post-Proto-East Polynesian speakers that had no common innovations other than those of Proto-East Polynesian, a language for which the cautious linguist remains loath to suggest absolute dates. Fischer feels that Tahitian must have "diverged" from other Central Eastern through migration when, in fact, it could just as easily have participated in a period of Central Eastern innovations *in situ*, innovations passing between dialect centers in the Marquesas, on the one hand, and Tahiti, the Cooks, and NW Tuamotus on the other. Fischer's attribution of "PSE" innovations (he has demonstrated none) spreading to Pitcairn, and so forth, rather than developing *in situ* after initial dispersion, lacks similar vision.

I find no evidence offered by Fischer to the effect that the Eastern Tuamotus, Pitcairn, Henderson, and Rapa Nui show evidence of Stratum 1 speech, his only "evidence" offered in support of "Southeastern Polynesian." He could not have offered such evidence, as he has not told us what is unique about Stratum 1 speech—what innovations it shares with other dialects in respect to Proto-East Polynesian speech. The exceptions are the unnamed, unnumbered lists of consonant correspondences (Fischer 2001:119–120), which show nothing unique about "SE" Polynesian or how they are justified in terms of the purported members of the purported group.

Fischer's "Southeastern" Polynesian is simply unclassified East Polynesian languages that may have diverged at any time before the development of Proto-Central Eastern. They are of the same phylogenetic status as Rapa Nui has been (divergent from other EP at the highest level), and, beyond Stratum 1 Mangarevan, Fischer has offered us no evidence of the "group's" membership.

Mangarevan is a Nuclear Marquesic language with a non-Central-East Polynesian substratum (c.f. Biggs 1978:713). The preponderance of phonological, lexical, and grammatical features is Nuclear Marquesic rather than whatever remnants of Southeastern Polynesian might eventually be reconstructed—if, as remains possible, such a group ever existed. As Fischer (2001) did not demonstrate a group, it is vacuous, inappropriate, and misleading for Fischer (2001:122) to present figure 2.

Fischer (1992) presented some trenchant, valuable comparative linguistics that concluded with a few sentences grounded in cursory and erroneous sociolinguistic notions.

Fischer (2001) not only contains inadequate sociolinguistics, it lacks disciplined comparative linguistic work as well. Fischer's (2001) contribution in sorting through and showing something of the extent of Stratum 1 speech in Mangarevan is important and should be welcomed. But his enthusiasm is far out in front of his organization in terms of the larger issues, and there is little that can be accepted from his presentation of the associated subgrouping or culture history problems.

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