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First, I want to thank the distinguished scholars who took the time and trouble to review my book so carefully and constructively. ¹ Responding to their comments provides an opportunity to clarify my views on some issues, to restate parts of my argument that have been misunderstood, and to add some new and important pieces of evidence that have come to light since *Melanesian Pidgin and the Oceanic Substrate (MPOS)* was written.

Let me begin with Siegel's commentary, since it sets out a useful summary of the argument of *MPOS*. This will allow me to note some points on which Siegel's reading differs from the one I had intended and to clarify some terminological differences.

An important starting point is the question of how various lexical items, and what became grammatical forms, got into the Pacific in the first place. I entirely agree with Siegel (and Baker and Clark and Troy and now Mühlhäusler) that many of the lexical forms that became established in Pacific Pidgin, and some incipiently grammatical forms (such as *-fela* and *-im*), came into the Pacific Islands by way of New South Wales English or Chinese Pidgin English or both.² I have developed the theme further in my unpublished paper on *-fela* (Keesing n.d.b). I have compared notes closely with Jakelin Troy on her research on New South Wales English for the last five years and its relation to my own. Both the eastern coast of Australia and the Pacific Islands were connected into a worldwide network of whaling and commerce;³ and the Australian frontier inherited the same traditions of military/maritime English and "native talk" as other margins of the expanding British empire. Elements of this tradition were, I argue, *reanalyzed* by Pacific Islanders to fit grammatical patterns broadly common to their own languages. Despite Mühlhäusler's comment, the early use of particular pronoun forms outside the Pacific Islands is irrelevant to my argument (although the ones he lists, citing Baker, may well have been introduced into Australia). I am talking about a developing linguistic code, not the lexical bits and pieces incorporated into it.

Siegel and I obviously are talking past one another regarding certain terms and issues. I see no point in arguing about whether a developing medium of intercultural communication in the Pacific at a particular point of time (say, 1855 or 1860 or 1865) was a "pidgin" or a "jargon," given the different senses assigned to these terms in the literature. In *MPOS*, no theoretical weight of any kind is hung on the distinction between "jargon" and "pidgin" (although I agree that greater theoretical clarity and consensus regarding this issue is needed).

Since Siegel misunderstands what I was trying to say about "stabilization," I need to clarify that. I used "stabilize" in an intentionally general sense to refer to the progressive regularization of linguistic patterns. One sort of regularization is grammaticalization of forms. The verb ending *-im* discussed by Siegel will serve to illustrate. I infer that in the 1840s *-im* was being used sporadically both by Europeans (drawing on their tradition of "native talk") and by Pacific Islanders, emulating the speech of whites and finding in *-im* an analogue to transitive suffixes

pervasive in their native languages. During the next forty years, progressively changed into a fully grammaticalized transitive suffix. Perhaps Siegel and I differ in our guesses (and that is all they can be, given the thin evidence) about how far *-im* had gone down this track by 1860, 1870, or 1880. A second sort of regularization is standardization of constructional patterns (such as the use of *blong [mi]* for possessives); a third sort is standardization of particular lexical forms.

When I talk about the degree of regularization or expansion of developing Pacific pidgin at any particular point in time, it is with reference to those speakers who had the most fluent command of the code and whose speech served as the target language in its further diffusion. In a zone where a pidgin is used, there will always be participants who speak it badly (that remained true of most Europeans through most of the nineteenth century and was true of Pacific Islanders on the frontiers of labor recruiting as they expanded). It should come as no surprise that in the 1870s we find in historical texts many instances of verbs used transitively without *-im*. I infer that the form was well on its way toward regularization in the 1870s, but had not become fully grammaticalized across the range of transitive verbs until the late 1880s. (Within the past century, further fine-tuning of the marking of transitivity has continued, a point Siegel mentions and one I illustrate with regard to a text below.)

A consequence of the differential command of a developing pidgin is that the discovery of historical texts (such as that I set out below) showing a more fully developed pidgin at a particular date than prevailing theories lead us to expect can force us to revise the time scale backward, but we cannot (contra Mühlhäusler) be similarly forced to revise the record forward by finding fragments of less developed pidgin, unless they constitute an extended corpus of the speech of ships' crews or other sophisticated speakers. Since my argument in *MPOS* hinges heavily on the most fluent speakers of a developing pidgin and their role in its expansion linguistically and its diffusion geographically (and since this seems to have been misinterpreted), let me again summarize my claims:

1. From 1855 (or so) onward, the most fluent speakers of a developing pidgin were *Pacific Islanders*, not native speakers of English (some of these islanders may also have commanded a register much closer to standard English, used when talking to Europeans).
2. They were primarily speakers of Oceanic Austronesian languages (speakers of Gilbertese, various Loyalties languages, Pohnpeian, Rotuman, and Fijian were prominently represented).
3. These most-fluent-speakers worked in key positions, notably on

ships but also on shore bases and as foremen in plantation settings, where they were the primary agents of diffusion of the code, both to Europeans and to fellow Pacific Islanders.⁴

Siegel errs in attributing to me the claim that "stabilization had occurred by 1860." Stabilization (in the sense in which I use the term) was a gradual process that was certainly still going on (in the New Hebrides, the Solomons, and New Guinea) in the early years of this century. As I show in examining pronominal forms, stabilization/regularization was further along in some parts of the developing grammatical system than in others (and I advance arguments about why this should have been so). I do argue that an important phase in the expansion and regularization of a developing Pacific pidgin probably had taken place by the time the Labor Trade began in 1865, an inference I base as much on sociolinguistic as linguistic grounds. I make very few specific linguistic claims regarding pidgin as of 1865, precisely because the textual evidence is so thin. What I suggest is that the code had expanded enough that by the 1860s it apparently could be a primary medium of ongoing quotidian social life on the ships, rather than a stripped-down medium for sporadic communication about work-related tasks. Although I speculate that there were probably fluent childhood speakers for whom this was a coordinate first language, this is only a guess, and one on which no argument in *MPOS* hinges.⁵ Whatever the linguistic nature of this code, I hypothesize that it constituted the initial medium of the Labor Trade, disseminated by the Pacific Islanders who acted as its key agents (hence, contrary to some widely held views, pidgin did not have to be invented by islanders thrown together as recruits and plantation workers).

My general claim is that, decade by decade, the ongoing process of stabilization/regularization/grammaticalization had advanced considerably further than most authorities have asserted. I argue that the entire time frame for the development of pidgin needs to be pushed back by ten or fifteen years from the timetable most specialists have posited, whatever index of that development we choose to use (i.e., whether we use regularization of usages or expansion of constructional possibilities or global syntactic complexity). Siegel himself comments that "Mühlhäusler simply has his dates wrong" (something the latter has yet to concede despite his professed reverence for "chronology"). I believe that Siegel (and Clark) and I differ more in our discursive practices than in substantive questions of who-was-saying-what-when. It is clear, however, that we read the same texts in different ways and that the texts presently available allow of such alternative readings. I believe

that additional new evidence (such as that set out below) will require them progressively to accept the general picture I present. Time will tell.

I believe Siegel misrepresents my position on a number of other points, although it would be tiresome to detail all of them. For example, my sketch of Oceanic subgrouping (pp. 65, 68) indicates quite clearly that I do *not* include Loyalties/New Caledonia languages or South Hebridean in a putative Eastern Oceanic subgroup. What I do claim, citing data from Iai (Uvea) and Sie (Erromanga),⁶ is that these languages incorporate all key grammatical elements of what I call the “core” Oceanic pattern (notably, pronominal elements referencing implied subject and object noun phrases), but that these elements are heavily cliticized, operating as bound particles marked on verbs and aspect markers. I leave to syntactic theorists the question of whether speakers of such languages could have been primary agents in the creation of an interlingual code in which the equivalents of such cliticized forms were free morphemes (as they would have had to be to be intelligible to superstrate speakers);⁷ but they certainly would have found such a code congruent with their native languages. Siegel appears to misunderstand my argument with regard to “core grammatical patterns” and a putative Eastern Oceanic subgroup. What I claim to be the “core” pattern for marking subject-reference and transitivity has been reconstructed for *Proto-Oceanic* (not only Proto-Eastern Oceanic). Elements of it are manifest in *all* Oceanic Austronesian languages I have examined. Some of these languages (including Southeast Solomonian, Nuclear Micronesian, North Central Hebridean, and Fijian-Rotuman, which are very provisionally and problematically subgrouped as Eastern Oceanic) are extremely conservative in preserving the “core” Proto-Oceanic pattern; others are considerably less conservative (including the New Guinea Oceanic languages, South Hebridean, and those of New Caledonia and the Loyalties, but also in some respects including Polynesian languages, as Belikov notes, although they are Eastern Oceanic).

On the question of separation between a putative Micronesian Pidgin and a Melanesian Pidgin, I believe Siegel overlooks the crucial linkages between the German plantations and colonial centers in the Marshalls and the plantations of Samoa and Neu Guinea. Hensheim came from Jaluit in the Marshalls to Neu Guinea; I note his observations, as cited by Schuchardt, that clearly indicate that the same pidgin was in use in both areas (pp. 58-59; Governor Solf implies the same). Hensheim told Schuchardt that this regional pidgin was represented only to a limited

degree in New Britain when he arrived in 1876, but that it had spread rapidly in his early years there.

I would not rule out the possibility that speakers of Bismarck's languages could have had some minor influence on the pidgin developing in Samoa and Queensland. However, these were fellow speakers of Oceanic languages (albeit ones that have substantially modified the "core" Oceanic syntactic pattern) and, demographically speaking, they were a drop in the linguistic bucket--a clear minority even on Samoan plantations until well into the 1880s. I remain convinced that the pidgin originally introduced into Samoa was the one spoken by the linguistic brokers I discuss above and that linguistic changes taking place in Queensland and its major recruiting zones were continuously fed into the Samoan plantations (for reasons I make clear in chapter 5). I have yet to see any clear evidence that "Samoan Plantation Pidgin" was a separate dialect, as Mühlhäusler claims quite unambiguously in several places. (Mühlhäusler's argument regarding the separate origin of Tok Pisin is ambiguous on several points but I cannot accept Siegel's reading of it, in the light of Mühlhäusler's repeated assertion that Tolai is Tok Pisin's only significant substrate language.)

I agree with Siegel that my interpretation of the early texts is colored by a theoretical argument; so, inevitably, is any counterinterpretation. That is why I rely so heavily on distributional evidence as a basis for inferring a stratigraphy of Pidgin development. Since Siegel mistakenly imagines that I break my own "ground rules" in discussing "prepositional verbs," let me state these "rules" yet again, and as clearly as I can.

1. Where we find a syntactic pattern or lexical usage in Tok Pisin, Bislama, and Solomons Pidgin, we can assume that it was present (although not necessarily fully regularized) in Southwestern Pacific Pidgin as early as 1885.⁸

2. Where we find a syntactic pattern or lexical usage in Bislama and Solomons Pidgin and *not* in Tok Pisin, we can assume that it emerged (although it was not necessarily fully regularized) after 1885 and prior to 1905.

3. Where we find a syntactic pattern or lexical usage in Bislama and not in Solomons Pidgin, or vice versa, we can assume that it emerged after 1905.

Some qualifications need to be made to these guidelines. One is that once crucial elements of a paradigm or pattern have emerged within a speech community, it is quite plausible that after separation of daughter dialects the pattern might be augmented or completed in similar ways in these dialects. A grammaticalization process set in train has a kind of

internal logic that can be realized in parallel among separated languages (that, I think, is what Siegel means by “embryonic structures,” “planted seeds,” and “mature forms”). I illustrate at some length how pronominal paradigms have inner logics, whereby a set of slots is filled in. Clearly the process of paradigm stabilization had not run its full course as of 1885, when Tok Pisin split off. Thus, while we find a close correspondence between the present pronominal paradigms, some interesting differences remain (such as the Tok Pisin use of *entupela* for “they two,” where Solomons Pidgin has simply *tufala*).

It is also plausible that a *very few* similarities in grammar or lexicon between Tok Pisin and Bislama/Solomons Pidgin (or between the latter two) have emerged after their geographical separation, either by chance or by diffusion (through continuing contact between plantation communities). What is not plausible is a massively long list of such similarities, such as those connecting Tok Pisin and the other two dialects. A further qualification is that it is quite possible that a form or pattern that was present in the regional pidgin as of 1885 subsequently disappeared in Tok Pisin (perhaps because of the lack of substrate support, a possibility Siegel notes). It is *possible* that “prepositional verbs” were being used in Queensland prior to 1885, but that they disappeared in Tok Pisin.⁹ But, in fact, I follow very strictly my own “ground rules” in accepting as more probable the emergence of prepositional verbs in Queensland and its recruiting areas during the period 1885-1905.

Talk of “embryonic forms” could distort the picture if it suggests that the Pidgin of the late nineteenth century was too limited in its syntactic resources or insufficiently regularized to permit connected and elaborated narrative discourse. I cite below a Pidgin text I recently discovered, recorded in the Solomons in 1893. It shows compellingly that by a century ago Pidgin had achieved virtually all the syntactic richness characteristic of contemporary “bush” Pidgin in the Solomons or Vanuatu and many of its present forms.

I believe that Siegel underestimates both the degree and historical importance of regional variation *within* modern Pidgin dialects. Siegel claims, for example, that in Bislama *-fela* is not used with demonstratives (*dis-fela*). Tryon’s and Charpentier’s evidence,¹⁰ however, shows that in different parts of Vanuatu three different demonstrative patterns occur (seemingly corresponding to different patterns in the substrate languages): *ples ia* ‘this/that place’, *dis-fela ples* ‘this place’, and *dis-fela ples ia* ‘this place’. In this case and a number of others, Tryon shows patterns supposed to distinguish Tok Pisin from Bislama are found in some regional dialects of Bislama and attested in nineteenth-century texts.¹¹

Belikov raises several interesting points. One is the issue of why speakers of Polynesian languages, which have gone off in some syntactic directions that deviate from the ancestral Eastern Oceanic patterns, did not leave a stronger imprint on a developing Pacific Pidgin. The Polynesian presence in ships' crews and beachcomber colonies was marked prior to 1850, declining sharply after that. My reading of the evidence is that such elements as transitive suffixes and resumptive pronouns as equivalent to Oceanic subject-referencing pronouns (SRP's) were only beginning to be used in the 1840s and 1850s. By the time the generation of fluent speakers of a developing pidgin emerged in the period 1850-1860, Polynesian speakers had become a marginal presence.

A second point is the question of relexification. I infer that lexical items in Tok Pisin derived from Tolai and other Bismarcks languages were introduced into New Guinea Pidgin after its separation from the regional pidgin from 1885 onward. Such forms as *diwai* and *liklik* replaced *tri* 'tree' and *smol* 'small', and so forth, which we know were present in the regional pidgin of the 1880s. (If it is accepted that Tok Pisin is historically derived from a regional pidgin used in Queensland and its recruiting areas as well as in Samoa, I don't see how this can be disputed.) The evidence Belikov gives does not address these forms, but simply indicates that all three dialects have continued to add (inevitably different) lexical resources from English in the century since Tok Pisin separated from the others. 12

What I mean by my claim that by the end of the 1880s "there was no room or need to expand [pidgin's] syntactic possibilities" should be clear from Pionnier's early 1890s texts and the text from Solomons Pidgin quoted below, from 1893. That is, by a century ago, Melanesian Pidgin incorporated syntactic structures that allowed complex, multiclausal sentences and extended, rich narratives. Obviously, room existed for further expansion, syntactically as well as lexically (and a good deal of that has occurred in the recent creolization of Pidgin dialects, as documented by such scholars as Sankoff, Romaine, and [for the Solomons] Jourdan 1985b).

Bickerton's comments are positive and helpful. As he notes, *MPOS* is by no means a blanket argument for substratomania. Rather, I am arguing (as he himself has) that Melanesian Pidgin is a very special historical case. First, the substratum languages are relatively homogeneous and their speakers had ample sociolinguistic room (in the context of shipboard and later plantation communication, with limited exposure to standard English) to leave a strong impress on the developing pidgin. Second, Pacific pidgin remained a second language, mainly learned by

adults, over the span of four or five crucial formative generations, without (in the New Hebrides, the Solomons, and New Guinea) either the hegemonic presence of a superstrate language or the breakdown of the plantation system. In these special circumstances, I claim (1) a pidgin can become much richer syntactically than pidgins are ever supposed to get, without turning into creoles; and (2) a pidgin can incorporate global patterns broadly common to substratum languages, where these are sufficiently congruent with superstrate patterns and compatible with general strategies of language learning/simplification. I had not intended to extend my argument to Atlantic creoles or to pidgins elsewhere in the world, or to enter into debates regarding the special Hawaiian case to which Bickerton refers.

Bickerton is right, and constructive, in suggesting that I should have given more examples from Gilbertese, Rotuman, and so forth (the data I have on these languages indicate that they manifest the Oceanic patterns I discuss, but further examples of this would have been helpful); and in pleading for maps. The difficulty with maps of the Pacific is that there is so much water and so little land. Perhaps a foldout map will be possible if a second edition proves feasible.

Romaine asks, "How can we separate substratum from superstratum influence?" I argue in *MPOS* that in many cases we cannot and need not. That is, where there is convergence between substrate and superstrate or congruence with universal patterns of minimal markedness, speakers of the different languages involved in multilingual interaction can get to the same place by different routes. However, in chapters 7, 8, and 9 I cite a series of morphological and syntactic constructions where Melanesian Pidgin incorporates patterns that are *unmistakably* modeled on Oceanic (rather than English or universal) grammar. Romaine asks why, if the inclusive/exclusive distinction is opaque to New Guinea speakers, "it was incorporated in the first place." I thought I answered that question. It was incorporated *somewhere else*, by speakers of languages where such a distinction is natural and obligatory (cf. Mühlhäusler's comments above; also in 1987a and 1989). Having been transplanted to "alien linguistic soil," as I put it, Tok Pisin is being pushed in the direction of radically different substrate languages. Scant wonder that such semantic distinctions, and the so-called predicate marker, are in some jeopardy.

Romaine devotes most of her attention to future marking. I have published a long article on future marking in historical perspective (Keesing 1985), which Romaine does not mention, so here I will be brief. First, I agree with her (on the basis of my textual evidence and

Solomons data) that two separate issues have become entangled in the literature: the reduction of *baebae* to *bae*¹³ and the incorporation of the form into the verb phrase as a grammatical element. My data on *bae* in rural and older forms of Solomons Pidgin and Jourdan's data on urban speakers (1985a) indicate that the use of the long or short form carries no grammatical weight.¹⁴ The short form is more common in the urban dialect, however, following a general pattern of streamlining and phonological reduction (Jourdan 1985a:76-78).

The question of grammaticalization is complicated, as Romaine indicates. I show (1985) that deciding what is "preverbal" and hence grammaticalized is by no means straightforward (because of a verb phrase pattern in which pronominal subject markers intervene between tense-aspect markers and verbs). I have argued that there was a continuous pull by Oceanic speakers to grammaticalize *baebae* within the verb phrase, Oceanic-style (and assign it an irrealis as well as time reference), and a countervailing continuous pull by English speakers to keep it in clause-initial position as "by and by." Of such linguistic tugs-of-war are pidgins fashioned. In short, I agree with practically all of Romaine's argument: with her separation of the *baebae* → *bae* shift from the question of grammaticalization, with her conclusion that "preverbal position was a potential slot for the positioning of grammatical markers long before creolization or extensive phonological reduction," and with her observation that this is "consistent with [my] claims for early stabilization."

Romaine misrepresents my disagreements with Mühlhäusler about counting of forms. She refers to a controversy about whether (as I claim) the Eastern Oceanic cast of the Solomons Pidgin pronominal system dates from the beginning of this century or whether (as Mühlhäusler has claimed in several papers) it represents a more recent and conscious linguistic change, an attempt by Malaitans (in the 1920s or 1930s) to distance themselves from Europeans. In demonstrating that he is wrong (Keesing 1988, 1991), I challenge the appropriateness of the statistical measures he proposes, but also show that even if we use them the numbers prove him wrong. The 1893 text I give below further establishes that I am right and Mühlhäusler is wrong (readers can count the "resumptive" pronouns and *him he's* if they like). As with *bae*, counting is valuable and useful provided you know how to count and what the results mean, both of which require an adequate grammatical analysis.¹⁵

Let me turn to Mühlhäusler's comments. First, as I have reiterated,

the ultimate origin of lexical forms and incipiently grammatical elements is irrelevant to the patterns into which they are constructed. Mühlhäusler writes, "Of the ten constructions that Keesing claims to have been common to southwestern Pacific pidgins in the late 1880s . . . the majority turns out not to have originated among speakers of Oceanic languages." But my argument had nothing to do with where the labeling bits and pieces came from but rather with their development into a highly expanded pidgin, which had attained much of its present complexity a hundred years ago.

Mühlhäusler does not confront the fact that the ten patterns I show to have been established in the pidgin of the southwestern Pacific more than a century ago have a far-reaching significance in terms of the historical interpretations he himself has proposed. He has previously argued that prior to 1880 Pidgin had such a simple "one- and two-part grammar" that complex constructions (such as periphrastic causatives) were impossible (Mühlhäusler 1980). I show that periphrastic causatives were recorded by 1869. He claims that *-fela* was used quite unsystematically until long after the 1880s. I document that *-fela* was being used by the 1880s in seven stable, interconnected grammatical slots. He has claimed the "they" pronoun was used as a plural marker only with human nouns until the end of the century.¹⁶ I show that *olgeta* was being used to pluralize inanimate nouns as early as 1880 (p. 129). To say, as Siegel does, that "Mühlhäusler simply has his dates wrong" will not suffice when so much theoretical weight has been assigned in the literature to the developmental sequences he has proposed.¹⁷

Mühlhäusler offers no evidence for 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 [1890]." We have texts showing both *yumi* and *mifela* in use by the 1890s (although it is true that no observers explicitly tell us that an inclusive/exclusive semantic distinction is being used). But does Mühlhäusler really expect us to believe that Tok Pisin and the Solomons and Vanuatu dialects separately evolved not only the same semantic distinction (which, as Romaine notes, is as opaque to many Papua New Guineans as it is to English speakers) but also exactly the same pronominal forms to fill these slots? He apparently would have us believe the same thing about *hem i*. The 1893 text below shows that *hem i* was thoroughly regularized in the Solomons by the early 1890s.

This text, which is of considerable importance in reinforcing the general argument of *MPOS*, was recorded in 1893 by the British naval offi-

cer Lieutenant B. T. Somerville in New Georgia, in the western Solomons. I employ the English-based orthography he used (Somerville 1897:450-451).

Long time before, one fellow man, name belong him he Tasa, him he go along Tomba, along canoe catch him fish along spear. By and bye one fellow makasi he come, him he catch him, him he put him along canoe. Close up another fellow makasi he come, he put him head belong him out of salt-water, he sing out, "What name you shoot him woman-makasi belong me? by and bye altogether picaninny belong me he die suppose he no catch him kaikai belong him."

Tasa, him he talk, "What name you talk him, suppose picaninny belong me he no kaikai makasi, he all o' same picaninny belong you, altogether him finish, he die." Man-makasi he sing out: "All right, you look out, me go talk him shark, by and bye he kaikai along you." Him he go away along salt-water.

Tasa he go, he shoot him plenty fish, sun he go down, he put him up sail, he go quick along Mungeri. Big fellow wind he come, rain he come, plenty thunder and lightning he come, canoe he capsize, canoe he broke, Tasa he swim, he swim along. Shark he come, crocodile he come, Man-makasi he come, shark he catch him Tasa along head, crocodile he take him along leg, he pull, he pull plenty hard. Tasa he sing out, no man he come, by and bye he broke, he finish.

Makasi he laugh: him he go place belong him, he catch him another fellow woman: picaninny belong him he no die.

[Translation: Long ago, a man named Tasa went to Tomba in a canoe to spear fish. After a while a *makasi* (fish) came and he caught it, and put it in the canoe. Then another *makasi* came and put its head out of the water and called out: "Why did you spear my *makasi*-wife? All my children will die if she doesn't get their food." Tasa said, "But (in relation to what you said) if my children don't eat *makasi*, they're just like your children, they'll die." The *makasi*-husband shouted: "Well, watch out then, because I'm going to go and tell a shark, and he'll eat you." He disappeared into the sea. Tasa went on and caught a lot of fish, and when evening came he raised the sail to get back to Mungeri quickly. A strong wind came up, it started to rain, there was lots of thunder and lightning, and the canoe capsized

and broke, so Tasa had to swim for his life. The *makasi*-husband came, bringing a shark and a crocodile; the shark seized Tasa by the head, the crocodile seized him by the leg and pulled, pulled really strongly. Tasa called out, but before anyone could come he was torn in half, and that was the end of him. *Makasi* laughed and went to his place; he took another wife; so his children didn't die after all.]

If we transpose this text to a plausible Melanesian phonology,¹⁸ only three minor changes distinguish this 1893 Solomons Pidgin from what one might record from older bush speakers in New Georgia or Malaita in 1989. In 1893 *wanem* (from English "what name") was being used as an all-purpose "wh" question marker. This is attested from other turn-of-the-century texts as well. In the twentieth century, a distinction emerged between *waswe* 'why?' and *wanem* 'what?'. In this text, *kaikai* 'eat' is used transitively with *long*. In this century, this has come to be expressed using the transitive suffix (*kaikai-em*). Finally, *tok-im* has been replaced by *tal-em*.

The commonalities with modern Solomons Pidgin vastly outweigh these minor contrasts. The Oceanic pronominal pattern, with the pronominal *i* following noun subjects and in *hem i* sequences, is exactly the one shown in my texts from older bush speakers (and which Mühlhäusler claims was not incorporated in Solomons Pidgin until the 1920s). The semantics of forms such as *kas-em* (to acquire s.t., to catch s.t., to reach a place) exactly correspond to contemporary usage. Forms such as *putimap* (and *leftemap*) can still be recorded from Solomons bush speakers. Note that by this time *olketa* was regularized both as plural marker and as third-person pronoun.¹⁹ The use of *-fala* as a suffix to quantifiers and demonstratives (*wan-fala*, *nara-fala*) and some common attributive statives (*big-fala*) in this text exactly corresponds to present usage.

This text²⁰ confronts us inescapably with a key fact that underlies *MPOS*: The major expansions and stabilizations of Melanesian Pidgin had occurred by about a century ago. To account for this, we have to postulate either an extraordinarily rapid crystallization and stabilization of Melanesian Pidgin at the end of the 1880s or the sort of progressive development through the 1860s and 1870s I have proposed. If, as Siegel seems to recommend, we take the most conservative and skeptical readings of the texts from these earlier periods, the highly expanded Pidgin syntax of the early 1890s has to be viewed as having emerged almost overnight.

The other key thesis of *MPOS* is that the close grammatical parallels between Melanesian Pidgin grammar and the core syntax of Oceanic Austronesian languages suggest that substrate patterns--interacting with superstrate patterns and universal faculties of language simplification and second-language learning--had a strong historical impress on the development of Pidgin. This thesis remains unproven; but I see no compelling challenges to it in these reviews and no alternative answers to the puzzle with which *MPOS* begins, of how and why Solomon Islanders are able to calque Pidgin morpheme by morpheme onto their native languages.²¹

NOTES

1. And to thank Christine Jourdan for helpful comments.
2. I do not think the evidence yet allows us to dismiss, as Mühlhäusler does, the possibility of significant inputs from Chinese Pidgin English into the Pacific (or, indeed, into Australia).
3. There were substantial numbers of Pacific Islanders in the ports of eastern Australia from the early nineteenth century onward (p. 14). We cannot date the first salient linguistic connections between Australia and the Pacific Islands to the onset of the Labor Trade.
4. I agree with Siegel that we know much less than we would like to about the pidgin being used by ships' crews at the onset of the Labor Trade (which, I made clear in *MPOS*, was in 1865, not 1870). Not least of all, this is because most accounts by Europeans quoting fragments of Pidgin deal with their interaction with Pacific Islanders on shore; their renderings of Pidgin were usually included for their exotic cast or amusement value. The everyday Pidgin of the ships is almost completely absent in the archival records.
5. My speculations about this, as I note in *MPOS*, were largely a response to Bickerton's suggestion that there might have been an early creolization and subsequent repidginization. My conclusion is that while there may have been some nativization, this probably would have had few linguistic consequences (p. 228). I find it hard, having just reread pp. 33-34 of *MPOS*, to understand how I could be misread on this point.
6. These interpretations could be reinforced by further data from Anejom (Aneityum) and other Loyalties languages such as D(r)ehu.
7. I assign them no such role in *MPOS*; see p. 29.
8. I assign that date to a separation that in fact took several years to run its course; 1884-1889 would be more precise.
9. I must say, though, that I have always suspected that prepositional verbs do occur in Tok Pisin, a point strengthened by Romaine's observations. The crucial diagnostic prepositional verb, however, is "with"; and despite considerable searching I have found no sign of *weit-im* or equivalent in Tok Pisin, where *wantaim long* seems pervasively established.
10. Set out by Tryon in a paper presented at the 1988 Fifth International Conference on Austronesian Linguistics in Auckland.

11. In Keesing n.d. *b*, I note the recorded occurrence in the New Hebrides in the 1880s of *dis-fela* and *dat-fela* as demonstratives preceding nouns and as demonstrative pronouns.
12. *Pato* 'duck' was probably introduced into New Guinea, along with the waterfowl, by Polynesian missionaries.
13. For many Solomons speakers, as in Vanuatu, the long form is *babae*. I use Solomons/Vanuatu (Oceanic) phonology as further commentary on Tok Pisin hegemony.
14. I have many texts in which the same speaker is using long and short forms interchangeably, in the same slots.
15. It is also necessary to be extremely cautious regarding what constitutes a corpus. The transcripts of the various Queensland inquiries are particularly treacherous documents for counting and other purposes (moreso, I think, than Clark acknowledges in his interpretation of prepositions cited here). As I note in *MPOS* (pp. 151, 157), a number of different voices and registers are discernible within the transcripts; moreover, the court recorders were evidently trying to represent roughly what was being said in Pidgin while producing a legal document intelligible to English speakers. The compromises they reached seem to represent a very unevenly anglicized text, misleading with regard to prepositions and almost everything else.
16. He is not quite precise about dates for this.
17. Theoretical weight has also been assigned to his claims that use of a "they" pronoun as plural marker and use of transitive suffixes to form causatives have no motivation in substrate languages. In *MPOS*, I show that both patterns are pervasive in Eastern Oceanic languages (pp. 124-126, 127-130).
18. In using the conventional contemporary orthography here, I am making no claim that --either in 1893 or 1989--native speakers of Roviana who learned Pidgin as young adults used exactly the pronunciations represented in the now-conventional Solomons Pidgin orthography: only that the Pidgin forms Somerville writes in a way modeled on English orthography would have been bent to follow Roviana phonology in a way probably not very different from the Pidgin phonology of older speakers in New Georgia villages in 1989. Edvard Hviding (personal communication, 1989), who has been working on the use of Pidgin by contemporary New Georgians, has provided helpful information on this.
19. In the Solomons, *olketa* is recorded as a plural marker for inanimate nouns as early as 1880 (p. 129).
20. Unlike Pionnier's almost exactly contemporaneous texts from Malekula, Somerville's has the virtue of representing a Melanesian story told by a Solomon Islander, rather than a Catholic religious text.
21. In forthcoming papers (Keesing n.d. *a*, n.d.*c*), I have carried this argument further, showing that Solomons Pidgin as spoken in the western Solomons is bent so as to follow quite different substrate patterns and showing how calquing has historically shaped (and shortcut) the processes of grammaticalization being uncovered in "natural" languages.

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