



Review: [Untitled]

Reviewed Work(s):

A Grammar of Hatam, Bird's Head Peninsula, Irian Jaya by Ger P. Reesink
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Book Reviews

Ger P. Reesink. 1999. *A grammar of Hatam, Bird's Head Peninsula, Irian Jaya*. Canberra: Pacific Linguistics. Series C-146. xv + 215 pp. Paper. Aus\$42.50.

A grammar of Hatam (GH) is one of a slew of publications that have, after a long drought, finally appeared on the linguistic horizon of the Bird's Head in the far west of New Guinea. Recently we have been treated to Abun, Mai Brat, and tidbits of Meyah, Moi, Mpur, Sougb, and Mansim (Baak, Bakker, and van der Meij 1995, Berry and Berry 1999, Brown 1990, 1991, J. Brown 1999, Dol 1999, Gravelle 1998, 2001, Gravelle and Gravelle 1991, and Reesink 2002 and the papers therein). GH, however, has the distinction of being written by a linguist who has proved his ability to describe, analyze, and present the relevant aspects of that analysis for outsiders (e.g., the still-current Reesink 1983).

GH is not as large as the sort of reference grammar that we have come to expect in recent years, with only 215 pages, of which 80 are texts, leaving only 135 pages of sketch description—R notes (8) that “this monograph does not pretend to give more than a preliminary description of Hatam.” It is, however, churlish to directly equate the thickness of a volume with the usefulness of its contents; many superlative descriptions have been much less convoluted than many impenetrable tomes. I know that I have produced better (though less broad) description in short works than in long ones, and we should expect that R's experience should lead to a more concise and less labored style, which it does. Indeed, GH is a convincing argument for linguists to dust off their field notes and publish less-than-reference grammars of those undescribed (or under-described) languages that they have some experience with.

Reviewing GH raises another issue that is in need of discussion—the level of competence in a language that authors should possess before their knowledge is published. In part, this question is answered by the editors and reviewers who make the publishing decisions, by those who examine the products of positive decisions in journal reviews, as well as by those who use and cite such grammar sketches in their own surveys, areal studies, and theoretical works. What these people will accept is, *de facto*, acceptable, and so sketches such as GH do reveal enough competence to be published.

But in larger part it is answered, unfavorably, by examining the overall maturity of the discipline as revealed in other ways. To a great extent linguists seem to firmly believe in the idea of the published article or book being the magnum opus—that a published work should be the last word on the subject, covering all facets of the phenomena that are mentioned in definitive detail and with perfect integration (reviewers for OL are a thankful exception). In effect, we are a discipline that discourages constructive debate through short articles. If we really believed in debate, more pages of more journals would be devoted to shorter, exploratory works, or squibs, that raise, address, or add to, an issue, without necessarily “solving” it. The lack of such collective discussion outside

of conferences leads to unnecessarily slow progress, because of overly slow dissemination of ideas (other than to an “inner circle” of prepublication recipients).

It should be clear that I am in favor of publishing, and letting the scholarly community do what it will with the results, and so am strongly in favor of the sort of publication that GH represents: it is not intended to discourage further research on Hatam, but to entice that research. This is the aim of a good grammar sketch: to present some of the more interesting or salient features of a language. R does not intend to spend years of his life working on this language, but it would be a waste indeed if the time he has spent could not be suitably shared with other linguists.

By its abbreviated nature, a grammar sketch is more documentary than descriptive, but R manages to make some areas of GH more analytical than might be thought likely in such a book. In 135 pages he provides us with an overview of the “essential” features of Hatam phonology and morphosyntax: phonology, morphology, and syntax are the primary divisions made in this traditionally organized grammar. In the syntax section we find a treatment of relative clauses and clause combining that goes beyond expectations for a small sketch. The level of thoroughness that can be found in the section on relative clauses shames many grammatical descriptions twice the length of this one.

The treatment given to “discourse level morphosyntax,” such as markers of textual deixis (3.11), conjunctions (3.15), multiple-predicate constructions (4.5), and different coordinating and subordinating constructions (4.6, 4.7) is impressive and reveals a bottom-up approach to the language and shows that R was not simply collecting questionnaire-style information, but patiently collecting a range of material (his methodology is spelled out in detail in the introduction).

R is too modest in some areas. Discussing the lexical category of adjectives, he states (56) that “it is not easy to find criteria to distinguish adjectives as a separate word class.” Despite this opening, he follows with a range of morphosyntactic tests that clearly show that adjectives do not have the same distributional properties as do verbs, or for that matter nouns. Although they take the same inflections when used predicatively, the differences emerge when we examine their use in attributive functions inside the NP, where the adjectives can modify without a relative clause being constructed. Furthermore, adjectives can appear in a nominal-like construction with the prefix that is used for inalienable possession, while verbs cannot. This approach, examining different words in different discourse functions, clearly reveals the differences between the word classes, and serves as a model of investigative methodology (see also Croft 1991, Donohue 1999).

As should be expected for any work dealing with an entire language, 135 pages are not adequate to answer all the questions that arise when reading the description. But it is to the author’s credit that these questions arise, as the clear and unconcerned presentation of enough data to let the reader come to agreement, or disagreement, with the analysis implicit in the presentation means that the volume is serving its function, namely to interest the reader and to instigate thought. I shall present my comments that follow not in the nature of comments that question R’s analysis of Hatam directly, but as ones that concern descriptive work, and analytical work based

on others' description. I shall address the Hatam data, but the questions that reading this sketch has brought up in my mind apply equally well to other works; the clarity of presentation in the Hatam monograph makes it a springboard for discussion.

Many of the problems that I have with GH are of an editing and copy-editing nature, rather than anything to do with the data or the analysis. There are some occasional inconsistencies in the glossing used. I noticed that the morpheme identified as the relativizer, *di-*, is not always glossed as such, sometimes being incorporated into the following morpheme (for instance, example 3.183 on page 74). In the phonology chapter there is little consistency in the use of phonetic transcription: some IPA is used, some apparent adaptations from Indonesian orthography, but no explanations, and much mixing of the two (or more?) systems. This is not just a quibble: it obscures a lot of phonetic detail that makes the evaluation of some of the phonological decisions troublesome, to say the least. But leaving these editorial points aside, the issues that I can raise with the author, and not the publisher, are more interesting, both for the analysis of Hatam and for general linguistic description, and the reading of that description.

Some phonological issues seem to me to be worth raising, not just as far as they are presented in GH, but as general issues in descriptive linguistics. Because I am addressing these questions to linguists involved in description in general, and not necessarily to R in particular (who makes not claim to have said the last word on the specifics of Hatam), I shall refer more to phonology than to morphosyntax in what follows, as the phonological issues represent a more easily generalizable set of issues that can be applied to other work as well. I shall concentrate on three issues: prenasalization, the phonological representation of glides, and epenthetic vowels (these last two points are related).

R notes that sequences such as [mb], [ŋg], and so forth can be found word-initially in Hatam, but believes that they do not represent a prenasalized series, but rather the sequence of two separate phonemes. The reasons that he gives for this analysis are: the nasals may be realized as syllabic nuclei (though this is not, to judge from R's transcriptions, mandatory); the stop component can be contrastively voiced or voiceless; there are prenasalized fricatives. Starting with the last of these reasons, the existence of phonemic prenasalized fricatives should not come as a surprise to anyone, and so can be discounted. The fact that (for instance) [n.ti] contrasts with [n.di] only shows that, if we are dealing with prenasalized unit phonemes, there is both a voiced and a voiceless series. The syllabification is a more interesting issue, but here R provides the answer. He does not propose a series of syllabic nasal phonemes, but suggests that this syllabicity is imparted to the nasal by virtue of its phonotactic environment: initially, before a stop, the nasal is realized as a syllable nucleus. There is, of course, no reason that the same argument cannot apply to a series of prenasalized stops. Often prenasalized stops are not realized as such word initially, because of phonotactic constraints: one common strategy, in a language with a /p/ vs. /mb/ contrast, but no /b/, for instance, is for the prenasalized stop to be realized simply as [b] initially. In Hatam, which has contrastive plain voiced stops, this would not be an option, and so syllabification of the nasal is found. R notes that the prenasalization is not always homorganic, but can be influenced by surrounding segments. One exam-

ple he gives is (orthographic) *Ni-mbut-mbut nyen* 1EXCL-walk-walk just ‘We were just walking around’ being realized as [nəm.bun.bu.ñen]. Here the *-t* of the first *mbut* causes the following nasal to be realized as an alveolar, and not bilabial, nasal. Again, there is no reason that this could not apply to a prenasalized stop: presumably the branching onset would allow for assimilation to apply to one, but not necessarily both, elements of the prenasalized phoneme. This same example also shows us that the nasal can be included in the reduplication template, which appears (though information is scarce) to apply only to single (phonological) syllables.

Glides, as found in sequences such as [aj], show a much greater tendency to be analyzed as a sequence of two vowels, /ai/, than does a sequence in the reverse order, [ja] (which is typically analyzed as /ja/). I am just as guilty of these prejudices as anyone else, and, based on my own most recent period of field work (last year) I show little or no sign of learning from experience. I know that I should think of them as the same sounds, but in my heart one is a sequence of two vowels, and one is a glide plus a vowel—why is this? Partly, of course, it reflects the prejudices I have acquired from my native language’s orthography, and the orthographies of other languages I have picked up along the way. Partly it probably reflects the conflicting desires to phonemicize a language, and to orthographicize it (in keeping with national language spelling principles, in which V+G sequences are written with two vowel graphemes) at the same time.

Classical (= structuralist) phonological principles tell us that we should be suspicious of the identity of two phonetic segments (or two nonsegmental entities) when they show alternations, or when they occur in complementary distribution. If, for instance, in the mini-data set described above (the analysis of [aj] and [ja]) I find that [aj] alternates with [ai], then suspicions should arise as to its identity: but still, we cannot know if this alternating sequence represents /ai/ (with optional desyllabification) or /aj/ (with optional syllabification), though the former seems more likely. The same arguments would apply to an alternation between [ja] and [i'a]. If, however, we found [ja] alternating with [i(j)a] when following another consonant, thus [pja] ~ [pi'(j)a], we have a more complex situation. We now have two equally viable options: /pia/, with optional desyllabification of the high vowel. This would mean that if there was a non-alternating sequence [pi(j)a], we would have to acknowledge contrastive stress rules for the language. Alternatively, if we analyze it as /pja/ we need to either have a rule of optional syllabification of the glide when it precedes a vowel, or else a general rule of vowel epenthesis between two consonants (CC → CVC), and the underspecified vowel then acquiring its identity from the features of the surrounding consonants or vowels (this analysis is that proposed by Laycock for the Ndu languages, or Pawley for Kalam), thus /pja/ → (“intermediate level” {pVja}) → [pi(j)a]. While this last option might seem to involve a lot of machinery for a simple alternation, it is not more than is involved in converting a specified vowel in a syllabic nucleus into an onset. R acknowledges that there are epenthetic vowels in the language (2.6). Despite the presence of epenthesis, he goes on to posit underspecified vowel units in his morphology (the 1SG subject prefix on verbs, for instance, is given as *dV-*). Concerning the allomorphy of these subject prefixes, R notes (23) that “before vowel-initial stems, V is elided

and /i-/ [the 3PL prefix] becomes [y],” and further describes the allomorphy as: $V \rightarrow [i, i]$ / ___ palatal, $V \rightarrow [u]$ / ___ /w/, $V \rightarrow V_{[\alpha \text{ features}]}$ / ___ $hV_{[\alpha \text{ features}]}$, and $V \rightarrow [i, \text{ə}]$ / elsewhere. Now, if we assumed that the underspecified vowel was merely an epenthetic vowel that acquired quality by the same rules, the 3PL would be described as an initial /j-/ being realized with syllabicity before consonants, /jpuj/ \rightarrow [ipuj] ‘they tell’, with optional friction or velar stopping being ascribed to the underlying consonantal nature of the prefix (R lists [ik>pΦuj] as an alternative to [ipuj]). The form of this prefix before an /h/-initial word ([jV], predictably, or [iç], unaccountably) is also predicted: if a form like [jahagom] ~ [içhagom] ‘they all’ was based on /j-hagom/ competing rules for epenthetic vowel identity would give us the allophonic surface forms. But if it was based on /i-hagom/ both forms are hard to explain.

An apparent ban on the simple /i/ (as opposed to what R analyzes as /ig/) occurring word-initially, and only a few cited attestations of an initial /u/, supports the idea that these vocoids (as R calls them, avoiding over-hasty classification) behave differently to the lower /a, e, o/. The lower vowels may occur initially, while all attestations of the higher ones involve friction or stopping of some sort; this suggests that they are underlyingly consonantal, rather than vocalic, and acquire their syllabic properties through assimilation with the epenthetic vowel that is generated by the phonotactic structure that is morphologically specified and its interaction with the universal preference for CV syllabic structure. A similar analysis has been applied to inflection in Mai Brat (W. Brown 1991, J. Brown 1999).

It is hard to understand why *doi* ‘song’ [dɔy] is assumed to represent /dɔy/, while *bui* ‘hit’ [buy] is assigned to the phonemes /bui/. Even more confusing is the assumption that the nonalternating [w]s in [kwɔ] ‘mat’, [ɲwɔy] ‘red’, and so forth, are assigned to vowel phonemes (R lists the phonemic representations of these two forms as /kuo/ and /nguoy/, respectively). Surely, in the absence of evidence for vocalic nature from alternations, there can be no question about the consonantal nature of this [w]. R states (28) that because “consonant clusters are highly restricted, the same elements (the glides [w] and [j]) are considered to be vowels when one or two consonants are present as onset,” but this is surely putting the analytical cart before the descriptive horse.

There is also a good argument for treating the rounded velar segments as unit phonemes as well, because they do not show the same sort of syllabification possibilities that are found with other C + w sequences: on page 26 we learn that both [bwak] and [buwak] are possible for ‘take’, and both [dwas] and [duwas] for ‘screech’ (unfortunately we are not told where stress is assigned in the epenthetic vowel cases), but [gwam] ‘sit’ does not alternate with *[guwam]. If the rounded velar stops were unit phonemes, then we would not expect them to allow epenthetic vowels to be inserted.

Another question that arises is, when is it justified to use an orthography that is in place for a language? Or, perhaps better phrased, when is it preferable to *not* use an existing orthography? We can only really judge this orthography (and so its suitability as a tool for representation in what is the first large[-ish] account of the language) by examining it, and that raises the question of the availability of Hatam dictionaries, or even likely availability in the future. R lists the dictionaries that he has used, an

invaluable tool in helping GH to achieve the quality that it does, but it is very unlikely that many other linguists are going to have access to these dictionaries. Because, then, this sort of preliminary work is of the nature of a documentary record at least as much as it is a descriptive one, it makes sense to present the material in as transparent a fashion as possible. The fact is, the orthography that has been used for Hatam is not representative of the sound system of the language. R acknowledges this, altering the orthography at some points where his interpretation of the language differs from Griffiths'. While it is understandable that R should employ the most convenient orthography (a preexisting one) for his sketch, it does seem to have obscured some of the data, and forced an analysis that R himself acknowledges is inadequate (as noted above). The use of the grapheme *i* to represent what appears to be at best an underspecified vowel, and at worst no phonemic vowel at all, is not very satisfactory for a preliminary description. Given that sequences that R transcribes as [dəbəpə'ley] are represented as *dibipilei*, there is a rather distant association between grapheme and phonological structure. Similarly, one suspects that the Hatam orthography is based on the Indonesian one, in which *c* and *j* represent [tʃ] and [dʒ], yet nowhere are we given a good description of the sound that R represents by *j*. Linked to the question of the appropriateness of using an orthography is the linguistic practice of the presentation of phonological material. It is usual practice for most linguists to present their analysis first, and then justify it: in effect, we act as lawyers, laying out the case for our analysis. Surely what we should be doing, especially in the case of lesser known languages such as Hatam, is to act as detectives, and start by presenting the phonetic evidence that we are presented with, with as little analysis as possible, and from there proceed to argue for our interpretation. I do not take issue with R here, but rather with the discipline of descriptive linguistics in general. I am just as guilty as R is, and I write this criticism hoping to remind myself to do better in the future, as much as anything else.

In the area of morphology and syntax we can only applaud R's detailed combination of textual analysis and paradigmatic checking. The value of any description lies not simply in demonstrating what patterns are found, but also in showing where the limits of those patterns are. We cannot make generalizations about the possibilities in a language that we are reading about without knowing about the *impossibilities* as well, because in the absence of information about them we cannot know whether gaps in the description represent the absence of these forms being attested, or the author's not having checked for them. In most areas it is quite clear that R has thoroughly investigated the syntactic phenomena he describes. He appears to have been blessed with good informants, but also has the experience of prior grammar-writing and grammatical analysis, essential tools for producing succinct and complete commentary.

R posits several clitics in his analysis as distinct from affixes. Nowhere, however, are his criteria for distinguishing clitics made clear. R mentions (43) that they can attach to a range of word classes; the pronominal prefixes, however, are also found on nouns, adjectives, and verbs, so this is not a sufficient criterion for declaring that these are clitics and not affixes. The description of the instrumental marking, which has its parallels in Meyah, is intriguing. In this construction, the instrument is (usu-

ally) introduced in a serial verb construction with ‘use’, but is also indexed on the “main” verb with the prefix *bV-*, apparently serving an applicative-like function. If this is indeed the function of this morpheme, then we might have evidence for a tendency in the language, at least with this construction, that the object must be object for all verbs in the construction (we are told that subjects must be identical, and that instruments cannot appear as obliques). I for one hope to see more analysis of this sort of thing from at least one language in this region, beyond the half-page that R gives us on page 54 and the few other examples on pages 101–102.

A quibble with R’s terminology, in the nature of an editing criticism, is in order here: R states that “Hatam does not allow Instruments as clausal constituents.” Of course it does! The fact that they are introduced in serial verb constructions, or with the main predicate prefixes with *bi-*, and that they often apparently serve as objects in the clause (though R gives no reasons or proof for this assertion), does not mean that they are not instruments, because that is a semantic role description, not a grammatical function description. I mentioned earlier that R’s coverage of clause combining puts many larger grammars to shame. The same is true for his description of the position of adverbials, and both the position and scope of negation in the language, which should serve as a template for others claiming to describe languages, or at the least a guide to the sorts of issues that should be relevant. It is this, the marriage of basic descriptive work to informed theoretical observation, that is likely to repair the rift in modern linguistics between field-oriented description and introspectively driven theoretical linguistics. While many fieldworkers may bemoan the perceived irrelevance of the bulk of most published theoretical progress to their work, it is also true that most theoretical linguists justly decry the irrelevance of most new, and old, descriptive work to theirs. Works such as GH (and we might also mention Klammer 1998) are the sort of descriptive studies that can make fieldwork relevant to the theoreticians.

The collection of texts that follows the grammatical sketch is a valuable tool, especially given R’s careful explication of their arrangement: he has not blindly imposed a narrative structure on the recordings that he transcribed by means of arbitrary line numbering, but rather indicates intonation units, the units actually used by speakers. This information is transcribed, making it recoverable to other users of the book, an important archival feature. An index would greatly improve the usefulness of the book as a reference, and in this era of word processing its absence is somewhat surprising. The detailed table of contents helps to address this lack, but it is still felt.

All in all, GH is a welcome addition to our knowledge of the Bird’s Head languages, a region of New Guinea that is fast changing from one of the less well-known areas of that island to one of the better documented ones. Maybe, if we’re lucky, this sort of sketch will serve as a springboard for more detailed work on the language in question, by either the same author or a different one.

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