This volume, as a collection of papers presented at a workshop at Stanford University in 1993, demonstrates to the readers the magnitude and diversity of forms and functions of complex predicates (henceforth CPs) in natural language. Indeed, the major contribution of this volume is its diversity, reflected in the theoretical frameworks employed and the languages and phenomena covered in the articles. On the other hand, the restriction of this volume is the relative shortage of space for the contributors to elaborate on the typological significance of CPs and its correlation with the syntax of the language. In this review, I first take an overview of the volume, and then go on to some questions related to the possible forms of CPs and their correlation with the typological characteristics of the languages. After that I examine a particular type of CP in Chinese, the Subject-Predicate/Verb-Object verbal compounds, and show that the role of syntax must be more extensive in the formation of CPs than has been assumed by most of the contributors. We conclude by claiming that syntax is indispensable in capturing the typological significance of CPs as manifested in the various possible forms in natural language.

In the introductory chapter (chapter 1), the editors define CPs as ‘predicates which are multi-headed; they are composed of more than one grammatical element (either morphemes or words), each of which contributes part of the information ordinarily associated with a head’ (1). A phenomenon of particular interest in this volume is the dual status of CPs as being both lexical and phrasal in a wide range of languages. For instance, Williams (chapter 2) argues that the kind of structure seen in wipe clean in English is in fact lexical in nature, though it has a phrasal representation in syntax. Similarly, the Preverb + Verb construction in Serbo-Croatian and Hungarian (Ackerman & LeSourd, chapter 4) and the Host N + Light Verb construction in Hindi (Mohanan, chapter 13) also show the lexical/phrasal dual status: on the one hand, the two elements seem to constitute a ‘single semantic word’
(Ackerman & LeSourd, 69) with word-like lexical/morphological properties, but, on the other, they are accessible to syntactic processes. The question as to how the lexical/phrasal dual status of CPs can be accounted for, then, constitutes a major concern in this collection, particularly in view of the lexical integrity hypothesis (e.g. the discussion by Ackerman & LeSourd, chapter 4). There are also other concerns. In this volume a significant portion of attention has been directed to questions related to the composition, or ‘fusion’, of argument structures. For example, Alsina (chapter 8) proposes to relativize the syntactic level where the predicate must be ‘complete’ (his Incomplete Predicate Parameter), which Butt (chapter 5) adopts in her account of VV compounds in Urdu. But Baker (chapter 9) suggests that there may not be such a thing as argument composition in the grammar, basing his claim on the observation that, in Mohawk and similar polysynthetic languages, morphological causatives can only be formed from unaccusative verb roots: if there were argument composition, this requirement would not be expected, since the external argument of the verb root could be fused with the causer argument of the causative morpheme. Questions related to polysynthesis also attract much attention. In addition to Baker, Foley (chapter 11) and Evans (chapter 12) investigate the syntactic behavior of Yimas and Mayali and argue that semantic/conceptual factors play a substantial role in determining the use of particular applicative morphemes (Yimas) and the possibility for particular nominals to undergo incorporation (Mayali). This wide range of concerns is a positive characteristic for a volume that aims to demonstrate the diversity of a given linguistic construction, and, as most readers would agree, it does it in a successful way.

The diversity of this volume is also reflected in the languages covered, which include Catalan, Chichewa, English, French, Hindi, Hungarian, Italian, Mayali, Mohawk, Serbo-Croatian, Urdu, Yimas, and others, ranging from polysynthetic/agglutinating to analytic/isolating languages. The forms of CPs, as a consequence, necessarily vary with the typological characteristics of the languages. For example, in polysynthetic languages, CPs refer to the constellation of verbal roots and a number of derivational affixes, and, on the other extremity of the spectrum, in other languages they can refer to a whole stretch of words making up syntactic phrases. The theoretical positions and proposed analyses also vary. Roughly classified, there are five different frameworks that the contributors assume in this volume: Government-Binding/Principles-and-Parameters (Williams, Hale & Keyser, Baker), Lexical Functional Grammar (Ackerman & LeSourd, Butt, Alsina, Mohanan), Relational Grammar (Rosen), Construction Grammar (Goldberg), and conceptual/semantic approaches (Durie, Foley, Evans, Kiparsky). An interesting point to note is that contributors assuming a particular framework tend to concentrate on the same issue and, explicitly or implicitly, emphasize that the particular framework can best cope with the data and provide
satisfactory analyses. This is particularly obvious for those authors who assume LFG: Ackerman & LeSourd on the ‘analytic paradox’ for the lexical/phrasal dual status of CPs vis-à-vis the lexical integrity hypothesis; Butt on the superiority of LFG in separating the argument structure of a CP (which is complex, in her analysis of Urdu VV and V+Light Verb constructions) from the functional and constituent structure (which can be that of a simple clause); and Mohanan on the function of a separate semantic structure as the locus for argument composition for predicates that map to a single clausal unit at argument structure. Another example is the GB/P&P framework: since the derivational approach is central to this framework, we see that Baker tries to decompose surface agglutinative verbal complex (and the serial verb construction) in polysynthetic languages into syntactically defined representations; similarly, Hale & Keyser try to extend syntax to the generation of lexical items – the denominal and deadjectival verbs – and suggest that the lexicon is subject to the same set of syntactic principles (their L-syntax). All these examples indicate that, to a large extent, interests and concerns are determined by the framework adopted.

I turn now to the definition of CPs and their typology. Though CPs are defined by the editors as multi-headed predicates with more than one grammatical element, each of which contributes part of the information, it appears that the nature of CPs, as discussed in the articles in this volume, has not been clarified in an entirely satisfactory way, and as a result some further questions can be raised. Basically, the editors’ definition ascribes two fundamental properties to CPs: (i) they have complex (syntactic) forms yet exhibit unitary behavior as one unit; (ii) the composing elements share arguments. Taking these two defining properties at their surface value, however, has the effect of including some constructions that don’t really fall within the domain of CPs. An example is Rosen’s discussion of auxiliation (e.g. *Eva is furious*) and serialization (e.g. *Eva becomes furious*). The major concern of Rosen’s work is to make it possible for Relational Grammar to handle multiple predicates in a single clause, and the analysis that is advanced is to relativize the order for the application of the advance-to-1 rule according to the valence of the outer predicate. Thus the focus of the analysis is argument sharing, with no bearing on the unitary nature of CPs. Another example is Goldberg’s discussion of the *way* construction in English. The major concern of Goldberg’s work is to show that the construction’s semantics (its sense of result and motion) cannot be predicted on the basis of the constituting parts, and hence the existence of such a construction points to the need for constructions as independent entities in the grammar. But since a construction is defined as an entity whose meaning cannot be predicted from its composing elements, its extendibility to more familiar types of CPs is quite dubious. These constructions, then, are at best peripheral for an understanding of CPs in general.

A more fundamental question is the full spectrum of phenomena regarded
as CPs, which range from the most simple to the most complex surface structures, as is illustrated in (1):

(1) 

<table>
<thead>
<tr>
<th>Simple verbs that are complex in sense</th>
<th>Predicates composed of morphemes or words</th>
<th>Predicates composed of independent words</th>
<th>Phrasal complex predicates</th>
</tr>
</thead>
<tbody>
<tr>
<td>Lexical</td>
<td></td>
<td>Phrasal</td>
<td></td>
</tr>
</tbody>
</table>

Though each of the phenomena as shown here in (1) more or less fits the definition of CPs as given by the editors, the question remains as to why the possible forms of CPs can expand along such a wide range in the lexical-phrasal dimension, and how. This seems to be a question with no less importance than giving a proper definition for CPs that encompasses all susceptible grammatical constructions into a single class. But surprisingly it receives little attention from the editors and contributors in this volume. Clearly, the form of CPs has to do with the typological characteristics of the language: agglutinating languages tend to have CPs toward the lexical end of the spectrum, and analytic languages tend to have CPs toward the phrasal end of the spectrum. Thus the forms of CPs are not arbitrary; they correlate with the syntax of the languages. Unfortunately, the editors and contributors miss an opportunity to elaborate on this issue, and hence the typologically significant question remains unclarified as to why, under a broad definition of CPs, there are all kinds of syntactic representations for CPs.

The ‘how’ question deserves special attention, too, and it is correlated with the ‘why’ question. As pointed out earlier, contributors of a particular framework tend to focus on the same specific issue which shows the superiority of that framework in accounting for the phenomenon. Meanwhile, however, there is virtually no cross-framework comparison in the articles in this volume, except for a few cases, like the criticism of Baker’s syntactic incorporation approach to polysynthesis (e.g. the chapters by Durie, Foley, and Evans). But Baker’s approach in fact represents a more restrictive way of accounting for the ‘why’ and ‘how’ questions on the various possible forms of CPs: except for the phrasal CPs, the possible forms of CPs as shown in (1) can be reduced to the question, given ancillary assumptions, whether incorporation applies or not, and how it applies (cf. Baker’s (1995) Polysynthesis Parameter). This theory can be correct or

[2] Ackerman & LeSourd (chapter 4) and Alsina (chapter 8) are exceptional in this regard, since their discussions directly touch upon typological issues, and therefore represent an important step forward for the typology aspect of CPs. But these authors still share the position of undervaluing the role of syntax in the formation of CPs. Like many other contributors, they take lexicon/argument structure as the locus for the formation of CPs. Syntax has not been considered as an important source for that purpose.
incorrect, but as an attempt to locate polysynthesis in the grammar, it
represents an effort toward a unified theory for CPs by syntactic means. On
the other hand, the LFG analyses of phrasal CPs (in the chapters by
Ackerman & LeSourd and Butt, for example) simply relegate the ‘how’
question to the mapping from complex argument structures to simple or
complex constituent structures, with no regard to how that mapping should
be constrained, what motivates such a mapping (but see the chapter by
Alsina), and so on. This point is particularly acute in view of the typology
question I raised earlier, i.e., the possible forms of CPs seem to correlate with
the typological characteristics of the syntax of the language and are
nonarbitrary.

Roughly, there are three theoretical positions in the articles in this volume:
syntactic (the form and function of CPs are determined by their syntax; e.g.
Baker and Rosen), strong lexicalist (the form and function of CPs are
determined in the lexicon, and the syntax only plays a subsidiary role; e.g.
Butt, Foley, Mohanan), and weak lexicalist (‘doing syntax in the
lexicon’; see below). This last position, the weak lexicalist approach, deserves
special attention, and includes work by Williams (in chapter 2), Hale &
Keyser (in chapter 3), and possibly Ackerman & LeSourd (in chapter 4) and
Alsina (in chapter 8). The gist of this approach is to pull the lexicon and
syntax closer to each other, for instance with Williams’ (chapter 2) lexical
rule $V \rightarrow V A$ for such phrasal CPs in English as wipe clean, or, in some cases,
even to extend the scope of syntax to lexicon, as with Hale & Keyser’s L-
syntax (chapter 3), or to carry lexical processes over to syntax, such as
Ackerman & LeSourd’s hypothesis that a phrasal CP already has a full head-
dependent structure as a result of lexical processes (chapter 4), and Alsina’s
Incomplete Predicate Parameter, which has the effect of relativizing the locus
of argument grid formation in the lexicon or in the syntax (chapter 8). This
cross-framework trend to emphasize the close relationship and interaction
between lexicon and syntax not only has the benefit of capturing the
lexical/syntax parallelism with respect to the formation of CPs, but also stays
in line with the important observation that I pointed out: the form of CPs
reflects the typological characteristics of the syntax of the language.

The other question I would like to raise is concerned with composition of
argument grids in CPs, a phenomenon taken to be a defining property of CPs
by many contributors. But it appears that argument composition is at best
a widespread phenomenon in CPs, not a diagnostic criterion for them – there
are languages where the complex argument grid is not formed via argument
composition or fusion. Chinese is an example. Here I would like to provide
a brief discussion of Chinese verbal compounds, a special type of CP in
Chinese, and by doing so add to the spectrum of CPs one more intriguing
instance for the multiplicity of the formation of CPs.

Chinese verbal compounds can be divided into three types according to
their internal elements (cf. Chao 1968, Li & Thompson 1981). The first type
is the familiar VV compound, where the relationship between the two Vs can be coordinate, causative-resultative, modifier-head, and so on. The other two types are much more interesting for our purposes. According to the insight of earlier researchers in Chinese grammar, there is a strong parallelism between sentence formation and word formation in Chinese – since sentences and compound words have similar or identical internal structures, sentence formation simply reflects the way morphemes combine to form compound words (cf. Chao 1968). Given this, it is not surprising to find compounds that have structures identical or close to sentences or full predicates. Therefore, in Chinese, there are Subject-Predicate (SP) and Verb-Object (VO) compounds. Below are some examples (cf. Li & Thompson 1981):

(2) (a) **SP compounds**

<table>
<thead>
<tr>
<th>Chinese</th>
<th>English</th>
</tr>
</thead>
<tbody>
<tr>
<td>tou-teng</td>
<td>‘be bothered’</td>
</tr>
<tr>
<td>head-ache</td>
<td></td>
</tr>
<tr>
<td>sheng-zhang</td>
<td>‘expose’</td>
</tr>
<tr>
<td>sound-open</td>
<td></td>
</tr>
<tr>
<td>yan-hong</td>
<td>‘be jealous’</td>
</tr>
<tr>
<td>eye-red</td>
<td></td>
</tr>
</tbody>
</table>

(b) **VO compounds**

<table>
<thead>
<tr>
<th>Chinese</th>
<th>English</th>
</tr>
</thead>
<tbody>
<tr>
<td>fu-ze</td>
<td>‘be responsible’</td>
</tr>
<tr>
<td>carry-duty</td>
<td></td>
</tr>
<tr>
<td>dan-xin</td>
<td>‘worry’</td>
</tr>
<tr>
<td>carry-heart</td>
<td></td>
</tr>
<tr>
<td>chu-ban</td>
<td>‘publish’</td>
</tr>
<tr>
<td>release-plate</td>
<td></td>
</tr>
</tbody>
</table>

Interestingly, though the SP and VO compounds have the form of a miniature sentence or predicate with the argument slots saturated internally, many of them can still take an extra internal argument, as shown below:

(3) (a) Laozhang hen **tou-teng** Xiaoli cizhi de shi

L. very head-ache X. resign MOD matter

‘Laozhang is bothered very much by Xiaoli’s resignation.’

(b) ni bie **yan-hong** renjia de qian

you don’t eye-red other-people MOD money

‘Don’t be jealous about other people’s money.’

(4) (a) wo hui **fu-ze** jintian de wancan

I will be-responsible today MOD dinner

‘I will be responsible for today’s dinner.’

(b) Xiaoli zong shi **dan-xin** ni

X. always be carry-heart you

‘Xiaoli is always worrying about you.’

The point, then, is how the extra internal argument is licensed. This question is especially puzzling in view of the fact that these SP and VO compounds can

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[4] But not all the extra internal arguments occur in the form of surface object; some of them need to occur preverbally and take a preposition. For example: *jie-hun* ‘(lit.) tie-marriage = to get married’, but *gen wo jie-hun* ‘(lit.) with me tie-marriage = marry me’. 

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be used phrasally, and hence the internal saturation of the arguments has really taken place. In other words, idiomaticity alone cannot account for the problem of licensing the extra internal argument:\(^5\)

\(5\) (a) Laozhang dui zhe-jian shi tou zhenshi teng de bu-de-liao
   ‘Laozhang is really extremely bothered by this matter.’

(b) Xiaoli zong shi dui ni dan-zhe yi-ge xin
   ‘Xiaoli is always worrying about you.’

In these examples, there’s no argument composition or fusion, yet the compounds behave lexically as a unitary item and phrasally as composed of discontinuous elements. It therefore seems that no approach in this volume can readily account for the licensing of the extra internal argument.\(^6\)

We believe that phenomena such as SP and VO compounding in Chinese indicate the importance of syntax in the formation of CPs. Typically arguments and their composition in CPs are assumed to be handled in the lexicon; cf. the lexicalist approach in earlier discussion. But now let’s suppose that some arguments in fact can be compositionally licensed along the way as the syntactic structures are being built (cf. the weak lexicalist approach mentioned above), and, furthermore, that internal arguments, in particular the objects, are in fact some sort of subjects – to use Bowers’ (1993) term, they are secondary subjects. Then the analysis of CPs in Chinese as developed in Huang (1992) can be readily applied to the SP and VO compounds in Chinese. Basically, in this analysis, the extra internal argument is regarded as being base-generated in the Spec position of VP – in other words, it is the secondary subject of the sentence. Its argument status is compositionally licensed by the \(V^\prime\) as a CP, just like the licensing of subject in the traditional view in grammatical theory. The \(V^\prime\), however, can be further subject to a process of reanalysis and turned into a \(V^0\), and after that undergo head movement to some higher light verb. The structure would be the following, taking (4b) and (5b) as an example (irrelevant details suppressed):\(^7\)

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\(^5\) The meanings of these SP and VO compounds are indeed idiomatic in some sense; cf. the gloss in (2). But it is clear that the idiomaticity doesn’t affect the argument saturation of S, V, and O. It is likely that they are semantically idiomatic but syntactically compositional.

\(^6\) But if we extend Hale and Keyser’s (chapter 3) L-syntax to syntactic derivations, then the Chinese SP/VO compounds may be well accounted for. This is what we will suggest later in the text for the analysis of the SP/VO compounds in Chinese.

\(^7\) The analysis of the SP compounds will be essentially the same, except that the compound-internal S should be considered as a ‘ternary subject’. Note that in our examples the Vs in SP compounds cannot license the extra internal argument, since they are all intransitive.
The boxed $V'$ is phrasal as it is generated. As such it is a phrasal CP that compositionally licenses the external object in the VP Spec, just like the light verb projection, $vP$, licenses the subject. In this way the phrasal (5b) is derived. But the $V'$ may be reanalyzed and turned into a $V^o$. In that case, it head-moves to the light verb $v$ and gives rise to the lexical (4b). Many important consequences follow from this analysis of CPs in Chinese, but due to limited space I cannot go into the details (cf. Huang 1992, 1997). What is presently relevant is that this analysis demonstrates well the critical role played by syntax in the generation of CPs in Chinese. Resorting to argument composition or lexical processes alone cannot account for the licensing problem with the extra internal argument. In fact, this analysis is quite compatible in spirit with the weak lexicalist approach alluded to earlier. The advantage of this approach is that it is now possible to capture the parallelism between sentence formation and word formation in Chinese, and, more generally, the correlation between the form of CPs and the typological characteristics of sentences in a language, as I have repeatedly emphasized.

Thus the traditional insight that words are parallel to sentences in Chinese is accounted for by the optional application of reanalysis at the $V'$ level of constituent structure. This is, however, just an instance of the participation of syntax in the formation of CPs; there can be ways in which syntax contributes to the shaping of the forms of CPs in natural language. An explanatory theory of CPs therefore must include a parametric subtheory that can yield the full spectrum of CPs. A possible parameter, I suggest, could be the following. Since the Chinese CPs are syntactically transparent and typologically analytic, the difference between Chinese and languages such as English could be that whereas the English CPs are lexical and hence fall within the domain of L-syntax (cf. the chapters by Williams and Hale & Keyser), the Chinese CPs are syntactic and formed via S(yntactic)-syntax. That is, in minimalist terms (Chomsky 1995), there can be a parametrization with respect to the syntactic levels – $V^o$, $V'$, $VP$, etc. – where a process of
‘lexical Spell-Out’ applies to feed Numeration. In English, the whole VP is lexicalized and turned into a $V^0$, as Hale & Keyser’s L-syntax depicts. In Chinese, however, all the ‘building blocks’ are directly accessed in Numeration and put into syntactic processing (cf. Lin 1999). As a result, a surface $V^0$ in English can correspond to a full VP in Chinese. The consequence for typology, then, is that Chinese is more analytic than English in having syntactically transparent compounds, and, conversely, English is more synthetic than Chinese in having word-internal complex structures. Limited by space, I cannot elaborate on this suggestion any further here. But it is clear that relativizing the boundary and relationship between lexicon and syntax may lead to rewarding explanatory theories for CPs and other grammatical constructions in human language.

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With this book, Bethin tries to bridge the gap between traditional and theoretical linguists. This twofold audience has been noticed in another review:

[1] I gratefully acknowledge the assistance of Marc Greenberg, Julia Fisher and Dan Jurafsky in the preparation of this review. Standard disclaimers apply.
Since I have approached this volume as a traditional Slavist, lacking a background in metrical syllabic theory, my review will necessarily reflect this bias. The ideal reader, of course, would have a background both in metrical syllabic theory as well as the basic facts of Slavic linguistics. (Feldstein 1998: 137)

Although Feldstein and I studied from some of the same teachers, our backgrounds are for the most part complementary; my overall orientation is generative and my current position is in general linguistics. Predictably, I share very few of Feldstein’s observations.² I therefore assess Bethin’s book with the non-Slavist in mind and refer any reader interested in a critique from the other perspective to Feldstein (1998).

Bethin divides the book into three roughly equal chapters. Chapter 1, ‘The syllable in Slavic: form and function,’ lays out several Optimality-theoretic constraints and provides an overview of the changes that took place up to Late Common Slavic. Chapter 2, ‘Beyond the syllable: prominence relations’, then discusses metrical-grid theory in combination with several very specific phenomena, especially in individual modern languages (e.g., the rhythmic law in Slovak). Finally, in chapter 3, ‘Theoretical considerations’, Bethin revisits many of the previously discussed phenomena from an even more theoretical standpoint.

The book’s front matter includes a detailed table of contents and list of abbreviations. However, the book’s sole map shows no more detail than the individual languages’ names (and Bethin’s somewhat non-standard divisions of Late Common Slavic (LCS) dialects). Where are readers expected to locate the various Serbo-Croatian dialects (e.g., Štokavian) geographically?

The back matter is equally surprising at both extremes: the endnotes (266–301) are very extensive; for this reason, footnotes probably wouldn’t have been a good idea. Bethin’s bibliography (302–346) is also thorough, although occasionally lapsing out of alphabetical order. On the other hand, the index (347–349) is skimpy and useful for looking up only names of languages and the most basic or traditional of terms.

A fundamental theoretical stance of the book is that ‘tone and stress are analyzed as different entities and if tone is characteristic of the mora,…stress is taken to be a property of syllables’ (116). Bethin expounds on this distinction throughout much of the book. Another central claim is that LCS was subject to Bethin’s MORAIC constraint, ‘Syllables must end in a moraic segment’ (28), which effectively limits the syllable rime (i.e., nucleus and coda) to moraic segments.³ For example, Bethin treats several fundamental

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² Indeed, the only criticism of Bethin’s book mentioned in Feldstein’s review (1998: 142) which I also noticed is the mistaken claim that all Slavic languages share word-final devoicing of obstruents (109). Serbo-Croatian has no such final devoicing.

³ See my concerns below about Bethin’s MORAIC compared to Prince & Smolensky’s CODA-COND.
processes in Slavic (e.g. velar palatalizations, iotation, vowel fronting) as syllable-structure effects – the result of constraints on feature-association not directly related to syllabic structure (38).

It’s also worth pointing out what Bethin’s book is not about: Slavic prosody, the main title (and the only title on the spine), is deceptively broad. However, the subtitle, Language change and phonological theory, is far more descriptive. Bethin’s approach, although a mixture of traditional and generative approaches, is decidedly diachronic.

Moreover, the book is limited to prosodic constituents up to and including the word, but not to larger constituents. That is, Bethin excludes discussion of clitic groups, phonological phrasing, or intonation. For example, in her discussion of penultimate stress in Polish (176–178) and antepenultimate stress in Macedonian (178–180), Bethin ignores the effect that enclitics have on a preceding word’s stress. This omission is unfortunate, considering the growing literature – summarized in Franks & Bański (1999) and Rudin et al. (1999), respectively – describing whether adding enclitics shifts the stress rightward. Thus, Bethin’s book is of little use to those who study larger prosodic constituents.

In a few instances Bethin’s theoretical notation departs significantly enough from the standard to cause confusion and even concern. For example, after writing that she takes no position on whether stress is best described by trees, grids, a combination of the two, or even alignment constraints (175), Bethin uses the following grid to formalize how Czech assigns initial stress (176):

\[
\begin{array}{c}
(1) \quad (s \quad w) \quad \text{Level 3} \\
(s \ w) \quad \text{Level 2} \\
(s \ w) \quad (s \ w) \quad (s) \quad \text{Level 1} \\
\sigma \quad \sigma \quad \sigma \quad \sigma \quad \text{Level 0} \\
\text{fi} \quad \text{lo} \quad \text{lo} \quad \text{gi} \quad \text{cký} \quad \text{‘philological’ (MASC.NOM.SG)}
\end{array}
\]

The bottom row shows the datum, with spacing added between syllables, in the standard orthography (where the acute accent signifies vowel length). To facilitate the following discussion, I’ve added the grid-level numbers.

Using \(\sigma, s\) and \(w\) instead of the standard \(x\) (or an asterisk) as grid marks deviates from convention, but this isn’t the problem; these are explained in an endnote. The confusion comes from Bethin’s introduction of level 2.

Level 0 depicts stressable units (= syllables). Level 1 then groups the syllables into bimoraic feet – either disyllabic trochees (in the first two feet) or a single heavy syllable (in \(eký\)); each \(s\) represents a primary- or secondary-stressed syllable. Level 3 then groups the foot heads into a prominence relation resulting in initial primary stress – i.e., the first foot is most prominent.

Bethin does write that level 3 ‘represents primary word stress and THE
INTERMEDIATE LEVELS [1 and 2] SHOW SECONDARY STRESSES on the third and fifth syllables’ (175, emphasis added). I agree that level 1 indicates secondary stress, but fail to see how level 2 contributes to this. Bethin’s inclusion of level 2 here is neither motivated nor explained.

Moreover, Bethin contradicts herself in the aforementioned endnote: ‘an asterisk on a higher level must have one below it on the immediately lower level’ (280). The w on level 3 has no grid mark under it.

To summarize my discussion of grid notation, then, I conclude that Bethin’s grasp of metrical theory is open to criticism. This is not to say that I dispute her proposals, either overall or in particular. For example, her basic claim with regard to (1) – that Czech can be formalized using trochees starting at the left edge of the word – remains valid.

Another area in which Bethin departs significantly from a theoretical framework is her MORAL constraint (discussed briefly above). She adopts this constraint to exclude various syllable-final elements in certain diachronic stages of Common Slavic. Crucially, Bethin requires a constraint that allows syllable-final liquids while ruling out other post-nuclear segments in the rime. Another constraint from Optimality Theory (OT) widely used for such purposes, which Bethin also adopts for separate reasons (38–39), is the —COD constraint (Prince & Smolensky, henceforth P&S, 1993: 85), which merely prohibits syllables from having codas; —COD serves essentially the same purpose as the so-called Law of Open Syllables, which traditional Slavists have used to account for the diachronic requirement that syllables in Common Slavic be C + V. Bethin’s reason for using MORAL, and not —COD, is that liquid codas remained during this period while glides and nasals were lost.

Bethin clearly makes the case for the —COD being insufficient to account for the so-called liquid diphthongs. My concern is that she overlooks another constraint in the OT literature: P&S (97–125) adopt a separate constraint, the CODA-COND(ition), which allows languages to draw a sonority threshold beneath which codas aren’t permitted. 4

Indeed, Bethin apparently harnesses —COD to account for phenomena generally handled by CODA-COND (46): ’Given that nasals are generally considered to be less sonorous than liquids, one might expect them to pose a more serious violation of the No Coda Constraint than would vowel plus liquid sequences’. This is just one instance which suggests that Bethin is not entirely conversant with OT.

One other concern I have, related to the previous ones, is Bethin’s treatment of antepenultimate stress in Macedonian (178–180). She utilizes final-syllable extrametricality, as in (2):

4 In the end, even CODA-COND fails to prohibit glides while allowing liquids as codas because the latter are less sonorous.
A recent approach to antepenultimate stress which doesn't rely on extrametricality is Hung's (1995: 141) proposal that an additional grid level be devoted to any footed syllable and a higher level devoted to foot heads, as in (3), using conventional notation:

(3) \( x \ x \ x \ 2 \) Foot heads
    \( x \ x \ x \ x \ x \ x \ x \ 0 \) Footed syllables
    pla ni na pla ni na ta

Hung argues, using Latin data, that the intermediate, footed-syllable level, along with her RHYTHM constraint (which requires a downward stair-stepping of grid marks), results in the same extrametricality effect. In Billings (1997, to appear), using Macedonian data, I support Hung's footed-syllables tier and a version of RHYTHM. Because Hung's work and mine are not widely known, I can't fault Bethin for overlooking them. It does seem strange, however, that Bethin's book is couched largely in OT, while portions rely on other frameworks of convenience.

The remainder of this review lists recurring problems with the book.

Bethin writes (42) that in Common Slavic ‘the long back vowel was simply /a/; /o/ could only be short’. This implies that there were no other back vowels. In fact, Common Slavic had a high, back vowel; /y/ < */a/ (44). Bethin seems to mean ‘the long back NON-HIGH vowel was simply /a/ …’

Bethin writes that ‘in… Macedonian what now appears to be a vowel plus liquid sequence once went through a stage with a syllabic liquid’ (69). Bethin then lists several vowel-liquid examples. However, she doesn’t explain two examples that appear not to have a vowel-liquid sequence, but rather a syllabic liquid: vrška ‘peak, summit’ and grlo ‘throat’. Agreed, most such sequences do have vowel-liquid sequences. So why list exceptions – without comment – when presenting the rule?

One obstacle in comparing Slavic languages is their varying orthographies, especially how prosodic properties are rendered. For example, Russian doesn’t usually render vowel-reduction, while neighboring Belarusian does; acute accents in Czech indicate vowel length, whereas their use in Serbo-Croatian indicates length and rising pitch. Certain prosodic symbols are discussed erratically – e.g., length/pitch diacritics (162); elsewhere, symbols remain unexplained (97). Overall, readers must decipher most diacritics on their own, especially in discussions of Slovene.

Only expert Slavists could peruse Bethin’s book without occasionally consulting other reference works.

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Not only are examples not tabulated, the notation is inconsistent. To be fair, Bethin appears to prefer using the individual languages’ orthography (or the standard transliteration of Cyrillic orthographies) whenever possible and shows this with italics; when various effects not rendered by the orthography are necessary, she uses square brackets. Unfortunately, Bethin occasionally slips and renders non-transliterated, presumably phonetic forms in italics (68): ‘R n’os (sic.) ‘he carried,’…’ In fact, [n’os] is the phonetic form but is spelled as nes.

A similar problem arises in a different context in Bethin’s use of Russian brat’ja (sic.) ‘brethren’ and ži’t’e (sic.) ‘life’ (90). In fact, these are transliterated as brat’ja and ži’t’e but pronounced [brát’ja] and [žit’jó] (or [brát’ja], [žit’jó]).

Bethin frequently uses lists of modern reflexes of historical phenomena. In many of these, very closely related languages with homonymous forms are nonetheless listed separately: ‘…B mraz, M mraz, …Cz mráz, Sk mráz, … R moróz, U moróz …’ (49), meaning ‘frost’. Why not just ‘…B/M mraz, …Cz/Sk mráz, …R/U moróz …’?

Occasionally, diagrams become confusing. For example, when two distinct diachronic stages are listed side-by-side in the same figure (61), it looked to me as though there were one three-stage development, which made no sense. By breaking this numbered diagram into a. and b. examples the graphics would be far easier to follow. All in all, the data are presented with the feel of a musty philological work.6

One common diacritic is using an apostrophe ‘ (or a prime ′) to indicate a preceding consonant’s palatalization. If only this convention were explained somehow for the sake of non-Slavists. Other diacritics are used inconsistently (albeit rarely): on successive pages (123–125) the first letter of the same Common Slavic form (glossed as ‘cart’) appears differently each time: first as u, then without the diacritic, finally with a slightly different diacritic (resembling an inverted háček) ů. For the most part, such typos are only mildly distracting.

All in all, this book is thorough and written by an authority in the field. It should therefore become a primary reference work on syllable- and word-level prosodic phenomena in Slavic diachronic and comparative phonology. Unfortunately, because of various problems with theoretical matters, I don’t foresee any major contribution to linguistic theory.

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[6] Contributing to this impression is Bethin’s convention of translating quotes only from non-Western languages: quotations in French, German and Spanish are in the original, while all those from Slavic languages are rendered into English.


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Blench and Spriggs took on a considerable challenge when they decided to edit the present volumes (at least two more have already been published); the subject area is so broad, and the range of papers rather mixed, that it must have been an editorial nightmare. The good news is that at last the historical linguists and the archaeologists are talking to each other and presenting their ideas and results in common fora, expanding and focusing a dialogue that will hopefully become normal and natural rather than awkward and exceptional. Nonetheless it remains clear that much of the ‘dialogue’ does not yet rise above individuals and small cliques speaking at cross purposes and without much in the way of shared assumptions.

Of course there has always been communication at some level between archaeology and linguistics, but a turning point was surely Renfrew’s 1987 book Archaeology and language: the puzzle of Indo-European origins. Renfrew’s bold attempt to synthesise archaeological and linguistic data, proposing models of language spread and chronologies of change, brought to the foreground many issues, laying down a challenge to the respective
research communities to seriously reconsider many assumptions and expand the interdisciplinary dialogue. We see now that Renfrew is simply one of many voices in a rapidly growing and still chaotic cross-disciplinary research effort that holds much promise in the worthy pursuit of better understanding human origins. Another, very similar collection of papers was recently published in Melbourne (McConvell & Evans 1997; reviewed in this volume, pp. 449–451), and it is notable that there is a great overlap of contributors and topics of papers between that book and the volumes under review. Much of the discussion revolves around correlating proto-language homelands and archaeologically attested settlements/cultures, and papers on this subject cover a diversity of regions and time periods.

In total there are 38 papers over two volumes, far too much material to discuss in detail here. The approach I have taken is to attempt to give some overview, while examining in more detail several papers which connect with my own expertise and special interest. My overall assessment of the books is that they contain much that is useful and insightful, but it takes a lot of careful digging to find it – just as often the papers also illustrate the problems of trying to grapple with topics that are beyond one’s normal specialisation: there are many pitfalls along the path to the ‘grand synthesis’.

Volume I presents 21 papers under four headings. These and the respective contributors are: Part I, Prehistory of language, Bernard Bichakjian, Gábor Győri, Paul Bouissac; Part II, Deep-level linkages/hypotheses, Irén Hegedűs, Marcel Otte, Colin Renfrew; Part III, Problems of method, James Mallory, Johanna Nichols, Ilia Pejros, Victor Shnirelman, Roger Blench, Lawrence Reid, Malcolm Ross, Anand Raman & Jon Patrick; and Part IV, Oral traditions, Lidia Ashikhmina, Joseph Eboreime, José Garanger, Daniel Frimigacci, Margaret Sharpe & Dorothy Tunbridge, Jerry Taki & Darrell Tryon, Inger Zachrisson.

Part I of Volume I might have been better titled ‘Evolution and language’, each of the three papers dealing with cognitive issues on the basis of biological and linguistic evidence. In the first of these Bichakjian kicks off by summarising what the scholarly world outside of America always knew, namely that the Chomskyan view of innate grammar without any specific phylogenetic history is an untestable nonsense based on tautological rhetoric, and remains so despite the various repackaging efforts on offer (such as Pinker & Bloom 1990). Emphasising an interdisciplinary approach, Bichakjian reminds us:

But linguists are not alone in their rejection of the prewired model. The evidence is also lacking at the neurological side of the interface. That the human vocal tract has been adapted to speech and that humans have developed neurons which together are capable of processing linguistic messages is of course true, but this cerebral material is a genetically coded form with a functional potential, within biological constraints…
The greater part of the paper is a discussion of apparently unidirectional changes observed (or at least reconstructed) in the typology of language change. The idea is that if such unidirectionality can be established, then we begin to have a basis for discussing evolution of language (as opposed to evolution of languages). Bichakjian discusses various developments from Proto-Indo-European phonology, morphology and syntax, e.g. the growing complexity of vowels systems; the emergence of passive voice and aspectual systems, to name a couple. However, I find myself questioning the validity of Bichakjian’s approach – linguistic reconstruction is based strongly on a uniformitarian assumption that any language we can reconstruct is not of a different type to known languages (otherwise the method would know no constraints) and furthermore the range of data presented falls somewhat short of a typology of unidirectional change.

The limitations of Bichakjian’s approach may be overcome to some extent by the prospects of cognitive archaeology, discussed in the next two papers by Gábor Györi and Paul Bouissac respectively. Like the previous paper they are both rather short, but they do raise very important issues. Without going into detail, they both make the point that language must have evolved over a long period of time within a cultural and cognitive complex that only archaeology can possibly throw any light upon. Thus consideration of evidence of cognitive activities, such as rock art and ceremonial practices, assumes an importance comparable to that which palaeography has for historical linguistics.

Parts II and III of volume I both deal with the ‘homeland’ problem. The contrast between papers is striking, and quite instructive. For example, we find Colin Renfrew and Johanna Nichols in separate papers discussing with some confidence their own models of ancient language spread, both discussing various language families but putting a lot of emphasis on Indo-European, in respect of which their views appear to be largely irreconcilable. Very interesting is James Mallory’s detailed evaluation of four Indo-European homeland proposals: (1) Baltic-Pontic(-Caspian), (2) Anatolia, (3) Central Europe-Balkans, (4) Pontic-Caspian (interestingly Nichols’ Turkestan option doesn’t rate highly with Mallory or just about anyone else for that matter). In each case he summarises and attempts to reconcile the linguistic and archaeological evidence, and after sifting through many contradictions and competing factoids ultimately favours a sort of hybrid of the Baltic-Pontic and Pontic-Caspian hypotheses. It is both a useful orientation to the issues, and a stunning reminder of how far we are from finding any consensus on this important question. Kathrin Krell, who in volume II discusses the PIE homeland question, is able to offer no more of a conclusion than the hope that:

Through the development of new theoretical models, further research on language change – both at the structural and at the sociolinguistic level –
and better interdisciplinary co-operation, the debate over the origins of PIE may perhaps one day be resolved. (281)

I found myself asking, if we cannot even agree on the localisation of the most thoroughly studied truly ancient proto-language, how can we begin to offer useful advice to the archaeologists?

Cutting straight to the chase Ilià Pejros boldly asks ‘Are correlations between archaeological and linguistic reconstructions possible?’ with the title of his paper. I say ‘boldly’ because so many of the other contributions assume a positive answer to this question without imagining the alternative, yet Pejros argues strongly for a conditional negative. The core of his case is that our knowledge of a language (or proto-language) allows us to draw conclusions about the world of the speakers, including the ecological, ethnic and cultural dimensions. However, this is the world as it is known to the speakers of that language. As there is no necessary connection between the forms of the lexemes which code the various meanings and the meanings themselves, the possibility exists (and is widely attested) that:

Members of two communities can speak the same language(s) yet have totally different material cultures, or having similar material cultures they can speak absolutely different languages. Change in one characteristic does not necessarily imply changes in others. (156)

That this is true is incontrovertible, yet the implications may be unwelcome to those who wish to correlate archaeological and linguistic data, namely that:

The only justifiable step we can take is to connect a proto-language with a territory, which according to archaeologists reveals the same deep representation of material culture as is found in our linguistic cultural reconstruction. In so doing we cannot talk about communities at all. What we can demonstrate is that the language was spoken in the area, but there is no way to show that it was the only language, nor that one single community occupied the territory. (157)

I find Pejros’ case is well argued and has wide-reaching implications. Consider the issue of the Chinese homeland – the archaeologist Peter Bellwood (1994: 400) is puzzled by the views of many linguists:

I strongly suspect […] that the Han Chinese cultural tradition has always been focused in the Yellow River basin, where the archaeological record has evidently been unbroken for 8000 years and where Chinese civilization eventually developed. If the Chinese migrated into this region from the mountains of inner Asia, as some linguists have suggested, then the archaeological record seems to have overlooked the fact.

Accepting Pejros’ understanding outlined above, we should not be surprised in the least to find no correlation between the continuity of material culture
found in the Yellow River basin and the suggestion that the language eventually adopted by those communities may have come from somewhere else – it is simply a case of language shift, and does not require entire population replacement by massive migration, although some population movement must have been involved. Contra this Bellwood simply states that there would have been no reason for the culturally advanced (pre-)Chinese to adopt the language of another group, but really this only means that he does not know of or accept any reason why such may have happened – clearly it will take more than just linguistic evidence and arguments to sway this archaeologist.

The last Part of Vol. I, *Oral traditions*, deals with correlating oral lore of indigenous peoples with history reconstructed by archaeology and/or linguistics. Among the various contributions I found Margaret Sharpe & Dorothy Tunbridge’s ‘Traditions of extinct animals, changing sea-levels and volcanoes among Australian Aborigines: evidence from linguistic and ethnographic research’ particularly interesting, and relevant to my own experience. Naturally one must be very careful with the use of indigenous ‘knowledge’ – Sharpe & Tunbridge begin by telling us of an aboriginal origin myth from New South Wales that when first recorded tells of three brothers coming to Australia in a canoe, while a more recent version has them arriving by sailing ship! The implication is clear – oral lore is dynamic, and we cannot presume that it correctly communicates the facts of history without additional confirmation. The difficulty however is how to establish a reliable correspondence between such stories and a history indicated by science.

A spectacular example of this problem is illustrated by various stories of flooding and/or land sinking which were related to white settlers by aborigines. Sharpe & Tunbridge (quoting from Blake 1991) tell us how settlers recorded Victorian aborigines explaining that Port Phillip Bay was once dry, only to be filled with water after some catastrophe. On one occasion an aborigine specifically explained that the heads of the bay are the old course of the Yarra River. These stories mimic the geological facts, which show that the sea level was so low as to make Port Phillip Bay a dry plain through which the Yarra flowed some 10,000 years ago. The astonishing suggestion is that there has been an unbroken communication of real events over such a period of time. I personally take a very skeptical view, preferring to see this as a case of chance correspondence by convergence rather than cognate stories. Around Port Phillip Bay, particularly Corio Bay, and along the Victorian coast at many places, one can observe large areas of sand bank and/or very shallow water at low tide, and within historical times there have been significant changes in the coastline – one does not have to be a geologist to turn such observations into stories about inundation of the land. As for the old course of the Yarra, it is obvious from any low hill that Port Phillip Bay is a bay with only one narrow outlet (known as The Heads), through which passes the infamous ‘Rip’. The tidal flow out of The Heads peaks at
around 8 knots, and looks like a tremendous river of water heading out to
sea to anyone standing at either of the points. It is not difficult to make a
parallel between this flow and outflow at the mouth of the Yarra.
Furthermore, standing at The Heads, one could readily imagine the
tremendous flow continuing and emptying the bay completely. I am sure that
the aborigines are as imbued with imagination and speculative ability as any
race.

Volume II presents 17 papers under three headings. These and the
respective contributors are: Part I, Correlating archaeological and linguistic
sequences, Kevin MacDonald, George van Driem, Charles Higham, Matthew
Spriggs, Peter Belwood, Malcom Ross, Nick Evans & Patrick McConvell;
Part II, Migration and expansion and their linguistic correlates: Eurasian case
studies, Juha Janhunen, Ludmila Koryakova, Johanna Nichols, Kathrin
Krell, John Hines; Part III, Linguistic models in reconstructing subsistence
systems, Søren Wichmann, Bruce Connell, Alexander Vovin, Ilia Pejros &
Victor Shnirelman, Waruno Mahdi.

A very refreshing and challenging contribution is made by Evans &
McConvell, well-regarded Australianists who have put a lot of thought into
the problems of Australian prehistory. The absolute level of material culture
of the aborigines, and the strong tendency for cultural diffusion between
aboriginal communities, make the traditional hope of archaeologists, to link
material assemblages with particular cultural groups, difficult in the
Australian context. Thus an especially heavy burden falls upon linguistics to
offer insights into the history of the peoples of the continent. Accepting the
common view that Pama-Nyungan languages represent a genetic entity that
spread over 7/8 of the continent leaving only residual areas of original
diversity (the Northwest, Tasmania etc.), Evans & McConvell try to propose
mechanisms for this expansion and correlations with the limited archaeo-
logical data. The linguistic evidence for Pama-Nyungan is discussed briefly,
and the authors are confident that it is a real family, comparable in age and
complexity to Indo-European. In addition archaeology lends some support
to these views by documenting the so-called ‘small tool tradition’ which
spread from northern Australia in a manner and at a time which corresponds
to (or at least suits) the proposed expansion of Pama-Nyungan. Of course
these views are hotly debated in Australia, but it is not the question of
whether or not Pama-Nyungan exists that interests this reviewer, but the very
interesting suggestions about mechanisms of language spread, which may
have much wider implications.

As Evans & McConvell state, ‘discussion of language expansion has
hitherto been dominated by agriculture-driven models’ (189) yet these can
only hope to explain a fraction of the language spread which has occurred in
human history. In this case a social mechanism is proposed which
reconstructs a shift from isolated to linked structures (e.g. endogamy to
exogamy, local ceremonies to joint ceremonies, etc.) ‘as a sort of trans-
continental chain-reaction, by the successive invitation of ‘isolated’ groups to become induced in the ceremonial culture of the ‘linked’ Pama-Nyungan groups (185–186). Clearly the implications go far beyond Australia – the same model may help explain the ancient expansions of African groups such as Niger-Congo or Khoisan, for example.

Rather closer to my own area of specialisation, Higham gives us a tour of language families of Southeast Asia, interspersed with archaeological dates, speculations and generalisations. The paper is illustrated with four maps, the usefulness of which is questionable – I note with concern that figure 3.1, *Distribution of the Austroasiatic languages in Southeast Asia*, completely omits the Khuic languages, a major group which dominates northern Laos and also occurs in Thailand and China, and also fails to indicate various Waic and other Mon-Khmer languages of China. These omissions are significant, because in a discussion about homelands one must carefully consider the actual language distributions. In this case the languages which are missed are those which are located closest to the presumed homeland site (Southern China)! Higham’s main point is to explore the consequences of accepting a relationship between Austroasiatic and Austronesian, a view which has received renewed support lately (e.g. Reid 1994, Diffloth 1994). However, Higham is far too enthusiastic when he characterises these as ‘confirmation of Schmidt’s Austric hypothesis’ (107), but how are archaeologists to assess the merit of conclusions based on linguistics and vice versa? Unfortunately the answer is that they cannot. Higham mentions a number of archaeological sites which tally with the spread of rice cultivation from southern China, but like his maps, the data is too fragmentary to be very useful. He concludes that linguistic evidence is useful for understanding the archaeological record, but it remains clear that it is still early days.

Pejros & Shnirelman also discuss linguistic evidence for the origin and dispersal of domesticated rice. Most significance is placed on words for rice reconstructed for various proto-languages (which I arrange in numbered columns below):

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<tr>
<td>PST</td>
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<td>*bria(s)</td>
<td>*majH ~ *malH</td>
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<tr>
<td>PA</td>
<td></td>
<td>*pag’aj</td>
<td>*beRas</td>
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<tr>
<td>PMY</td>
<td>*mblai</td>
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<tr>
<td>PMK</td>
<td>*Casaj</td>
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PST (Proto-Sino-Tibetan), PA (Proto-Austronesian), PMY (Proto-Miao Yao), PMK (Proto-Mon-Khmer)

The rather obvious resemblances between PST and PA forms in columns 2 and 3 are striking, and constitute prima facie evidence for ancient contact between PA and PST communities. For a combination of archaeological and linguistic reasons Pejros & Shnirelman suggest that the domestication of *O.*
Oryza *japonica* occurred in an area overlapping the Sino-Tibetan homeland (sub-Himalayan to Upper Yangtse), so that either Sino-Tibetan speakers themselves domesticated the crop or they acquired it from another local group. The further suggestion is that the proto-Austronesians subsequently took both *O. japonica* and *O. indica*, and words for them, from the Sino-Tibetans. This poses the question of the Austronesian homeland—the presently favoured view among Austronesianists is that the localisation must be on Taiwan or perhaps the adjacent Chinese coast. Pejros & Shnirelman argue that the sub-tropical climate suggested by the PA lexicon puts the homeland further north ‘probably in Jiangsu or even Shandong’ (381). In or not far from such locations Sino-Tibetan speakers were certainly already settled by the time of PA, which sits well with the linguistic evidence. More questionable is their suggestion that PMK *Casaj* is the source of PA *pag’aj* and PMY *nuhlaj*. They state matter-of-factly ‘Either they represent an ancient, probably Proto-Austric, term for rice, or alternatively the word was borrowed from one language group into another after the break-up of the proto-language’ (383). Frankly I see no formal basis for treating these as related forms, nor do the writers give us any reason to (i.e. the resemblance is poor and no system of correspondences is given or referred to). Thus the appeal to a ‘probably Proto-Austric term for rice’ (383) is one that will impress few specialists.

On balance, I am very pleased to see this series in press, and I congratulate the editors on their achievement. Even though the contributions are somewhat mixed and one can find various contradictions and inconsistencies among and between them, they have succeeded in presenting a valuable contribution to interdisciplinary cooperation.

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REVIEW


Reviewed by James P. Lantolf, Pennsylvania State University

This volume contains selected papers from the 1996 Sheffield University cognitive studies conference. Its aim is to awaken interest in the relationship between language and thought, which, according to the editors, has been dichotomized into a communicative and a cognitive conception. The former hypothesis asserts that language and thought are autonomous entities with language serving only to express thoughts formed, ostensibly in Mentalese, by our central cognition. The supra-communicative version of this conception maintains fundamental independence of thought and language but allows language to facilitate and augment thinking. Although the cognitive conception accepts Fodor’s modularity hypothesis, it argues that internal speech, in the form of natural language sentences, and not Mentalese is the medium through which the submodules of central cognition conduct their business. A stronger, Whorfian, version of the cognitive conception sees mind itself as shaped by language. The volume is organized into three sections: language, development and evolution; language, reasoning and concepts; language and conscious reasoning.

While the book has much to offer, it has one important shortcoming. In their introductory chapter the editors mention the important Russian work on the language/thought interface. I was pleased to finally encounter some recognition of this research in the cognitive science literature, but unfortunately, too little attention is paid to the rich literature that the Russians, and followers of their tradition in the West, have developed over much of the course of this century. Symptomatic of the problem is the disagreement between proponents of the alternative conceptions of language on how the Russian research figures into the picture. The editors seem to believe that the Russian work supports the cognitive conception. Clark’s chapter, however, evokes Vygotsky’s, and related, work to bolster the supra-communicative hypothesis. I am inclined to side with Clark; but this is not my point here. There is a relevant literature out there that could have been more thoroughly mined. As it turns out, one of the few bibliographic citations of this work, Vygotsky’s well-known Thought and language, is inaccurate. Although the English-language version of Vygotsky’s book indeed appeared in 1962, its translator, contrary to the bibliography (315), was not Alex Kozulin, but Hanfmann and Vakar. Kozulin, in fact, translated and edited an expanded version of Thought and language in 1986.

The initial set of chapters considers evidence from abnormal language acquisition, pathological language use and evolutionary speculation. Goldin-Meadow and Zheng report on a case study of a deaf child of hearing parents.
capable of expressing the notions of ‘path’ and ‘agent’ with motion verbs that could not have been learned from a conventional language model. If, as the authors suggest, although with appropriate caution, this knowledge is part of what children bring to the language-learning setting, there are at least some thoughts in which language is not implicated – some support for the weak communicative hypothesis.

Using evidence from Williams syndrome children (i.e., mental retardation with relatively intact language ability) and from children with Specific Language Impairment, or SLI (i.e., language impairment with relatively normal mental ability), Boucher’s chapter raises problems for conceptions of language. If language were implicated in thinking, Williams children should be normal thinkers; but they are not. On the other hand, if language were constitutive of thought, SLI children should show normal non-verbal cognitive abilities; but they do not. Although the editors argue that the evidence from SLI children fails to support an autonomous Grammar Acquisition Mechanism, the picture which emerges from the Williams children does not fully support the general processing alternative proposed by Bates and MacWhinney either. For Boucher the matter remains open pending more precise definitions of intelligence and cognition.

Gomez, in his chapter, explores human cognitive/linguistic evolution. His partly speculative discussion of human linguistic and cognitive phylogenesis challenges the discontinuity hypothesis. According to Gomez, the problem with this view is that it identifies language with syntax and overlooks the fact that syntax itself serves a larger communicative system – a system we share with our closest relatives, the great apes. Great apes can deploy a Shared Attention Mechanism, which allows them to signal and infer an intention to communicate about specific entities in the world. Although the common ancestors of humans and apes may have shared a system in which communication and thinking were intertwined, the problem is to account for the evolution of syntax. To make a long story short, one branch of the primate tree evolved the capacity to use verbal sounds instead of gaze to pick out objects of intended mutual attention. The capacity for second order representations bestowed upon the hominid branch a clear advantage over its relative, because now mutually shared attention could be achieved more efficiently. This in turn precipitated a race between LAD (i.e., Language Acquisition Device), or those capable of producing linguistically complex descriptions of the world, and TOM (i.e., Theory of Mind), or those better suited to interpret these utterances. An even greater advantage was enjoyed by individuals (e.g. modern humans) who controlled both processes. Although LAD and TOM remain separate systems in modern humans, they are open to mutual influence.

A potential problem with Gomez’s story, as work in corpus linguistics shows, is that interactions between relatives and close friends favor greater reliance on paratactic rather than syntactic speech. If similar interactions
were prevalent in the life of our early ancestors, it is difficult to imagine how there could have been sufficient evolutionary pressure for syntax to complexify beyond what is required for casual familiar conversations. This same problem confronts Carruthers’ evolutionary story, which is predicated on the idea that cognition is freed up for other purposes through the development of grammar.

Carruthers attempts to make the case for the cognitive conception of language. Although he accepts Fodor’s brand of modularity for the peripheral input systems, he rejects Mentalese as the language of the central processor and instead posits a quasi-modular (i.e., not fully encapsulated) central cognition. Information from the peripheral modules, including verbal input in the form of **internal speech**, is capable of penetrating central cognition and serves as a medium of communication between its quasi-modules. Internal speech then interfaces with central cognition at the level of Logical Form and is therefore syntactic in nature. This is at odds with Vygotsky, who conceived of inner speech as pure meaning. Carruthers may be right, but to make his case convincing he will need to deal with the alternative perspective in some depth.

Carruthers predicts that if the communicative hypothesis is correct, language pathologies should not disturb any of the central processes, and therefore, disturbances of the language system should impact on such central processes as theory of mind, social detection, causal-explanatory attribution, and counterfactual thought. He further argues that the supra-communicative conception can’t be correct because internal speech occurs not only when the mind is working on some problem, but also when we are engaged in such leisure activities as day-dreaming. But Sokolov’s research shows, as Vygotsky predicted, that increases in problem complexity correlate with increases in the frequency and intensity of inner speech. Luria’s (1973) research with brain-damaged individuals also has to be brought into the picture. His work shows, for example, that patients with frontal lesions generally have intact grammatical systems, but lose the regulatory function of speech, which often results in difficulties in constructing narratives but not in echoing utterances of others. This seems to jibe better with the supra-communicative perspective.

The chapter by Varley offers some support for the communicative hypothesis. Varley considers the theory of mind and causal reasoning abilities of a rare agrammatic and apropositional patient exhibiting problems with false belief tasks who was only able to complete them satisfactorily after training. The patient produced normal responses on reality or causal attribution tasks. She offers three potential explanations for this performance: despite production and comprehension problems, the patient still possesses implicit grammatical knowledge at some deeper level; Mentalese, rather than natural language, underlies his thought processes; the tasks do not tap into propositional representation. Varley notes that the patient was not language-less and had fairly rich nominal resources that he
could have relied on to complete the tasks. A fourth possibility, not entirely unrelated to this one, is derivable from Vygotsky’s notion of inner speech. Due to the patient’s language difficulties, the experimenters communicated with him about the tasks through gesture. McNeill (1992) proposes that gesture and speech are linked at the level of inner speech. Given that the patient was able to interpret the experimenter’s gestures, his inner speech was most likely undisturbed by the trauma; this could have allowed him to perform the experimental tasks. In his chapter, Siegal, in fact, suggests something similar. Thus, as Varley’s third explanation suggests, the patient’s conceptual structure was to a degree intact, which could be construed as supporting the cognitive hypothesis.

Siegal argues that a correlation between immature theory of mind and immature language in ontogeny fails to tell us anything about the directionality of the relationship. It could be that theory of mind is a precondition for, or consequence of, syntax of attitude reports; or it could be that some other feature is the source for both processes. Siegal directs the causal arrow from syntax to theory of mind, since in an utterance such as ‘When did the little boy say how he hurt himself?’ it is difficult to imagine how a theory of mind would lead a child to answer the embedded question.

Clark’s chapter argues for the supra-communicative conception of language. He considers six domains in which language serves as a tool for cognition: memory augmentation (e.g. diaries, shopping lists), environmental simplification (e.g. words that group together specific entities and events), coordination and reduction of on-line deliberation (e.g. planning what to do and how to do it, which reduces cognitive load), taming path-dependent learning (e.g. education, in which ideas migrate from mind to mind and build on previous ideas), attention and resource allocation (e.g. mental rehearsal to focus and monitor behavior), data manipulation and representation. Language can satisfy these mental functions because it serves these same functions in the public domain.

Echoing Vygotsky, Clark speculates that the mind may extend beyond the head and into those domains mentioned above. Only one of the components of mind, the brain, is physically located inside of the head. Clark then pulls back to a more conservative position in which language is seen to be merely implicated in our computational capacity. I wonder if it is too presumptuous to speculate that Clark might be more willing to embrace his own speculation if he had more intimate contact with the Russian research?

Clark believes that Dennett’s take on things represents a version of the supra-communicative conception of language. For Clark the difference between himself and Dennett resides in the nature of the effect of language on neural organization. He attributes to Dennett a Whorfian stance in which language not only mediates mental functioning (also Clark’s viewpoint), but also reprograms the brain itself. Interestingly, the editors see Dennett’s views as supporting the cognitive hypothesis.
Sperber & Wilson’s chapter, based on their relevance theory of communication and cognition, argues that since speakers have a richer conceptual than lexical store, language is neither constitutive, nor fully reflective, of mind. People can convey meanings even when they do not have a corresponding public word for a concept. Hence, shared concepts are not a necessary condition for comprehension and words and utterances serve as indexicals to a speaker’s contextually intended meaning. Rommetveit (1992), in his dialogical approach to human cognition and communication, would call this ‘attunement to the attunement of the other’.

In his chapter, Laurence rejects a Gricean-based natural language semantics in favor of a model based on Chomskian grammar. He believes that the assignment of the semantic features of utterances is independent of thought and intention and occurs at the interface of language and our conceptual systems. According to the editors, this position is consistent with either the cognitive or the communicative conception of language.

In one or two places I found Laurence’s discussion problematic. As an argument that language is not solely a cultural artifact, he cites Pinker’s claim that if language were indeed an artifact we would expect to find a correlation between linguistic complexity and cultural development, but supposedly we don’t. This is worrisome, however. For one thing, I suspect there would be considerable disagreement about what constitutes cultural development. How do we measure it? And measures of linguistic complexity have been notoriously problematic. The fact that biology is implicated in language acquisition does change its status as a cultural artifact. Humans have the capacity to convert a wide array of biological entities into cultural artifacts, including our physical bodies.

The final set of chapters addresses the connection between language and conscious thought. Davies’ chapter is sympathetic to the kind of anti-Mentalese, pro-natural language, hypothesis proposed by Carruthers. He points out, however, that to substantiate this perspective requires evidence that sentences serve thought at the sub-personal, and presumably subconscious, level and not just at the conscious level, as Carruthers contends. It would be worthwhile for Davies to consider Searle’s recent proposal (1992) that there is no sub-personal computational level at all. Mind, for Searle, encompasses the non-conscious bio-chemical processes of the brain and conscious processes carried out in part via language.

Frankish, in his chapter, proposes a two-tiered system of mind – a non-linguistic, passive, tier that we share with animals, and an active, uniquely human, conscious tier that is primarily language (i.e., inner speech) based. On Frankish’s account, inner speech is used to construct mental states through premising, carried out in linguistically formulated propositions, which allow us to manipulate, adopt or reject available alternatives. Although Frankish claims his analysis supports a version of the cognitive conception of language, the following statement appears to be more in line with the supra-
communicative perspective: ‘it might be better to say that we think with language rather than in it; language provides a kind of scaffolding for our premising activities’ (266).

Again echoing Vygotsky’s theory, Perner’s chapter focuses on higher-order thinking processes, including planning, decision-making, inhibiting habitual-responses, and trouble-shooting. According to Perner, higher-order thought is meta-representational and therefore entails language. The key resides in intentional control of our actions, which requires that we represent to ourselves the situation, the action and the goal or a potential action. The representation is hypothetical and exists only in thought, and it is this knowledge that brings its content into awareness. Perner points out that since in producing and coordinating normal language, we coordinate ‘the linguistic vehicle with its intentional content’, meta-representational mental processes also implicate ‘the intentional relationship between the linguistic vehicle and its content’ (283–284).

The final chapter, by Dennett, is based in part on his concluding address at the Sheffield conference. Dennett reiterates his case for natural language-based consciousness. Like Carruthers, he believes that consciousness allows us to tell and question others, and ourselves, about what we are doing. Perhaps, however, as Vygotsky believed, it is (goal directed) linguistic activity itself that constitutes consciousness. Through linguistic activity, we tinker with, and refine, our resources, ‘turning our brains (and all the associated peripheral gear we acquire) into a huge structured network of competencies’ (292). Moreover, like Vygotsky, Dennett sees a dialectic connection between our acts and our being in that we are the agents of our acts and our acts are the makers of who we are. In what seems to be a position very close to that of the great Neapolitan scholar, Giambattista Vico, Dennett brings our inner life back into focus – a perspective that has been missing in mainstream psychology and cognitive science for too long.

The book is worth reading for those concerned with the interface between thinking and speaking. However, I don’t believe this issue can be adequately broached without paying serious attention to the work of Vygotsky, Luria and those who work within their theoretical perspective on language and mind.

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Embedded in the Minimalist Program, the book is concerned with how linguistic structures are derived and represented. The general ideas of the Minimalist Program are rather simple. The grammar essentially contains a computation system of the human language (C_n), which builds or generates linguistic structures out of lexical items drawn from the lexicon. C_n is restricted to the operations Merge (concatenation), Move and Erase, which are subject to economy principles of derivation like the Minimal Link Condition, Procrastinate or the Shortest Derivation Condition. The structures legitimately derived by C_n are said to converge and are further constrained by the principle of Full Interpretation, the economy principle of representation. A structure conforming to all these principles is the representation for a given set of selected lexical items, the numeration (chapter 1).

Kitahara suggests enriching the operation Merge to include not only concatenation, but also replacement, taking as input a phrase-marker Σ containing K and α in (1a) to form a new phrase-marker Σ’ as in (1b), where t(α) is the trace of α (chapter 2):

(1) (a) \[ \begin{array}{c} \ldots \\ \Sigma \\ \ldots \\ K \end{array} \] (b) \[ \begin{array}{c} \ldots \\ \Sigma’ \\ \ldots \\ L \end{array} \]
Kitahara then shows how **Strict Cyclicity**, barring operations applying to a smaller domain if some operation has applied to a larger domain, can be derived from the Shortest Derivation Condition, requiring that the number of elementary operations (now including Replacement) necessary for convergence be minimized. Thus, the non-cyclic derivation of the example in (2) is excluded since it involves two elementary operations applying to *what*: one Merges it with the embedded clause, and the other Replaces the complement of *wonder* with the new phrase marker formed by Merging *what* with the embedded clause, as shown in (3):

(2) *How did John wonder what Mary fixed?*

(3) (a) \([\text{cr} \text{ how } [\ldots \text{ Cwh } \ldots \text{ John wondered } \text{ cr }] \text{ Cwh } \text{ Mary fixed what t(how) } ]]]]]]]
(b) \([\text{cr} \text{ how } [\ldots \text{ Cwh } \ldots \text{ John wondered } \text{ cr }] \text{ Cwh } \text{ Mary fixed t(what) t(how) } ]]]]]]

The cyclic derivation of (2), however, is admitted, since it involves no application of Replacement, as shown in (4):

(4) (a) \([\text{cr} \text{ what } [\ldots \text{ Cwh } \ldots \text{ Mary fixed t(what) how } ]]
(b) \([\text{cr} \text{ how } [\ldots \text{ Cwh } \ldots \text{ John wondered } \text{ cr }] \text{ Cwh } \text{ Mary fixed t(what) t(how) } ]]]]]]

But the derivation in (4) violates the Minimal Link Condition, according to which the matrix C should attract the closer *wh*-phrase *what* in the embedded SpecCP, rather than the adjunct *how*.

Kitahara argues that the Procrastinate principle in the Minimalist Program, according to which the number of overt operations necessary for convergence must be minimized, can also be derived if Erasure is taken to be an instance of Replacement, which replaces a formal feature \(F\) in \(\alpha\) with an empty element \(\emptyset\), mapping a structure like the one in (5a) to the one in (5b):

(5) (a) \[\Sigma \]

```
Σ
...
(\{\text{F}\})
```

(b) \[\Sigma' \]

```
Σ'
...
(\{\})
```

Kitahara proposes to revise Chomsky's (1995) formulation of the **Strong Feature Condition**, viz. `Suppose that the derivation \(D\) has formed \(\Sigma\) containing \(\alpha\) with a strong feature \(F\). Then \(D\) is canceled if \(\alpha\) is in a category not headed by \(\alpha'\),` as `Spell-Out applies to \(\Sigma\) only if \(\Sigma\) contains no category
with a strong feature’, and demonstrates how together with the Shortest Derivation Condition the Strong Feature Condition derives the timing of verb movement in English (cf. *John often kisses Mary vs. John often kisses Mary, Pollock 1989). Kitahara assumes the theory of feature movement according to which overt verb movement raises not just the formal features FF(V) of the verb, but also the phonetic and the semantic features, PF(V) and SF(V) respectively, whereas covert verb movement raises only FF(V). Now, if the verb moves to T in overt syntax, then FF(V), PF(V) and SF(V) all move; consequently, at least one application of Erase must be exercised to eliminate SF(V), presumably at Spell-out since the representation would then be sent to PF where SF(V) is not interpretable. But the derivation with covert verb movement requires no such application of Erase, and hence must be selected to satisfy the Shortest Derivation Condition. In a similar fashion, Kitahara shows how the timings of object shift in Icelandic, and expletive insertion (cf. *there seems to be someone in the room vs. there seems someone to be in the room) can be derived.

While it is perhaps unsurprising that the Superiority Condition, which bars moving a wh-phrase over a c-commanding wh-phrase (cf. I wonder who bought what vs. *I wonder what who bought), may be derived from the Minimal Link Condition, a more interesting case is the grammatical contrast in (6), which used to fall under the purview of an independent Proper Binding Condition (Fiengo 1977), as t(who) in the wh-phrase in the matrix SpecCP in (6b) is not bound (chapter 3):

(6) (a) \( ?\text{[who \ did \ [ which picture of t(who) ] \ [ \text{[John likes t(which picture of who) ]\]}}} \]
    (b) \( *\text{[who \ did \ [ which picture of t(who) ] \ [ John likes t(\text{which picture of who) ]\]}} \]

The representation in (6a) is derived by first moving the wh-phrase which picture of who to the embedded SpecCP and then moving who out of it. This derivation involves only one violation of the Minimal Link Condition, as the matrix C should attract the whole wh-phrase which picture of who from the embedded SpecCP, assuming here that \( \alpha \) is closer to \( \gamma \), \( \gamma \) c-commanding \( \alpha \), than \( \beta \) is if \( \beta \) is a constituent dominated by \( \alpha \). By contrast, the representation in (6b) is derived by first moving who out of the wh-phrase object of the embedded verb likes to the embedded SpecCP, violating the Minimal Link Condition since the embedded C should attract the whole wh-phrase, and then moving the object of likes to the matrix SpecCP, again violating the Minimal Link Condition, since the matrix C should attract who in the embedded SpecCP. The Shortest Derivation Condition excludes the derivation in (6b) with two Minimal Link Condition violations in favor of that in (6a) with one.

Kitahara proposes an ingenious account for the distinction between adjuncts and arguments with respect to extraction, having to do with the
Chain Formation Condition: ‘Move to form \( \geq 1 \) chain(s) only if it is legitimate (= violation free)’ (chapter 4). In the derivation in (7a), there is no chain headed by the adjunct how, owing to a Minimal Link Condition violation by the move to the matrix SpecCP; the matrix C should attract whether:

(7) (a) \*\(\lbrack\text{do} [\text{whether} \lbrack\text{John fixed the car}\rbrack]\rbrack\rbrack\)
(b) \?\(\lbrack\text{what} [\text{you wonder} [\text{whether} [\text{John fixed}]]]\rbrack\rbrack\)

Consequently, how has no variable to bind, resulting in vacuous quantification (cf. \*who did you see Bill? where who binds no variable); Full Interpretation is violated.

By contrast, in the derivation in (7b), even though movement of what to the matrix SpecCP also violates the Minimal Link Condition, and no chain headed by what can be formed given the Chain Formation Condition, there is an independent difference between arguments and adjuncts having to do with Case. According to the copying theory of movement, which Kitahara assumes, when a phrase \( \alpha \) moves, it leaves behind a copy with formal features FF(\( \alpha \)); crucially, the Case features of the argument are left in the position from which it moves. Now, since the Case features of arguments must be checked, a major assumption in the Minimalist Program, the Case features of what must move to the verb fixed to get checked. The movement of the Case features of what thus creates a variable for what in matrix SpecCP to bind. This option is not available to adjuncts, however, since they do not have Case features that need to be checked, even though they also leave behind a copy with formal features. Consequently, no variable is created, and the adjunct how binds no variable, resulting in vacuous quantification; Full Interpretation is violated.

This account also explains why subject extraction out of a wh-island as in (8) is also sharply ungrammatical:

(8) \*\(\lbrack\text{what} [\text{you wonder} [\text{whether} [\text{t(what) fixed t(what)}]]]\rbrack\rbrack\)

The movement of what in (8) to the matrix SpecCP violates the Minimal Link Condition, just like in (7); hence no chain can be formed given the Chain Formation Condition. In contrast with the object, the Case features of the subject are already checked in SpecTP; so there would be no moving of the Case features of the copy from argument position for checking. As a result, no variable is created, and what binds no variable, violating Full Interpretation. The ungrammaticality of (8) is thus on a par with that of (7a). In the same vein, Kitahara shows how quasi-objects bearing no Case features behave the same way, and gives an account for why adjuncts like where and when are more easily extracted out of a wh-island than why and how (cf. 428).
where did you wonder whether John fixed the car vs. *why did you wonder whether John fixed the car?). Essentially, where and when are complements of an empty preposition, while how and why are not. When where and when move out of a wh-island, they form no chains. But the Case features they leave behind move to the empty preposition for Case checking, creating a variable for them to bind.

I enjoyed reading the book, not only because it is clearly written, but also because it gave me the feeling that a new perspective is opening up for the various constructions that have long been studied, offering a satisfactory explanation for some of them. For instance, the Minimal Link Condition is arguably conceptually and empirically more adequate than the subjacency-based account (Chomsky 1973) or one appealing to the Empty Category Principle (Chomsky 1981) or to a separate condition like the NEEDED DEPENDENCY CONDITION (Pesetsky 1982), especially in the light of the contrast in (9):

(9) (a) ??what did you wonder whom John persuaded t(whom) to buy t(what)?
(b) ??whom did you wonder what John persuaded t(whom) to buy t(what)?

There is only one Minimal Link Condition violation in (9a), when what moves over whom, but two in (9b), one when what moves to the embedded SpecCP over whom in the base-position, and one when whom in the base position moves over what in the embedded SpecCP. The grammatical gradation in (9), as well as that in (6), is quite straightforwardly captured in terms of one vs. two violations of the same constraint, an issue that previous accounts did not come to grips with very easily, as it is not obvious how different degrees of grammaticality can be measured for different constraints in a principled way.

The account of Strict Cyclicity in (3) relies crucially on the operation Replacement. But it is not at all clear in (1) how L formed by concatenating α and K replaces K, as K is a constituent of L. Intuitively, when A replaces B in a structure C, then B no longer figures in C. It is not that one cannot state the mapping from (1a) to (1b); one can certainly take Σ’ as the result of concatenating α and K, creating a new node, a segment of K (cf. May 1985). Rather, one might wonder whether the operation Replacement is simply a way of stating the non-cyclic concatenation, which is precisely what needs to be excluded on principled grounds. The operation Replacement is all the more curious when it is considered in the two contexts in (1) and (5). It is not obvious that the two contexts are the results of the same operation.

The analysis in (7b) hinges on Case-checking by moving the Case features of the objects, with the effect that a variable is created for the wh-phrase object to bind. But one might ask why the checking cannot be done directly
with the Case features on the \textit{wh}-phrase itself. One can imagine the \textit{wh}-phrase moves through a position where it checks its Case features before reaching its final destination. If variable binding is possible by creating a variable with feature movement as Kitahara suggests, then we would have two types of operator-variable pairs according to whether the operator and the variable form a chain or not. This does not seem to be in the spirit of the Minimalist Program, where legitimate objects should be restricted to the minimum.

The account of the example in (7b) has an empirical bearing, however. One should expect under Kitahara’s theory that it is not possible to extract out of a \textit{wh}-island the subject of the infinitival complement of an \textit{Exceptionally Case-marking} verb. Consider in particular the structure in (10):

\begin{align}
\text{(10) } \left[ \text{I} \text{w} \right. \text{what you wonder [ \text{I} \text{p} \text{ Bill believed [ \text{I} \text{p} \text{ t(what) to have been fixed t(what) } ] } ] } ]
\end{align}

The movement of \textit{what} from the object position of \textit{fixed} to the embedded subject position is triggered by Case considerations, as is standardly assumed; so the Case features of \textit{what} should already be checked. But the Chain Formation Condition excludes forming a chain for \textit{what}, for movement of \textit{what} to the matrix SpecCP violates the Minimal Link Condition; the matrix C should attract \textit{whether}. Now, if no variable can be created in (8) for the \textit{wh}-phrase \textit{what} to bind, the Case features having already been checked when \textit{what} moves to the embedded subject position, then the same should hold in (10). The mild ungrammaticality of the example in (10), in stark contrast with that in (8), indicates that the explanation for (8) is empirically limited, if not incorrect.

It is interesting to observe how Kitahara brings the very few assumptions that he makes to bear on a variety of data, ranging from general phrasal movement and verb movement in French and English, to scrambling and topicalization in German and Japanese as well as the multiple subject construction in Icelandic. The fact that these constructions require additional, theory-internal auxiliary assumptions, for example, that different features trigger different types of movement or that feature strength varies in certain ways, seems to indicate that the explanatory power of the Minimalist Program is compromised by its empirical scope.

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Reviewed by Edwin Battistella, Wayne State College

The book jacket of Frederick J. Newmeyer’s *Language form and language function* is illustrated with Diego Rivera’s mural *Detroit industry*, which shows a busy automobile factory. In the foreground are workers moving a cart full of engine blocks. Some are pushing and some are pulling but all are working toward the same goal. So it is with the work of linguistics: some researchers push linguistic theory by emphasizing the autonomy of syntax; others pull by connecting grammar to external influences. *Language form and language function* examines and attempts to clarify the tension between the two orientations to the study of language alluded to in Newmeyer’s title: the formalist and the functionalist. Newmeyer argues that ultimately there is no principled incompatibility between these and that formalism and functionalism can inform one another in an industrious partnership of equals.

Formalist and functionalist orientations are nothing new in linguistics. The tradition preceding the 20th century emphasized the evolution of language, a tradition which Saussure departed from by establishing the autonomy of *langue* as a guiding principle. And the American tradition of the post-Bloomfieldians contrasts with the orientation of Sapir and of European functionalists like Baudouin de Courtenay, Trubetzkoy, Jakobson, Martinet and others. Most recently the form/function dynamic has been played out between Chomskyan generative grammar and approaches labelled typological-functional.

*Language form and language function* follows from Newmeyer’s earlier work on the history of generative-transformational grammar (Newmeyer 1980) and on the autonomy of grammar (Newmeyer 1983).¹ Newmeyer

¹ Some sections, revised here, appeared as separate pieces in the early and mid 1990s. Newmeyer’s study was largely underway before Optimality Theory began to become influential. However, Optimality Theory, which provides for competing motivations within
(1980) touched briefly on the issue of functional explanation (section 7.4) and on the relation of generative semantic approaches to pragmatic, discourse and perceptual explanations, themes which recur in much current functionalist work. And Newmeyer (1983) devotes a chapter to ‘Formal grammar and extragrammatical principles’, providing a discussion of Givón’s On understanding grammar and Bolinger’s Meaning and form as well as a summary of some generative work drawing on discourse principles. Language form and language function may thus be seen as part of a long-term concern of Newmeyer’s with the permeability of the boundaries of formal grammar.

The book is divided into 6 chapters, followed by a comprehensive bibliography of over 800 sources. In chapter 1, titled ‘The form-function problem in linguistics’ (1–23), Newmeyer illustrates the issues he wishes to raise by presenting a dialogue between two imaginary new Ph.D.s, who had been undergraduate classmates and who meet at the Linguistic Society of America annual meeting, where they are both competing for an open-specialization position: Chris Funk, from the University of California, Santa Barbara, and Sandy Forman, from MIT. In chapter 1 Newmeyer also outlines his goals, which are to provide a defense of formalism (as characterized by the Principles and Parameters model of generative grammar) while at the same time broadening formalists’ perspective by challenging them to incorporate functionalist insights.

Newmeyer notes that formalism and functionalism are slippery terms, with ‘formal’ being used for a range of work from mathematical to structuralist to the competing versions of generative grammar. Similarly ‘functional’ covers a range from external functionalism, which rejects the view that there are purely syntactic rules ‘of any great generality’ (14), to integrative functionalism, which acknowledges arbitrariness but which sees such a close connection between grammar and external factors that grammar cannot be viewed as self-contained, to the extreme functionalism of the Columbia (Diverian) school, which maintains that all grammar follows from semantics and discourse. Newmeyer focuses his attention on external functionalism, citing Role and Reference Grammar, Functional Grammar, the Competition Model, Systemic Grammar, and Cognitive Linguistics as examples. External functional approaches, Newmeyer suggests, share several features (drawing on Goldberg 1996): semantics is based in speaker-construal, with semantics and pragmatics forming a continuum; categorization involves prototypes rather than discrete categories; formal distinctions are useful to the extent that they support the primary function of language to convey meaning; grammatical constructions are pairings of form and meaning, with grammar

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a generative framework, has potential to incorporate aspects of variation and typology and may provide a further bridge for formalists and functionalists (see Nakamura 1999 for some discussion).
serving as an inventory of such pairings. Newmeyer represents formal approaches, by contrast, as typified by the Principles and Parameters approach – involving a multi-level, modular, transformational theory in which constructions are often epiphenomena.2

Chapter 2, ‘The boundaries of grammar’ (23–94), defends the key principle of formalism – the autonomy of syntax. Drawing on Croft (1995), Newmeyer describes three positions regarding autonomy of form and meaning: the autonomy of syntax, the autonomy of knowledge and the autonomy of grammar. The first (labeled AUTOSYN) claims that there is a system of syntax with non-derived primitives whose combination can be stated independently of external factors; the second (AUTOKNOW) refers to the idea that linguistic competence should be characterized independently of language use; the third (AUTOGRAM) refers to the idea that there is a cognitive system whose primitives and principles are specific to language.

Newmeyer begins by placing these autonomy theses in the context of American linguistics, noting, for example, the relation between Chomsky’s position in Syntactic structures and that of the post-Bloomfieldians. Newmeyer then moves on to his defense of the autonomy of syntax. He argues first that syntactic primitives are not necessarily derived from semantic ones and that (at least some) syntactic generalizations are system-internal rather than discourse-based. Newmeyer also argues that some generalizations are best expressed by independent syntactic principles (such as the principle of Lexical Government) and that constructional properties (such as wh-fronting or subject-verb inversion in English) are not semantically unified.

The discussion of AUTOKNOW focuses on the model of Emergent Grammar developed by Hopper and Thompson, which sees grammar as intrinsically linked with principles for constructing discourse: in fact, grammar can be said to be the result of strategies for producing discourse and language viewed as an epiphenomenon of discourse. Emergent grammar is connected closely to the ideas of prototypical discourse functions for nouns (discourse manipulable participants) and verbs (reported events). Newmeyer notes, however, that a knowledge of discourse options nevertheless requires a considerable amount of prior knowledge (presumably) stored in a grammar. Newmeyer also argues that the facts of language change support AUTOKNOW in that many changes are usage-based, with adults changing their speech but not their grammar. Following Andersen (1973), Newmeyer suggests that change may involve a functional superstructure (used by adults)

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which becomes part of the formal grammar acquired by subsequent generations. Once again, such a superstructure requires a grammar upon which to rest.

With respect to the autonomy of grammar, Newmeyer argues that while some expositions of functionalist linguistics appear to reject AUTOGRAM, many functionalists do not. In Newmeyer’s view, the autonomy of grammar is a pseudo-issue. Newmeyer stresses that formal grammar does not entail rejecting connections with external factors and he shows several lines of investigation in formal syntax which are concerned with how the mental representation of the world is related to language (such as, for example, Jackendoff’s conceptual semantics, which bridges formalist and cognitivist concerns).¹

Chapters 3, 4 and 5 examine aspects of the functionalist program. Chapter 3, ‘Internal and external explanation in linguistics’ (95–164), considers the notion of explanation. Newmeyer argues that, while internal explanations (those relying on the deductive structure of grammar) are often associated with autonomy and thus formalism, functionalists sometimes themselves appeal to internal explanation and so cannot dismiss deductive explanations out of hand. Newmeyer also discusses and assesses various types of external explanatory factors, arguing that information flow, frequency and economy are generally irrelevant to syntax. He also suggests that synchronic explanations relying on competing external factors are problematic because it is impossible to find independent evidence for the strength of competing motivations within a language and for the range of competing motivations more generally.

With respect to iconicity, which plays a prominent role in some functionalist research, Newmeyer discusses a number of ways in which grammatical structure has been suggested to reflect conceptual structure: iconities of distance, independence, order, complexity and categorization. Contrasted to this are instances of structure-discourse iconicity – explanations based on the iconic reflection of discourse function by syntax. Newmeyer argues that the latter fail to meet three crucial tests of PRECISION, LINKAGE and TESTABILITY. Newmeyer illustrates the problem with analogies to other forms of causative explanation, such as medical research: to demonstrate causation requires that causative factors be stated precisely, that there be a linkage shown between a proposed cause and some specific effect, and that the causation must be shown to have measurable typological consequences. Newmeyer sees functional explanations having to do with information flow (based on Prague School communicative dynamism) and on frequency as unable to meet these criteria.

¹ Newmeyer also notes, in chapter 2, many instances in which generative linguistics has embraced parsing principles (work by Bever and Langendoen, Fodor, Berwick and Weinberg) and discourse-based explanations (work by Kuno, Prince and others).
Two current functional approaches have real promise in Newmeyer’s view: Hawkins’ Early Immediate Constituent (EIC) approach and the principle of structure-concept iconicity. Both meet Newmeyer’s three conditions. Hawkins’ EIC centers on the principle of Early Immediate Constituents, that ‘The human parser prefers linear orders that maximize the IC-to-non-IC ratios of constituent recognition domains (CRD)’ (Hawkins 1994: 77), where CRDs are nodes that need to be parsed in order to recognize a particular constituent and all of its parts. The EIC explains various heaviness phenomena such as the contrast between I consider everybody who agrees with me and my disciples about the nature of the cosmos to be smart vs. I consider to be smart everybody who agrees with me and my disciples about the nature of the cosmos, since the former will have a greater IC-to-non-IC ratio than the latter. Newmeyer sees the EIC as providing the best explanation of ordering constraints, better in fact than attributing such constraints to innateness.

In chapter 4, ‘On syntactic categories’ (165–223), Newmeyer defends the traditional view of categories as discrete, algebraic primitives. He discusses three alternative views to the traditional approach: that categories are organized in a prototype structure, around a best case from which radial members deviate; that category boundaries are fluid and gradable rather than discrete; and that categories are defined by necessary and sufficient semantic criteria rather than syntactic ones. Theories of prototypes serve an important role in some functionalist theories, as a descriptive technique and as part of the underlying theory. Prototype theory embodies the view that formal structures are too idealized to serve as a model for the analysis of grammatical constructions as prototypes. Newmeyer begins by discussing some of the history of prototypes in linguistics in Ross’s work on so-called NP squishes. The bulk of the chapter concerns consequences of prototype theory – that prototypical structures will show greater syntagmatic simplicity than nonprototypical ones and that they will also exhibit greater complexity. Newmeyer argues, however, that it is unnecessary for grammatical statements to refer to degrees of prototypicality. He provides alternative explanations for some prototype effects (such as the failure of measure verbs to passivize and the status of there as an NP), and he suggests that prototypicality reduces to relative markedness. Newmeyer also argues that examples of fuzzy
categories do not entail that categories lack discrete boundaries and that attempts to provide necessary and sufficient semantic definitions of categories (such as that of Langacker 1987) leave too many questions open to be considered a plausible alternative to syntactically primitive categories. Of special interest is the Appendix to the chapter (208–223) in which Newmeyer discusses at length the position that prototypes are needed to explain the concept of grammatical constructions, arguing against Lakoff's (1987) analysis of deictic there and here in terms of a central notion of physical pointing and a network of radial extensions.

In chapter 5, ‘Deconstructing grammaticalization’ (225–95), Newmeyer examines the process of reanalysis and associated semantic change in which more independent grammatical items are recategorized as less independent ones. Often grammaticalization involves reanalysis of categories (such as the shift of certain modal main verbs to auxiliaries in English or the reanalysis of the have of possession to the have of obligation). Some functionalists see grammaticalization as posing an especially serious explanatory challenge to formal grammar and to the ideas that grammatical description is synchronic and that the form-meaning relation is arbitrary. Newmeyer argues, however, that grammaticalization is an epiphenomenon rather than a distinct process. He teases grammaticalization apart into more basic processes of morpho-syntactic reanalysis, phonetic reduction and semantic change and suggests that grammaticalization is only one type of the possible combination of these processes. Newmeyer concludes that grammaticalization fails to always show discrete steps and that bleaching and reanalysis are not in a fixed temporal relation. He also provides counterevidence to the unidirectionality of grammaticalization (noting that while complete reversals are rare, deg grammaticalization and regrammaticalization are not). Nevertheless, he agrees that the directionality of grammaticalization is a natural process.

Chapter 6, ‘Language typology and its difficulties’ (297–364), takes up the problem of establishing valid crosslinguistic generalizations—that is, the problem of language typology. Functional typology has as its goal the discovery of crosslinguistic generalizations and their external explanation, often in terms of common function. Such approaches assume that the sequence of grammatical information follows the sequential flow of information and that more widespread grammatical constructions are ones which enable the most efficient expression of meaning and the most efficient strategies for recovering meaning. Newmeyer discusses some of the methodological difficulties with language typology—the representativeness of available languages, sample size, genetic and areal bias (drawing on Tomlin 1986, who suggests that some typological results are due to the geopolitical successes of language families). And as Newmeyer notes (315):

If we cannot be confident that any sample is free of areal bias, how confident can we be that any typological study presents findings that have
Newmeyer also discusses the uniformitarian hypothesis central to much functional-typological work. This is the hypothesis that present-day typological universals should also be assumed to apply to ancient and unattested languages, an assumption that Croft (1990) sees as being as essential to functional-typology as innateness is to generative grammar. However, Newmeyer raises the intriguing possibility that unattested languages could have manifested properties not shown in the pool of languages available today. Chapter 7 is a brief ‘Conclusion’ (365–369), which is followed by References (366–416) and Indices (Name and Subject, 417–428).

*Language form and language function* is a excellent work, supported with extensive research and lucid and crisp writing. And while Newmeyer is not neutral on formalist-functionalist issues – he clearly sees syntax as autonomous and formal study as a prerequisite to typology and functional explanation – he makes every effort to be evenhanded, critiquing narrow perspectives and incomplete proposals on the formalist side as well as the functionalist side. Newmeyer succeeds in encapsulating issues without oversimplifying; however, space limitations make it impossible for him to treat every topic in detail. Thus, one way in which the reader sometimes feels hurried is that there are areas where a longer treatment and more exploration of issues would have been more satisfying (the discussions of the implications of second language acquisition and genetic dysphasia, to name two). I also found myself curious at various points how functionalists would respond to some of Newmeyer’s arguments – that it is hopeless to think in terms of competing motivation from external forces, that the levels of form and meaning are iconic in generative grammar (through D-structure), that autonomy itself might be an adaptation, that prototypes are epiphenomena, or that functionalism often relies on internal explanations. My hope is that functionalists will react to Newmeyer’s arguments and stimulate needed further debate on these questions.

As I read the book, I was struck by the extent to which formalism and functionalism connect linguistics with biology – formalism through the connection with abstract learning theory and the (frequent) assumption that linguistics is ultimately an aspect of biology; functionalism through ties with perception and cognition and through the model of language based in adaptation to communicative function. Given this connection, it is worth emphasizing that form and function in biology coexist quite nicely. Daniel Nettle (1999) notes that there are four types of questions relevant to behaviors, drawing on Tinbergen (1963): the adaptation of behavior to purpose; the psychological, neural and cognitive structures involved in
behavior; the factors that govern and influence the development of behavior; and the ancestral history of the behavior. Formalists and functionalists each begin with one of these questions and take that as a starting point for the investigation of the others. One's choice of where to begin is no doubt related to one's interests (and to where and how one was trained). Rather than viewing formalism and functionalism as incompatible approaches to research, a better way of viewing them is in terms of where one wishes to begin looking for explanations.

Newmeyer's most important contribution in this book may be in helping formalists and functionalists arrive at this view – that both groups will profit from a more constructive engagement that will help them to delimit some issues on which formalism and functionalism need not debate and some on which they should. Among the former, the following seem to be good candidates:

- Claims about innateness are distinct from claims about autonomy.
- The description of language should characterize what speakers find it possible to express.
- Some purely syntactic constructs play a role in describing the synchronic structure of language.
- Factors of discourse, cognition, parsing and perception play roles in language use.
- Language use affects language structure.
- Causation should be precise, linked and testable.
- Language typology must consider issues of sample bias.

Among the latter issues – those on which debate is expected – I would include the issues mentioned above: the soundness of explanations involving competing motivation; the extent to which generative models are sensitive to external factors such as iconicity; the connection of the types of autonomy to functionalist and formalist positions; and the relation of innateness to parametric models and prototypes to functionalist models.

Overall, Newmeyer's book (together with the proceedings of the 1996 University of Wisconsin—Milwaukee Linguistics Symposium, see Darnell et al. 1999) signals a welcome rapprochement between formal and functional linguistics. And finally, it is interesting that Newmeyer opens and closes his work with Sandy Forman and Chris Funk – hypothetical new Ph.D.s. Perhaps certainty in the correctness of one framework is a luxury of those recently trained, for the longer one does linguistics, it seems, the more value one sees in diverse approaches and in bridges among them. Newmeyer never reveals which of the two gets the job – but we can hope for a time when both would be colleagues and collaborators in the same department, one pushing and the other pulling.
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