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Reviewed by Martin Haspelmath (Max Planck Institute for the Science of Human History)

The book under review which deals with grammatical case-marking (especially accusative, ergative, dative, and genitive) in the world's languages, is an important work that I enjoyed reading and found very stimulating, and that I recommend to all linguists interested in case-marking typology, whether they follow B's methodological orientation or not. It deals with a wide variety of languages and construction types, it is very well written, and even readers who are not well-versed in Chomskyan syntactic notions will understand large parts of it. This also has the advantage that it can be seen fairly clearly how the approach ultimately fails (more on this further below).

From my perspective, the main attraction of Baker's main contribution, his DEPENDENT CASE THEORY, lies in its egalitarian treatment of accusative and ergative alignment, and similarly of indirective and secundative alignment of ditransitive constructions. In this respect, it follows the earlier typological literature (e.g. Comrie 1978; Dryer 1986; Haspelmath 2005; though only the first of these is cited), and the similarities between B's and Comrie's approaches are noted explicitly (p. 57). The earlier generative literature on case (following Chomsky 1981) has typically been biased toward accusative alignment, and toward languages that index only subjects but not objects, thus favouring the idea that nominative case is licensed by subject agreement, and inviting the further generalization that all arguments are licensed by agreement. But B shows nicely that object case-marking is rarely in line with object indexing (§ 2.2.2, § 2.4), and he also points out that the reverse relationship, with indexing/agreement licensed by case, is just as usual (§ 2.5). Thus, he departs radically from the Chomskyan paradigm and proposes a set of schematic case-assignment rules that look like those in (1) (e.g. p. 182; the formulation is simplified here).

(1) a. High case in TP (clause) is ergative.[1]
b. Low case in TP (clause) is accusative.

1. A fuller version of this is: „If XP c-commands ZP in the same TP, then assign ergative case to XP" (p. 80). More complex versions occur elsewhere, e.g. on p. 169 and p. 174.

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c. High case in VP is dative.
d. Low case in VP is secundative (called “oblique” by B).
e. High case in NP is genitive (there is no low case in NP).
f. Unmarked case is nominative-absolutive.

The eventual picture is much more complicated, so it fills over 300 pages, but the essential point is that case on nominals is not licensed by more or less abstract functional categories, but by a CASE COMPETITOR in a local domain (TP, or VP, or NP). In a configuration with two nominals in a clause (TP), the high nominal may get ergative (if the language has rule (1a)), or the low nominal may get accusative (if the language has rule (1b)), and in the elsewhere case a nominal gets unmarked case (nominative-absolutive by rule (1f)). A language may also have both rules (1a) and (1b), resulting in an ergative-accusative pattern in transitive clauses and nominative-absolutive in intransitive clauses (i.e. tripartite alignment).

In addition, Baker highlights syncretism patterns that typologists have often been intrigued by. For example, Eskimo has the same case for ergative and genitive (2a), and Ika has the same case for ergative and dative (2b).

(2) a. High case in TP or NP is ergative-genitive.
b. High case in TP or VP is ergative-dative.

What I also liked was B’s recognition that there are some cross-linguistic regularities that he has no explanation for and that may very well be explained functionally, e.g. the fact that tripartite case patterns are rare, and horizontal patterns (with transitive subject and object flagged in the same way, differently from unmarked nominative case) are even rarer (p. 57–58).

But I also have many critical questions and remarks, of course, some of them fairly fundamental. Let me state at the outset what I see as four distinct goals of theoretical linguistics, listed in (3).

(3) a. language-particular comprehensive description (for observational adequacy)
b. language-particular elegant description (for descriptive adequacy)
c. cross-linguistic comparison and detection of universal tendencies
d. explanation of universal tendencies

Perhaps the most basic problem in the book is that Baker is unclear about his goals and seemingly wants to achieve (3b–d) in one go. On the one hand, he says that he wants language-particular “principles [= rules] of case assignment that are as unified as possible” (p. 10), i.e. he regards (3b) as an important goal. But on the other hand, he aims for “a theory that has universal aspirations” (p. 6), i.e. (3c), and he proposes general principles of case assignment with some limited parametric variation, in the spirit of Chomsky’s (1981) principles-and-parameters idea (thus hopefully achieving

(3d)): "Ideally, this parametric variation should be rich enough to be descriptively adequate, and restricted enough to be explanatorily adequate" (p. 79).

In practice, the attempt to do all these things at the same time generally fails, in my view, so I have favoured an approach that sidelines elegant description (3b), makes comparison (3c) independent of description (3a), and explanation (3d) independent of both (cf. Haspelmath 2014). Why would one want to do all of (3b–d) at the same time? And why is (3b) so important? B does not really tell us, which is not surprising, given that he works in mainsteam generative grammar, where it is normally simply assumed that one should proceed in this way, on the basis of the idea that a substantial amount of grammatical categories and architectures (including presumably (1)–(2)) is innately prespecified, and that learners look for maximal generalizations. B does briefly justify his goal of elegant description ("Ockham's razor, elegance, learnability, and so on", p. 11), but in view of its subjectiveness, one should be skeptical about elegance as a truly scientific goal (as opposed to pedagogical – of course, grammatical descriptions should be readily understood by human readers, and therefore avoid tedious repetition). B emphasizes the "explicitness and precision" of generative grammar (p. 1), but more than these I would value objectiveness, testability and replicability, and these are difficult to reconcile with (3b).[2]

To his credit, in quite a few places Baker recognizes that there would be alternative possibilities, and that he is forced to "let the theory decide", i.e. where there are no empirical considerations, so that "the reader will have to decide whether or not [the author] exercised good judgment" (p. 16). Personally I often find it interesting (even inspiring) to consider B's judgments, but ultimately only testable and replicable claims are satisfactory.

Clearly, some of the highly abstract case-assignment rules such as those proposed for Sakha (in the present book and earlier in Baker & Vinokurova 2010) have the virtue of being "unified" and thus "elegant", but to what extent does B achieve the other goal of cross-linguistic explanatory adequacy (3c–d) at the same time? This would be easier to tell if B actually provided a list of testable cross-linguistic universals that should hold given his theory. After reading the book carefully, I came up with the list of universals in (4a–h) that he seems to have an account for.

(4) a. There are no languages with an ergative case pattern and an accusative agreement pattern (p. 76)

2. B keeps using negative expressions such as "horrendous complications" (p. 25), "it is awkward to say that…" (p. 40), "this goes too far" (p. 26), "a stretch" (p. 38), and "theoretical gymnastics" (p. 39). These make the reading lively, but they also point to the serious problem with (3b), especially since some readers will find B's own approach "horrendously complicated" etc.

b. When experiencer constructions with two arguments have a case pattern with two identical cases, the pattern is always double nominative-absolutive, and never double accusative or double ergative (p. 88)
c. Nominal possessors are (almost) never coded with accusative case (p. 163)
d. The domain of case licensing is never larger than the clause (p. 113)
e. Ergative case is never excluded from reduced (non-finite) clauses (p. 45)
f. Genitive case that is identical to ergative should not be able to occur twice in a possessed nominal ('Ali's picture of Istanbul', p. 169)
g. The case of an adpositional complement is hardly ever triggered by a case competitor (p. 186)
h. Adverbs can trigger dependent case marking only on other adverbs, not on arguments (p. 293)

These look as if they were testable, but some of the most famous universals of coding, those listed in (5), are not explained by B's system, even though he discusses most of the relevant phenomena, and they are far more robustly attested than most of those in (4):

(5) a. Where there is a case system, the only case which ever has only zero allomorphs is the one which includes among its meanings that of the subject of the intransitive verb. (Greenberg's 1963 Universal 38)
b. If in a language the verb follows both the nominal subject and nominal object as the dominant order, the language almost always has a case system. (Greenberg's 1963 Universal 41)
c. When the overt coding of the P argument depends on its prominence, languages always reserve overt coding for an animate, definite, specific, and/or topical P.
d. When a language neutralizes the ergative-absolutive distinction depending on the prominence of the A argument, the neutralization always starts at the higher end of the prominence scales (especially person and topicality).
e. When a language has an inverse coding pattern, it always uses extra coding for the inverse/upstream scenarios, and less coding for the direct/downstream scenarios.
f. In all languages, the great majority of two-argument verbs follow the case-marking pattern of physical-effect verbs like 'break' and 'kill'.
g. Flags (case-markers, adpositions) coding peripheral relations (instrument, location, beneficiary, time, manner) tend to be less minimal than flags coding core relations (S, A, P).

Thus, readers (like me) who are primarily interested in the explanation of universals (because these are testable and replicable, thus discussable in objective terms) are bound to be somewhat disappointed – or from a different angle, functional linguists who think they have good explanations of these universals are not seriously challenged in their views by B's book. Regarding (5d) (prominence-based ergativity splits) in particular, Baker makes an interesting comment (p. 23, n. 23) that is telling about his thinking: He says that he is "not always fully persuaded by the cogency" of a functional (Silversteinian) account of overt ergative marking, because in specific languages, other factors sometimes play a role (e.g. the singular/plural distinction, which is relevant in Diyari, but is not expected from any universal hierarchy). However, in an approach that separates description (3a) from comparison (3c) and explanation (3d), this is not an issue: Diyari has some rules that are in line with universal trends, and other rules that are simply language-particular and are not (obviously) in line with any cross-linguistic trend. Functional explanations account for universals, not for language-particular rules. It is only if one conflates explanation/universals and description that a problem arises: A language-particular rule that partially follows a general trend and is partially idiosyncratic cannot be accommodated naturally by B's approach, even though in my experience most rules are of this sort.

As I mentioned earlier, Baker recognizes that the rarity of tripartite and horizontal case patterns is functionally motivated, and he might similarly accept a functional account of the generalizations in (5a–g). There is nothing incoherent with assuming that some generalizations are due to universal grammar (e.g. those in 4), while others are explained functionally. But then the question arises whether even more universals can be explained functionally, e.g. some of those in (4). For example, (4a) has been explained in terms of the topicworthiness of person forms, which favours accusative alignment of indexing/agreement (or secundative alignment in ditransitive constructions, cf. Haspelmath 2005).

Moreover, some general patterns might be due to general tendencies of language change. Thus, while ergatives may come from ablatives, like genitives (thus accounting for many cases of ergative-genitive syncretism), accusatives and genitives simply do not have intersecting paths of development, or very few (cf. Lehmann 2015 [1982]: 119; Narrog & Ito 2007: 283). This might be sufficient to account for (4c), thus obviating any need for an explanation in terms of innate grammar. To his credit, B does not completely ignore this kind of diachronic explanation, and in fact he invokes it himself a number of times (e.g. to explain away some exceptions to (4c) on p. 171).

However, after this lengthy discussion of my pet topic of universals, I should note that B's primary interest is not really in universals. He never highlights them by numbering and setting them off as I did above,[3] and throughout the book, he is

3. I suspect that this is because he is not as optimistic about testability of universals as I am. If universals are not based on observationally adequate descriptions (3a), but on elegant descriptions (3b), then there may always be a way of explaining exceptions away, unless we have the

clearly more interested in formulating elegant rules for different individual languages that have as much in common with each other as possible. Thus, when he explains marked-S languages (to use Handschuh's 2014 term) by negative c-command conditions ("Assign marked nominative if there is no higher nominal in the relevant domain"), this is imaginative, and it works surprisingly well, but this move has no place in my typology of theoretical goals in (3a–d), and no real place in orthodox Chomskyan thinking either. Perhaps one should add another theoretical goal (3e): describe a new language in as similar terms to other languages as possible. In practice, this is of course what linguists typically do, but more often out of practical considerations (or lack of imagination) than as an explicit theoretical goal. At least in my understanding of classical generative grammar, the primary goal is to have a restrictive framework that can describe only those kinds of languages that are actually attested (Haspelmath 2014). In B's system, there is not much restrictiveness, and the emphasis is not on this aspect. Following his approach to marked-S languages, one could ask whether negative c-command also plays a role in the VP domain (giving rise to a special kind of ditransitive construction), or whether the domain condition could perhaps also be negative ("Assign case C to XP if it c-commands YP outside the local domain ZP"), or whether even the whole rule could be negative ("Do not assign case C to XP if it c-commands YP in the local domain ZP"). So while some minimal modifications of the basic rule schema yield interesting possibilities, it seems that most don't, and one could say that the rule schema overgenerates vastly. In a number of cases, B's interest is in syncretisms, but some of these are very rare (ergative-dative syncretism, attested only in two languages, Ika and Ubykh, p. 136), and his observations do not really amount to any restrictions either, because he does not rule out accidental homophony of case forms. He does feel strongly about the lack of accusative-genitive syncretism, however, though it is unclear to me why. When he says: "I take it to be a robust fact that a syntactic convergence of accusative and genitive is resisted by human speakers" (p. 171), I find this quite baffling, because he can hardly mean that this kind of syncretism is not learnable, or not expressible in his system (given that there are few restrictions on morphological realization of syntactic case features, as discussed in § 1.2.2).

Moreover, in view of the fact that B allows both agreement-sensitive case and dependent case, i.e. two very different mechanisms, one wonders whether there might be even more ways in which nominals could get case. Thus, again there is little restrictiveness, and little typological explanation (in the sense of accounting for unattested types by a framework that cannot describe everything).

definitive account of a language (which in practice always remains elusive). Moreover, B probably wants to explain only exceptionless universals, and many of those in (5) are known to have exceptions (though this applies to (4g), too).

One virtue of Baker's thoroughness and the space that the book allows him is that one gets a clear sense of some serious gaps in the motivations for his system. The most striking cases are an alleged zero predicative marker in predicate nominal constructions (§ 5.5) and zero adpositions (§ 5.1). B needs to be credited for discussing predicate nominal constructions and the way in which they seem to fail his predictions: Even though the subject is higher than the predicate nominal, we almost never get accusative or ergative case. B proposes that there is an abstract element E that is almost always empty but that has the desired effect of preventing dependent case assignment. This is of course the kind of circular reasoning that one wants to avoid, and B admits that "there are no stunning successes" here (p. 227).[4] A semantic account based on meaning similarity between (typically transitive) physical-effect situations and other kinds of situations would work much better here, given that predicate nominal constructions are semantically remote from physical effects. Likewise, I found the postulation of zero adpositions for all kinds of oblique cases (§ 5.1.1) unmotivated, other than in order to get the right effects on nominative, accusative and ergative. B makes a distinction between case and adpositions on the surface, but at the relevant abstract level, surface adpositions can also realize case (n. 9 on p. 18), and surface case can also realize an adposition (n. 2 on p. 2). Thus, the surface realization does not tell us whether we are dealing with an NP or a PP, but the distinction is often crucial for case assignment, because a PP that c-commands an NP cannot be a case competitor and hence cannot trigger accusative case, and a PP that is c-commanded cannot trigger ergative case on the subject because only NPs do. So why is this not circular? Here B says a bit more than about predicate nominals, but not much more. On p. 13, we read that we should "hope that one can find some fine-grained syntactic properties which distinguish the two kinds […] : a process of clefting, perhaps, or quantifier floating – the sorts of syntactic phenomena known to apply to NPs but not to PPs in some languages".[5] But do we really "know" that clefting or quantifier floating is universally restricted in this way? If we cannot be sure of this, what have we learned if we find such a distinction? The distinction could be connected to a PP vs. NP contrast, but it need not. Again, there is a serious danger of circularity. There is one passage where B discusses the difference in some detail, with data from Tamil (p. 188–194), where he claims that there is a contrast between dative NPs as subjects (triggering accusative

4. A very weakly motivated zero element is also postulated in possessive nominals „to get the domain requirement in order" (p. 167).

5. It seems to me that this strategy represents precisely the sort of methodological opportunism that is criticized in Baker & Croft (2017).

case on the object, e.g. "DAT understands ACC") and adpositional NPs (not triggering accusative, thus showing a nominative coargument, "to-DAT is-needed NOM"). There is an interesting difference in particular with control constructions, with the first allowing the dative nominal to be controlled ("I want [$Ø_{\text{DAT}}$ to understand ACC]"), and the second allowing only the nominative to be controlled ("I want [to-DAT be-needed $Ø_{\text{NOM}}$]"). But this is hardly compelling evidence for a difference between PP and NP, because control phenomena are known to be often affected by semantic factors,[6] and they are not widely known to be affected by the distinction between different kinds of categories (Stiebels 2015).

The circularity in the two cases of zero predicate markers and zero adpositions leads me to another point of criticism: Throughout the book, B assumes that c-command relationships are crucial for case licensing, determining which of the two nominals is "high" and which is "low" for the rules as in (1). But there is no argumentation for these c-command relationships – B just assumes that in a transitive clause, the A-argument is higher than the P-argument, and in a ditransitive clause, the R-argument is higher than the T-argument, in all languages.[7] This seems to follow from Uniformity of Theta Assignment (p. 290), i.e. indirectly it follows from semantic roles after all. So why have syntactic trees at all, if they are fully determined by semantics? B basically just gives a single argument, from some very intricate facts in Amharic, relating to two different kinds of ditransitive patterns (p. 81–86). He mostly resists the currently fashionable approach of arguing from binding and quantifier-variable relationships for particular c-command relationships (cf. n. 2 on p. 84), citing Barker (2012) for serious doubts concerning the connection between bound variable anaphora and c-command. So why use c-command? There is another interesting comment on p. 290 concerning the way in which semantics influences case assignment indirectly via the syntax, rather than directly:

> it predicts that the influence of lexical semantics on structural case will be limited and coarse-grained. It seems quite clear that lexical semantics itself can make a myriad very fine-grained distinctions, as shown by our very rich and subtle intuitions about how one verb differs in meaning from another… In contrast, part of the charm of syntax is that it is relatively coarse-grained […]

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6. Baker claims that in general „only the highest NP argument in a nonfinite clause can be controlled", but this does not seem to be based on any systematic research, and it is well-known that agentivity also plays a role (the DAT-ACC verbs are apparently of the more agentive sort in Tamil).

7. B does not say how he would deal with languages that have P-A order, or T-R order (the latter happens fairly frequently, cf. Heine & König 2010; an example is French). Presumably he would assume that A-P and R-T is always the underlying order, hoping to find independent evidence for this.

This is no doubt true, and earlier approaches in terms of a small set of grammatical relations (Perlmutter 1980), proto-roles (Primus 1999), macro-roles (Van Valin 2005), or typological role-types (Haspelmath 2011) have of course come to the same basic conclusion.[8] But I see no argument for c-command here.

C-command should have to do with constituents, i.e. the grouping of words, and almost the only place where this is relevant to Baker's concerns is with differential object marking, as in the following minimal pair from Sakha (p. 4–5), which shows that accusative is found only on a definite object:

(6) a. *Masha salamaat-y türgennik sie-te.*
Masha porridge-ACC quickly eat-PST.3SG.SBJ
'Masha ate the porridge quickly.'
b. *Masha türgennik salamaat sie-te.*
Masha quickly porridge eat-PST.3SG.SBJ
'Masha ate porridge quickly.'

B claims that this is due to dependent case assignment as well, because the object is in the same domain as the subject only when it raises from the VP as in (6a). In (6b), where the object stays in its base VP position, it does not count as a case competitor in this kind of language (though this is of course not true for all languages). B's way of integrating this insight into his overall story is very complex, though, and one wonders how successful he is in capturing the kinds of generalizations that seem to be obvious, in terms of P prominence (cf. (5c) above, and Aissen's (2003) well-known appeal to prominence scales). In a language where definiteness is not as consistently correlated with word order but still determines differential object marking (e.g. Hebrew), again constituency (and thus c-command in its literal sense) would be irrelevant.[9]

Baker could of course say that differential P marking in such a language is due to some other factor (recall that he does not claim to account for the universal in (5c) in general), and indeed he notes in several places that his system contains redundancies in that it offers multiple ways in which very similar case patterns can be generated (pp. 87, 89, 130). But if the system allows cross-linguistic redundancies (and thus is not as fully restrictive as it would ideally be), one wonders why it

8. Dixon and Van Valin sometimes pretend that these entities are semantic, but it is clear that they are nonsemantic to a significant extent.

9. Of course, there is a cross-linguistic tendency for specific and given nominals to precede other material, presumably for good functional reasons. The position of the definite object in Sakha is thus not an idiosyncrasy of the language, but its functional motivation may well be quite independent of the functional motivation for its case.

could not also allow language-particular redundancies such as two different rules for accusative assignment in Sakha. Actually, B admits that he sometimes needs multiple rules for particular languages, e.g. two different rules for dative assignment in Sakha (p. 13), so I see no deep reason for linking the Sakha accusative variation to word order and constituency. B clearly finds this view attractive, so he explores its implications in great detail, but I did not find it compelling.

On the contrary, it is clear that there is a larger generalization here, even larger than the statements in (5c–d) suggest, and it does NOT have to do with word order. B seems to think that it is a particular virtue of generative approaches that they reason in a more deductive, theory-driven way (p. 129), but actually, highly general statements have been proposed in the functionalist literature, though they are not well-known (and I do not fault B for not knowing about them). My current favourite formulation is as in (7).

(7) Deviations from canonical associations of role rank (A > P, R > T) and referential prominence (i.e. prominence of person, nominality, animacy, specificity, givenness) tend to be coded by longer grammatical forms.

I discussed (earlier versions of) this generalization for ditransitive constructions in Haspelmath (2004; 2007), and I plan to extend it to monotransitives in future work (see also Malchukov 2008 for a similar approach). It subsumes most of the generalizations about differential object marking and prominence-based ergative splits, and also about inverse patterns (e.g. Klaiman 1992; Bresnan et al. 2001) and patterns determined by coargument prominence (B claims that functionalists have not known about these, and indeed they are not very widely known; but see, e.g., van Lier 2012: 3 on "co-argument conditioned marking".)

To the extent that the generalization in (7) holds up, we have a very good explanation of many different case patterns (and voice marking in addition), because there is a straightforward functional explanation in terms of expectedness. Canonical associations of role rank and referential prominence are more frequent and thus should be coded with less effort in an efficient semiotic system. The generalization in (7) was thus arrived at deductively from a basic functional principle, and by starting out from it, I have discovered quite a few additional facts that I would have overlooked otherwise.

Baker also claims that his approach explains why it is primarily the specificity of the object, but not of the subject, that affects case patterns: "Whether a subject is specific or not is not expected to have much impact on whether it is ergative, nor on whether the object is accusative" (p. 130). But referential prominence of the subject has been noted as an important factor in Malchukov (2008: 2015–2016) and is by now well-known to affect the obligatoriness of ergative case (McGregor 2010), and at least the person of the subject has repeatedly been seen to affect accusative marking, for example in Yukaghir (where objects lack accusative marking in a person-downstream scenario, i.e. when the object is 3rd person and the subject 1st or 2nd person):

(8) Kolyma Yukaghir (Maslova 2003: 89, 10)

a. *met es'ie tet pulut-kele kudede-m*
my father.NOM your husband-ACC kill-TR.3SG
'My father has killed your husband.'

b. *met tolow kudede*
I.NOM deer.NOM kill.TR.1SG
'I killed a deer.'

In general, person effects on case patterns (which are particularly widespread in ditransitive constructions) play no role in B's book at all, probably because they cannot be easily integrated into a framework that models prominence as raising into a higher spell-out domain. B's commitment to using c-command as the basis for hierarchical relationships apparently prevents him from providing a fully general account.

Thus, even though Baker proposes a very general (and highly intricate) system for dependent case assignment, I do not regard it as successful, because it neither yields fully elegant language-particular description (which is impossible to judge objectively anyway), nor does it provide explanations for robust cross-linguistic generalizations in the domain of argument coding (5a–g). Those generalizations that it does seem to explain (in (4a–h)) are much less impressive, but it may be worth following up on them in future work.

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Reviewer's address

Martin Haspelmath
Max-Planck-Institut für Menschheitsgeschichte
Kahlaische Strasse 10
07745 Jena
Germany
haspelmath@shh.mpg.de