REVIEW ARTICLE

Case and grammatical relations: Studies in honor of Bernard Comrie. Ed. by GREVILLE G. CORBETT and MICHAEL NOONAN. (Typological studies in language 81.) Amsterdam: John Benjamins, 2008. Pp. x, 290. ISBN 9789027229946. $149 (Hb).

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The book under review is a collection of twelve papers in honor of Bernard Comrie.[1] Though the genre of a festschrift does not impose rigid thematic restrictions, especially when the festschriftee is a scholar who has contributed to as many diverse areas of linguistics as Bernard Comrie has, the title *Case and grammatical relations* is indeed justified. All of the contributions to the volume (except the one by Maria Polinsky) deal with various issues having to do either with morphological case, or with various properties of grammatical relations, or with both.

The contributions to the volume include studies dealing with individual languages (Russian, Hungarian, Ingush, Swedish dialects, Central Pomo, Malagasy) or with groups of genetically or geographically related languages (North-West Caucasian and Kartvelian, Bodic, Kiranti, Germanic and Romance), as well as wide-scope typological studies. Most of the papers are, in addition to being empirically grounded, also theory-oriented, aiming at elucidating some analytical, methodological, or terminological issues against the particular material, or bringing forth new approaches to the data. A broadly understood functional-typological approach is the framework the contributors adhere to, though not all of them state this explicitly; this does not mean, of course, that the volume shows absolute theoretical unity.

The volume opens with a brief preface (vii–ix) by the editors, stating the goals of the book and giving useful short summaries of the individual chapters. Though the editors decided not to divide the volume into several thematic sections, the organization of the book follows a certain pattern.

The first two papers deal specifically with morphological case, and contain both discussions of interesting (though not previously unknown) data and important theoretical and methodological conclusions. GREVILLE G. CORBETT, in 'Determining morphosyntactic feature values: The case for case' (1–34), extends his 'canonical' approach to typology (see e.g. Corbett 2005) to the category of case, and illustrates it with data from Russian, whose case system includes both more canonical and less canonical case values. Since the canonical approach is relatively well known, I do not think it is necessary to give an outline of it here. With respect to case, it allows one to formulate a whole array of morphological, syntactic, and semantic criteria defining the range along which cases (and, indeed, almost any reasonable morphosyntactic features and their values) can vary. The second part of the paper, which discusses the problematic case values in Russian (the vocative, the so-called 'second genitive' and 'second locative', the adnumerative, and the 'inclusive', the latter usually not analyzed as a separate case value), is particularly interesting, especially for those linguists who are not well acquainted with Russian data. The canonical approach allows one to give a principled and explicit ac-

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count of the important differences between the 'central' and the 'peripheral' cases in Russian, and can, moreover, be useful for typological comparison. Finally, it must be acknowledged that Corbett does full justice to the literature on case published in Russian, even to the lesser-known papers.

ANDREW SPENCER, in 'Does Hungarian have a case system?' (35–56), analyzes the morphosyntax of what has been traditionally considered 'case' in Hungarian and argues that in this language it is not necessary to posit a morphosyntactic feature 'case' at all. Spencer bases his argument on the assumption that in order to justify a genuine case system in a given language, the following two questions (the so-called 'Beard's criterion', Beard 1995, Spencer & Otoguro 2005) must be answered: (i) 'is there a need for a [Case] attribute in morphology to capture generalizations purely about forms?', and (ii) 'is there a need for a [Case] attribute in the syntax to capture generalizations about the parallel distributions of sets of distinct forms?' (37). Thus, in languages like Russian—where case values are not expressed by dedicated morphemes, being fused with number and (in adjectives) gender, which is further complicated by the existence of several inflectional classes and by various instances of syncretism, and where case concord is exhibited by adjectives, numerals, and pronouns—a [Case] attribute is undoubtedly needed. Spencer argues, however, that in languages where each alleged case value can be identified by a unique morpheme identical across all possible targets and where case concord is not attested, positing a [Case] feature is superfluous. For Hungarian, Spencer specifically argues that the traditional cases are 'fused postpositions', and this analysis is supported by the fact that in this language it is indeed not easy to draw a strict borderline between traditional cases and postpositions.

I am quite sympathetic with Spencer in the empirical part of his paper, where he carefully shows that the system of markers of grammatical functions of noun phrases (including both traditional cases and postpositions) in Hungarian is indeed quite distinct from case systems in 'flective' languages, and I agree with his conclusion that perhaps a formal grammar of Hungarian indeed does not need a morphosyntactic feature 'case'. I would like to point out some possible pitfalls of such an approach, however, particularly relevant for the crosslinguistic discussion of case.

Beard's criterion, which Spencer considers to be of primary importance for making decisions about whether a language has a case system, actually makes little sense from a typological point of view, I believe. It is now well known that case can employ quite diverse formal means, which are not necessarily affixal. What does Beard's criterion have to say about case expressed by stem-internal change (e.g. in Siuslaw; see Frachtenberg 1922) or by tone (as in many African languages; see e.g. König 2008)? It seems to me that the answer to this question might depend on the particular model of the morphology-syntax interface the individual linguist adheres to. A crosslinguistic definition of case must be as theory-neutral as is reasonably possible, however. Of no less importance is the fact that inflectional classes (whose presence in a language arguably makes the [Case] feature indispensable) may be much less straightforward than those of the well-known Indo-European languages. In some languages evidence for inflection classes is very scanty, but is at the same time fairly unequivocal. Consider Mongolic languages (Janhunen 2003:10), where stems ending in /n/ fall into two completely synchronically unpredictable classes: members of the one class always retain the final /n/, while members of the other drop it in certain morphosyntactic environments. In all other relevant respects the system of nominal dependent marking in Mongolic languages is similar to that of, for instance, Turkic languages. Now, would any reasonable linguist, just on the basis of the quirky behavior of the final /n/ in Mongolic, claim that

while Turkic languages, similarly to Hungarian, have no case, Mongolic languages do have case?

Similar observations are true of the notion of cumulative exponence. For instance, in Hua (Haiman 1980:228–40), genitive and ergative show cumulation with number (and also with person), while other cases, standing in paradigmatic opposition to these two, do not. Does Beard's criterion require the linguist to consider only genitive and ergative as 'true' morphosyntactic cases in Hua? In Mangarayi (Merlan 1989:57) case is expressed simultaneously by both (a) suffixes more or less similar to Hungarian 'fused postpositions', and by (b) the change in the shape of the noun-class prefixes. Certain syntactic environments (e.g. locational cases with feminine nouns) trigger only (a), and certain others (e.g. core syntactic cases) require only (b), while still others involve an interaction of both types of exponence. It is not straightforwardly evident how many morphosyntactic features are needed to adequately describe the Mangarayi system, nor whether cumulative exponence is involved here. What is beyond any doubt, by contrast, is the fact that this complex and unusual morphology is employed in more or less the same functional domain as the case systems of such more familiar languages as Russian or Sanskrit.

The syntactic part of Beard's criterion is not unproblematic either. As is convincingly argued by Spencer himself (Spencer 2003, 2006), case agreement or concord should be regarded as a single syntactic feature assigned to an NP/DP and realized on several subconstituents thereof. Unequivocal instances of case agreement can be found in Russian, for example, where the case-number-gender suffixes on agreeing adjectives bear little or no formal similarity to the corresponding suffixes on nouns (cf. nom.sg. *bol'š-**oj** dom-**Ø***, nom.pl. *bol'š-**ie** dom-**a***, gen.sg. *bol'š-**ogo** dom-**a***, gen.pl. *bol'š-**ix** dom-**ov*** 'big house'). But this must not always be the case. For instance, in Martuthunira (Dench 1994:Ch. 8), where all the subconstituents of a noun phrase must agree in case, the suffixes are in general identical or straightforwardly related by phonological rules. Is this 'true' case agreement or merely some sort of 'affix copying'? Moreover, if we consider Old Russian, we find there quite numerous and more or less regular instances of 'preposition copying' (Klenin 1989, Minlos 2007), for example, ***po** mostu **po** velikomu* 'over the great bridge'. Is this an instance of 'concord', and if not, then what are the theory-independent reasons to distinguish this and similar phenomena from concord proper? Returning to Australia, let us consider Nyigina (Stokes 1982:59–60), where case suffixes usually occur only once per NP, but may attach to any of its subconstituents. Though this language arguably has no or very little case concord in the strict sense of the word, an analysis in terms of an abstract [Case] feature assigned to the whole NP and realized on at least one of its subconstituents is feasible.

To summarize, I believe that, taken to its logical endpoint, Beard's criterion will leave linguists with a handful of languages, mostly Indo-European, having a [Case] category complicated by various morphological quirks, sometimes clearly marginal from the point of view of the general system (as the Mongolic 'unstable /n/'). Most importantly from the typological point of view, the approach advocated by Spencer highlights the rather superficial, though by themselves very interesting, differences among languages, such as presence vs. absence of case syncretism, synchronically unmotivated inflectional classes, or case agreement irreducible to 'affix copying', while blurring the important and sometimes quite nontrivial functional similarities displayed by systems of dependent marking irrespective of the aforementioned formal properties.

The papers by both Corbett and Spencer raise the question of the nature and definition of case as a morphosyntactic phenomenon, and provide valuable discussions of

nontrivial empirical data. Both papers surely deserve attention from all doing research on case, and it would be fair to conclude that while Spencer's approach is best suited for the construction of formal grammatical descriptions of individual languages, Corbett's canonical approach is best suited for a crosslinguistic investigation, where gradual notions are indispensable.

The next two papers deal with grammatical phenomena of the languages of the Caucasus. JOHANNA NICHOLS, in 'Case in Ingush syntax' (57–74), focuses on morphological and syntactic ramifications of alignment in Ingush (Nakh-Dagestanian). Morphological case marking in Ingush is consistently ergative both in nouns and pronouns, and so is verbal agreement. However, other morphosyntactic phenomena are less uniform in their alignment. Reflexivization (both local and long-distance) and infinitive complementation are predominantly subject-oriented regardless of case (subjects in Ingush may bear absolutive, ergative, dative, and genitive cases), whereas converb constructions again pattern ergatively. Finally, relativization is virtually unconstrained (however, no examples that could show this are provided in the paper). Nichols also discusses certain lexical phenomena that show no uniform alignment, such as ambitransitive verbs, derived inceptives, causativization, and complex verbs. She concludes that in Ingush those syntactic phenomena that are sensitive to case marking pattern ergatively, while those that show accusative alignment are independent of case. From the diachronic and comparative point of view, Nichols hypothesizes that accusative traits in Ingush must be innovative.

The data presented in Nichols's paper are interesting, and her general conclusions seem to be well justified. However, the overall impression of the paper is that it is somewhat disappointing. First, the use of terminology is rather messy. Putting aside obvious misprints (e.g. ergative instead of absolutive at the top of p. 58), I cannot judge to be precise and scientifically correct such formulations as 'the issue is a purely syntactic one of weak crossover or command or the like' (63). Both notions ('weak crossover' and 'command') are well defined and do not admit of such fuzzy uses, and, most importantly, their application to particular data requires certain empirical justification, which Nichols does not provide.

A more substantial objection is raised by §3.2, where Nichols discusses the phenomenon she calls 'case climbing', 'in which the subject of a modal or similar auxiliary takes the case of the subject of its infinitive complement clause' (60). As far as I may judge from the few examples provided, as well as from the terminological discussion in n. 5 (61), the alleged 'case climbing' could be better analyzed as involving either backward control, with the subject remaining inside the infinitive clause (Polinsky & Potsdam 2002; Nichols does not refer to this important contribution, nor to Polinsky and Potsdam's 2001 article on long-distance agreement in Tsez, which is also relevant), or restructuring, whereby the modal and the infinitive form a single complex predicate. Nichols's statement that 'the Ingush infinitive always has a shared subject' (61, n. 5) is compatible with both types of analysis, whereas her claims that the relevant noun phrase syntactically belongs to the matrix clause must be supported by constituency and scope tests, which she does not provide. All in all, had Nichols adduced robust evidence that (i) the subject in these constructions indeed belongs to the 'upstairs' clause, and (ii) that the construction does not involve restructuring (which would mean, for example, showing that the modal and the infinitive admit independent modification by adverbials and negation), the Ingush 'case climbing' would constitute a previously unattested type of construction, running counter to the commonly accepted assumptions about locality and directionality of case assignment. These very interesting data call for a careful and

sophisticated analysis grounded in a strict and unequivocal use of terminology and informed by current theoretical work.

George Hewitt, in 'Cases, arguments, verbs in Abkhaz, Georgian and Mingrelian' (75–104), presents an extensive and quite interesting discussion of patterns of argument marking in various verb classes in three geographically close languages of the Western Caucasus, belonging to two different language families, viz. North-West Caucasian (Abkhaz) and Kartvelian (Georgian and Mingrelian). The article discusses the very complex systems of verbal agreement in these three languages, coupled with no less complex patterns of case marking of core arguments in the Kartvelian languages. Various morphosyntactic derivations such as causative, potential, and expressions of unintentional actions further complicate the situation discussed by Hewitt. A large body of the paper is devoted to the long-standing debate on the 'active' vs. 'ergative' characterization of Georgian. Based on various interesting evidence, Hewitt again (cf. Hewitt 1987) claims that the 'active' traces postulated for Georgian and other Kartvelian languages by some scholars (e.g. Alice Harris (1981, 1985)) have been misanalyzed, and concludes that 'the traditional categories of ergativity and transitivity still provide the best framework for understanding the aspects of Georgian, Mingrelian and Abkhaz verbal morphology, argument structure and associated case-marking' (103).

The size of this review does not allow me to fully discuss Hewitt's claims, but I must confess that I consider them largely unjustified and grounded in limited knowledge of the literature on the topic (see Harris 1990 for a critical evaluation of Hewitt's earlier claims on the topic). The paper does not contain references to such major publications on 'active/stative' languages as Mithun 1991 and Donohue & Wichmann 2008, let alone to the vast 'formalist' literature on unaccusativity, or, most dramatically, even to important contributions discussing Georgian itself, such as Van Valin 1990 and especially Holisky 1981. Though Hewitt is correct in pointing out that there is much more to the Georgian verbal system than an active-inactive distinction, his claim that 'there are no grounds in Georgian to justify classifying it as manifesting in any part of its morphosyntax the Active-Inactive opposition' (95) is far too strong. The distinction between the two classes of intransitive verbs in Georgian is indeed neither between 'stative' vs. 'active', nor between 'patientive' vs. 'agentive' (and, as far as I know, no one has ever proposed to analyze Georgian precisely in these terms). This does not mean, however, that the dichotomy does not admit of a semantic characterization, or that it cannot be regarded as a possible instantiation of so-called 'semantic alignment' (see Donohue & Wichmann 2008). As has been shown already by Dee Ann Holisky (1981), the most important semantic parameter underlying the distinction between the so-called 'intransitive proper' and 'medial' verbs in Georgian is telicity with respect to atelicity. This is by no means the whole story, and Hewitt's paper provides the reader with a rich array of fascinating data that do not always fit well into Holisky's generalization. Nevertheless, not doing justice to the well-known proposals of experts in the field and fighting with straw men instead is not, to my mind, very productive.

Another point to be made about Hewitt's paper is that it is rather hard to get through his argument. The paper is not explicitly structured; moreover, while presenting extremely complex material, Hewitt does not always make the examples clear and the conclusions he draws from them uncontroversial. To give just a couple of examples, on p. 78 Hewitt tells the reader that the two types of bivalent verbs in Abkhaz are distinguished by stress pattern, but he does not explicitly indicate how precisely this disambiguation is instantiated, and the stress marking in the examples is less than evident. It is at least very unconventional to call the Circassian verbal prefix *fe-* a 'benefactive postposition' (85); needless to say, no references to authoritative sources such as

Smeets 1984, 1992 or Paris 1989 are found in Hewitt's discussions of West Circassian data. On p. 86 the Mingrelian examples 31 and 31′ and the Georgian example in n. 21 all contain different prefixes, and this variation deserves explication and discussion. Glossing the Georgian 'series marker' *-eb* as 'intransitive' in example 46 on p. 95 is also quite controversial. To conclude, I think that the editors should have suggested that Hewitt rewrite his article to make the text more readable and the argumentation better justified and better informed in contemporary typological work.

The next two papers also deal with morphological case, now from a predominantly empirical perspective. ÖSTEN DAHL, in 'The degenerate dative in Southern Norrbothnian' (105–26), presents very interesting material virtually unknown to the general linguistic audience (see also Dahl & Koptjevskaja-Tamm 2006). He explores the relationships between number and definiteness marking in several vernaculars of Northern Sweden, where certain constructions require that the noun appear in the form going back to the older dative plural. There are two types of this construction: the one contains quantifiers such as 'many', 'little', 'most', 'some', 'dozen', and so forth, and the other involves adjectives. From a typological perspective, Dahl argues, such former datives can be compared to the better-known Persian 'ezafe'. From the diachronic point of view, the development of these constructions poses a whole variety of problems. As Dahl shows, initially there might have been a preposition governing the dative after the quantifiers (as in English *many* ***of*** *the fences*), which later was dropped. But the precise path of an analogical extension from the quantifier construction to the construction involving premodifying adjectives is less clear. Whichever way the Norrbothnian noun phrases actually did evolve, this material is indeed fascinating, and Dahl can only be praised for bringing forth the data contained in the Swedish sources and interpreting it from a typological point of view.

The late MICHAEL NOONAN, in 'Case compounding in the Bodic languages' (127–47), starts with laying out a comprehensive typology of the phenomenon of case compounding. This phenomenon, though quite pervasive in the languages of the world, has not yet received due attention from linguists (the only major contributions are the classic paper by Dench and Evans (1988) on Australian languages and the *Suffixaufnahme* volume Plank 1995). Extending Peter Austin's (1995) typology, Noonan proposes to distinguish between CASE STACKING, where 'two independently occurring case affixes are used together to describe a complex trajectory' (128), DERIVATIONAL case compounding, where 'one case serves as the "basis" for another, which is not found independently without the first' (129), REFERENTIAL case compounding, where constituents 'are marked with one case indicating location or direction and another referencing the NP it modifies or refers to' (129), and several types of ADNOMINAL case compounding. The latter vary according to such parameters as presence vs. absence of an overt nominal head and the presence vs. absence of case marking on this head. A special kind of adnominal case compounding is the COMPLEX ATTRIBUTIVE NOMINAL, where 'a case-marked noun is further marked with a nominalizer-attributive affix, and the resulting noun may be further case-marked' (131). Noonan's classification of case compounding (limited to adnominal and relational uses of case and excluding case on verbals and the so-called 'modal' case) is, to my knowledge, the most comprehensive typology of this phenomenon found in current literature. The main body of the paper is devoted to the presentation of case and especially case compounding in Bodic, a branch of the Tibeto-Burman family, analyzed from both a synchronic and a diachronic perspective and containing valuable and interesting data.

MARTIN HASPELMATH and SUSANNE MICHAELIS, in '*Leipzig fourmille de typologues*—Genitive objects in comparison' (149–66), discuss verbal complements expressed by genitive case or genitive adposition in several European languages (French, Italian,

Latin, German, English). Genitive objects in these languages are found with several types of verbs, including location (German *wimmeln (von)* 'swarm'), change of location (positive: French *tapisser (de)* 'paper'; negative: English *deprive (of)*; in some languages, such as English, genitive objects are found only with negative verbs of change of location), possession (both positive possession: French *disposer (de)* 'have', and negative possession: Italian *mancare (de)* 'lack'), cognitive (Latin *memini* 'remember', German old-fashioned *vergessen* + gen. 'forget'), and emotional (Portuguese *gostar (de)* 'like'). Special subclasses of verbs allowing genitive objects are constituted by reflexive and subjectless predicates, for example, German *sich bemächtigen (von)* 'acquire', French *se souvenir (de)* 'remember', Latin *pudet* 'be ashamed', and so on. The authors propose that genitive objects all have a common function, that is, are background themes, and speculate on possible diachronic sources of this form-function mapping. In this connection, I think that taking into account a broader range of data (e.g. the relatively well-described Slavic and Baltic material, where genitive verbal complements are quite widespread) might be fruitful, especially from the point of view of possible areal connections.

The next two papers present large-scale typological studies. JOHN A. HAWKINS, in 'An asymmetry between VO and OV languages: The ordering of obliques' (167–90), uses the database of the *World atlas of language structures* (Haspelmath et al. 2005) to investigate the crosslinguistic distribution of possible orders of direct objects and obliques with respect to each other and to the verb. Hawkins shows that while VO languages are very consistent in ordering obliques after the object, OV languages allow for all logically possible orders and also show a larger proportion of intralinguistic order flexibility. Explanation of this asymmetry is based on the PERFORMANCE-GRAMMAR CORRESPONDENCE HYPOTHESIS (Hawkins 2004), which claims that 'Grammars have conventionalized syntactic structures in proportion to their degree of preference in performance, as evidenced by patterns of selection in corpora and by ease of processing' (171). The most interesting parts of the paper, in my opinion, are the sections discussing the corpus data on the variable ordering of long and short objects and obliques in English (a VO language) and Japanese (an OV language), which support Hawkins's performance model. He also observes that the lack of a strong positional preference for obliques in OV languages may be due to a lower degree of structural differentiation between the two types of phrases in head-final languages (which tend to mark both objects and obliques with bound case morphemes) than in head-initial languages (which more often have prepositions marking obliques), and also to larger variation in head positioning in OV languages. Interestingly, Hawkins shows that while XOV and OXV languages almost exclusively have postpositions, a full one-third of OVX languages have prepositions, and a similar tendency is observed in the NP domain.

Summarizing, Hawkins proposes that various logically possible orders of verb, object, and oblique to different degrees conform to two general principles (see Hawkins 1994, 2004 for details): MINIMIZE DOMAINS (favoring V and O adjacency, and putting O and X on the same side of the verb) and ARGUMENT PRECEDENCE (favoring O before X). The three tendencies (V and O adjacency, O and X on the same side, and O before X) converge and reinforce each other in VO languages, yielding consistent VOX ordering, but are in partial conflict in OV languages, which results in greater variability. The degree of preference for different orders correlates with the number of tendencies they conform to: VOX (3) > XOV/OXV/OVX (2) > XVO/VXO (1) (p. 187).

BALTHASAR BICKEL, in 'On the scope of the referential hierarchy in the typology of grammatical relations' (191–210), challenges the commonly assumed typological pre-

diction that nominals ranking higher in the referential hierarchy (RH; 1 > 2 > 3 > animates > inanimates) are more likely to be accusatively aligned, while those ranking lower are more likely to be ergatively aligned; compare proposals by Silverstein (1976) and Comrie (1978a, 1981). Bickel tests the predictions of the RH-based hypothesis against large typological databases on verb agreement and nominal case marking and concludes that the number of relevant languages (i.e. those exhibiting accusative vs. ergative splits) is too low to be indicative of a strong linguistic universal (for a more extensive discussion and explicit statistical data, see Bickel & Witzlak-Makarevich 2008). Moreover, Bickel presents data from the languages of the Kiranti branch of Sino-Tibetan, which demonstrate diachronically stable patterns contradicting the RH-based predictions: in verb agreement, the first person aligns ergatively, while the third person aligns accusatively. Though in the domain of nominal case marking the results are slightly better for the RH-based hypothesis, this support is not very strong, both because of the small number of the relevant languages and due to the existence of some quite robust counterexamples (see Filimonova 2005). Similarly, in domains other than case marking and agreement, the evidence for RH-based distributions is inconclusive: ‘With regard to relative constructions, for example, there are both languages where the relativizable G[rammatical] R[elation] favors higher-ranking arguments and languages where the same GR favors lower-ranking arguments’ (205). Bickel concludes that ‘where statistical testing is possible, we find no support for a general trend linking accusative alignment with high RH positions and ergative alignment with low RH positions’ (207).

These results, which might be rather disappointing for those who consider the RH-based hypothesis ‘among the most robust generalizations in syntactic markedness’ (Aissen 1999:673), are very instructive, at least for the following reasons. First, they show that in order not to propagate linguistic myths and speculative post hoc ‘explanations’ of the kind ‘the speaker is the prototypical agent, hence first person agents are unmarked’ (this logic, though put to trial as early as in Mallinson & Blake 1981, has not been abandoned up to now), one must first conduct a large-scale crosslinguistic study and make sure that the generalization one wishes to explain is exemplified by a statistically significant number of independent cases. As Bickel shows, with the RH-based hypothesis this is not the case. Second, the RH story shows how well individual developments attested in several language families—which are probably reducible to such better-understood phenomena as grammaticalization channels of case markers and special morphological properties of pronouns (ultimately having to do with their high text frequency)—can mimic the ‘universal tendencies’ that linguists are still tempted to explain with the aid of such controversial concepts as ‘iconicity’ or ‘markedness’ (see Haspelmath 2006, 2009).

Marianne Mithun, in ‘Does passivization require a subject category?’ (211–40), argues on the basis of data from Central Pomo against a conception of passive that hinges upon the much-discussed notion of subject. Mithun shows quite convincingly that there is almost no language-internal evidence for a subject category in Central Pomo. Overt case marking in Central Pomo is semantically driven (exhibiting an agent/patient system described for this language in Mithun 1991), while various morphosyntactic operations like imperative formation, plurality indicators, conjunction reduction, relativization, and switch reference are either agent-oriented or show no direct sensitivity to grammatical relations or semantic roles at all. After having established this, Mithun turns to the construction she terms ‘passive’. In Central Pomo, ‘passive’ does not involve any change in the grammatical relations (since there are none) or morphosyntactic properties of arguments, its primary function being to eliminate the agent when it is generic, unimportant, or unknown. In this respect, the ‘passive’ in Central Pomo is similar to the *-ta*-passive in

Ute, as described by Givón (1988), and belongs to the class of agent-backgrounding morphosyntactic operations.

I like the empirical part of Mithun's paper and consider the data (especially those that I have not seen in her previous publications) very interesting and the analysis mostly convincing. However, two comments are in order from the theoretical side. First, it is not always clear in which sense the term 'subject' is used by Mithun, especially when it is opposed to the agent, as in the following passage: 'The antecedent of "his" is the subject but not the agent of the immediately preceding sentence' (227). If, on the one hand, subject is not a universal category easily identifiable in all languages, and if Central Pomo does not have a robust language-specific subject, how can one determine on independent grounds which noun phrase is the 'subject' in any given sentence of this language? If, on the other hand, the 'subject' of the particular Central Pomo sentence the passage above refers to is simply the noun phrase corresponding to the subject of its English translation, then why not extend this simple and straightforward 'definition' of subject to all other Central Pomo sentences? Second, I must confess that I do not think that the main point of this article—that is, that passives can exist without subjects—is at all worth making. If passive BY DEFINITION is a morphosyntactic operation crucially referring to grammatical relations and involving a promotion of the former object into the subject position, then the Central Pomo construction is (again by definition) not a passive, regardless of any functional similarities it has with genuine passives and especially of the fact that it is 'typically translated as a passive' (239). But if we assume a prototype approach to the notion of passive, following Shibatani (1985), then the Central Pomo construction will be a nonprototypical instance of passive falling in a well-defined subclass of passive-related constructions. Under this approach, however, the problem of the subject category in Central Pomo is largely irrelevant for the discussion of this construction, since the nonprototypical instances of passives, undoubtedly, are not required to refer to grammatical relations. To conclude, the question put in the title of Mithun's article, viz. 'Does passivization require a subject category?', is, in my opinion, purely a terminological one, and thus only of marginal interest; compare Shibatani's (1985:822) formulation that 'the familiar controversy ... over whether a given construction should be considered a passive is pointless'.

EDWARD L. KEENAN, in 'The definiteness of subjects and objects in Malagasy' (241–61), discusses the so-called definiteness duality, whereby direct objects may freely be indefinite without special marking while definite direct objects often require special marking (Comrie 1978b), whereas subjects show an opposite distribution, and some languages may even prohibit indefinite subjects. Since Western Austronesian languages have often been considered to instantiate definiteness duality, Keenan investigates the effects of these tendencies with respect to objects and subjects in Malagasy. In the section devoted to object-related definiteness duality, Keenan recounts (without many references to the vast literature on the topic, however) the well-known fact that, crosslinguistically, differential object marking is not limited to definiteness but may involve special conditions such as word order (Turkish) or more intricate semantics (Mandarin Chinese). Turning to Malagasy, Keenan observes that the locative preposition *an-* obligatorily appears with proper names and is optional with the 'previous mention' article *ilay*, but does not occur with the definite article *ny* and thus cannot be considered a marker of definite objects per se. Indefinite objects in Malagasy must be adjacent to the verb, however, whereas the definite ones have no such restriction.

Turning to subjects, Keenan shows that though in Malagasy articleless indefinite NPs are not allowed in the clause-final subject position, there are several types of NPs that

are built with the aid of the definite article *ny* and freely appear as subjects, but semantically are indefinite. These are various quantified expressions, involving numerals and cardinal ('many', 'some', etc.), universal ('all', 'each'), and proportionality ('ninety percent', 'half') quantifiers. A separate section is devoted to the discussion of an interesting property of these subject NPs, which they share with the ordinary definites, namely their ability to outscope negation. Though it might be tempting to think that the quantified NPs have some 'definite flavor', Keenan rejects this hypothesis in favor of a purely structural account. He shows that negation in Malagasy takes in its scope only the predicate phrase, while the subject attaches higher in the syntactic structure. Keenan concludes that 'we simply don't know how this usage [semantically indefinite quantified NPs in the subject position—*PA*] compares with other languages in which subjects have been claimed to be definite, as commonly the claims are just illustrated with the simple cases of definites ... So more empirical typological work is needed' (259–60). This is a field of investigation where an interesting and mutually enriching collaboration between typologists and formal semanticists might be possible.

MARIA POLINSKY, in 'Without aspect' (263–82), analyzes the encoding of aspect in incompletely acquired (Heritage) Russian. The first section of the paper presents the characteristic features of Heritage Russian and outlines three approaches to its investigation. After having briefly recounted the most important properties of aspect in standard Russian (predominantly derivational in nature, and combining more lexicalized and more grammaticalized means of expression), Polinsky turns to the state of affairs in Heritage Russian. She shows that the deterioration of verbal morphology in Heritage Russian has led to some important changes in the expression of aspect, such as regularization of aspectual paradigms and the impoverishment of the set of affixal exponents of aspect. Second, the very use of aspectual forms in Heritage Russian is often deprived of aspectual semantics, imperfective forms being used in perfective contexts and vice versa. Polinsky presents the results of an experiment showing that the speakers of Heritage Russian choose one of the aspectual forms arbitrarily even in those contexts where the choice is unequivocal in the standard language. Addressing the question of possible reasons for retaining the perfective or imperfective member of former aspectual pairs in Heritage Russian, Polinsky shows that frequency alone is not always a sufficient factor. Finally, Polinsky discusses how universal aspectual distinctions are expressed in Heritage Russian in the absence of the former aspectual system, and shows that light verbs are used instead.

The volume also includes indices of authors, languages, and terms.

The overall impression left by the book is surely very positive. All of the contributions contain interesting and sometimes quite novel material, and the analyses presented are definitely worth considering even if not always entirely convincing. As a festschrift to Bernard Comrie, this volume is almost ideal, focusing as it does on the central field of his typological research, that is, the study of case and grammatical relations, and sometimes even challenging his own proposals (especially Bickel's paper). The inclusion of Polinsky's paper on aspect in Heritage Russian, which, strictly speaking, does not very well fit into the general topic of the book, is certainly justified by Comrie's seminal contribution to the field (Comrie 1976), as well as by the long-standing collaboration between the two scholars. The range of languages covered in the volume is also very impressive, from the well-known European languages to Sino-Tibetan, Central Pomo, and Malagasy by way of lesser-known European varieties (Swedish vernaculars and Heritage Russian) and the languages of the Caucasus.

Several articles of this collection, in my opinion, deserve special attention from theoretical linguists and typologists, since they make important contributions to our understanding of case (Corbett), raise serious methodological questions (Spencer), shed new light on lesser-known phenomena (Noonan, Haspelmath and Michaelis), or present valuable large-scale typological research (Hawkins, Bickel), sometimes challenging the common assumptions of the community. Two of the more empirically oriented papers, viz. Dahl's and Keenan's, are also of great value in that Dahl introduces the wide linguistic audience to the quite exotic material of Southern Norrbothnian nominal morphology, and Keenan urges typologists and descriptive linguists to pay more attention to quantified noun phrases and scope relations.

The main points of criticism and discussion about the individual chapters have been already presented above, so here I would like to make some more general remarks. Though *Case and grammatical relations* is far more coherent than an ordinary festschrift, I believe that it could have been even more so. For instance, both Corbett and Spencer could have discussed some of each other's conceptions in their respective chapters, which would have been of great value (of particular interest could be an assessment of the Hungarian data Spencer discusses against Corbett's canonical approach to case). Hewitt, when quoting Ingush, could have referred to Nichols's article, where some of the data directly relevant to his analysis are presented. Again, in Corbett's canonical typology the widespread phenomenon of case compounding discussed by Noonan could also have found its place.

More editorial work, I think, could have been done. I have already mentioned above that Hewitt's paper is rather reader-unfriendly, and this perhaps could have been amended. In several places data are cited in such a way that it is not always easy to guess from which language they come (e.g. the Avar case paradigm on p. 136 and the Rumanian examples on pp. 242–43); sources of data are not always provided (e.g. the Tocharian A paradigm on p. 129). Some terms are not clarified (e.g. Nichols should have probably explained what is meant by 'Type 5 clitic' on p. 66), as well as some local specialties (what is *sädesskylar* in the translation of a Norrbothnian example on p. 113 of Dahl's paper?). Typos and mistakes are also found; for example, Russian *v zabyt'i* in Corbett's paper (20, n. 30) means 'out of consciousness', not 'in oblivion'. Words and glosses are misaligned in exx. 28–31 on pp. 118 and 120 of Dahl's paper; an ERG(ative) gloss is missing from ex. 8d on p. 131 in Noonan's paper.

To recapitulate, the collection of papers in *Case and grammatical relations: Studies in honor of Bernard Comrie*, despite certain weaker points (both conceptual and technical), is a very valuable contribution to the typological and empirical study of case and grammatical relations, and, last but not least, it is indeed worthy as a festschrift to such an eminent scholar as Bernard Comrie.

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