Clause Types and Verb Types: the author's reply

Amaya MENDIKOETXEA
Universidad Autónoma de Madrid

I have read with a sense of perplexity Enrique Bernárdez's «On Unaccusativity: Clause types and verb types reconsidered» in this volume, which is supposedly inspired by some of the ideas I discuss in my own «Clause-types and verb-types: Implications for descriptive and pedagogical grammars of English» (EIUC, 3). The number of inaccuracies and misconceptions, the general tone of the discussion, and the position he ascribes to me regarding, for instance, the relation between formal and functional grammars and the particular analysis suggested have, unfortunately, made this reply necessary – a task which I undertake rather reluctantly (and hastily), but in the hope that it will serve the purpose of clarifying my position on some of the issues addressed in my paper, as well as others raised by Enrique Bernárdez's paper itself.

The aim of this reply is to show that Enrique Bernárdez's paper (EB, for brevity) is based both on a fundamental misconception as to what are the essential claims and leading purpose of my own paper (AM, for brevity), as well as on a crucial misunderstanding of my approach to the syntax-semantics of unaccusative verbs. These two factors underlie a significant number of the objections that EB raises against some of my own observations and ideas concerning (a) linguistics at large and approaches to language study (basically, sections 1, 2 and 3 in EB), and (b) the particular analysis of unaccusative verbs offered (basically, section 4 in EB). I focus on each of them in turn.

1. THE USE OF THE TERM 'FUNCTIONAL' IN AM, AND LINGUISTICS AT LARGE

EB attributes to my article a purpose and a significance which is, to say the least inaccurate, if not entirely misleading: (i) I do *not* argue that unaccusativity as a semantic and a syntactic phenomenon should be introduced into functional/cognitive approaches to grammar as a whole [1], and (ii) I do *not* even faintly suggest that functional grammar should follow the «dictates» of formal grammar.

Regarding the former, my paper argues for a «non-functional» approach to clause types and verb types, where non-functional is to be understood (by the objective reader) as an analysis which does not refer to **syntactic or grammatical functions** (GFs) (i.e. Subject, Object and so on), such as the one commonly found in descriptive and pedagogical grammars of English, as EB correctly points out in section 2.1. Insofar as the functional grammars (FGs) quoted in EB make use of such classifications, my claims could indirectly affect them, but there is no criticism in my paper of how FGs classify clauses and verbs, contrary to what EB appears to be suggesting in section 3.

In a very restricted sense, EB may not be entirely wrong when he says that I offer an incomplete picture of present-day functional models. However, to say that this is based on «prejudices to be found in the consideration of non-generative models by generative grammarians» (EB: sec. 2.2) and that it serves the purpose of providing a «tailor-made adversary which one's own ideas can confront with superiority» (EB: sec. 2.2) is a misrepresentation of the facts. Statements like these would be rather too serious to be lightly dismissed even if they were well grounded, but in this case they are clearly intolerable since, as I will show, they are unjustified. EB fails to mention that the reference to functional grammars in AM is purely incidental and is there merely to clarify (apparently without much success) the use of the term «(non)-functional» in the Introduction to my paper. Crucially, my use of the term «functional» differs from that of EB's. In my paper, as stated in the Introduction, functional is mainly used in relation to grammatical functions (GFs) (and this is what the analysis concentrates on). In EB's paper, functional is mainly used in relation to a particular approach to the study of language ((cognitive-)functional grammars (FGs)). Since I have already said that the objective of my article was to argue against an analysis of clauses and verbs which makes use of GFs — and *not* against an analysis of clauses and verbs in FGs — I find EB's observations, at best, irrelevant [2].

This basic misunderstanding is also apparent in section 3 in EB, on the issue of GFs. This section is devoted to showing that the objections in AM against functional explanations in relation to the transitive-intransitive dichotomy and the use of GFs are not valid. However, again, he is missing the point: I argue against the use of S(ubject) and O(bject) in the classification of

clauses/verbs in descriptive and pedagogical grammars (e.g. Greenbaum & Quirk 1990), but I do *not* say anything about the use of these terms in FGs or in any other theoretical model [3]. Questions like whether theoretical grammars such as those mentioned by EB in section 3 should make use of GFs and whether GFs are primitive or derived are beyond the scope of my paper, as clearly pointed out in the Introduction:

> «...my purpose is not to evaluate different theoretical frameworks according to their use of GFs, under the assumption that terms like S and O have no clear pretheoretical reference and that any answer to the questions above is inevitably linked to the explanatory power of the different theories, an issue well beyond the scope of this paper.» (AM: 135)

Incidentally, since my own view, from a Chomskyan perspective, is that GFs are derived terms, I am glad to be informed that functionalists like Dik (1989) and Nuyts (1992) (mentioned in EB) also support that idea [4].

As for the position that functional grammars should follow the «dictates» of formal grammars (see (ii) above), I would understand EB's reaction in section 1 if I had said anything which could even remotely be interpreted like that. EB provides no proof to substantiate that this is actually my position (he does not quote me either on (i) or (ii) above). To say that «AM's stance is not quite as extreme» (EB: sec. 1) is simply not enough, since I do not express a stance on this matter at all, and therefore it can by no means be used to justify what comes afterwards in his paper. Simply, my paper was about something else.

Perhaps characteristic of EB's reaction to my paper is the fact that he crucially misquotes when he attributes to me the following position «much of the recent effort in linguistics has been devoted to deriving lexical properties from **syntactic** properties» (AM: 141 in EB: sec. 2.2, my emphasis). Unless my own copy of that volume, the offprints, and the original are wrong, where EB claims I said «syntactic», it actually reads «**semantic**». A closer and more objective study of the text may have prevented this kind of error, as well as the other misconceptions that are evident in EB's reading. EB may not be aware that deriving lexical properties from semantic properties (and syntactic properties from lexical semantic properties) is currently a popular line of research among formal grammarians (including myself), and one in which formal linguistics has benefitted from work carried out in other (more functionalist) frameworks [5].

2.   THE ANALYSIS

Section 4 both in AM and EB contains the particular analysis of unaccusative verbs proposed. There are two basic misunderstandings in EB's paper,

if it is to be seen as a response to mine, concerning (i) the overall purpose
and (ii) the nature of the analysis.

The purpose of section 4 in AM was to show that a descriptive/pedagogi-
cal grammar incorporating Perlmutter's Unaccusative Hypothesis «is able to
make generalizations and provide explanations which would not be made
and provided otherwise.» (AM: 140). Whether there are alternative ways of
providing the same generalizations and explanations or not is something the
paper does not discuss, given its necessarily limited scope, but it can certainly
be part of an engaging and fruitful debate regarding the analysis of clauses/
verbs in different theoretical frameworks and the way they should inform de-
scription and pedagogy. Not even the slightest reference is made in AM to
the idea that cognitive-functional grammars cannot account for the facts that
the Unaccusative Hypothesis is designed to account for, since I hope that it is
by now clear that it was not the purpose of my paper to evaluate this hypo-
thesis from a theoretical perspective. Therefore EB is wrong when he says:
«My point here, **as was also partly AM's in her article**, is whether (cognitive)-
functional grammar can account for these facts, and whether they have ac-
counted for them in different ways» (EB: sec. 4.1, my emphasis).

As for the nature of the analysis, a critical assumption in AM is that the
syntactic properties of unaccusative verbs follow from their lexical semantic
properties, following ideas in Levin & Rappaport Hovav (1995) (L & R,
henceforth) [6]. Despite this, EB insists on ascribing a «basically syntactic» ap-
proach to both my analysis and that in L & R, on which my account is
based [7]. L & R start from the assumption (following Perlmutter 1978) that
unaccusativity is **both** *semantically* determined and *syntactically* encoded (see
140-1 in AM). There have been, in fact, two distinct approaches to unaccu-
sativity (cf. the introduction to L & R's book): (i) a syntactic approach, which
denies that unaccusativity is semantically predictable (see e.g. Rosen 1984);
and (ii) a semantic approach, which denies that unaccusativity is syntactically
encoded (see e.g. Van Valin 1990). While it is clear that EB adopts the latter
position, it is false that the former approach is the one adopted in my paper
and especially in L & R, despite the fact that this is what the quotations se-
lected by EB from my own work and, particularly, from L & R's seem to
show [8].

An example of this is is the following quotation given by EB in section
2.4. from L & R (2-3):

«The Unaccusative Hypothesis (...) is a syntactic hypothesis that claims that there
are two classes of intransitive verbs, the *unaccusative* verbs and the *unergative*
verbs each associated with a different underlying syntactic configuration»

These words belong to section 1.1 in the Introduction to L & R's book when
they first present the Unaccusative Hypothesis. Where EB has chosen to omit

information by using (...), L & R say «as first formulated by Perlmutter (1978) within the framework of Relational Grammar and later adopted by Burzio (1986) within the Government-Binding (GB) framework (Chomsky 1981)». But this position is by no means representative of the one adopted by L & R throughout their book (see note 8). The following quotation reflects much more faithfully these authors' approach:

> «The goal of this book is to provide support for Perlmutter's hypothesis that unaccusativity is syntactically represented but semantically determined. To achieve this goal, we provide evidence bearing on both parts of this hypothesis.» (L & R: 30)

In fact, L & R concentrate most of their efforts on providing lexical semantic representations of unaccusative verbs and as such their analysis is more semantic than syntactic. One of the criticisms that can be leveled against their account of unaccusativity is that they do not establish a direct relation between lexical representations and syntactic structures, beyond rather primitive linking rules concerning the syntactic position of semantic arguments (see chapter 4) [9]. This — and the fact that they do not account for certain morphological and syntactic features of unaccusativity in languages in which these are shown overtly (auxiliary selection, participle agreement, partitive clitics, and so on in e.g. Italian and Dutch) — indicates that they do not fully achieve the goal of showing that unaccusativity is syntactically encoded, though they extensively prove that it is semantically determined [10].

As for my own approach, I am essentially following that of L & R, though the purpose of section 4 is simply to show how «unaccusativity is syntactically encoded» (AM: 141) and, hence, I do not go into the semantics of these verbs, other than incidentally. Thus, EB's characterization of my (and L & R's) position in this matter in section 4.4.2. is incomplete, and therefore questionable. Statements like «they [unaccusative verbs] form a syntactic class of verbs irrespective of their semantics» (EB 2.4.2.) are not representative of these authors' (and my own) work, even when followed in parenthesis by «(not quite so, however, as «semantics also plays a role»)», since they undermine the role played by the lexical semantics of the verbs involved. The section on resultatives further illustrates that EB is offering an incomplete picture of the account in AM following L & R [11].

EB objects to a syntactic analysis of resultative phrases and argues that a semantic analysis of these constructions renders reference to the syntax unnecessary. He downplays the role attributed to semantic considerations in AM: no mention is made of the fact that resultative phrases derive accomplishments from activities, or of the thematic role of «theme» in structures with resultative phrases (AM: 144) [12]. This may be seen as more serious when applied to his account of L & R's views on the matter. Though it is true

that the *Direct Object Restriction* is a syntactic restriction in L & R (as quoted by EB in section 4.4.1), it is also true that L & R crucially derive this restriction from semantic considerations (see section 2.2.2 «Explaining the Direct Object Restriction», and especially section 2.3 «Semantic Restrictions on the Resultative Construction» in L & R's book). What L & R argue in chapter 2 (as throughout the book) is that a purely *semantic* approach cannot account for the facts they discuss, but taking this to mean that a purely *syntactic* account does actually explain the facts is a fundamental misunderstanding of L & R's whole approach to the matter (see above).

As for EB's own account in terms of functional and cognitive grammar, this may not be the most appropriate context to discuss it, especially since the author himself admits that it is not a full analysis but «a general background on which an alternative explanation of the facts can be built.» (EB: sec. 4.2). Even so, I find the author's exposition rather obscure. Statements like «resultatives are ultimately a special case of causativity» need to be further clarified [13]. Reference to terms such as volition, controller, agentivity are vague, and the analysis offers more questions than solutions (Why does the subject of an unergative V like *work* and *laugh* require its agentivity to be «heightened» by means of a reflexive? Why can't agentive Vs like *build, construct, kill, destroy, bring, take*...etc. have resultative phrases? Why is *the clock ticked the baby awake* possible?) Finally, Goldberg's (1995) account of resultative phrases with unergatives (if I have correctly understood it from the summary of this linguist's ideas offered in EB's paper) does not add much to the discussion in L & R, who also argue that in unergative-based resultative constructions the «added» NP (not necessarily a reflexive) is *not* an argument of the verb (see also Carrier & Randall 1992):

> «We will, then, make the crucial assumption that the arguments of a verb are expressed in the same way in the resultative construction as they are when the verb appears in isolation. All that the formation of a resultative construction involves is the addition of a resultative XP (and sometimes..., a subject for that XP).» (L & R: 47)

Regarding the four constraints required for resultative phrases, they can be more accurately characterized by L & R's representation of the lexical meaning of unaccusative verbs or by Pustejovsky's (1991, 1995) theory of event composition (also mentioned in L & R) [14], and which have the further advantage of explaining why resultative phrases of some sort may appear with unaccusative verbs (while Goldberg's constraints in EB seem to apply only to transitive verbs) [15].

The conclusion reached by EB that the diagnostics for unaccusativity are a special case of a semantic-syntax configuration is perfectly in accordance with the ideas put forward by L & R and adopted in my paper in a very sche-

matic way. But this does not imply denying the existence of unaccusative verbs as a particular class of verbs. The facts can be perfectly explained by assuming that unaccusative verbs belong to different semantic classes characterized in terms of predicate composition (i.e. associated with different Lexical-Conceptual Structures), which have the same syntactic structure: projecting an internal argument, but no external argument. The alternative undermines the role played by verbs in the syntax and semantics of constructions like those discussed by L & R, and, in particular, the resultative construction, thereby making the connection between these structures and their semantics relatively mysterious.

3.   CONCLUDING REMARKS: ON THE AUTONOMOUS SYNTAX THESIS

I want to end this reply to EB's paper by clarifying the meaning of the autonomous syntax thesis in current formal approaches to language, and in particular in Chomskyan linguistics, a thesis whose content has often been misinterpreted. Autonomous syntax does not mean a syntax which is totally unconnected to other components of the language faculty or other subsystems of the mind/brain. Thus, the fact that aspects of lexical meaning influence syntactic structures (even in standard GB theory, where Deep Structure is a representation of argument structure) cannot in itself be understood as a denial of the autonomous syntax thesis: syntax is as autonomous as ever in that there are syntactic principles and operations which cannot be derived from other (semantic / phonological) principles. These principles explain among other things, to mention an example which is relevant to unaccusative verbs, why in Spanish we can say things like *se abrió la puerta* with a postverbal subject, but in English **opened the door* is ungrammatical (the explanation being based on Case assignment in GB and the checking of strong [- Interpretable] features in the Minimalist Program).

Explanations in Chomsky's (1995) Minimalist Program emerge at the syntax-phonology and syntax-semantics interfaces, and computation is driven by formal features contained in the lexicon, but insofar as there are purely syntactic principles and operations, the autonomous syntax thesis is not undermined [16]. A different position, within the same research program, is adopted by Baker (1995), who points out that the minimalist conception could blur the distinction between formal and functional approaches which focus on the connections between syntax, cognition, lexical semantics and discourse pragmatics if the relations between lexical structures and L(ogical) F(orm) became entirely natural, i.e. isomorphic. The study of unaccusatives and other aspects of verb meaning indeed has «implications for the relationship of language to thought and beyond» (Baker 1995: 43). All of this is remi-

niscent of the debate concerning generative semantics in the 60s and 70s, but, fortunately, many of us are dealing with these issues from more open, more flexible, and more enriching positions than that currently adopted by EB. His approach, sadly, can only serve to widen the rift by insisting on attaching old labels to formal (generative) linguists and adopting attitudes which, for many of us, do not belong to linguistics in the 90s.

NOTES

[1] In this sense, claim (c) in EB's section 1 «(c) it is convenient for functional, descriptive models of grammar to introduce those types of verbs as substitutes for their analyses of clauses» reflects a misunderstanding of my paper. My argument is that the distinction between the two types of intransitive verbs (unaccusative and unergative) is useful for *any* descriptive and pedagogical grammar of English, whether the orientation of these grammars is formal or functional. In fact, the discussion centres on the approach to clause-types and verb-types in Greenbaum & Quirk (1990) as an example of a «functional» classification of clauses (see AM: 136-137). This (descriptive) grammar can, by no means, be considered «functional» if this term is used to characterize a particular approach to the study of language (i.e. the way EB uses this term, but not the way it is used in AM, as will be shown below). See Greenbaum (1988) on this matter.

[2] In academic contexts, statements like these are tantamount to accusations of dishonesty. In fact, such accusations against formal linguists are not absent in EB's paper (see his notes 2 and 6). Regarding the claim that Perlmutter's hypothesis has been annexed without acknowledgement in Chomsky (1981) and Burzio (1986), Burzio (1986: xiii) says «I must thank David Perlmutter for the single most important idea in this book, the one that he later termed the "Unaccusative Hypothesis"» (see also Chomsky 1981: 282, fn 35). Perlmutter's work on the Unaccusative Hypothesis is dutifully acknowledged in perhaps the two most influential book-length studies of the GB decade concerning the syntactic representation of verb arguments: Marantz (1984) and Baker (1988). It has to be said that while present-day American linguistics is often cavalier when it comes to giving credit for ideas, regrettably this defect is not exclusive of generative linguistics.

[3] The fact that these issues are addressed under section 2 «The functional classification of clauses and Vs» cannot justify EB's claims, since «functional» here is simply to be understood as an approach based on grammatical functions, as clearly specified in the Introduction of my paper and repeated throughout this reply.

[4] Similarly, I find the content of section 3 on how cognitive grammars classify verbs as both interesting and enlightening, though in no way a response to my own, since nowhere in the paper do I suggest that FGs take functions as primitives.

[5] The literature on this is vast but, to mention one of the most recent and significant works quoted extensively in both EB and my own paper, see Levin & Rappaport Hovav (1995) and references cited there.

[6] Actually, I used in my paper an earlier (manuscript) version of L & R's book (Levin & Rappaport 1992), since at the time I wrote it, L & R's book had yet to be published.

[7] I therefore fully agree with EB that classifying verbs in terms of syntactic properties may lead to «a rather anomalous piece of circular reasoning» (EB: 4.1). This is precisely my point about the definition of transitive/intransitive verb/clauses in descriptive grammars like Greenbaum & Quirk (1990).

[8] Standard GB analysis have concentrated purely on syntactic and morphological factors like those mentioned above (see Burzio 1986), and in that sense L & R cannot be taken to be representative of GB approaches, contrary to what EB seems to be suggesting.

[9] For instance, while much of the discussion is devoted to characterizing the difference between external and internal causation, there is no attempt at representing the difference in syntactic terms.

[10] As an example of an analysis which goes from lexical-semantics to syntax and morphology, see Zaenen (1993) on unaccusativity in Dutch.

[11] I am not discussing here section 4.3. on the discourse analysis of *there*-construction and locative inversion. In my opinion, discourse-based approaches like that adopted by L & R fail to distinguish what Milsark (1974) calls «inside» verbals from what he calls «outside» verbals in constructions with *there* and locative inversion. Distinguishing both types seems to me crucial to explain what locative inversion constructions are diagnostics for unaccusativity and what are not (see also Bresnan 1994). These issues are not addressed in my paper, though in the conclusion reference is made to the «presentative» function of *there* and it is pointed out that «there are aspects of *there* insertion which are closely related to discourse matters» (AM: 147); it is also suggested that *formal* and *functional* approaches are not incompatible when combined into descriptive or pedagogical grammars of English.

[12] Also ignoring the fact that, as said above, the whole section is devoted to characterizing how «unaccusativity is syntactically encoded» and not to offer a full description of the semantic (and syntactic) properties of unaccusatives with resultative phrases.

[13] In this sense, I cannot see what EB's Samoan example in (23) is meant to illustrate. EB's interpretation of the Samoan construction does not coincide with the translation offered, in which *faa-lelei* is clearly translated as a manner adverb and not as a resultative phrase. But this could simply be due to my own lack of understanding of the facts.

[14] See Davis (in press) for an adaptation of Pustejovsky's ideas to the analysis of unaccusative predicates in Salish and, from a different perspective, Hale & Keyser (1993; in press).

[15] Surprisingly, no reference is made of Van Valin's (1990) paper, which explicitly addresses these issues in a framework akin to that defended by EB (see EB's section 2.2 where Van Valin's Role-and-Reference Grammar is mentioned among functional/cognitive models).

[16] As Marantz (1995: 381) points out regarding the Minimalist Program «syntax...stands between interfaces and is neither a phonological nor a semantic component. And, as always, syntax trades in representations that are neither phonological nor semantic».

Departamento de Filología Inglesa
Facultad de Filosofía y Letras
Universidad Autónoma de Madrid
Ciudad Universitaria de Cantoblanco
28049 Madrid
e-mail: mendiko@ccuam3.sdi.uam.es

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