

## REVIEW ARTICLE AND DISCUSSION

### FOCUS IN FOCUS. REPLYING PROFESSOR SZWEDEK

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#### 1. Introduction

There are three main reasons why Professor Szwedek's review of my book *Aspects of the grammar of syntax. A minimalist view* calls for a detailed and principled reply. First, it aims at undermining my academic reputation by raising the accusation of improper academic practice in ignoring earlier research on the same topic. Second, it offers a long list of rather demeaning commentaries on my treatment of linguistic data. Third, it totally rejects and caustically repudiates the theoretical framework adopted in the book. These three are the major motifs of Szwedek's criticism throughout the whole article, though the list of sins I should confess to according to the reviewer is much longer. The overall impression of the review, despite its extremely selective approach, is that the whole book is unacceptable academic work, a conclusion deemed to be enhanced by occasional confrontation of my proposals with the reviewer's own illuminating findings from his previous work.

I do not agree with Prof. Szwedek's criticisms in any of the three respects mentioned above. I find it ungrounded, unprofessionally emotional and simply unfair – an instance of poor reviewing, for which I will present evidence in this rejoinder. Before I start, let me make one crucial confession straightaway; I do not think my book is perfect. I probably began realizing this the moment I typed the last word of the final draft. I am therefore open to any constructive criticism of the ideas I expressed in my book. Nevertheless, Prof. Szwedek's criticism misses the points I think may need some revision. What is more, it fails to relate to major theoretical proposals of my book. It seems as if the reviewer just browsed the book in search of straw men to elaborate on, without bothering to

read the whole chapters or even sections. Some of the offences so spotted find explanation or justification just a few pages before or later in the text. Such a scanning mode of reviewing the text probably explains why most of his reproaches are insubstantial rather than insightful.

The organization of this article is as follows. First, I briefly outline the functional and formal domains for the study of focus. I find it necessary to return to these well-understood points to put much of Prof. Szwedek's criticism in the right perspective. Next, I relate to the accusation of the omission of major functionalist work on focus, and then turn to the issue of the treatment of linguistic data in the book. Then, I discuss the reviewer's assessment of my analyses, and finally, I address the reviewer's points concerning the selection of the theoretical model. In conclusion, I briefly return to the issue of academic professionalism.

## 2. Approaches to focus. Sentence types and sentence tokens

Consider sentence (1) below, taken from the review:

1. I was *reading* the book.

A point of agreement for a linguist working within a generative paradigm (myself) and one adopting a functionalist framework (Prof. Szwedek) would be that the position of focus in (1) calls for some explanation. A functionalist could propose an explanation like the one cited in the review: the reason why the stress falls on *reading* is that "in the absence of a 'new' noun in the clause, the stress is assigned by default to other categories; in that case: a) the stress has to fall on an item as far towards the end of the clause as possible; b) the stress must not fall on a 'given' noun, if such is present, in which case it will fall either on the preceding lexical item, or on the following lexical item, if such is present" (Szwedek 1986: 86).

Why is such an explanation unsatisfactory (and uninteresting) from a formalist's point of view? Simply because a formalist would take example (1) to be a sentence *type*, not *token*, hence a de-contextualized syntactic construct, with a specific hierarchical structure that has its meaning derived solely on the basis of the form. What specifically disqualifies Szwedek's rule from a formalist's point of view is that it uses notions such as "new noun" and "given noun" when what is "new" or "given" can only be established in a context, not available in the formal description.

From a formalist's perspective, Szwedek's rule would not be a rule of grammar at all, but rather a description of language use. To define a noun as "new" means to relate it to a speaker who is to decide. What Szwedek's rule really tells us is that since a speaker does not intend to use any "new" noun in

sentence (1), but rather wants to make the verb “new”, which is a special situation, he places a sentence stress on the verb.<sup>1</sup> The grammar itself cannot decide where to place sentence stress; the speaker is indispensable to judge what is “new” and what is not. As Prof. Szwedek himself states: stress assignment is “governed by what the speaker wants to say in a given context”.

This sort of explanation of the placement of sentence stress on *reading* in (1) (Szwedek’s (1986)) would, from a generative perspective, merely be “descriptively adequate” in the classical three-grade measure of the adequacy of grammars.<sup>2</sup> It is certainly possible to stop linguistic investigation at the point of deriving such a descriptive generalization.<sup>3</sup> However, an attempt to promote it to an explanatory level must fail because the explanation it provides is circular: Why is the sentence stress on *reading* in (1)? Because the noun in (1) is “given” and *reading* is an item which precedes it.<sup>4</sup> How do we know that the noun in (1) is “given”? Because it does not carry a sentence stress (or because the speaker intends it to be “given” and hence does not mark it with sentence stress).

Why should (1) still be of interest from a formalist’s perspective? The task of generative research, which is, roughly, to reveal autonomous grammar-internal principles of stress placement in sentence type (1), begins where it stops for a functional approach. A particular sentence stress is considered to be one aspect of sentence form. In other words, one of the possible sentence types is (1), in which a sentence stress falls on *reading* and not on the final constituent, which would be an unmarked case. It would be a formalist’s job to explain what grammar-internal mechanisms are responsible for the “switch” of sentence stress from a sentence final to some other position, e.g. to the verb *reading*.

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<sup>1</sup> Szwedek’s rules 1 and 2 are to account for the “principles of sentence stress assignment in neutral utterances” but then he claims that in sentence (1) (Szwedek’s [54]) “... the stress falls on the verb preceding the ‘given’ noun ... and the clause will also result in a corrective interpretation, as predicted by Rule 2”. The confusion is even greater if one reads earlier that: “for contrastive/corrective stress there are no rules”.

<sup>2</sup> The three levels of adequacy that grammars may reach are: observational, descriptive and explanatory. Thus, a grammar reaches the lowest level of success if it “presents the observed primary linguistic data” (Chomsky 1962: 62). A grammar is *descriptively adequate* “to the extent that it correctly describes the intrinsic competence of the idealized native speaker ... in a substantial and significant class of crucial cases” (Chomsky 1965: 24). Finally, a theory meets the condition of *explanatory adequacy* if it “succeeds in selecting a descriptively adequate grammar on the basis of primary linguistic data [i.e. *the information available to the child in the process of language acquisition*]” (Chomsky 1965: 25).

<sup>3</sup> There remains a question of its precision, though. Can a neutral utterance distinguish between “new” and “given” elements? If a neutral utterance is “all new”, why cannot a rule just state: sentence stress should fall on a noun, which by definition would be “new”?

<sup>4</sup> Another problem with the application of Szwedek’s rule is that it does not explain why sentence stress must fall on *reading* and not on *was* or *I*, which also precede the “given” noun.

Some mechanisms would also have to be discovered for the placement of sentence stress in the unmarked, i.e. final position, but also for its possible placement on *I* or *was*.

The reason why in formalists' accounts (like mine) there is still a place for reference to contexts in which sentences are uttered and interpreted is that such descriptions are informative in telling us what sentence types the grammar must provide for various communicative needs.<sup>5</sup> For example, before determining a formal rule for a non-neutral stress placement rule applicable in (1), it makes sense to consider if a sentence-type (1) meets any special communicative need. The description of the interpretation that a sentence-type (1) may have when used in a given context is nothing more than the statement of the empirical fact. In particular, such a description of the interpretation with the elements of the context is not treated as explanation, any scientific finding, revelation, theory, account or discovery, but as just plain fact or empirical observation, an issue to which I return.

The purpose of the above brief characterization of the domains of formal and functionalist approaches to focus, which may be generalized to a host of other linguistic phenomena, is to highlight a major methodological chasm between the two approaches.<sup>6</sup> To put it bluntly, what constitutes a functional explanation is for a generativist just description of the linguistic facts with the use of some meta-language (e.g. with reference to categories such as sentence stress, "given", focus, etc.), while explanation of the rules (principles) of grammar is yet to be provided.<sup>7</sup> I believe that clarification of these points is particularly revealing for the assessment of two major aspects of Prof. Szwedek's critique; the omission of the legacy of functional linguistics, and the treatment of linguistic data in my book.

Finally, since the fact of the "division of labour" between functional and formal approaches to syntax has been rather generally well-understood and agreed on by the linguists from the two camps for more than two decades (see e.g. Newmeyer 1986, 1995; Kuno 1987; Lambrecht 1994; Jackendoff 2002), I must admit I was genuinely surprised by Prof. Szwedek's belated attack on the postulate of the autonomy of grammar. The impression can be that of "flogging a dead horse" or starting a new episode in the "linguistic war" which everybody thought had long ago ended.

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<sup>5</sup> This certainly is related to the "interface condition" perspective adopted for the explanation of the linguistic facts fundamental in my book and totally ignored by the reviewer.

<sup>6</sup> I still believe that the foundation for the chasm may be found in the classical competence-performance dichotomy.

<sup>7</sup> What I am saying here applies to a variety of approaches representing, or stemming from, the Prague School tradition. It does not extend to the endeavors of Cognitive Linguistics.

### 3. Why I do not confess a sin of omission

#### 3.1. Prague School tradition and Professor Szwedek's work

The reviewer comments on the title of my book, saying it is on focus rather than on minimalism, because minimalism is mentioned only in a subtitle while the word *focus* appears in the title. I find this sort of implication bizarre. Surely, it is not a book about minimalism, but a book adopting a minimalist framework to account for focus, and I genuinely wonder how one could interpret it differently. Furthermore, it seems obvious that adopting a given theoretical framework does not imply adopting some other theoretical framework at the same time, unless otherwise stated. A reader of my book is most explicitly informed of my choice of methodology on the second page of the introduction (cf. Tajsner 2008: 12), and for those who would not intend to read it, I added the subtitle.

The reviewer says that the omission of reference to his work is “particularly perplexing” to him because we earlier “listened to each other’s presentations on focus and word order” and I cannot claim ignorance of the reviewer’s views and works, just as I “cannot claim ignorance of other research, for example, by the Prague School”. That is right; I surely do not intend to claim the ignorance of either. As for the earlier exchanges between us, I particularly remember one at a conference in Turawa in 1986. What I remember best from this debate is that Professor Szwedek quite fiercely expressed his lack of interest in the type of syntactic analysis undertaken in Generative Grammar. Such a firm, strictly functionalist position has been confirmed in all the reviewer’s earlier and subsequent written work. The topic of my book is the grammatical basis of focus, where grammar is conceived narrowly to methodologically exclude any functional explanation and in which “any reference to speakers’ intentions in justifying linguistic form is avoided” (Tajsner 2008: 12). This means that the domain of functionalist interest in the study of focus has been explicitly and purposefully excluded from the scope of my book. My decision not to include reference to the works by functionalist linguists (Prof. Szwedek’s work included), whose descriptions are essentially dependent on functional explanation and in which the analyses of speakers’ intentions are commonplace is thus only most natural.<sup>8</sup>

I have much respect for Prague School’s linguistics, as well as for Halliday’s London School and the whole tradition of Functional Sentence Perspective. I have expressed it openly in my earlier publications (Tajsner 1998, 1999) in

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<sup>8</sup> I found a natural place for mentioning and citing Prof. Szwedek’s views and work in the context of my earlier book *Minimalism and functional thematization* (Tajsner 1998). In the first chapter of this book, entitled *functional perspective and formal grammar* I compare the methodologies adopted for the analysis of word order variation by Szwedek (1976a) and Willim (1989).

which I speak in favour of the “division of labour” and cooperation between the followers of the “two syntactic camps”. There, I also argue for the complementarity of the two types of study: “Neither of the two approaches fully characterizes the properties of a broadly defined “syntactic structure”. Some of its properties may be carefully examined and determined within a formal approach, but nonchalantly disregarded by the functional one. Another set of principles may be significant from a functional point of view, but entirely disregarded from the formal perspective” (Tajsner 1998: 15). But, my autonomous decision not to return to the results of functional research within a narrower scope of the book under review is fully justified.

It is clear that more extensive reference to the Prague School would be necessary and substantial in three types of studies on focus; historical, comparative and polemical. Thus, if my book was designed to include (a) a survey of the views on focus in different linguistic traditions, (b) a part devoted to the comparison of function-based and form-based approaches to focus, or (c) a polemical discussion on the primacy of the latter with respect to the former, in all these instances some extensive analysis of the Prague School’s view of focus would be unavoidable. It just so happens that my book has been designed not to raise any of these issues, and instead it has been designed to have its scope limited to just one theoretical framework, i.e. Generative Grammar.

It is not true that, as Prof. Szwedek writes, “Of many scholars of the Prague School for whom focus was the main theme in a span of 40 years of research, only Mathesius and Sgall are mentioned.” The reviewer has overlooked note 87 on page 70 where I write: “What we refer here is the whole tradition of functionalist linguistics following Prague and London Schools with many modifications and incarnations (Halliday 1974, Daneš 1974, Mathesius 1975, Sgall 1975, Givón 1984, Lambrecht 1994)”. More importantly, however, it seems that the reviewer has overlooked the discussion in the context of which I make the above note. In subchapter 1.15. I raise the issue of the theoretical scope of two related notions: *rheme* and *focus* (in its two varieties; information and identification): “A perspective in which the rheme – focus distinction should be seen more broadly is that of the opposition between competence and performance studies. Typically, the division of utterances into *rhematic* and *thematic* parts has been the concern of performance theories, as in the framework of functional sentence perspective or theory of information structure. Such studies do not partition the linguistic reality into the performance and competence spheres, and argue against a concept of sentence abstracted from its use” (Tajsner 2008: 70).

There is one more important reason why I believe that I was not obliged to pay special tribute to the Prague School’s studies in the context of my work. I think of focus as a linguistic phenomenon, not a theoretical construct. It is just like other linguistic phenomena, e.g. null-subject, anaphoric binding, or control

in that it may be described from the variety of viewpoints, and no linguistic tradition should monopolize it. I do not include in my work specific reference to the studies on focus performed within other grammatical paradigms such as, e.g. Role and Reference Grammar (Van Vallin – La Polla 1997; Van Valin 1999), Functional Grammar (Dik 1997), Lexical Functional Grammar (Vallduvi – Engdahl 1996), or HPSG (Pollard – Sag 1987), and by the same token, I did not feel obliged to extend the discussion of the treatment of the phenomenon by the Prague School linguists.

What is more, contrary to what the reviewer implies, the origin of the very notion of focus is not to be sought in the Prague School tradition. Focus was first more explicitly referred to by Halliday (1967), and then it was Chomsky who in his article “Deep structure, surface structure, and semantic interpretation”, which appeared in 1968 and was published in 1972, defined focus in opposition to *presupposition* and outlined its prosodic and semantic dimensions: “The focus is a phrase containing the intonation center; the presupposition, an expression derived by replacing the focus by a variable” Chomsky (1972: 100). It is the linguistic tradition based on formal categorization and not on pragmatic inference to which I subscribe in my book. Evidence for this is found throughout the whole text, most evidently in the above mentioned subchapter 1.15., where I explicitly argue for distinguishing the notion of *rheme*, stemming from Prague School tradition from the notion of focus. It is thus inadequate to say that for Prague School scholars “focus was the main theme in a span of 40 years of research”. I am far from being scrupulous about terminology but it would be more appropriate to apply this statement to *rheme*, *new information* and *comment* rather than to focus.

Finally, Prof. Szwedek’s accusation of the omission of the works of Prague School linguists aims at giving the impression that such an omission is something uncommon, exceptional, an instance of academic neglect. The reality is, however, different. Here is just a selected list of recent (“first league”) generative books on the issues related to information structure. In none of these can one find more than little, if any at all, mention of Prague School linguistics: Erteschik-Shir (1997), Steedman (1996), Zubizarreta (1998), Winkler (2005), Sabel and Saito (2005), Molnar and Winkler (2006), Reinhart (2006), Green (2007), López (2009), Zimmerman and Féry (2010). What Prof. Szwedek impugns is thus common practice, which I find well-motivated.

### 3.2. Examples and interpretations from earlier research

A different aspect of the “sin of omission” raised by Prof. Szwedek is his accusation of my repeating “examples and interpretations from earlier research without citing them”. It sounds like a very serious reproach, which I cannot

accept. I have not used other authors' examples; all examples are mine, unless otherwise explicitly stated. Before I undertake a detailed analysis of particular examples, let me comment on this accusation in a more general sense. As already noted, the role of linguistic examples in generative work is different than in functionalist works. They are not supposed to provide explanation, but illustration of the independent workings of the grammar. Thus, e.g. a provision of a context for sentence (1) above appears indispensable for its explanation in functionalist terms: what is a "new noun" can only be determined in a context. No such dependence on the interpretation in a context is needed in the generative domain: my explanation of the reason why sentence stress falls on *reading* is that the grammar provides formal means for the avoidance of default stress placement, by so called Nuclear Stress Rule on the most embedded (first merged) item in the structure underlying a sentence type (1). A sentence making use of such a formal device is used in a specific context and with a specific interpretation in which *the book* is understood as "given".

Moreover, the instances which Prof. Szwedek finds derived from older research are very simple illustrations of the basic facts about how people interpret sentences with different positions of sentence stress. I find these interpretations easily accessible to any more literate user of language, not to mention professional linguists. They are not dependent on any theory of language, just common sense and simple linguistic reasoning. Consider a parallel from a study on the use of morphological case in Polish. A new, theoretical proposal in this respect would have to be preceded by simple illustrations of case use with particular grammatical functions, e.g. examples like; *Chłopiec spotkał dziewczynkę* ('A boy met a girl') or *Tomek dał Marysi jabłko* ('Tom gave Mary an apple'). Would the use of such simple examples be repetitive with respect to hundreds of earlier accounts of morphological case in Polish using similar data? Would enumeration of these earlier works and their authors be expected even if their descriptions of such simple facts were based on completely different methodologies? Such a requirement seems very odd.

Prof. Szwedek argues that in a discussion of the scope of focus on page 254 I repeat "what Chomsky (1971) and many other linguists have written, without acknowledging the source" and thus create "the impression that the analysis is the result of (my) own original research". Had the reviewer read (and not just browsed) the previous parts of the book he would have found numerous reference to earlier works in which broad and narrow scope of focus was postulated: e.g. page 82 (reference to Zubizarreta 1998, and Cinque 1991), page 92 (reference to Reinhart 2006), page 208 (reference to Erteschik-Shir 2006), page 228-229 (reference to Irurtzun 2006 and Kiss 1998 [note 40]) etc. In chapter five, in which I illustrate the narrow – broad scope distinction in Polish with the examples questioned by the reviewer, I first enumerate the major topics discussed in



previous chapters (surveying other authors' proposals) and then I explicitly state: "in the present chapter we draw extensively from this reservoir of valuable ideas in providing our own proposal of the account of focus in English" (Tajsner 2008: 254). It really escapes me how the reviewer could not deduce from these words that the observed contrast is one of these "valuable ideas". The whole section 5.2. (which is three pages long) containing what the reviewer calls "the analysis", is nothing more than the setting of the simple empirical ground for my proposals in the following pages. A careful reader should have absolutely no doubt whatsoever that I do not claim the authorship of the discovery of the contrast in question. What is more, in a partial summary (section 5.6.) in which I enumerate the original proposals I make in the preceding sections; there is no mention of this contrast in the scope of focus.

The reviewer objects to the statement I make in a footnote on page 51 that "Reinhart (2006) convincingly argues that there is nothing like absolute stress" and suggests that to avoid the shaft of his critique I should have rather used the phrase "Reinhart follows traditional description of stress".<sup>9</sup> Thus, the reviewer seems to assume that the view on stress akin to the metrical phonology approach formalized by Liberman and Prince (1976) is the only conceivable one, and, what is more, is the only "traditional description of stress". In other words, theirs is the absolute truth and the state of the matter for which it does not even make sense to argue, however convincingly. In fact, the term "absolute stress" I use here may be linked to the notion of the absolute integer values of stress like [1 stress], [0 stress] proposed by Chomsky and Halle (1968) in their SPE system (certainly, part of tradition!). Besides, my further comment in the same footnote, which the reviewer ignores, explains why I make any reference to relational stress. Part of my proposal is that emphatic (contrastive) stress may (exceptionally) be an outcome of the underlying phonological feature [Emphasis], typical for the use of words in isolation, and probably also for other inherently emphatic forms.<sup>10</sup> The postulation of such a feature may be viewed as controversial given the lack of "absolute stress", though I argue that "the relational status of main stress is only evident for its phonetic value, while its conceptual sense may be non-relational" (Tajsner 2008: 51 n. 56).

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<sup>9</sup> Reinhart argues against absolute stress in a specific context of considering the option of introducing the feature [focus] to code main sentence stress in abstraction of other stresses in a context. The argument thus holds against some proposals that one feature [focus] can code both prosody and interpretation (e.g. Erteschik-Shir 1997).

<sup>10</sup> The candidates for such items, apart from citation forms, could be e.g. English contrastive auxiliaries (e.g. like in *I DID turn left!*), or Polish emphatic pronouns e.g. *jego* (like in *JEGO widziałem w kinie*) as opposed to inherently unstressed pronominal clitics e.g. *go* (in *Widziałem go w kinie*). The latter examples are not mentioned in the book, though.

Next, the reviewer finds problematic my remark that “there is no lexical semantic feature [new information], what is new may only be established in a context”. It is not that there is anything wrong with the idea I state; the reviewer objects to the fact I do not refer to Lanin (1977), Pakosz (1981), the Prague School or Prof. Szwedek’s work on the occasion of saying this. This misses the point that the statement here is part of the argumentation for the non-grammatical nature of information focus (in opposition to both non-exhaustive identification and exhaustive identification focus). My point is that information focus is not coded formally, unlike the other two above, and that in particular a lexical feature [new information] is not a good candidate for such a coding. To the best of my knowledge, none of the four sources mentioned by the reviewer have presented the issue in a similar perspective.

Another good example of the reviewer’s careless reading of my text may be the objection he raises to my statement on page 54 the crux of which is that “any type of phrase, whether equipped with a lexical marker of definiteness/specificity or devoid of such a marker may be fronted”. In his comment Prof. Szwedek implies the assertion is obvious (in view of what e.g. Mathesius 1939 said) and he wonders who I am defending the thesis against. If the reviewer had chosen to read the whole subchapter he would know; I am defending the thesis against a grOxford University Press of generative authors, e.g. Holmberg (1999), Zwart (1993), Miyagawa (1997), Richards (2001), Bailyn (2003), or Kiss (2003), who advocate accounts based on a recurring prediction “that the grammar formulates a formal mechanism which is used by the semantic component for specific/definite interpretation of the moved constituent” (Tajsner 2008: 52).<sup>11</sup>

What the reviewer calls an example of a “cavalier attitude” on my part is my statement: “We assume that sentence prosody may be marked or unmarked with respect to focus. The unmarked instance is a case in which no special focus intonation is used in the sentence. ... By contrast, a marked focus intonation will be a sentence melody in which the placement of a pitch accent is unpredictable from the general rule” (Tajsner 2008: 58). What the reviewer skipped in this quotation by using the mark “...” is the major point I am making in the passage: the unmarked prosody is related to the non-parameterized version of the Nuclear Stress Rule proposed by Cinque (1993). None of the authors mentioned by the reviewer make an analogous prediction. Why my interpretation deserves being called “cavalier attitude” remains a mystery to me. Besides, I have two comments relating to this point: (i) it appears I do not share with the reviewer the semantic understanding of the English verb *assume*. For me, *as-*

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<sup>11</sup> My views on “sentence initial position” and its relation to the traditional view of theme (e.g. Halliday’s 1976) are expressed broadly in Tajsner (1998).

*sume* means to ‘take as true’ and does not mean that I am saying something new. If I wanted to say these findings were mine, I would have probably used *propose*, (ii) Secondly, by slicing a paragraph into two parts and dropping its most important part, it is possible to shape any quotation according to one’s needs.

I was particularly puzzled by Prof. Szwedek’s comments on my allusion to Reinhart’s (2006) example [22]. First, the reviewer criticizes me for not mentioning Chomsky’s and Akmajian and Jackendoff’s earlier uses of similar examples, and then proceeds in details with an analysis of such examples (involving various types of cross-reference) presented in his own works (Szwedek 1976, 1980). I do not understand why he does so, given that Reinhart’s (2006) analysis is concerned with entirely different matters. I think it is made absolutely clear in my text that Reinhart discusses the distribution of stresses in examples using the formal mechanisms she provides in her system: NSR, anaphoric de-stressing and stress shift. To this, I added a critical comment about the way in which the pronoun *she* is made anaphoric in her example (by D-linking, i.e. discourse-linking, to *Sue*). There is no place in the context of my discussion of these two points for a consideration of the possible or impossible co-reference of the noun phrases used to which the reviewer could contribute his own analyses. It is not that such considerations would be uninteresting, but they simply are irrelevant to the discussion. In my view, the reviewer’s comments at this place prove that he did not care to read deeper into the text reviewed.

Another instance of alleged omission criticized in the review is my reference to the use of *even*, *only* and *too* in focus sentences, which I call “focus sensitive particles” without citing earlier work on the role of these particles by such authors as Horn (1969), Fraser (1971), Anderson (1972), Grochowski (1986), Jackendoff (1972) and Szwedek (1986). Let me explain that, like elsewhere in the book I have limited my interest here to generative accounts. Besides, I do not offer my own proposals on the syntax of *only* and other particles, and hence I limit the discussion to a brief reference to the views of other authors. It is not true, however, that the only source I mention is Rooth (1992a). It may be easily checked on page 243 that I also refer to Kiss (1998), Brody (1990), Chomsky (1976) and Drubig (1994). These all are proposals in which the interpretive role of the particles in question is related to the structural properties of focus sentences.<sup>12</sup>

At another point, the reviewer protests against my account of the Polish example *Dźwig uszkodził spychacz*, where I note that given the morphological isomorphism between nominative and accusative case, the sentence should be

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<sup>12</sup> Contrary to the reviewer, I find Rooth’s (1992a) interpretation of the role of *only* to be original and new because it is related to the framework of alternative semantics for focus she develops.

ambiguous, which it is not. This shows there is preference for SVO order in Polish. What I say is obviously true, but, according to Prof. Szwedek, I should have mentioned that similar sentences, e.g. *Ciełę widzi kurczę*, have been discussed before (by Świeczkowski 1969 and Szwedek 1976). For me it is obvious that such sentences must have been discussed hundreds of times before. Analogous examples cannot be missed in any fuller description of Polish word order patterns, but I am sure that here no reference to “previous research” on the issue has to be made. Such a simple observation can even be made by a “naïve” native speaker of Polish after a middle-school course in grammar.

By the way, I comment on the interpretation of *Dźwig uszkodził spychacz* in various contexts in order to advance a hypothesis of a formal account: “For the leftmost DP in (5) (*Dźwig uszkodził **spychacz***) to be an object, the grammar must provide some way of moving the object to a front position. One way of fronting a DP is by a form of topicalization which is triggered by a need to derive a new interpretive effect. (...) We will assume after Chomsky (1999b, 2001) that the trigger is the occurrence of the EPP feature” (Tajsner 2008: 326). This is a key aspect of the discussion which the reviewer completely ignores. As standard in his review, Prof. Szwedek criticizes me on a selected point without paying any attention at all to a wider context in which the point is made. As a result, he does not come any closer to the main point but prefers to “beat about the bush”.

When commenting on a set of examples I provide to illustrate the variations of word order in Polish the reviewer recalls his own research in which he also discussed all possible word order configurations in Polish. Even though it is not made explicit, Prof. Szwedek seems to imply that the striking similarities between my examples and his (compare, for example, my *Ani dał Marek **książkę*** with his *Mężczyzna dał chłopcu **książkę***) are not accidental. I leave it for the reader to judge whether it could it not be by mere coincidence that the two authors independently thought of using the most prototypical ditransitive verb *give* and one of the most common nouns (especially within an academic setting) – namely, *book*. Let me only add that nothing hinges on this similarity. My examples are there to illustrate grammar internal processes (such as raising to Spec.TP, the satisfaction of the EPP, or Spell Out) operative in their derivation. It is standard practice in generative analyses to first provide examples of sentences and then to discuss their structure and derivation. The instances with variations of word order (in which stress is uniformly sentence-final) perform exactly this function. It only takes a little good will and some literacy in recent minimalist syntax to see that the accounts of such examples offered by Szwedek and myself are totally different.<sup>13</sup>

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<sup>13</sup> Let me make it clear. I cannot believe that the simple facts of the possible variation of word order in Polish, as well as the description of possible differences in interpretation such a

### 3. Data and interpretations

Professor Szwedek formulates all sorts of objections to the linguistic data I use in my book. Despite generalizing statements like “sometimes Tajsner uses linguistically unacceptable structures or interprets them wrongly (Sauren’s ‘no respect for data’ )” or comments such as “some are in incorrect English, many interpretations are questionable”, nevertheless of the totality of over 400 hundred original linguistic examples I use in my book, he questions the correctness or interpretation of just six examples.<sup>14</sup> It is based on these six examples that the reviewer makes a sweeping judgment that I have no respect for data. I am not sure how to interpret such reasoning. I understand that the review has to be limited to the discussion of selected points, but in view of such slashing generalizations about my use of examples (“no respect for data”), I would at least expect a longer enumeration of the number of questionable examples. Given the lack of such evidence, I again have the impression that the reviewer treated my text undiligently.

In the concluding part of the review he additionally blames me for using invented examples (“almost all his examples are invented”). I understand that Prof. Szwedek’s examples, either in his own earlier work or in the review, have not been invented by the author, but taken from some corpus of utterances, literary works or, maybe, TV shows. This probably also applies to examples like: *Nie. Książkę czytałem, a pisałem wiersz*, or *I saw a cat under the table* (Szwedek 1975b: 14). If so, it is a pity that the original reviewer’s texts do not identify the sources of these “live” examples.

The reviewer objects to my interpretation of the example *Jack brought flowers to Jill*, as a possible answer to the question *What’s up?* He states that if “Jack and Jill are known from the universe of discourse (which is my assumption), they are not new information items, and so the stress cannot fall on either of them”. However, it seems relatively easy to envisage a context in which the parameters I mention are true. Imagine a situation in an office where both *Jack* and *Jill* work. The boss comes to the office in the morning and asks in a friendly manner: *What’s up?* When asking the question (an instance of an “out-of-the-blue question”), the boss has absolutely no idea what to expect. The message: *(Nothing special) Jack brought flowers to Jill* passed on to the boss by the secretaries may be understood as “all new”, because even though *Jack* and *Jill* are known from the “universe of discourse” (i.e. the office), they are not part of old

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variation invokes can still be considered major findings, analyses or research results. They are just well understood and widely attested empirical facts, truly elements of linguistic “common knowledge”.

<sup>14</sup> This calculation is only of my own examples excluding those I quote after other authors. The number does not include bracketed structures of sentences.

information in the sense of having been present in the immediately preceding context (there has not been any relevant preceding context for this expression). In particular, despite their presence in the general “universe of discourse” and hence not being “new” as possible referents, they have not been identified specifically as the agent and recipient of the action of bringing flowers. I am not claiming here that *Jack* or *Jill* are new in any ontological sense; I am only saying that the whole message (representing a specific argument structure) is interpreted as new.<sup>15</sup>

Let me add that the example I use is a sentence type, not a sentence token, as earlier noted. My point in the description of its interpretation is that the new information such a sentence type carries may extend to the whole clause, its verb phrase, or the prepositional phrase only (which are well known facts from earlier research, not my own findings). In the first case, all parts of the message are new: that it was *Jack* who did the action, that the action was bringing flowers, and that it was *Jill* to whom the flowers were brought. A sentence of a similar kind would be *A boy brought flowers to a girl*. What I want to determine is the technology of stress placement in various sentence positions. Thus, I argue (following Cinque 1993, and others) that for both *Jack brought flowers to Jill* and *A boy brought flowers to a girl* the same strategy is used of placing sentence stress by the application of plain Nuclear Stress Rule to the most embedded item in the sentence’s structure. When the grammar applies the rule, it does so “blindly” without any respect to such context-dependent categories as “givenness”, “newness”, co-“reference”, topicality, etc.<sup>16</sup> It will now be a task of the extra-grammatical interpretive component to interpret such sentences. It is also possible that not all sentences so derived may be successfully interpreted. Some outcomes of the application of the grammatical technology may thus turn out as “weird” or “gibberish”. However, I am arguing that sentence type *Jack brought flowers to Jill* can be interpreted as “all new” or “all focus”, given the right context - a possibility that the reviewer excludes.

The reviewer raises an almost identical case one more time when referring to my example *Jack saw the movie*. He is completely wrong claiming that this

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<sup>15</sup> The controversy at stake is evidently that of “text boundness” and “sentence boundness” for defining what is “given”. For some authors “given” refers only to elements mentioned in the previous context; for others, for example Dahl (1974), “given” is defined generally as “a certain picture or model of the world which the hearer has in his/her mind” (Dahl 1974: 38). I am closer to the former, not the latter interpretation, hence I do not think *Jill* and *Jack* in the sentence under investigation are “given” elements. I am far from trying to settle this (largely terminological) controversy, though.

<sup>16</sup> It also turns out to be blind with respect to the markers of “definiteness” vs. “indefiniteness”, i.e. the use of definite vs. indefinite articles. If this were not so, no “unique reference” examples like *they looked at the sky* could ever be interpreted as “all new”. This is obvious given that the grammar itself cannot determine which elements have “unique reference”.

sentence can only be “contrastive or corrective”. I am surprised that he is apparently unaware of obvious contextual possibilities. For example, imagine that *Jill* is *Jack’s* fiancée and there is a private movie in which *Jill* kisses another man. *Jill’s* colleagues know there is such a movie, but *Jack* hasn’t so far. Then somebody asks: *What’s happened? Why is Jill crying?* Somebody else explains: *Jack saw the movie*. The sentence fits the context perfectly, it is “all new”, and the sentence stress falls on *movie*. Likewise, to paraphrase Prof. Szwedek’s own example, the sentence *She saw The Terminator* fits perfectly in the context of *What’s happened? Why is she crying?* It is strange that the reviewer, who otherwise believes so strongly in the power of the context, misses such obvious possibilities.

Generally, a noun phrase in a sentence final position (most deeply embedded), which is assigned sentence stress “by default” may be interpreted as part of new information (i.e. part of an “all new” message), regardless of the grammatical exponents of definiteness. In other words, not all definite noun phrases and proper names in sentence-final positions must automatically be given the status of old information.<sup>17</sup> I evidently share this view with many other generative authors dealing with similar matters. For example, Erteschik-Shir (2006) compares two examples: [he ATE the CAKE]<sub>loc</sub> and he ate [the CAKE]<sub>loc</sub> noting that “The stress on the verb is reduced when the speech-rate is increased, yet a distinction can be easily detected between the pronunciation of [these two examples]. This distinction has been ignored since Chomsky (1971), where it was claimed that only the final constituent of the focus is stressed. That this is not the case can be shown even more perceptibly in the following all-focus sentence: Q: Why are you so upset? A: [the CHILD ATE the CANDY]<sub>loc</sub>. Here again, in faster speech, the stresses on the subject and the verb are reduced...” (Erteschik-Shir 2006: 8). Likewise, Zubizarreta (1998) ascertains that example *John ate the pie* (notation for main stress mine) “(with main prominence on *pie*) is compatible with F-structures” a. [<sub>F</sub> John [ate [the pie]]], [What happened?], b. [John [<sub>F</sub>ate [the pie]]], [What did John do?], c. [John [ate [<sub>F</sub> the pie]]], [What did John eat?]. Adger’s analogous example to illustrate sentence final stress under neutral intonation is: *The baby ate the gloop* (Adger 2007: 8); one of Reinhart’s examples to illustrate the same pattern is simply: *I read the book*, and Lopez (2009) has the following: [<sub>F1</sub> Mary [<sub>F2</sub> greeted [<sub>F3</sub> Jane ]]], and so on.

As for the native speakers’ responses, I am not sure about the methodology applied in the questionnaire annexed to the review. If the respondents were just confronted with the written description of the short dialogue in question (like

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<sup>17</sup> Szwedek (1975) notes the fact but restricts it only to definite NPs used in adverbials like in “I am going to the cinema”, or to the instances of nouns marking “unique” reference like “I looked at the sky” (Szwedek 1975: 16-17).

the one in the appendix), with perhaps an additional instruction that bold type means emphasis, no wonder that the answers were as they were.<sup>18</sup> I asked a few native speakers a different question: Can a sentence *Jack brought flowers to Jill* uttered with a neutral intonation and with sentence-final stress, be a felicitous answer to the question *What's up?*, asked by the boss in the above described "office situation". The responses I gathered differ significantly from those cited by Prof. Szwedek: all the seven respondents gave me a plain *yes* response to such a question.

Next, the reviewer questions my distinction between information focus and identification focus. The reviewer would certainly have had every right to dispute me on this point only if he had not tried to ascribe to me views which I never expressed in the text. Although it is true I state that "information focus and identification focus exclude each other", I never stated that the sentence *Jack brought flowers to Jill* does not identify *Jill*, nor that in *Jack brought flowers to Jill* the word *flowers* is not new information. The reviewer goes on to ascribe to me the following views: "Thus according to Tajsner, when we use *Jack brought flowers to Jill*, as an answer to *What's up?* it carries new information, but does not identify anything; neither the event, the subject, the verb nor the object." The reviewer has not found such statements in my text, hence he decided to make them up and put them to my mouth. Let me then repeat what I state explicitly in my text. With reference to example *Jack brought flowers to Jill*, I refer to the role of focus in this sentence "identificational, in that it specifically identifies a value of variable *x* (something) of a proposition: *John brought x (something) to Jill*" (Tajsner 2008: 254). Thus, if sentence *Jack brought flowers to Jill* does not perform a "completive function, as a response to a specific wh-question" (Tajsner 2008: 255), i.e. if "the context is unrestricted, the whole utterance constitutes pure information focus" (Tajsner 2008: 253). What I propose is thus that the use of *Jack brought flowers to Jill* provides new information when "neither specifically anticipated nor provoked by the formulation of a wh-question" (Tajsner 2008: 256). In particular, I argue that when used in such a context, the main sentence stress on *Jill* does not specifically identify a value of variable *x* (somebody) of a proposition *Jill brought flowers to x (somebody)*.<sup>19</sup>

The purpose of the distinction I draw is thus to define more precisely the two uses of main sentence stress. If the reviewer does not agree with such a formal partition I am waiting for the arguments against it. I cannot accept, however, a

<sup>18</sup> That such an additional instruction could have been given may be deduced from the answer given by the respondent B: "the emphasis on *Jill* suggests that *Jack brought flowers to her* and not to someone else."

<sup>19</sup> I apologize for the misprint in the word *proposition*, which appeared in the text wrongly as *preposition*.



pseudo-argument based on a supposition that I assume that identification is not providing any new information or that part of new information cannot be identifying. Within my proposal there is a difference between the concept of new information and the category of information focus which I define explicitly. Likewise, I differentiate the idea of (general) identification and the category of identification focus for which I provide the necessary formalization. The reviewer completely misses these distinctions. Certainly, identifying (also identifying specifically) is adding some new information in a general sense, just like providing new information may contain an element of identification.<sup>20</sup> It would be difficult to formalize such general intuitions and I am far from plunging into such speculative areas. All I did was introduce some technical and terminological partitioning to lay grounds for subsequent theoretical proposals.<sup>21</sup>

The second questionnaire which Prof. Szwedek provides concerns native speakers' judgments on the appropriateness of the use of my example *Jack brought flowers to Jill* as an answer to *What did Jack do to Jill?* The native speakers point out that such an exchange is "very unnatural" ("it does not seem the response fits the question") mainly because "He didn't do anything to Jill" or "the phrasing ... suggests some sort of violent act, physical or mental". The reviewer makes two points; first the sentence "is unacceptable in the meaning intended because it means that he brought flowers **to** Jill, but not necessarily **for** Jill" and then, "(it) is totally unacceptable in the context of *What did Jack do to Jill?*". The right responses would be e.g. *John spanked/hurt/offended Jill*. As for the first reproach, I must admit I cannot get the reviewer's point; bringing flowers *for Jill* and bringing flowers *to Jill* may be just two (slightly) different events, and I do not understand how the reviewer knows I intend the former, not the latter.<sup>22</sup>

The second qualm can be answered if the context is slightly enriched: A: *What did Jack do to Jill?* B: *Nothing. He (just) brought flowers to Jill*. My point is this. On asking the question A does not know what exactly might have happened between *Jack* and *Jill*. Nothing is excluded at this stage, even some "sort of violent

<sup>20</sup> On pages 294-295 I argue, however, that the co-occurrence of wh-movement with identification focus is excluded, thus, e.g. in *Whom did JACK/Jack\* see in the pub?* *Jack* can only be contrasted with other alternative persons (exhaustive identification) and cannot be plainly (non-exhaustively) identified. Thus, I argue *Jack* cannot be here part of new information in the technical sense, though, obviously, singling out *Jack* from other alternatives is also saying something new in a general sense. The discussion at this point shows, however, that the reviewer is wrong attributing to me the above claims.

<sup>21</sup> If identification is understood as "variable identification", then under information focus the variable only extends to the whole utterance (i.e. for which *x*, *x* happened). Thus, there is no specific identification of the value of any variable within a proposition, but only of the value of the whole proposition.

<sup>22</sup> None of the native speakers has raised the issue.

act, physical or mental”. All A knows is that something happened between *Jack* and *Jill*. The act of asking such a question is aimed at exactly filling this gap in A’s knowledge. The purpose of my argumentation presented in the book is to show that the span of focus in the answer excludes the items *Jack* and *to Jill* but subsumes the sequence *brought flowers*.<sup>23</sup> I agree that the sequence in question is unnatural when not preceded by some phrase like *nothing ... (just)* whose role would be to discharge a possibly unwarranted suspicion that what happened between *Jack* and *Jill* might even be some “sort of violent act, physical or mental”. However, I think that adding such a phrase to an utterance does not affect anyhow the argumentation presented in my book. First of all, it involves absolutely no change in the gist of the interpretation of the example: what matters is how the question asked aligns with the scope of focus of the answer. Thus, the question about a possible event involving *Jack* and *Jill* (in which *Jack* was an agent and *Jill* a patient) determines exactly the scope of focus in the answer given. Second, in considering examples of contexts and interpretations possible additional elements may be disregarded if they are irrelevant from the point of view of the main issue under discussion. What is relevant in the present case is whether there may be a felicitous link between a question and the answer, i.e. whether the answer *Jack brought flowers to Jill* fills the information gap expressed by the question *What did Jack do to Jill?* despite pragmatic nuances.<sup>24</sup>

A different range of objections relating to my use of linguistic data and their interpretation concerns the issue of emphatic (contrastive) stress. Prof. Szwedek notes that I describe sentences like **JACK** *brought flowers to Jill* (identificational, exhaustive focus) as “bearing an emphatic/contrastive stress which is audibly (and mentally) distinct from sentence nuclear stress assigned by NSR (Nuclear Stress Rule)”. He further says “Tajsner offers no evidence for the above statement. It would be easy to determine acoustic parameters of emphatic stress, but Tajsner has made no attempt to ask or, even better, record native speakers to find out whether there really is an audible difference, or if it is only mental”. I quote the whole passage *in extenso* because Prof. Szwedek’s interpretation of my argumentation may be best related to his own words. Note that the statement for which “I offer no evidence” does not mention “acoustic parameters of emphatic stress” at all, it only mentions audible (and mental) distinctiveness. My understanding of the concept of audible is “able to be heard” (*Webster’s dictionary of the English language*). If hearing is form of perception then

<sup>23</sup> I argue it is an instance of narrow focus because despite its extension to the V+NP sequence it excludes the PP complement of V (cf. Tajsner 2008: 255).

<sup>24</sup> A strategy analogous to mine is used, e.g. by Lopez (2009) who gives an example of sentence  
Mary [<sub>F4</sub>greeted] Jane as an appropriate reply to *What did Mary do to Jane?* (Lopez 2009: 81).

I see no substantial difference between my statement and Prof. Szwedek's own formulation by which emphatic stress is "unexpected and therefore perceived as ... emphatic" (Szwedek 1986: 88). It looks like the reviewer comes close to opposing his own views.

Next, nowhere in my text do I make any definite claim that emphatic/contrastive stress is acoustically distinct. I only say in note 76 on page 60: "there is an open question of the phonetic (and acoustic) reality of such an (extra-strong) contrastive stress. We tentatively assume its existence and, more importantly, we believe that its phonological presence is unquestionable". Prof. Szwedek calls the above note "an extraordinary declaration", and returns to it at the end of the review when he calls it "ambiguous". The offence I commit is that I first "tentatively assume and believe, and then immediately find my assumption and belief unquestionable". The way the reviewer comments on these words is characteristic of his style of reviewing, an issue to which I return in the final part. Certainly, in the note I tentatively assume one thing and believe a different one to be unquestionable; I tentatively assume the phonetic and acoustic reality of emphatic stress, while what I find unquestionable is its phonological (mental) representation. Why the reviewer finds a logical equivalence between phonetic and acoustic reality on the one hand and phonological presence on the other remains a mystery for me.<sup>25</sup>

Prof. Szwedek says that "Tajsner offers no evidence" for the statement that emphatic/contrastive stress is audibly (and mentally) distinct. The reviewer thus evidently has taken no notice of the discussion in the whole subchapter 5.10. in which I first admit that when sentences displaying each type "are used separately" it is difficult to assess the claim that the difference between contrastive/emphatic stress and plain "identification" is audibly noticeable, and that in such cases "it may be claimed that they are only perceived as distinct, depending on the context of utterance" (Tajsner 2008: 308). I further pass on to the evidence in favour of the postulated audible difference based on an instance when the two types "are not used separately". The relevant cases are originally provided by Winkler (2000) in the context of her discussion on the interaction of focus and ellipsis. There is a surprising and revealing difference in the grammaticality status between sentences (2) and (3), below (my original examples [222] and [224]):

- 1) *The **wallet** was found, but the **passport** wasn't.*

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<sup>25</sup> Just for clarity, I follow the view of phonology as part of mental grammar in which "rules of syntax and phonology, at least, are organized in terms of 'autonomous' principles of mental computations and do not reflect in any simple way the properties of the phonetic or semantic "substance" or contingencies of language use" (Chomsky 1980: 246).

- 2) \**The **WALLET** was found, but the **passport** wasn't.*

The contrastive stress on the word *wallet* would only be possible if the auxiliary *wasn't* was also contrastively stressed, like in (4):

- 3) *The **WALLET** was found, but the **passport WASN'T.***

If however, there was no audible difference between (2) and (3) why should there be a difference in grammaticality between the two? It looks like (2) is okay because the two words *wallet* and *passport* are perceived as carrying both the same type of stress (interpreted as identification focus), while (3) is out, exactly because the stress on *wallet* is audibly distinct, hence blocking such an interpretation. It is further obvious, that the availability of the interpretation of both stressed words in (2) as identification focus vs. the unavailability of analogous interpretation in (b) can be assessed in isolation from any context.

There are three more arguments I provide for the principled distinction between exhaustive (contrastive) and (non-exhaustive) types of identification focus for which I postulate correlation with two audibly different types of stress. First I point to the synonymy between cleft-sentences and sentences with contrastively/emphatically stressed phrases.<sup>26</sup> Next, I show that there is a restriction on the co-reference between a preceding pronominal and a contrastively focused phrase, which cannot be derived either from pragmatic considerations or from binding effects, but may be attributed to Weak Crossover effects. This is significant because WCO effects are dependent on LF movement, which is a property of contrastive/emphatic stress but not of plain identification or informational focus.<sup>27</sup> Finally, I argue that there is a context-independent difference in interpretation of the mutual scope relations between universal quantifiers and focused phrases which correlate with audibly different stresses.<sup>28</sup> It is a pity the

<sup>26</sup> I argue, contrary to Kiss (1998) that e.g. *No, she put a **PASSPORT** into a pocket* and *No, it was a **PASSPORT** she put into a pocket*, are synonymous and may be used alternatively as responses to *Did she put the wallet into a pocket?*

<sup>27</sup> The relevant example is *??His<sub>i</sub> mother gave **JOHN**<sub>i</sub> a kiss*. The pragmatic explanation of its deviance proposed by Kiss (1998) is that a pronominal *his*, which is old information, should precede the co-referential *John*, which is new information. However, I argue that a contrastively stressed phrase is not, strictly speaking, new information, hence the pragmatic restriction does not hold for exhaustive identification (only for pure identification). What is more, reference to Binding Condition C is not possible since *his* does not c-command *John*. Hence, a solution is recourse to Weak Crossover effect, which implies Focus Movement to the left periphery.

<sup>28</sup> The relevant examples are:

(5) Every detective found the **boys** in a pub.

(6) Every detective found the **BOYS** in a pub.

Under Prof. Szwedek's predictions, there is no systematic difference in pronunciation be-

reviewer has ignored all these proposals and simply proclaimed, contrary to facts, that “Tajsnér offers no evidence”.

In my book I postulate that the answers given to two questions: *Who told you to come?* and *Did your parents tell you to come?* would be audibly different. The former could be answered with a lighter variant of focal stress to mark non-exhaustive interpretation: *My mother told me to come*, while the latter would rather make use of a contrastive/exhaustive stress: *My MOTHER told me to come*. The reviewer has subjected my non-native judgments to empirical verification by native speakers. The methodology of the questionnaire he used remains obscure, but I can gather from the answer sheet presented in the review that the native speakers were asked if stress on *mother* in the answer to the first question would really be audibly distinct (stronger) than in the answer to the second question. The results of the questionnaire are: 3 native speakers confirmed my predictions while 7 native speakers noticed no difference (1 judgment was contradictory to my prediction). The conclusion drawn from the survey is that my “respect for data is unprofessional” and “my claim is totally unsupported”.

In response to these accusations let me first note that an analysis of the recordings could only be of relevance from the acoustic, and not from the phonological point of view. Given that in my argumentation I refer to audible (and mental) and not to acoustic dimensions of stress, the analysis of the recordings, though generally instructive, would “miss the target”. More importantly, however, I think the methodology of the questionnaire was inappropriate. Given that only one context is provided for each of the answers, the prosodic difference between *mother* and *MOTHER* in fact may become indistinguishable. The minimal conditions for marking an utterance contrastive/emphatic may in fact be met by *mother*, and no recourse to *MOTHER* may be needed. Different results may be achieved, however, with a change of methodology. The native speakers should be asked about the possible contexts for the two sentence variants in question. Specifically, sentences *My mother told me to come* and *My MOTHER told me to come* could be read to native speakers with a conspicuous (perhaps even exaggerated) difference in the stresses on *mother*. They could then be asked to envisage a

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tween (5) and (6) (two different tunes or pitch heights) which could be related to a contrastive vs. non-contrastive meaning. Thus, if the difference in interpretation arises it can only be derived from the context, not the sentence in isolation. However I argue that only in (5) and not in (6) can the phrase *the boys* be interpreted as taking wide scope interpretation over the quantifier. The argument is based on two well-known premises: (i) quantifiers take wide scope interpretation with respect to ordinary nominal phrases at LF (logical form) (ii) exhaustively (contrastively) focused phrases can take wide scope positions at LF. My point is thus that there is a formal (not discourse-related) correlation between types of stresses and scope properties.

context in which each sentence might be uttered. If the native speakers really had no “mental” distinctions between emphatic/contrastive and non-emphatic sentence stress, they should not respond to the differences in intonation at all, or simply judge them as impossible or unnatural. If, however, they noted the change and could relate it to emphatic/contrastive vs. simply informative use, it would mean that they associate audible differences with specific interpretations and my claims would be vindicated.

When preparing this reply I asked a few (seven) native speakers for their judgments applying the above described methodology. I read to them two sentences: *I saw a cat on the roof* and *I invited all my friends to the birthday party*. I read each sentence twice, once with standard sentence stress on *cat* and *friends*, and then with stronger (exaggerated) stress on these words, asking if any difference in interpretation may be involved. They all responded similarly; that in the second pair “I specially mark it was a cat not some other animal” “I want to make a distinction between real friends and others”, “I emphasize it was a cat”. These are exactly the sorts of answers I expected, and they corroborate my position; no matter if acoustic difference is used in casual speech, if such an acoustic difference is purposefully imposed it implies special, emphatic or contrastive interpretation.

Next, I must admit I am confused by Prof. Szwedek’s own views on the nature of contrastive stress. In one instance he criticizes me for postulating audible (and mental) distinctiveness of emphatic stress, and then he himself ascertains that if sentence stress is not in the expected position but in “another position, the impression is that it is stronger – stronger than it would be in the neutral interpretation”. So it looks like he assumes that all “shifted” instances of focus have the same “mental” representation; they are perceived as stronger, and all are interpreted as emphatic (contrastive), while stresses in expected positions may not be perceived as emphatic. This view is, however, in sharp disagreement with the views he expresses elsewhere. For example in Szwedek (1976b: 113) he says: “(35) seems to be the most natural response to (32) although with a stronger stress it may also have a contrastive meaning.”<sup>29</sup> Thus, he admits that the expected (neutral) position of stress may be perceived as stronger or weaker. In Szwedek (1975a: 211) he says that in *I bought the book yesterday* the neutral sentence stress falls “on *yesterday*, leaving *book* unstressed” but then he adds “notice that if the stress on *yesterday* in (8) is to be contrastive it must be much stronger and the whole intonation pattern changes.”

At one point the reviewer questions my native speaker’s competence of Polish, elaborating on the fact that the example *Co dźwig uczynił?* (which Prof.

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<sup>29</sup> (32) Have you *bought* the book?  
 (35) I *haven’t* bought the book (Szwedek 1976b: 113).

Szwedek translates as ‘What did the crane execute?’ instead of my plain translation: ‘What did the crane do?’) is ungrammatical because a crane cannot execute anything. I guess the point is that the Polish verb *uczynić* (‘perform’, ‘execute’, ‘do’) assigns an Agent theta role to its subject and *dźwig* (‘crane’) cannot be an Agent. It is strange that the reviewer elsewhere in the texts informs me that “language is replete with ungrammatical but acceptable sentences” and “‘grammaticality’ becomes a partial determiner of acceptability in interaction with other factors” (Beaugrande – Dressler 1980: 129) but here he is so orthodox in defending “the rules of grammar”. Here I think Prof. Szwedek one more time simply misses the main point. My use of *Co dźwig uczynił?* is not to illustrate a possible conversation (e.g. between myself and a crane operator, as he scornfully suggests) but to mark a contrast with a more specific (narrower) question type *Co dźwig uszkodził?* (cf. Tajsner 2008: 329). Maybe the use of *zrobił* instead of *uczynił* would appease the reviewer’s dissatisfaction in this respect, though I am not sure since *robić* also assigns an Agent theta role to the subject, and then recourse to some *cielę* (‘calf’) as of *Cielę widzi kurczę* (‘A calf sees a chicken’) would really be necessary.<sup>30</sup>

## 5. The analyses

The main thrust of the critique, in the part rather misleadingly called “weird interpretations”, is Professor Szwedek’s objections to a few selected analyses from a wide range of theoretical proposals I included in chapter five. Unlike earlier accusations, which centered mainly on the appropriateness of interpretations and the question of their originality with respect to previous work, the ones raised in this part are strictly related to generative and minimalist methodology adopted in the book. Before I address the specific complaints and reproaches, let me first raise a general point. In an earlier part of the review, Prof. Szwedek writes “Any linguist can adopt a methodology that he thinks best serves his purposes. I expect the same attitude from my minimalist opponents. If they have serious arguments against cognitive linguistics, they have the right to bring them up. However, nobody has the right to criticize, as Tajsner does cognitive linguistics, any approach or field he lacks knowledge about...”. I must agree with this statement; it is not reasonable to criticize proposals one does not understand. However, I find some difference in the way in which the reviewer and I apply this wise advice in practice.

Maybe my knowledge of cognitive linguistics is comparable to the reviewer’s understanding of minimalism. Prof. Szwedek openly admits “I am not competent to discuss these issues” when mentioning some minimalist concepts.

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<sup>30</sup> Can *cielę* (‘calf’) be an Agent though? I am afraid I am not able to decide.

Nevertheless, the obvious lack of competence in minimalism he proves beyond doubt in the article, does not restrain him from expressing stinging criticism of the specific technical implementation of the approach. In my opinion, such an attitude contrasts sharply with mine; nowhere in the book do I criticize specific analyses proposed within cognitive grammar. The only references I make to the approach are those on pages 21 and 23 of my book. First, I briefly compare the concept of iconicity known from cognitive linguistics to the minimalist proposals on the role of the “interface condition” (also known as “bare output condition”) in the design of the language system.<sup>31</sup> I argue there is a “flaw in the logic of the “iconicity” argumentation” because “the properties of language may be designed so that they successfully meet the external requirements”, which does not entail that these requirements must be “reflected” in these properties (cf. Tajsner 2008: 21). The point I raise is not critical, maybe just mildly polemical. I further argue that talking about “convergence” between Cognitive Linguistics and minimalism is therefore unwarranted. If minimalists say that language is designed for specific “external” tasks, it does not mean it is necessarily iconic.<sup>32</sup>

Next, I make a note related to the methodological differences between the two approaches: “the point of departure for many explorations within CG is the formulation of the highly intuitive associations and analogies allegedly existing between language and other systems, external to language. The existence of such analogies is not rejected *a priori* in recent generative projects ... but it is claimed that the study of language cannot be preceded by any intuitive speculations concerning the relations between language and the world” (Tajsner 2008: 23).<sup>33</sup> These are general remarks of a conceptual and methodological nature, which I uphold. They are worded cautiously, and the reviewer’s over-reaction to them is really surprising.<sup>34</sup>

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<sup>31</sup> The idea of “interface condition” is, roughly, that language is shaped as a response to the requirements of the external world.

<sup>32</sup> By raising the issue I rather express a slightly “defensive” position taken by minimalism which has to somehow rationalize its position about “interface condition” *vis-à-vis* the concept of iconicity.

<sup>33</sup> I have to acknowledge my mistake here, correctly pointed out by the reviewer. The dichotomy in question is not *figure – trajectory* but rather *landmark – trajectory* (or *figure – ground*).

<sup>34</sup> The only assessing phrases used in these passages are: *highly intuitive associations* and *intuitive speculations*. I find these phrases appropriate given that one finds in cognitive linguistics literature such speculative statements, as “...the order of cognition is from objects to relations between them with the result that once the relation between the objects has been encoded/lexicalized on the basis of their properties, the lexicalized relation will necessarily entail/invoke these properties. Thus, the pivotal role of the verb in language model is contrary to psychological reality/organization of cognition, but is justified on the grounds of ease of processing in language modeling” (Szwedek 1995a:72).



The first set of his specific objections is directed at my analysis of the sentence *The baby disappeared*. The explanation I propose for a non-final location of sentence stress under neutral interpretation in such sentences rests on a well-grounded view that unaccusative verbs select their “surface” subjects as objects.<sup>35</sup> I offer my proposal as an alternative to Adger’s (2007), which “calculates” the number of stress grids in a metrical tree corresponding to a syntactic tree.<sup>36</sup> Prof. Szwedek’s harsh and uninformative comments on the use of the copy of the phrase in the original object position are a vivid illustration of his ignorance of minimalism. The reviewer is clearly unable to assess the validity of the proposal that NSR may apply to a copy within the adopted theoretical framework. He points to a presence of a copy as something strange or unexpected, while in fact it is the absence of a copy that would have to be specifically argued for and proven.<sup>37</sup> It is unclear how to reply to such criticism. The point is that either one knows the theoretical paradigm and then may assess the validity of a new proposal within its bounds, or one does not know the theory and then, in my view, should not raise objections.<sup>38</sup>

The reviewer scornfully remarks that “It would appear that in minimalist linguistics one can add, move, delete, etc. anything to satisfy some rules and to achieve the desired result without any regard whatsoever as to what functions such manipulations serve”. The question of function has been addressed already and will be raised again in the part devoted to theoretical reflections. Let me only recall that the generative position is that the functions are imposed on the outputs of the grammar (i.e. sentences) only when these are put to use. By definition, there is no question of the function in the description of the derivation of sentence types, when these are not sentence tokens. As for “adding, moving and deleting”, these really are (approximately) the most primitive syntactic operations the grammar has at its disposal.<sup>39</sup> It is only that each of these operations is heavily constrained: e.g. movement must be local, where locality is strictly defined, furthermore, it must be driven by lexical features and performed only to a root node, not to a structure-internal position. Adding an element to a structure,

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<sup>35</sup> Such a view is rather uncontroversially adopted in recent generative accounts. It is well-motivated by the properties of theta marking and Case marking (unaccusative verbs do not assign accusative Case nor external theta role).

<sup>36</sup> I point to some inadequacies of Adger’s (2007) proposal in view of a minimalist premise, known as Phase Impenetrability Condition.

<sup>37</sup> Specifically, it is absolutely wrong that “*the baby* is added in order to get stress in the final position”. The phrase appears in the final position because it is the only initial argument position in this structure.

<sup>38</sup> Likewise, the deletion of one of the copies is part and parcel of the minimalist view of syntactic derivation.

<sup>39</sup> More appropriately; merging and moving with deletion understood as “pronunciation of a single copy”.

or rather merging it with another element, requires a specific relation between a “selector” and a “selectee”, where the former projects, and typically triggers theta role assignment. Deletion of all copies but one at PF (and one at LF) is conditioned by the interface condition and the requirement of Full Interpretation. The most significant point, a distinguishing trait of minimalism, is however that the constraints in question are not independently stipulated but “can be reduced to general considerations of computational efficiency, or to properties that any system pairing sound/sign and meaning would have to meet to be usable at all” (Boeckx 2008: 11). The minimalist program offers a whole agenda for determining “how the gross features of FL (language faculty) result from the combination of general cognitive operations and principles, plus a very small number of innovations (preferably one) specific to LF (Hornstein 2009: 106).

I realize that a description of the properties of syntactic derivation with the use of terminology and categories internal to a specific theory is hermetic. If one wants to understand what an account in such terms really amounts to, it is necessary to first make an effort to get acquainted with the basic metalanguage. Therefore, the technical discussion I propose in chapter five has not been inserted “out-of-the-blue” but preceded by a detailed description of the minimalist machinery of derivation which I present in chapter one. Had the reviewer read subchapters 1.2., 1.3., 1.4., 1.5., 1.6., 1.7., he would have had a chance to familiarize himself with the elements of the derivational system, such as *copy*, *move* or *delete*.

One specific point raised by the reviewer with reference to the example *The baby disappeared* may require a more detailed reply. The reviewer objects to my proposal that “the ultimate order is a result of PF stylistic fronting of the subject which restores a canonical SV order”. It has been pointed out to me by another (generativist) reviewer that the view of the PF stylistic fronting as a kind of “repair mechanism” outside “narrow syntax” may actually be seen as “too powerful” and that I should have really clarified my point on the issue. One thing I point out earlier in the text is that I allow for “overt movement operations taking place in the phonological component, after SO (i.e. spell out), but they would necessarily be without any semantic effect (e.g. the cases of Japanese Stylistic Scrambling, see Bošković (2004))” (Tajsner 2008: 35). I also note further that such operations are “ill-behaved” (as defined by Chomsky 2002) and different from “core syntactic processes in that they do not iterate (are not successively cyclic)” and are not driven by Last Resort (feature checking). They represent “properties ascribed to the phonological component involving ‘heaviness’ like Heavy NP Shift, Right Roof Constraint (and probably also other operations like Extraposition, Thematization...)” (Tajsner 2008: 36).

The other point of relevance in this connection is a general question of how the ultimate word order (linearization) is related to syntactic derivation. There are

two major viewpoints in this respect represented by Chomsky (e.g. 1994, 2001, 2007), and Kayne (1994). According to the first view, vividly expressed by Horstein et al. (2005: 219) “linearization is what we get when we try to force a two-dimensional structure through a one-dimensional channel. Phrase-markers are 2D objects...”, word order is “however, one-dimensional; a string of sounds and signs”. Further they conclude: “linearization is essentially a PF-affair; an interface requirement imposed by the A-P system”. Chomsky himself defines this view as follows: “There is no clear evidence that order plays a role at LF or the computation from N to LF. Let us assume not. It must be, then that ordering is part of the phonological component ... It seems natural to suppose that ordering applies to the output of Morphology, assigning a linear (temporal, left-to-right) order to the elements it forms” (Chomsky 1994: 25). Thus, the order of elements is determined by PF (directionality) filters. One such filter is a specific value of a Head Parameter, so that languages may be head-initial (e.g. English) or head-final (e.g. Japanese). The other filter is “the near universal SPEC-H ordering – which is narrowed to subject-H ordering” (Chomsky 2007: 7).<sup>40</sup>

In opposition to a view of linearization based on directionality “filters”, Kayne (1994) proposed his Linear Correspondence Axiom (LCA), by which order is encoded in hierarchical syntactic structure through a relation of asymmetric c-command to the effect that an element X asymmetrically c-commanding Y must also precede it in linear order at PF. Thus, even though the ordering remains the PF “affair”, it has its “roots” in narrow syntax. Under such a view, the postulation of purely stylistic PF displacements would be problematic.

Of the two views on the nature of the relation of syntactic structure to order, I adopt the former in postulating the “repair” PF mechanism for restoring canonical order. Moreover, I make it explicit that operations such as “stress avoiding movements to escape hatches” or topicalization, bringing about semantic effects do not belong to the class of stylistic PF movements.<sup>41</sup> The four relevant instances in which such “repair mechanisms” are applied, as discussed in my book are: (i) the case (just discussed) of *The baby disappeared* when *the baby* having been assigned nuclear stress and transferred to PF, undergoes a stylistic fronting to restore the SV order, (ii) example [72] *Jack saw the movie*, (iii) example [90] *Jack saw the movie on DVD*, and (iv) example [91] *Jack gave Jill a bunch of flowers* (Tajsnér 2008: 278).

With respect to the first example, the reviewer further objects to calling the “repair” movement stylistic, since it would mean that “the difference between

<sup>40</sup> Some other proposals for determining order at PF have also been advanced. “To mention one, it would not be implausible to seek a parsing account for properties of ordering: often justified (e.g., rightward displacement of complex phrases)” (Chomsky 2007: 7).

<sup>41</sup> I follow here Chomsky (2001) in assuming that “surface semantic effects are confined to narrow syntax”.

*The baby disappeared* and *Disappeared the baby* is stylistic only, and both are fully grammatical”. Let me explain that the term “stylistic” is used here in the sense of “semantically vacuous” in agreement with the convention used by other authors (e.g. Saito 1989; Poole 1995; Saito – Fukui 1998, etc.). The role of the syntactic structure is to determine all syntactically relevant relations (e.g. Case, theta role, S-V agreement). All these relations are properly accounted for in the structure (5) given below, before it is transferred to PF.

5) [<sub>TP</sub> The baby disappeared {the baby}]

For example, the Nominative Case is checked on the higher copy of *the baby*, the theta role is assigned to a lower copy, and the agreement features are checked. Furthermore, only the higher copy is retained for semantic interpretation (at LF (SEM)), and only the lower one is transferred to PF and gets nuclear stress. In particular, there is no special LF (semantic) interpretation of such a sentence. In pragmatics, the sentence will simply be interpreted as purely informative (an instance of information focus). The repair mechanism at PF is purely stylistic in the sense of adjusting the PF string to the canonical SV order typical for English.<sup>42</sup> Under such reasoning, the ungrammaticality of *\*Disappeared the baby* is derived not from any serious derivational (syntactic) problem, but from breaking a strong preference stylistic constraint.

Consider also (90). I propose that “the linearization in this case adheres to PF constraints, one of which is that Verb-Object-PP Adjunct order is restored for proper Case realization through the adjacency of the object to a Case assigner” (Tajsner 2008: 278). What I am aiming at is to show that the special semantics of this sentence arising apparently from “stress shift” is in fact determined in the syntactic derivation and not in the phonological component. Namely, the phrase *on DVD* is first removed from the most embedded position due to a stress avoiding movement to the escape hatch (Spec.vP). This step is triggered by the presence of the feature EPP in v. Next, the object *the movie* is transferred to PF, and as a result, it is assigned prominent stress by NSR. The remaining parts of the sentence are only transferred to PF at a later (i.e. CP phase). Hence, they have no chance of getting nuclear stress. Once again, all syntactically relevant relations (Case, theta, agreement) are satisfied in the (simplified) structure (6), below:

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<sup>42</sup> Note that corresponding unaccusative sentences in other languages (more liberal with respect to word order) may notoriously be either VS or SV, e.g. Polish: *Przyjechał Tomek/Tomek przyjechał* (‘Tom arrived’), Italian: *Arrivata Giovanni/Giovanni Arrivata* (‘John arrived’), Braz. Portuguese: *Chegou a Maria* (‘Mary arrived’), French: *Il est arrivé trois hommes* (‘Three men arrived’), Spanish: *Llegó María* (‘Mary arrived’), etc.

## 6) [CP Jack saw [vP on DVD [vP the movie ]]]

Such a structure is transferred to PF in two chunks, where, after nuclear stress assignment, a “repair” mechanism restores the required SVO-PP order.<sup>43</sup> The special interpretive effect of such a sentence (identification focus) will then be achieved in pragmatics by the appearance of nuclear sentence stress on the movie, i.e. in a “shifted” position. Analogous mechanisms are proposed for the derivations of [71] and [91].

I hope the readers will forgive me this rather lengthy, technical description. I wanted to show that the reviewer’s unfamiliarity with minimalism cannot serve as accuse for classifying my proposals as “methodologically incorrect” or “completely” unconvincing. It is at this point and in numerous other places that Prof. Szwedek displays his frustration at the fact that other people may have cogent views on language completely different from his.

Next, the reviewer objects to my analysis of unergative structures (e.g. *The boy slept*) in which nuclear stress may fall alternatively on the subject or on the verb. A key part of my proposal in this respect is derived from the idea that “unergatives are hidden transitives” in that they take a “cognate object” (like in: *slept [a sleep]*). Within generative grammar, the idea was first applied by Hale and Keyser (1993).<sup>44</sup> There are two objections raised in this context. First, the reviewer wonders why I use the word “alternatively”, implying that no difference in meaning follows from the change of stress placement. Wrong! I am not saying that there is no difference in meaning, but I simply assume that both versions are neutral and “all new”, i.e. may be used in response to *What happened?*<sup>45</sup> The point is uncontroversial, for example David Adger (2007) (a native speaker of English) contrasts *The BABY’s crying* vs. *The baby’s CRYING* (notation of the original) with the same prediction. I was really amazed to note, however, that two pages later the reviewer quotes his own analogous examples; *John died* and *John died*, saying they are both correct in the context of *What happened?* Either the reviewer got completely lost at this point, or just wanted to ascribe to me straw man views I did not express in order to attack them afterwards – a maneuver he resorts to on several occasions.

<sup>43</sup> There is a crucial difference between the derivation of transitive and unaccusative structures, since the latter are transferred to PF at one phase (there is no vP phase), while the former at two phases.

<sup>44</sup> It is true that the idea had been around before Hale and Keyser (1993), which they admit, but their contribution was to instantiate it in generative terms. By the way, it is strange that the reviewer would require that Hale and Keyser (1993) should have noticed that such structures were mentioned in (1996) edition of Quirk et al.

<sup>45</sup> The importance of such a classification is that the derivation of unergatives need not recourse any special mechanism for avoiding assignment of nuclear stress (e.g. a stress-avoiding movement to an escape hatch).

The second point is technical. Prof. Szwedek frowns at my explanation stating “dropping an element at some phase, assigning the stress at another without explaining the motivation, appear to me like purely arbitrary decisions.” He may be dissatisfied with the solution but I do not think his dissatisfaction gives him the right to criticize the analysis he does not understand.<sup>46</sup> It seems necessary to inform the reviewer: in minimalist accounts “what is essential **is** (emphasis PT) the derivation not the speaker’s intention” and “such manipulations” **cannot** be semantically motivated, because we (generativists, minimalists, formalists) take the grammar to be autonomous from semantic motivation. I will leave for later a more thorough discussion on these points.

The theoretical proposals in my book are not placed in a vacuum. Quite the opposite, they relate strictly to well-grounded minimalist views, accounts and analyses advanced by numerous scholars over the years. Whenever my own accounts with respect to focus were related or alternative to other linguists’ views, I first tried to present these other views in some detail. I devoted three chapters of my book to a discussion of recent views on focus. However, when discussing more general proposals, not strictly related to focus, I often adopted some theoretical framework without giving it a detailed description. I find this to be a sound practice. Therefore, I was surprised by the reviewer’s critical comments when discussing my way of handling sentences like *Jack saw the movie on DVD* in which sentence end-focus falls on an adjunct. I explicitly refer to Chomsky’s (2001) account in which adjuncts are “freely adjoined ‘at a separate plane’ to V, or alternatively to VP” and the addition of adjuncts is an instance of “pair-Merge” rather than a more common “set-Merge” operations. The reviewer says “it would also be nice to know what accounts for the ‘free adjunction’ and ‘at a separate plane’”. It is not the place here for an extended discussion of the issue; hence I direct the reviewer (and any interested reader) to the author for explication (Chomsky 2001).

One of my original proposals is that reference to “stress avoiding movements” is a way of accounting for the effect of “shifted focus”. I argue that “the nuclear stress may be said to invariably fall in the same structural position in both types of sentences – those representing pure information focus and those representing non-exhaustive identificational focus” (Tajsner 2008: 268). In the latter case, a constituent escapes from the position of the assignment of nuclear stress to an “escape hatch” and some other element can receive stress prominence. The reviewer frowns at this proposal saying: “the sole purpose of escape hatch and other manipulations is to avoid nuclear stress assignment”. Let me inform him that the presence of escape hatches (here Spec.vP positions) has

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<sup>46</sup> I do not think it necessary to explicate the details of my solution here since its consistency with the minimalist program is not questioned. (For details see Tajsner 2008: 264-265).

been part and parcel of generative derivations for about forty years, i.e. since the invention of consecutive cycles (e.g. Bresnan 1970; Emonds 1970; Chomsky 1973). What I propose is a specific instantiation of the independently motivated minimalist version of the escape hatch in the account of focus. The reviewer is also wrong in saying “Tajsner has nothing to say about the ultimate aim to which all these manipulations should lead, that is, the differentiation of meaning”. The whole logic of my argumentation about “escape hatches” is that they allow formal variants which get special pragmatic interpretation. Certainly, given the reviewer’s cursory mode of reading the text, he had little chance of spotting (frequent) clues on this issue in the text, e.g. “If, for example, a subject DP is to be interpreted as identificational, non-exhaustive focus, then the derivation aims at designating it for nuclear stress assignment” (Tajsner 2008: 280).

There are also less technical objections to my proposals. The reviewer does not accept the very distinction between “pure identification” and “exhaustive identification”. He asks; “Can you identify an individual non-exclusively?” It is not clear if the reviewer objects just to taking my example [52] (Tajsner 2008: 60) as an example of non-exhaustive (non-exclusive) identification, or in fact to the whole concept of non-exhaustive identification. It seems he means the latter, which is very odd. The novelty of my approach is its tripartite distinction: information, non-exhaustive identification and exhaustive identification, but the very distinction between exhaustive and non-exhaustive identification by focus has been long known and cogently argued for. It is enough to mention the set of Szabolcsi (1981)/Kiss (1998) exhaustive identification tests, on which I elaborate in sections 1.12., 3.8.2., 5.3., 5.7.4., 5.10. One of these tests applies in the following manner: of two English sentences *Mary picked a hat for herself* and *It was a hat that Mary picked for herself*, only the latter represents exhaustive identification because it is not entailed but rather contradicted by sentence *It was a hat and a coat that Mary picked for herself*. The conclusion Kiss draws is that only cleft sentences (allowing also for sentences with the use of *even*, *only*, *also*) represent exhaustive identification, while others are instances of “pure”, non-exhaustive (non-exclusive) identification.<sup>47</sup> Given such obvious logical grounds for the distinction, the reviewer’s objection cannot be sustained.

Elsewhere, objections are raised to the concept of “out-of-the-blue” sentences. The reviewer says: “... there are no ‘out-of-the-blue’ sentences. Everything is used in a context and/or co-text”. I have not invented the term myself. It has been used by almost every author dealing with matters related to information-structure within generative grammar. For example Jackendoff (2002) comments on the use of the sentence *JOHNSON died* (notation for focus of the original): “... it can also be used to announce the event of Johnson’s death ‘out of the blue’, with no previ-

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<sup>47</sup> I argue that such a restriction is too severe. See discussion in sections 1.12. and 5.7.

ous common ground in the conversation that bears upon either Johnson or this event” (Jackendoff 2002: 412). This is exactly the sense I apply to such sentences when discussing cases like *Jack brought flowers to Jill* or *Jack saw the movie* uttered with unmarked sentence stress. I assume there has been “no previous common ground in the conversation” when such sentences are used. Maybe such compelling evidence could make Prof. Szwedek revise his views; it is true that almost everything is used in context, but if there is no “previous common ground” the form of the sentence in such instances is not affected.

An interesting phenomenon related to word order and sentence stress in Polish is the option of deriving sentences in which so called scrambling and topicalization coincide with (apparent) “stress shift”, e.g. *Wiadomość o spotkaniu TOMKOWI przesłał Janek*. I discuss such cases in sections 6.5.5. and 6.5.6, and put forth a hypothesis that focus in such cases must be emphatic/contrastive and not purely identificational. I support this proposal on both empirical and conceptual grounds in terms of such matters as scope of focus, co-occurrence with wh-questions, cleft-like interpretation and derivational economy. This whole discussion I gave does not draw the reviewer’s attention at all. Instead, he prefers to relate his criticism to a single statement I make with respect to the above hypothesis in the introduction.

He calls “bizarre” and judges untrue my claims that (i) “the variation of word-order alone in Polish is an effective strategy for deriving various options for identification focus”, and (ii) “any instance of ‘stress shift’ on the non-canonical order should be analyzed as an instance of ‘polarity’ focus in which the focused phrase is strongly emphatic and gains ‘contrastive’ or ‘corrective’ meaning” (Tajsner 2008: 17). The reviewer says that his examples *Książkę czytałem* and *I was reading the book* prove his point. I do not think they do. First, the English example is irrelevant since it does not feature any change of word order; the strategy of combining stress shift with non-canonical order I discuss is available for Polish, but not English. The second example *Książkę czytałem* is interesting. It seemingly contradicts my prediction; it features non-canonical order and “stress shift” but it may represent information focus, and need not be emphatic/contrastive. I did not mention such cases in my book, but it looks like their account in my system is straightforward. If *Książkę czytałem* truly is a stylistic variant of *Czytałem książkę* (without any change of information structure) – a judgment that we both agree on, then the difference may be simply derived in PF without any effect on LF (SEM, interpretation). It is interesting to note that the same alternation is not possible if a subject is added; the sentences *Książkę czytał Tomek* and *Tomek czytał książkę* are not just stylistic variants; the former has contrastive/corrective interpretation, unlike the latter.

The reviewer says that “separating word-order from the sentence stress is a purely speculative endeavor”. I find this statement astonishing. This implies that



word order and stress are part of the same grammatical phenomenon and that they always go hand in hand; changing stress implies change of word order, and vice versa. But the reviewer himself uses examples in which this linkage is falsified; shifting of focus occurs without change of word order, e.g. *I bought a **book*** and *I **bought** a book*, or the order changes with stress staying on the same element, e.g. *Bił ja **MEŻCZYŻNA*** and ***MEŻCZYŻNA** ja **bił*** (Szwedek 1975: 212) (the notation for stress of the original). I take word order variations and the mechanisms responsible for stress placement to be separate phenomena which interestingly interact. What is more, I find the kind of “holistic” approach to focus, stress and word order proposed by the reviewer to be methodologically wrong. I therefore sustain my claims about the interaction of non-canonical order and “shifted” stress and strongly reject the reviewer’s opinion that they are “bizarre”.

## 6. Theoretical reflections

The recent two decades have not seen much of the “linguistic wars”; and with some notable exceptions (cf. Seuren 2004; Sampson 2005), the linguists from different camps have learned to co-exist, or even to collaborate. There is no doubt that a “cross-model” linguistic debate is a virtue in itself, but I think it must be based on mutual respect and the acceptance of the rules of “fairplay”. The primary condition of such a debate must be that the sides make an effort to understand each other’s views. I do not think Professor Szwedek’s critical review of my book meets these criteria, which is a pity because a debate on the limitations of the functionalist vs. generative approaches to the phenomenon of focus would be worthwhile.

The theoretical reflections that Prof. Szwedek offers in the review reveal his misunderstanding of the generative view of language in general and of the conceptual foundations of the minimalist program in particular. What is more, it seems he has made no effort to get an update on minimalism from my book. Instead, he repeats arguments known from many publications in the past in which their authors militate against views and assumptions which they erroneously, or purposefully, ascribe to generative linguists.<sup>48</sup>

One of the recurring motifs of such critiques, prominent also in Prof. Szwedek’s review, is the issue of the autonomy of language and grammar. Prof. Szwedek writes: [the methodological position adopted by the minimalists] “has nothing to do with ontology, that is with the psychological reality of language

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<sup>48</sup> Some of these attacks are so fierce that one has an impression that personal and emotional factors may be involved (e.g. Robinson 1975; Givon 1979b; Seuren 2004; Sampson 2005). It is worth adding that there is usually very little retaliation from the generative side, which I am sure need not be understood as lack of arguments.

use and also with the ultimate goal of linguistic research which is to build a model of language". This statement is a major distortion of the minimalist position. It is enough to take any introductory manual to minimalism (or generative grammar) to find reference to two related goals of generative research which are: (a) building a theory (model) of the (internalized) language system (language organ, Language Faculty) and (b) explaining language acquisition.

I understand that Prof. Szwedek's reference to ontology has been provoked by my use of the term in the following passage: "... the autonomy thesis so conceived is in fact a methodological not an ontological thesis. *As such, it does not presuppose the ontological autonomy of language from all other aspects of reality* (emphasis PT). The fact that language should be studied in its own terms, without the guidelines from the observation of the surrounding reality does not imply that the organization of language may not in the essence be like the organization of other aspects of reality" (Tajsner 2008: 22). What I am saying is exactly opposite to what the reviewer imputes to me. Actually, the whole discussion in chapter one aims at summarizing the recent minimalist position by which "we can regard the explanation of properties of language as principled insofar as it can be reduced to properties of the interface systems and general considerations of computational efficiency and the like" and "crucial aspects of language can be studied as part of the natural world" Chomsky (2006: 181).

Thus, the study of language aims at discovering the real nature of language, hence its ontology. Nowhere in my book or elsewhere in minimalist literature will the reviewer find justification for his accusation that "ontology is rejected" in minimalism. I included the whole passage from my book, because when quoted in the review it was sliced in two, and the bolded key sentence in the middle was simply dropped, the means by which the reviewer distorted its sense. The reviewer's preconception on generative grammar's view of the ontology of language seems to reflect frequent misunderstandings of grammar as an "artificial" and "arbitrary" system which has nothing to do with the mental reality. Such a view has never been part of the generative theory of language.

The methodological constraint of language (and grammar) autonomy that generative grammar imposes on itself follows directly from the "Galilean style in science" it adopts. The crucial elements of this style are idealization and selection. By the former, the "distracting" factors are eliminated in empirical research so that the real nature of the things observed may be revealed. Thus, when studying language (conceived narrowly as the internalized system of rules), non-linguistic factors, such as, e.g. the context of utterance, speakers' intentions and other pragmatic factors must be sifted out (cf. e.g. Chomsky 2000; Smith 2000). Furthermore, as stated by the physicist Chen Ning Yang (1982: 28); "Galileo ... taught the world of science the lesson that you must make a selection, and if you judiciously select the things you observe, you will

find that the purified, idealized experiments of nature result in physical laws which can be described in mathematical terms” (after Boeckx 2006: 124). The “things selected for observation” in generative research are the formal properties of phrases and sentences, while all elements pertaining to interpretation are excluded. They remain, however, objects of research of the independent study of language use, the domain of functional linguistics, and pragmatics, etc.

The reviewer writes: “Tajsnér’s arguments for the autonomy of language are not convincing.” No wonder, since I do not provide any arguments for language autonomy. It was not my intention to defend the thesis in my book. I simply took it for granted as part of the adopted generative methodology. But, the arguments are there, easily available in the literature. Newmeyer (1998) argues extensively for three subparts of the autonomy thesis he calls Autosyn (autonomy of syntax), Autoknow (autonomy of the knowledge of language) and Autogram (autonomy of grammar). There is no place to present this compelling argumentation at length here, and hence I refer the interested reader to Newmeyer’s book. Let me only mention one extended argument for Autosyn, which I take to be decisive. If function and meaning determined syntactic structure, there should be a one-to-one relationship between form and meaning. Empirical study easily proves it is not the case. For example, consider the syntax of English inverted auxiliaries. First, they are associated with many diverse semantic functions: (yes-no questions, wh-questions, pre-posed negative adverbs, bare subjunctives). Next, within a given semantic function there are constraints: thus auxiliary inversion is blocked for embedded yes-no and wh-questions and for pre-posed positive adverbs and the subjunctives introduced by *if*. Finally, there are formal idiosyncracies that all these uses share, e.g. each of them allows only one auxiliary to be inverted.

Furthermore, evidence from language acquisition provided by Crain and Nakayama (1987) shows that children, when acquiring the auxiliary inversion structure, “pay no attention to the semantic properties of the subject NP. In particular, there are no semantic prototype effects. All NPs are integrated into the system at once, whether referential, expletive, abstract or whatever. ... Presumably any linguist who wished to deny the reality of autonomous structures (or, more properly, the autonomous principles underlying them) would have to take the somewhat peculiar position that each time a new semantic function is learned for the inverted auxiliary, the construction itself would have to be learned from scratch” (Newmeyer 1998: 49). There can only be one conclusion arising from these empirical observations: “the environments in which the inverted auxiliary structure occurs defy a uniform semantic characterization” (Newmeyer 1998: 48).

In addition, that author shows there is no functional or pragmatic basis for auxiliary inversion either, since its use has a variety of discourse effects, such as: “conveying a question, a request, an offer, an exclamation of desire, and a

statement of enthusiasm". What is more, these five different types of speech acts can also be performed with other formal devices. This all is compelling evidence that "the principles involved in characterizing structures formally must be distinguished from those involved in determining their use in discourse. ... There are pervasive structural patterns that form part of our knowledge of language and there are general form-based principles responsible for producing them" (Newmeyer 1998: 49).

The points made with respect to inverted auxiliary structures apply generally. One finds "pervasive patterns of form" unrelated to meaning or function everywhere in syntax. The relations of complementation, specification, modification within phrases, the syntax of *wh*-constructions, locality constraints, "island" conditions, are just a few of a long list of syntactic phenomena which require syntax-internal characterization. They constitute the substance of language without which no semantic relations between lexical items could be established and no functional (pragmatic) interpretation of sentences could be added in the context.

I was amazed reading Prof. Szwedek's pronouncements like "syntactic decisions, even in minimalism, are determined by semantics", "it is the function that is the essence of language", "no language element is independent of its interpretation". I leave aside the empirical incorrectness of such claims, indicated above. I can understand one's strong attachment to the selected methodology, but why impose one's views in such an arbitrary manner on others?

There are all sorts of misinterpretations of both my views and the minimalist position in the review. Prof. Szwedek seems to think of minimalism as a model of language performance, which it is not. He writes: "Once a particular language category is selected, for example VP or PP adverbial, the choice becomes semantically charged, as every category involves a functional significance and representation by sets of meaningful lexical items." On the contrary, the minimalist derivation is blind to the "semantic charge" of a phrase or a lexical item. The only "semantic" elements involved in the derivation are theta roles and formal agreement features (number, person, gender) introduced by lexical items. As for "functional significance", there is none since it can only be determined in a context. The minimalist derivation is not a mental process in the mind of a language user occurring in "real time". It is simply not part of performance but a grammar-internal procedure, which characterizes our knowledge of language.

Some of the reviewer's other odd statements are: "I assume that before minimalists start derivation, they have to select a language structure that will eventually function in communication, that is make sense for language users". For one thing, minimalists "do not start derivation", they can only describe the way it proceeds; hence, they do not "select a language structure". If such imprecise statements are only a "metaphorical" short-cut to point to the relation be-

tween sentence form and its function, let me explain what I already said in this reply and made explicit throughout the whole book. The goal of minimalist research is to show how different sentence types are consecutively built in the grammar. How such sentences function in communication is determined outside the grammar. Nevertheless, it is true that the starting point for minimalist studies is the elucidation of what sentence types the grammar should be able to derive to perform various communicative needs.

The reviewer raises objections to American structuralists, saying “they believed that they could describe Indian languages objectively ... . However, they also had to adopt a procedure of asking native speakers whether a structure was correct. The native speaker was the ultimate judge, the ‘semantic component’ in language analysis.” I find this statement very peculiar. Why should an informant judging about the grammaticality of sentences be called “the semantic component”? If a linguist asks an informant if a sentence is correct, what he expects is usually a plain “yes”, “no” or “I am not sure”, not some elaboration on its interpretation.

Prof. Szwedek calls my statement “the speaker can do with words and stresses only what the grammar allows him to do” an “uninformed statement about cognitive linguistics”. I must say I find it difficult to believe that this is anything else than an intentional attempt to mislead the reader. It is absolutely obvious from the text that my statement does not relate to cognitive linguistics. What I meant by this is immediately explained in the next sentence which the reviewer chose not to quote: “The speaker’s intentions can thus be realized only within the limits imposed by the formal system of grammar which he has at his disposal.” Thus, for example, if the speaker’s intention were to ask someone not to smoke in his car, he could say, *Please don’t smoke in the car*, but not *\*Please don’t in the car smoke*.

Further, the reviewer objects to my words in a footnote: “We abstract here from the obvious cases of figurative use of language, poetry, word-play and other instances of the intended breaking of the rules of grammar” (Tajsner 2008: 12). Moreover, Prof. Szwedek imputes that I would judge the sentence *It’s difficult for me to put my ideas into words* to be an instance of breaking the rules of grammar, and then says: “Regrettably, Tajsner does not offer any arguments why such sentences are ungrammatical.” Surely, no generativist (or minimalist) would think of this example as ungrammatical. I think this has been obvious at least since the relevant notions were properly defined, i.e. since the time of the discussion of *Colourless green ideas sleep furiously* back in the sixties. What is more, the description of the formal workings of the grammar given in my book leaves no doubts: such sentences cannot be excluded from the range of well-formed structures, and their status as “figurative language” can only be determined at the semantic/pragmatic interface.

But the reviewer has caught me on an imprecise statement here. Let me explain that by mentioning figurative use of language in a note I meant idiomatic expressions like *It's raining cats and dogs*, *Let's talk money*, or *He was hand in glove with her*, in which the rules of syntactic selection are breached.<sup>49</sup> However, it is true the term “figurative use of language” is certainly not confined to such cases, hence I should have added the phrase *some instances of* in the note, saying: “we abstract here from the obvious cases of some instances of figurative use of language...”<sup>50</sup>

At places, the reviewer imputes my having mean intentions. When I write “... we will not treat focus as mere ‘surface phenomenon’, an incidental property of the output of syntactic derivation...”, he reacts as if I was almost hurting his feelings: “... such impressionistic statements, almost offensive expressions like ‘incidental’, without any argument and support whatsoever, are discreditable in scientific discourse”, and further he asks; “What is Tajsner’s evidence for the incidental character of focus in functional approach?” What I, in turn, find “discreditable in scientific discourse” is such a careless treatment of the text reviewed. Had the reviewer read the passage in full and joined it with the following ideas in the book he would have known I am not referring at this point to functional approaches and only to early generative views on focus (e.g. Chomsky’s 1972).<sup>51</sup> Besides, I cannot understand why the term *incidental* should be judged as “almost offensive”.

## 7. The epilogue. The question of manners

The reviewer accuses me of bad academic manners. In his conclusions, he says that Seuren’s words taken from his lampooning critique of the minimalist program, which Prof. Szwedek uses as a motto, fully apply to my book.<sup>52</sup> It is not

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<sup>49</sup> For example, the verbs *rain* and *talk* are not transitive, and a noun without an article taking a PP adjunct is not a possible predicate, cf. *\*He was teacher from local school*.

<sup>50</sup> As for the overwhelming use of metaphors in language, I am not sure of the estimate the reviewer so facetiously gives, but I cannot agree that I “flippantly reject some 80% of language as ‘intuitive associations’”. I simply do not raise the issue of metaphors in language at all. What might be of interest from my perspective is that these metaphors preserve the formal requirements of grammar, i.e., for example, it is okay to say *It's difficult for me to put my ideas into words*, but not *\*It's for me difficult to put my ideas into words*, or *\*It's difficult for me to putting my ideas into words*.

<sup>51</sup> Cf. Section 2.1, where I write: “The earliest generative frameworks (Chomsky 1972) viewed focus as a mere ‘surface’ property defined at PF (surface structure). ... Under this view, focus was coded overtly and superficially” (Tajsner 2008: 73).

<sup>52</sup> Seuren’s book *Chomsky’s Minimalist Program* may find acclaim with the fans of the web sites like *Chomsky watch*, who have desperately needed proof that his great intellectual powers they grip with in the domain of political debate can be questioned and ridiculed in the domain of linguistics. Seuren’s attempt is a failure, and an example of bad academic

that I have to retaliate in the same vain, but in this final part I will show that Prof. Szwedek's review itself does not represent good academic standards (nor professionalism).

First, Prof. Szwedek calls his paper a *review article*, which I think it is not. I would rather stick to a popular view that “[r]eview articles are an attempt by one or more writers to sum up the current state of the research on a particular topic. Ideally, the writer searches for *everything* relevant to the topic, and then sorts it all out into a coherent view of the ‘state of the art’ as it now stands”.<sup>53</sup> Surely, Prof. Szwedek's paper does not accomplish such goals. I would think of it more as a *critical book review*, but then – according to prevailing standards – the reviewer should “be fair to the author. It is not the reviewer's role to be critical in a negative sense. It is to give the reader a balanced view of the book, its arguments, its strengths and weaknesses”.<sup>54</sup> Furthermore, I would think that “[w]riting the critical review usually requires you to read the selected text in detail and to also read other related texts so that you can present a fair and reasonable evaluation of the selected text”.<sup>55</sup>

I doubt whether Prof. Szwedek either read “the text in detail” or “read other related texts”. In the foregoing rejoinder I have pointed out a number of places where the reviewer finds an issue to raise which is explained elsewhere in the text, sometimes just a few pages (or lines) away. Such an oversight is recurring. One more example of this sort is when he criticizes me for using a “confusing convention” in the discussion of examples [22] (*Jack brought flowers to Jill* (, not Billy)). and [23] (*Jack brought flowers to Jill*). Szwedek notes that “... non contrastive [23] does not differ from contrastive [22]”, which he finds to be an instance of confusing notation. However, if he had read the whole passage he would have noticed that before I propose my notation I refer to Kiss's (1998) views, who does not distinguish between contrastive/emphatic and pure identificational foci in English non-cleft sentences, hence the use of [22] is at this point in accordance with her original notation, not mine (cf. Tajsner 2008: 256-257). At some other point he says: “Almost accidentally he remarks that [44] (*Książkę dał Ani Marek*. PT) “fulfils the completive function by providing an answer to a potential *wh*-question: *Who gave Anna a book?*” However, he has

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manners itself, in that Seuren attacks the whole Minimalist Program without making any reference to Chomsky's work in minimalism which appeared after the publication of the *Minimalist Program* in 1995.

<sup>53</sup> This is a part of the definition provided by Life Science Library of The University of Texas at Austin, available at <http://lib.utexas.edu/lsl/help/modules/review.html>

<sup>54</sup> From Allestair Mc Culloch “How to write an academic book review”, available at [http://academicwriting.suite101.com/article.cfm/how\\_to\\_write\\_an\\_academic\\_book\\_review](http://academicwriting.suite101.com/article.cfm/how_to_write_an_academic_book_review) (date of access: 5 June 2010)

<sup>55</sup> As defined by The Learning Centre, The University of New South Wales, available at <http://www.lc.unsw.edu.au/onlib/critrev.html> (date of access: 5 June 2010).

no grounds for calling it an accidental remark in view of the fact that the *completive* function of focus is discussed at length in a few other places in the book (e.g. 4.5.5., 5.7.5, 6.4).

As already noted, one has the impression that the reviewer just scanned the book without properly reading it. This normally would result in misunderstanding or, maybe the cursory impression is just a cover-up for intentional distortion of my views. Consider his remark relating to my statement that "... as commonly assumed, the canonical word order in Polish is SVO, and SVO<sub>i</sub>O<sub>d</sub>". When quoting, Prof. Szwedek intentionally emphasizes words "commonly assumed" and says that "the only source supporting this 'commonly assumed' view is Witkoś (2007) mentioned in a footnote". The reviewer noticed there is reference to Witkoś (2007) in the footnote, but evidently did not care to read the whole note. If he had, he would have understood that Witkoś (2007) is not my source of knowledge on SVO order for Polish, but rather I refer to an earlier polemic between Witkoś and Tajsner on the status of the two post-verbal objects in ditransitive constructions in Polish. In the note I admit that now I accept Witkoś's (2007) view on the ordering of the two objects which I earlier contested.

The reviewer's comments are often unrelated to the text reviewed. He puts forth a thesis, says it is mine, and then comments on it the way he finds suitable. For example, he says "He wouldn't have had to merely assume the similarity of behaviour of focus in English and Polish, and raise the problem of the variety of word order, had he read my papers and books in which I not only speculatively, but also experimentally demonstrated this similarity beyond doubt..." This disregards what I really explicitly state. I apply the same model of derivation for English and Polish, but I do not advance any claims or assumptions about the similarity between the two languages. In fact, I point to a major difference between them; the availability of scrambling in Polish but not in English, which results in two strategies for achieving functional effects, namely "shifting stress" (English) or word order alternations (Polish).

The arsenal of ill-natured means the reviewer adopts is rich. I already mentioned a few: taking statements out of context, cutting the citations in two and dropping their essential parts, imputing to me views and statements which are not mine, exerting his authority and implying he anyhow "knows better", missing the main points and elaborating on peripheral issues. Here is yet one more example of the unfair distortion of my views, seasoned with irony. He writes: "Finally, Tajsner discusses [36] (*Jack saw the movie*) about which he makes 'a crucial assumption' and 'sensational discovery'". This is to imply that the sensational discovery is my observation that "[e]xample [36] represents an instance in which a new informational effect is achieved..." This certainly is not any revelation or discovery, given the preceding discussion in which the point was



raised a number of times. However, if one reads the paragraph to the end it becomes clear that the “crucial assumption” I really want to make at this point is that “surface semantic effects are restricted to narrow syntax, thus any displacement operation leading to a change in information structure has its place in narrow syntax, and not in the phonological component” (Tajsnér 2008: 268). This point is really essential, given my further proposals concerning both “focus shift” and scrambling.

Finally, I think that Prof. Szwedek’s review does not meet formal standards in that it does not present an overview of the whole book. The reader cannot learn from the review either of its size or of its structure. There is no description of the contents of particular chapters. As mentioned earlier, the approach is extremely selective. The book is 379 pages long (excluding references) while the critical comments relate to just 36 pages (less than 10%). The reviewer has concentrated on a few selected parts (especially on the following sequences of pages 12-22, 51-63, 242-257, 264-273 and 321-336). By doing so, he has left 90 percent of the text practically untouched. Of course, the author is not in a position to tell the reviewer which parts of the text he should focus on in his review. The problem is that Prof. Szwedek uses sweeping generalizations about the content of my whole book without addressing 90% of its contents. I have found just one single reference to the contents of chapter two, (page 88), one reference to chapter four (page 242), and no reference at all to chapter three. I understand the reviewer may not be interested in the present state of research with respect to the generative views on focus. Neither need he be interested in my proposals about the implementation of the recent minimalist technology to the analysis of the phenomenon. But if Prof. Szwedek decided to read only the parts of interest to him, I would then expect him to admit he had read some 40 pages of the book, and just browsed the rest.

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