

ASPECTS OF THE RELATIONSHIP BETWEEN  
THEORIES OF GRAMMAR AND THEORIES OF PROCESSING

Juan Carlos Acuña Fariña  
*Universidad de Santiago de Compostela*  
iacanoe@usc.es

The aim of this paper is to analyze the pros and cons of the relationship between theories of grammar and psycholinguistic research conducted in laboratories. It is argued that current optimism about the benefits that psycholinguistic research may bring to those interested in the nature of language may be hard to reconcile with the difficulties there are in synchronising the research agendas of linguistics and psycholinguistics. The main appeal of psycholinguistic research lies in its superior testability standards. Through a brief examination of the simple notion of apposition, it is argued that linguistics is indeed irremediably circular and inconclusive, and that that is why some look to experimental research in language as a way of circumventing the endemic weakness of grammatical theories. However, psycholinguists cannot avoid a dependence on syntactic descriptions in order to formulate their own predictions. This makes for more undesired circularity. Finally, the paper seeks to show that at least some experiments can shed light on grammatical structure. A key issue will be to ponder how psycholinguistic tools can help us meet the challenge of psychological adequacy.

KEY WORDS: linguistics, psycholinguistics, language processing, empty categories, perception predicates, apposition.

## 1. Introduction

At a recent AEDEAN conference held in Granada, Juan Uriagereka expressed his view that the future of linguistics may lie in psycholinguistics, particularly in the realm of sentence processing.<sup>1</sup> As a linguist who has been involved in psycholinguistic research on the processing of language in the last few years, I would like to focus here on whether the Spanish linguist's opinion points in the right direction. In section 2, I discuss the serious difficulties involved in synchronizing the research agendas of linguistics and psycholinguistics. This is a section with a pessimistic orientation. In section 3, by discussing the simple notion of apposition, it is argued that linguistics is irremediably circular and inconclusive, a fact that might make psycholinguistic research a more desirable option. Section 4 shows how experimental research on language can shed light

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on issues pertaining to the elucidation of grammatical structure, issues that are usually the preoccupation of theoretical linguists. Section 4 is therefore more optimistic. Most of the pros and the cons of a shared agenda that I will refer to will involve research on empty categories, some of it still in progress. A key issue will be to ponder if, and especially how, the challenge of psychological adequacy, as posed by linguists (Dik 1978; Dik 1991; Kaplan and Bresnan 1982; van Valin and Lapolla, 1997), can be met using the tools of psycholinguistics.

## 2. Psycholinguists Needs Linguistic Analyses

Since the Chomskyan derivational theory of complexity was put to the test in laboratory-based studies, and proved wrong (Fodor and Garrett 1967; Johnson-Laird 1974; see Tannenhaus 1988, for a review), till recently, psycholinguists have generally steered clear of any direct dependence on the postulates of linguistic theory (by which they usually mean generative grammar), and linguists, also till recently, have been wary enough to make their analyses dependent on empirical refutation in laboratories. The problem with the distinct agendas is that psycholinguistics cannot afford to turn its back on linguistics. Take the following structure of Spanish:

- (1) Juan se va a vivir finalmente a Londres. *Se lo oí decir a su hermana esta mañana.*  
 a. *Juan finally moved to London. I heard his sister say so this morning.*  
 b. *Juan finally moved to London. I heard him say so to his sister this morning.*

The segment of interest is the italicised part containing the perception predicate *oír* and one of its possible syntactic projections, an infinitival complement clause. The structure in (1) is ambiguous between an interpretation in which *Juan* is the understood subject of the infinitival complement, and another in which it is *mi hermana* that functions as such. The latter is by far the most obvious interpretation, at least to the conscious mind. An obvious psycholinguistic question to ask when faced with the processing of strings like (1) is the following: how is the “gap” in the subject position of the infinitive actually recovered in the on-line time course of processing? The stringent needs of linearization explain why recoverable material is often dropped in languages (Beukema and Verspoor 1991). This explains why the subject of the infinitive is not expressed in (1), but it does not explain what sort of heuristic the mind uses to fill the gap. In the psycholinguistics literature, this kind of “filling” process is unsurprisingly referred to as a *filler-gap dependency*.

Formal theories of parsing have two mechanisms to capture filler-gap dependencies: the Active Filler Strategy, AFS (Frazier 1987; Frazier and Flores d’Arcais 1989; Clifton and Frazier 1989) and the Minimal Chain Principle, MCP (de Vincenzi 1991, 1998). The AFS is aimed especially at *wh*-traces, to use the habitual GB terminology for ellipted constituents (which will be adopted in the remainder of this article). It instructs the parser to “rank the option of a gap above the option of a lexical noun phrase within the domain of an identified filler” (Frazier and Flores d’Arcais 1989: 332). That is, the AFS assumes that the parser can identify fillers like *wh*-elements (displaced constituents after movement) and it mandates that these fillers be assigned to the first empty slot available in a syntactic tree in a left-to-right parse. It is, therefore, filler-driven (as opposed to gap-driven) parsing.

In (1), however, there is no easily identified displaced phrase before reaching the PRO gap, so the AFS is of no use in dealing with the processing of such a gap.<sup>2</sup> This leaves the MCP as the only candidate that formal models of parsing can wield to explain the processing of that kind of filler-gap dependency.

Formal, or principle-grounded models of parsing like the Garden Path model (Frazier and Rayner 1982), and its derivative Construal (Frazier and Clifton 1996), maintain that the first operation conducted by a specifically linguistic module of the mind is to subject the incoming linguistic stimuli to a syntactic analysis done by a specifically syntactic submodule. The first pass is blind to information coming from other hypothesized later ranks, like meaning, pragmatic adequacy, including context fit, and even statistical knowledge of lexical and syntactic co-occurrences. In sum, then, only after a tree has been formed can semantic and pragmatic integration commence, which means that tree-building is mandatory and the fastest processing stage. In its turn, this simply means something obvious which psycholinguists often appear to recognise only reluctantly: namely, that they need linguists to provide them with the trees. And here is the first important problem: trees, more often than not radically disparate trees, abound in the world of linguistics, which means that it is in principle hard for experimenters to make solid claims about the processing of specific syntactic structures. The justification of claiming that syntax is or is not the first filter in the time course of processing is therefore seriously compromised.<sup>3</sup>

In order to understand precisely how a dependence on trees and syntactic analyses affects the routine of research on syntactic processing, let us first examine the specifics of the MCP, before we evaluate how it can be applied to the specifics of the syntactic structure in (1). De Vincenzi (1991: 13) postulates her MCP as follows:

Minimal Chain Principle: Avoid postulating unnecessary chain members at S-structure, but do not delay required chain members.

Following Rizzi (1990), de Vincenzi (1991) defines a chain as a set of co-indexed elements which bear the same thematic role and the same grammatical case. The essence of the MCP is that the parser is expected to prefer postulating a singleton chain to postulating multi-member chains. Thus, it predicts that in the ambiguous Italian construction with PRO in (2):

- (2) Ha chiamato Gianni  
 a. PRO called Gianni  
     "he/she/it called Gianni"  
 b. PRO<sub>i</sub> called Gianni<sub>i</sub>  
     "Gianni called"

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2. In fact, *lo*, as a preposed clitic, is a displaced object, but that need not detain us for the moment since we are concerned with displaced material that can grammatically fill a subject position.

3. The elucidation of the time course of processing is essential to the whole experimental agenda of psycholinguistics, which seeks to reveal the functional architecture of the linguistic mind. This may be modular or interactive, serial or parallel, or mixed, but the only way to cast light on that architecture is to map the time course of all the forces that may reasonably play a part in shaping interpretations. These include syntactic, semantic, pragmatic, lexical, and statistical biases (Carreiras 1997).

the parser will opt for (a) because in (b), assuming a preferred SVO order, the movement of Gianni from the canonical preverbal subject position to the displaced position after the verb must be processed additionally. Notice that (2a)-(2b) involve choosing between two different PROs: (i) a full (-dummy) PRO that retains case and theta-role like any other lexical pronoun, and therefore forms a singleton chain, since it is in a position where it can directly receive both case and theta-role; and (ii) an expletive (+dummy) PRO which forms a chain with the inverted subject to which it transmits case and theta-role, thus incurring a complex chain (Chomsky 1981).

Notice that the single chain preference actually amounts to saying that the parser prefers a non-movement alternative to one involving movement. The reason is computational economy: in multimember chains the parser has to link all the members of the chain before phrasal packaging can commence. De Vincenzi's theory shares the inspiration of current linguistic theorising by Chomsky (1995), that sees movement as an extremely costly operation in terms of grammar as well. It is maybe as well to point out that de Vincenzi's thinking precedes the inception of Minimalism.

Let us now consider how the MCP, as a formal theory of parsing designed to deal with unannounced gaps, can cope with the processing of (1) above. In order to do so, let us dispense with the complexities of cliticization and its word order repercussions initially, and focus on a structure like (3):

- (3) Oigo cantar a María  
*I hear Mary sing*

In principle, (3) admits the following three syntactic analyses, and the three have in fact been proposed (see Hernanz 1999, for a review):

- (4) Oigo [cantar a María]  
 (5) [Oigo cantar] a María  
 (6) Oigo [a María]-[cantar]

The structure in (4) shows the infinitival proposition as a clause, on a par with its finite counterpart *oigo que las campanas tocan*. That in (5) puts the grammar of perception predicates on a par with so-called "verbal periphrases" like *ir a tocar*, *tener que hacer* and so many others typical of the Spanish language. The hyphenated bracketing of (6) shows perception predicates as object predicatives. It intriguingly equates *siempre dejan correr el grifo* with *siempre dejan abierto el grifo*, and, more to the point, *vi desfilar a los soldados* with *vi a los soldados cansados* or *vi cansados a los soldados*.

Notice from (7)-(9) below, however, that the understood subject of *sonar* is in the accusative case, as if it were an ordinary DO of *oír*. It would appear that a control analysis might solve that:

- (7) Oigo PRO cantar a María  
 (8) Oigo a María PRO cantar  
 (9) *La oigo PRO cantar* (compare object control in: *Juan animó a Ana a PRO ir/Juan la animó a PRO ir*)

In that case, to (4)-(6) we must add another analysis, as in (10) below:

- (10) Oigo [a María] [cantar]

However, many insist that that analysis is incorrect. They argue that *oír* selects just one argument (*oír las campanas* or *oír que las campanas suenan*, or *oír a María*), not two. In control structures like (11)-(12) the intervening NP/PP and the infinitive do not make up a constituent (a clause, as in [4]), which means they take two different places in the tree. Yet that may be good for classic control verbs like *obligar*, *invitar*, or *prohibir*:

- (11) Vamos a invitar [a Ana] [a cenar/a la cena]  
*We are going to invite Ana to have dinner/to dinner*  
 (12) Obligaron [a Ana] [a venir/a que viniera]  
*They made Ana come here/“They made that Ana came here”*

but questionable for *oír* or *escuchar*:

- (13) Oigo sonar las campanas/\*[el sonido] [las campanas]  
*I hear the bells toll/the sound of the bells*  
 (14) Escuchamos las noticias/\*/?[las noticias] [a Juan]  
*We listened to the news/the news from Juan*

So critics of the control theory of perception predicates prefer to think that the accusative feature of the postverbal constituent comes from Exceptional Case Marking (ECM): there is a “restructuring” of the barrier between main and dependent verb, such that the barrier in question becomes weak, resulting in the higher verb trespassing the domain of the lower one and governing its subject. Since, relative to the higher verb, that subject is in an object position, it gives it Acc. case. That view is compatible with (4) above. The label “Exceptional” clearly indicates the extraordinary nature of such a syntactic analysis. Be that as it may, not even the ECM explanation can account for the fact that real, unidealized, linguistic data show that, sometimes, at least some perception predicates may allow two phrasal complements, especially if a clitic appears in preverbal position:

- (15) Vimos ensayar a Shakira/Le vimos [el ensayo] [a Shakira]  
*We saw Shakira rehearse/“We saw the rehearsal to Shakira”*  
 (16) Oímos cantar a Juan/Le oímos [el disco] [a Juan]  
*We heard Juan sing / “We heard the record to Juan”*  
 (17) Le vimos [el coche nuevo] [a Pep]  
*“We saw the new car to Pep”*  
 (18) Le oímos [esa historia] [a Rosa] ya en otra ocasión  
*We heard that story from Rosa on another occasion*

Seen in isolation, such cases reveal a path that, together with the *Acc* marking, leads to a control analysis again. Seen in combination with the cases that do not admit the two phrasal complements, however, they simply cast a complex film in motion, instead of a frozen picture. However, to go back to (1), seen from a psycholinguistic perspective, the two radically different analyses have unfortunately very little impact on differential psycholinguistic predictions. Indeed, it would be ideal to untie such theoretical knots of linguistics using the precision of psycholinguistics, a discipline capable of superior testability standards. At first sight, it may seem we can do just that. One might argue that

if an ECM analysis is assumed, and *a mi padre* (not *Juan*) is to be generated by the mind as the subject of the infinitival clause, then *a mi padre* must be repositioned in the subject position of the infinitive, which means there must be a movement transformation and, therefore, a complex chain. This in its turn means that slower latencies should be registered for that interpretation (which is, however, the preferred interpretation). Conversely, if a control analysis is preferred, then since the very ambiguity of the control co-indexing (both *Juan* and *a mi padre* can actually control the subject of the infinitive) shows this to be a case of non-obligatory control, *a mi padre* need not c-command PRO, which in its turn means that no transformation would be needed and that complex, time-consuming chains ought not to be expected. In short, that the psycholinguistic route to the right interpretation (*a mi padre* controls the PRO) is clear. Notice that, interpreted in this fashion, if an ECM approach is taken, then the MCP is wrong because the complex-chain interpretation is indeed the preferred interpretation. Conversely, if non-obligatory control is assumed, then the MCP is right because the right interpretation can be formed without resorting to complex chains. It is tempting to conclude that psycholinguistic evidence backs up one syntactic analysis (control) over another (ECM).<sup>4</sup>

Sadly, however, the situation is more complicated. If a theory of transformations is assumed when making use of such notions as ECM or Control, then transformations should not be forgotten when they apply for independent reasons. The fact is that not even under a control analysis can one dispense with movement if the right interpretation (*a mi padre* is the one saying something) is to be given a syntactic form. The reason is that, independently of the gap-filling process, *a mi padre* must move anyway to occupy the position of object of the object-control matrix verb *oir* before it can control the subject of the subordinate verb *decir*:

- (19) Se lo oí [OBJECT GAP] [SUBJECT GAP] decir  
 1. a mi padre    2. a mi padre

Seen in this light, the only difference between an ECM analysis and a control one is that while the former needs movement to reposition a subject, the latter needs movement to reposition an object. It might be argued that the ECM alternative is more economical since it directly relates the moved constituent to the gap, instead of relating it to a position from which it can control the gap. However, there is nothing in the MCP that makes room for such considerations: the MCP simply states that movement implies complex chains and that these are costly in the time course of processing because they delay phrasal packaging.

It was suggested in passing that possibly the best view of a research agenda shared by linguistics and psycholinguistics would involve the latter using its objective experimental tools to confirm or disconfirm the former's analyses. That is in effect still the dream of psychological adequacy and, one may surmise, the reason for Uriagereka's words at the AEDEAN conference mentioned in the introduction. However, it has just become clear that such a combined research programme is not an easy task. We have seen how, faced with two perfectly distinct theoretical analyses, the MCP is unable to provide a route to discrimination. It is also true, however, that in the last few paragraphs we have

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4. Notice, however, that such an approach turns *a mi padre* into a suitable candidate for control, but it does not rule out *Juan* as controller, as would be desirable.

circumscribed our syntactic analysis of perception predicates to ECM and control.<sup>5</sup> In fact, the reader will recall, the situation is rather more complex. The predicative analysis suggested in (6) should by no means be discarded. And something equivalent to the “verbal periphrasis” analysis receives support from modern syntactic thinking in terms of *avalanches* (MacWhinney 2001) as strings of words which usage congeals or precipitates together. Not too different from such an approach might be another view of perception predicates and their complement clauses as *constructions*, in a manner suggestive of a construction grammar (Goldberg 1995; Croft 2001). All these latter interpretations are aided by the fact that perception predicates are indeed a very restricted set of verbs incurring a high degree of specificity.

These other syntactic approaches are reminiscent not so much of a psycholinguistic theory like the MCP, but of another one like Tuning (Mitchell, Cuetos, Corley, and Brysbaert 1995). Tuning is another syntactocentric psycholinguistic processing model which, unlike Garden Path or Construal, bases its reliance on tree-forming as the first processing stage not so much on intrinsic geometrical properties of the trees themselves, but on their frequency of occurrence. Being an exposure-based model, it merely claims that, in the face of an ambiguous segment, the mind will opt preferentially for the syntactic structure that is more frequently used by actual speakers and readers. In the case at hand, a possible research project might involve checking the implications of a construction/avalanche approach to the grammar of perception predicates against a corpus analysis of that group of predicates. If such an analysis were to prove that at least the most common perception predicates (like *hear*, *listen*, *look* or *see*, as opposed to *behold* or *contemplate*, for instance) do occur more often with infinitival complements than with other kinds of complements, then the construction/avalanche approach would appear to be intriguingly appropriate since it is free to emphasize holism and de-emphasize internal structure. That is just what is needed to dispense with hypothesized derivations (re-orderings) which must tidy things up to (hypothesized) perfection. If (1) is seen as an avalanche, then it is “generated” holistically with *a mi padre* in a position which need not be syntactically reconducted to a more abstract level of syntactic organization. Seen in MCP terms (which would not be the right terms any more), *a mi padre* would act as object of the higher verb and subject of the lower one *in situ*, and that could explain why it is preferred as a subject, instead of *Juan*.<sup>6</sup>

Precise psycholinguistic data showing differential processing of very common perception predicates relative to rather uncommon ones would strengthen such a line of thinking. We are currently working on that, but unfortunately, whatever the result of that psycholinguistic work, the fact remains that proponents of ECM or GB-style control can

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5. Felser (1998) provides a minimalist analysis of English direct perception predicates in terms of a functional projection Aspect that allows a control relation to be established between the time expressed in the perception predicate and the time of its non-finite complement. She speaks of “event control.”

6. Also Lexical Functional Grammar and Head Driven Phrase Structure Grammar might in principle dispense with the need for syntactic reconstruction of the controlling phrase, since in these models control does not have a syntactic reflex and is relegated to other levels of grammatical organization such as argument structure. On the implications of handling several distinct strata of linguistic organization see a little ahead in this section.

simply claim that their theories would only be falsified empirically *if one assumes the MCP*. But what if it is the MCP that is wrong? One cannot help noticing that, some thirty years after the derivational theory of complexity was deemed psychologically unreal by psychologists who—sharing, or stimulated by Chomsky’s ideas—were eager to prove it right, now two Chomskyan notions of linguistics are once again at odds with a Chomsky-inspired theory of language processing again. Notice that both now and then a trademark of Chomskyan thinking, *reconstruction*, is at stake. I cannot offer a solution to the way in which the grammar of perception predicates and theories of processing might fully converge. For now, I merely make the point that the shared agenda is a real challenge, and that something like the grammar of the interaction between theories of language and theories of processing still awaits a more enlightened day. The crux of the problem seems to be a double vicious circle: on the one hand, psycholinguistics needs syntactic analyses to make solid claims about the psychological reality of linguistic structure, but on the other: (i) there are too many syntactic analyses to choose from; and (ii) when experimental research in laboratories backs up one or another syntactic analysis, one can never be sure that the conclusions reached do not reflect psycholinguistic theory bias (like the belief in a preferred SVO order, or the belief in reconstruction as a derivational process accomplished by the mind). In section 3, I suggest scattered glimpses of a light at the end of this tunnel.

### 3. Circularity in Linguistics

One might still think, however, that the problems of convergence between linguistics and psycholinguistics that we have referred to above are due to the complex grammar of perception predicates, which makes so many different analyses possible and thus complicates ties with a neighbouring discipline such as psycholinguistics. Maybe it is not a good idea to start off on a road of cooperation between the two disciplines precisely by tackling the most challenging obstacles. Physics, after all, did not start with the quark but with coarser matter. The problem with this optimistic approach, however, is that linguistics *does* seem to be irremediably circular and inconclusive, no matter the structure one wishes to focus upon. As evidence of that, let us give careful consideration to that in (20), arguably the most basic kind of syntactic relation there is:

(20) *Chomsky’s bare syntax, the latest development in the model, is meeting with mixed reactions in the literature*

Essentially, the structure in (20), which traditional grammar has labelled *apposition*, involves the mere juxtaposition of two NPs, so dealing with it obviates the need for hypothesized complex syntactic reconstructions or any other seemingly complicated “generative” process. If grammar could be seen as a complex building, with Control Theory or ECM a high-tech department in it, apposition would just be whatever ensemble would be formed by placing one brick beside another. One would imagine that explaining a structure like (20), therefore, is not such a tall order. Furthermore, the number of possible arguments that linguists have traditionally used to account for it is uncharacteristically low. Let us consider them briefly.

For some (Fries 1952: 187; Roberts 1966; Bogacki 1973: 19), what distinguishes

appositive structures like (22) from other syntactic constructs is that (20) is made up of two juxtaposed NPs which are co-referential. Others (Hockett 1955; Sopher 1971; and Burton-Roberts 1975) believe that what is truly remarkable about (20) is the fact that it is constituted by two non-coordinated constructions which are functionally equivalent, irrespective of their class membership. Still others (Norwood 1954: 270; Hadlich 1973: 234; and Dupont 1985: 41) believe that the appositive status of (20) rests crucially on the obligatory presence of intonational boundaries. A fourth group of linguists (Pignon 1961: 252–57; Mathesius 1975: 90–94; and Koktová 1985) have emphasized the predicativity that underlies the pattern. According to this view, an appositive would be a second-level predication which, detached from the sentence proper, affects a nominal constituent of that sentence. Finally, a fifth, and to our purposes here, last feature of paradigmatic appositions like (20) is the fact that they allow insertion of so-called apposition markers like *or*, *namely*, or *viz*. Introduce a marker between the two NPs and, if it fits, an apposition has been detected (Sopher 1972; Burton-Roberts 1975).

The problem with appositive structures, however, is that by choosing different parameters as criterial for apposition linguists have ended up classifying as appositive a large list of seemingly (and surely) different constructions (Meyer 1992; but see also Kortman 1994; Acuña 1999; Acuña 2000). For instance, the following have all been regarded as “more or less appositive” by Meyer (1992):

- (21) *England international left-winger Eddie Holliday* will never touch “bubbly” again
- (22) The Association of Head Mistresses warmly welcomed *the Newsom report*—“*a vital and moving human document*” . . .
- (23) Waterlogging leads to *three kinds of changes in the soil: physical, biological and chemical*
- (24) Well, *it’s a jolly nice place, the new university* . . .
- (25) He spoke of *his desire to promote the abolition of slavery by peaceable means* . . .
- (26) We might, therefore, ask *two things about a new theory of a change of state:*
  - (a) *Does it give a satisfying physical picture of what is probably happening?*
  - (b) *Is the numerical agreement with the observed facts in keeping with the number of adjustable parameters, or is the theory unduly “forced” in this respect?*
- (27) She was getting herself so excited at *the thought of my auntie Elsie coming and knitting and knitting and knitting and driving her mad.*
- (28) We were willing to consider analogies but don't forget that the analogies between teaching *something relatively simple like late medieval poetry was very different from Anglo-Saxon*
- (29) People are beginning to *rouse themselves, stretch, you know*

Examples (21)-(29) far from exhaust the range of so-called appositive structures. Other possible patterns include “tag exclamations” (Quirk et al. 1985: 1304), or, as they are termed in French grammar, “dislocations qualitatives” (Forsgren 1988: 140 ff.; and Milner 1976: 153 ff.), as in:

- (30) *He* confessed, *poor lamb*, that he had been misled

Or the pattern illustrated in (31):

- (31) *Orateur remarquable*, *Atticus* était un écrivain médiocre (Tesnière 1976 : 165; also Sopher 1971 : 403)

Intrigued by the elusive nature of such a simple-looking structure, I have traced linguists' theories of it (Acuña 1996; Acuña 1999; Acuña 2000). The reader will surely be surprised to find out that they span every possible theoretical option:

1. Apposition as juxtaposition of coreferential NPs: Fries (1952), Roberts (1966), Bogacky (1973).
2. Apposition as coordination (hence symmetric): Hockett (1958), Allerton (1979), Brown and Miller (1980).
3. Apposition as dependency (hence asymmetric): Poutsma (1904), Curme (1947), Hadlich (1973).
4. Apposition as a third kind of syntactic relationship (defined in various ways, but different from coordination and subordination): Hockett (1955), Sopher (1971), Delorme and Dougherty (1972), Bitea (1977), Koktová (1985).
5. Apposition as a spectrum, exhibiting features of both coordination, dependency and sheer peripherality: Matthews (1981), Quirk et al. (1985), Meyer (1992), and, to a certain extent, also Burton-Roberts (1993).
6. Apposition only explainable through a mixture of levels of description: Paula-Pombar (1983; coordination at a deeper level, but apposition at the surface), Dupont (1985; coordination at a deeper level, but dependency at the surface), and Martinet (1973; both coordination and dependency at the surface).
7. Apposition is not a syntactic relationship at all: Zemb (1968), Longrée (1987), to a certain extent also Burton-Roberts (1975, 1993), Huddleston and Pullum (2002).

Matthews (1981: 236) points out that the best way of explaining the elusiveness of the notion is to recognise the features of paradigmatic cases like (20) and be willing to assume that, in a Wittgensteinian way, the paradigm relates to a network of other structures “by various forms of [family] resemblances,” and that “(w)here the resemblances end is naturally indeterminate.” Currently, the true structural nature of apposition *vis-a-vis* other structural constructs with which it appears to be at least intuitively related remains obscure. Burton-Roberts (1993: 184), sees unresolved connections between apposition and no less than: (i) conjunctive (*and*)-coordination; (ii) disjunctive (*or*)-coordination; (iii) restrictive modification; (iv) subordinative verbless absolute clauses (e.g. *A shy man, Olsen seldom speaks*); (v) attributive NPs; (vi) relative clauses; (vii) complement *that*-clauses; and (viii) parentheticals. Huddleston and Pullum (2002: 1350) have recently placed them beside supplementative structures of both an integrated and a parenthetical kind. Indeed the concept is reminiscent of a radial category (Lakoff 1987).

In the face of such sheer failure to fully understand the simplest of notions pertaining to syntactic structure, it is easy to see why some linguists look to what is done in labs to reassure themselves that language *can* be studied scientifically, and that solid conclusions *can* be reached. Others simply prefer to shield themselves under the protection of linguistic theory bias.

#### 4. Where Psycholinguists Can Help Linguists to Study Linguistic Structure

Consider the structure in (32) now:

(32) That boy must weigh *ten tons*

*Ten tons* cannot become a subject through passivization (*\*Ten tons must be weighed by that boy*), it does not agree with the subject, and it does not enter a paradigmatic contrast with adjectives. This means it is neither a prototypical object in a transitive structure (as *the boy* is in *I weighed the boy*) nor the prototypical subject complement in a copulative structure (as in *I am the boy*). Its obligatoriness (*\*That boy must weigh*) indicates that it is not a prototypical adjunct either. What is it? Psycholinguistic research has shown that NP arguments are processed more quickly than NP adjuncts. For instance, using self-paced reading and eye-tracking, Kennison (2002) compared sentences containing NP arguments and adjuncts preceded by biased transitive verbs or by biased intransitive verbs (e.g. *Meredith read/performed every play/week*). The results of her experiments showed that, following biased transitive verbs, NP arguments were processed more quickly than NP adjuncts. However, when NPs followed biased intransitive verbs, there was no significant difference between arguments and adjuncts. One may imagine that applying the same methodological tools to the likes of *ten tons* in (32) above might be of interest. Consider (33) now:

(33) I finally cut the branch of the tree that was occluding our view of the sea

This sentence contains what is perhaps the most widely studied (but still unresolved) syntactic pattern in the history of psycholinguistics: a relative clause which follows a complex NP. The structure is ambiguous because any of the nouns in the complex NP can attract the RC. The most logical hypothesis about the processing of strings like (33) was that the computational economy that recency grants should make of *tree* the preferred host for the RC (Frazier and Rayner 1982; Frazier 1987). However, in a pioneer study, Cuetos and Mitchell (1988) showed that Spanish readers preferred anti-economic high adjunction to *branch*. Since then, a host of experiments, languages, and theories have flooded the specialised journals, with the general tendency found to be a high adjunction preference. Gilboy, Sopena, Clifton, and Frazier (1995) compared Spanish (general high attachment preference) and English (general low attachment preference) complex NPs and reasoned that their internal structure might have something to do with the disparate results observed in different experiments. In particular, they argued that theta-marking (i.e. predicative) prepositions like *with/con* (expressing accompaniment) or genitive *of/de* (expressing possession) might behave differently from non-theta-marking prepositions, which are mere case assigners like *of/de*. In that pioneer questionnaire study, they confirmed their predictions. Later, research in French and Italian (Frencke-Mestre and Pynte 2000a; Frencke-Mestre and Pynte 2000b), corroborated those results using an eye-tracking methodology. The results served proponents of Construal to advance one of the pillars of their theory: the processing of modifiers (secondary relations) is done within the current thematic processing domain, this being the extended maximal projections of the last theta-assigner (Gilboy et al. 1995: 134). That is in itself a psycholinguistic hypothesis couched

directly in very precise linguistic terms. But equally importantly, one wonders, why not use inverse logic and check for either the predicative or non-predicative nature of particular prepositions by analysing the scope of their nouns' RC attachment preferences.

To conclude, let us turn back to the question of empty categories and consider the speed with which the right antecedent of PRO is activated. That is surely a psycholinguistic question to ask, but I hope to show that it offers interesting angles for theoretical linguists as well. The first thing that strikes one's attention is that measuring antecedent activation depends on the particular methodology employed, as different results have been obtained with different methodologies. A pioneer experiment by Nicol (1988) found no trace of fast activation using cross-modal priming.<sup>7</sup> However, more recently, Demestre et al. (1999) used electrophysiological measures like Evoked Response Potentials (ERP) while Betancort, Carreiras and Acuña (forthcoming) chose eye-tracking to show that PRO gaps activate their antecedents extremely fast.<sup>8</sup> For instance, with experiments involving auditory stimuli like:

- (34) Pedro/\*María quiere PRO ser rico (masc)  
*Peter/Mary wants to PRO be rich*

Demestre et al. reported an early negativity with prominence at anterior and central sites followed by a centroparietal positivity in the violating condition, that is, a typical biphasic pattern in syntactic, as opposed to semantic, anomalies (Osterhout 1997; Coulson, King, and Kutas 1998). That showed that the brain reacts to NP-adjective gender agreement violations with intervening PROs quite quickly after the critical stimulus (the adjective), which in its turn means that PRO must have been properly co-indexed very promptly. After the first priming results, Sag and Fodor (1995) had reasoned that since PRO, but not *wh*-trace, had shown slow activation, it is unlikely to figure in syntactic trees. Since, additionally, theories of grammar differ in the way they conceive of empty categories, they concluded that the priming tests provided indirect evidence in favour of theories like HPSG or LFG (also RRG, one may guess) where control ties are dealt with by

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7. Cross-modal priming (Swinney 1979) combines auditory and visual perception. Subjects read sequences while sounds reach their ears through headphones. The sounds (both words and non-words) occur at pre-specified portions of the reading process. If, relative to a baseline, fast time reactions are detected, then it is assumed that the words heard must have been primed or pre-activated.

8. ERPs are recordings of electrical activity in the brain. ERP research is especially adept at revealing the time course of processing and the locus of the reaction in the brain. ERPs have a positive (P) or negative (N) wave form. Syntactic anomalies are usually registered in front parts of the scalp through a negative component of the wave at around 125/200 milliseconds after the onset of the anomaly (the so-called N125 or N200 effect). Semantic anomalies also typically peak negatively at around 400 mscs (the so-called N400 effect) in centro-parietal regions. Serious breakdown in the reading process or reprocessing usually reflect a P600 effect (positive wave, 600 mscs after the critical stimulus). Eye-tracking is done with a device that is precise enough to map the position of the eye relative to each character on the screen millisecond by millisecond. This methodology is the most widely used in syntactic processing. It provides probably the best time course of the reading process: first pass, second pass, regressions to right and left, total reading time, and a myriad of other possible measures.

a level of structural organization equivalent to argument structure, or conceptual structure, not by syntax (Osterhout and Swinney 1993). Since the relationship between fillers and gaps is presumably a semantic one of coreference, Nicol and Swinney (1989) and Fodor (1989), among others, made the point that the late activation results obtained in the priming experiments reveal that, if something involves reference, then it is after all semantic, and if semantic, then it should occur late, only after phrasal packaging has been completed.

The way such syntactocentric thinking is expressed in processing terms is surely interesting in itself. However, in view of the more recent fast activation findings obtained with ERP and eye-tracking, it seems that a very profitable line of research into such issues as syntactocentrism or the different representational levels that must accommodate the different empty categories reduces at least in part to a matter of methodological adequacy. That is no doubt a far cry from quintessentially inconclusive linguistic theorizing. After all, it is easier to discriminate among competing methodological tools than among competing (and perpetually idealized) syntactic analyses. Although this is not the place to resolve such matters, there can be little doubt that ERP, eye-tracking and Functional Magnetic Resonance Imaging (fMRI) tap into syntactic processing more naturally than any other existing methodology.<sup>9</sup> Priming and probe recognition tasks are, however, best suited to capture lexical processes (which are very probably processed independently in parallel; Swinney 1979). In the short term, this means that PRO cannot be ruled out from phrase markers and that, contrary to Sag and Fodor's line of thinking, the theory that makes use of this notion has not been proved wrong.

Even if, as recent evidence suggests, PRO has a role to play in syntactic computations, it is not clear what kind of a role this is. Investigating that is another interesting project. A recent ERP experiment conducted by Featherston, Gross, Munte, and Clahsen (2000) revealed that raising (as in *Pep seems to be fine*, where *Pep* is raised from the position of subject of *be fine* to the actual position of subject of the main verb *seem*, leaving a trace behind, according to standard GB thinking) is harder to process than control (as in *Pep tries to be fine*, where the subject of the main verb *tries* originates already there and that of *be fine* is a coreferential covert PRO, which means there is no trace left behind and no transformation either). That result awaits confirmation, but Featherston et al. propose that raising is harder to process because it involves untying the workings of a movement transformation (de Vincenzi 1991; see section 2), whereas in control there is no need to do anything of the kind as PRO and the surface main clause subject do not move anywhere. An intriguing contradiction seems to lurk here: how is it possible for control, a presumably semantic issue involving coreference, to be easier and faster than presumably syntactically-governed raising? It is customarily assumed that semantic representations ought to "limp slightly behind" syntactic ones (Fiebach, Schlesewsky, and Friederici 2001: 328; see also Fodor 1983). This means that: (i) such an assumption is more questionable than was thought; or (ii) that there is an alternative explanation for the rapidity of long-distance control ties.

That explanation might take the following form. Bearing in mind that, since Rosenbaum (1967), virtually all linguists of all persuasions have agreed that control is

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9. fMRI reveals activity in the brain by registering magnetic fields around the skull. It is characterised by an excellent topographic resolution.

lexically regulated (with perhaps the notable exception of Hornstein 1999 and Boeck and Hornstein 2003, who insist that it is *raising*; see Culicover and Jackendoff 2001 for a review), and that there is no reason to believe that lexical specification partakes more of semantic structure than of syntactic structure, one might say that lexical specification and syntax are interfaced. The nature of that interface reveals itself both in linguistic and psycholinguistic theorizing. Construal (Frazier and Clifton 1996), for instance, rests fundamentally on a lexically-specified distinction between primary and non-primary syntactic constituents. It is true that construal aims to capture adjunction liaisons, but there is clearly not much of a difference between a lexical dictum that constituent Y must first and foremost be adjoined (that is “related”) to constituent X, and another lexical dictum that constituent Y must first and foremost be cross-referenced with (that is “related” to) constituent X. Construal itself shares the philosophy of virtually all existing theories of grammar of viewing the lexicon as the platform from which syntax is projected. That is evident even in syntactocentric approaches to the nature of language. Generative grammar, for instance, has moved progressively away from the strong autonomy of strict overgenerating syntactic rules in the 60s and 70s, to the lexicon and lexically-projected X-bar syntax in the 80s and early 90s (GB), and from these even further away from such a radically syntactocentric view of language to an emphasis on interface levels into the new millennium (Chomsky 1965; Chomsky 1981; Chomsky 1995). The distinction in RRG between *core* and *periphery* is another way of endorsing the same view. In short, finding evidence of fast activation of PRO antecedents does not automatically entail that semantics takes the wheel in processing; rather, it may simply mean that lexical specification and syntax are more intertwined than is usually believed by some, and that, contrary to what others think, since true semantic processes are surely processed late, lexical specification is *form*.

But again, more important than the direction that this debate may take (for present purposes) is the fact that fusing linguistic and psycholinguistic theorizing makes the debate both richer and more amenable to empirical refutation. After all, if it makes little sense to design, say, cars and motorcycles with no roads and motorways in mind, why design grammars that must be “portable” in brains with no brains in mind? In section 1, it could be seen that synchronizing the research agendas of linguistics and psycholinguistics is not an easy task. But in view of the seemingly irremediably inconclusive nature of linguistic studies, of the little solid, tangible residue left after two thousand years of circular theoretical speculations, and of the first tangible glimpses provided by empirical research conducted in laboratories, it seems that Uriagereka’s words at the AEDEAN conference might just be pointing in the right direction.

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