

## DOCUMENT RESUME

ED 454 714

FL 026 771

TITLE Revista de Documentacao de Estudos em Linguistica Teorica e Aplicada (DELTA): Novos Estudos em Gramatica Gerativa (Journal of Documentary Studies in Theoretical and Applied Linguistics [DELTA]: New Studies in Generative Grammar).

INSTITUTION Associacao Brasileira de Linguistica, Sao Paulo.

ISSN ISSN-0102-4450

PUB DATE 2000-00-00

NOTE 255p.

PUB TYPE Collected Works - Serials (022)

LANGUAGE English, Portuguese

JOURNAL CIT Revista de Documentacao de Estudos em Linguistica Teorica e Aplicada; v16 spec iss 2000

EDRS PRICE MF01/PC11 Plus Postage.

DESCRIPTORS \*Applied Linguistics; Diachronic Linguistics; Foreign Countries; French; \*Grammar; \*Linguistic Theory; Negative Forms (Language); Phonology; Phrase Structure; Portuguese; Romance Languages; Speech Communication; Tenses (Grammar); Uncommonly Taught Languages; Verbs

IDENTIFIERS Brazil

## ABSTRACT

This issue contains the following articles: "Resumption and Last Resort" (Joseph Aoun); "Existentials, A-Chains, and Reconstruction" (Norbert Hornstein); "How Long Was the Nineteenth Century" (David Lightfoot); "Formal Features and Parameter Setting: A View From Portuguese Past Participles and Romance Future Tenses" (Lucia Lobata); "Revisiting Universal Grammar" (Jurgen M. Meisel); Wh-Extractions and Relative Clauses in Brazilian Portuguese" (Esmeralda Vailati Negrao); "On the Nature of Licensing Conditions of N-Phrases in Portuguese" (Joao Andrade Peres); "Some Remarks on the Diachrony of French Negation" (Ian Roberts); and "Some Thoughts on Economy within Linguistics" (Juan Uriagereka). References are found at the end of each article. (KFT)

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revista de

D Documentação de  
E Estudos em  
L Lingüística  
T Teórica e  
A Aplicada

Novos Estudos  
em Gramática  
Gerativa

Homenagem a  
Mary Kato

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Vol.16 - N.º Especial - 2000

# DOCUMENTAÇÃO DE ESTUDOS EM LINGÜÍSTICA TEÓRICA E APLICADA

D.E.L.T.A., Vol. 16, Nº ESPECIAL, 2000

Revista publicada com o apoio oficial da ABRALIN - Associação Brasileira de Linguística

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Programa de Pós-Graduação em Lingüística Aplicada e  
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D.E.L.T.A.: Revista de Documentação de Estudos em Linguística Teórica e Aplicada.

Vol. 1, 1/2 (fev/ago 1985)

São Paulo: EDUC, 1985

Semestral, no. Especial desde 1992

Revista da Pontifícia Universidade Católica de São Paulo/PUC SP e  
da Associação Brasileira de de Linguística/ABRALIN

Resumo em Português e Inglês em todos os artigos

1. Linguística Teórica - periódicos. 2. Linguística Aplicada - periódicos. I. Título:  
Revista de Documentação de Estudos em Linguística Teórica e Aplicada. II. Pontifícia  
Universidade Católica de São Paulo III. Associação Brasileira de Linguística

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ISSN 0102-445

CDD 405

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Os textos publicados na revista são indexados no LLBA (Linguistics and Language Behavior Abstracts), Linguistics and Language Behaviour Abstracts Database, no MLA (Modern Language Association) Directory of Periodicals e International Bibliography, no Sociological Abstracts, no ULRICH's International Periodicals Directory e no ERIC.

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Novos Estudos  
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Homenagem a  
Mary Kato



Vol. 16 - N.º Especial - 2000

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A Revista D.E.L.T.A. publica estudos de caráter teórico ou aplicado, oriundos de qualquer área referente ao fenômeno lingüístico, desde que se trate de contribuições inéditas.

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Os originais apresentados não devem ter sido publicados ou submetidos simultaneamente a outra revista.

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## UMA PALAVRA INICIAL

Charlotte Galves e Jairo Nunes me procuraram com um volume organizado por eles e Eduardo Raposo, com contribuições de seus colegas, parceiros de trabalho na Unicamp, para homenagear Mary Kato; consultavam se a D.E.L.T.A. se disporia a publicá-lo. Havíamos terminado de elaborar um regulamento para envolver o Conselho Consultivo e orientar a decisão quando tivéssemos pedidos de números especiais homenageando colegas. O regulamento inclui pronunciamento sobre a oportunidade de tal publicação com base na produtividade do/a homenageado/a, sua influência na comunidade a nível nacional, não restrita a sua instituição, e no desenvolvimento de sua área.

Mas, tratava-se de Mary Kato. Seriam necessárias formalidades para uma decisão nesse caso? Mary não só é uma das criadoras da D.E.L.T.A. mas foi sua Editora nos dez anos de implantação – e é a ‘dona da casa’ permanente. Não só a grande mentora da Revista, mas inspiração de muitos de nós como profissionais, amigos, ou pessoas – e não nos restringimos à equipe da Revista, ou à PUCSP – onde completou sua formação – ou ao LAEL – que por 16 anos ajudou a desenvolver e consolidar – ou à Unicamp – onde contribui há 13 anos. E não esqueçamos seus orientandos de mestrado ou doutorado, muitos deles hoje figuras fundamentais na Lingüística e na Lingüística Aplicada. A Mary Kato, que, diretamente ou com seu entusiasmo e generoso estímulo, tem contribuído com tantos lingüistas brasileiros; Mary Aizawa Kato, que ‘fez escola’.

Mas, o regulamento foi cumprido. Houve, como seria de se esperar, unanimidade entre os Conselheiros consultados, como já havia na comissão executiva e como, estamos certos, ocorrerá com os leitores da D.E.L.T.A.

É portanto, com total satisfação que a D.E.L.T.A. agradece os organizadores e os autores deste volume e aproveita para, ao homenagear essa grande criadora, inaugurar a introdução de títulos orientadores de conteúdo para os números temáticos.

Mary, como é bom ter você em nossa equipe!

Leila Barbara  
Pela D.E.L.T.A.

## UMA HOMENAGEM PARA MARY KATO

(A Festschrift for Mary Kato)

Charlotte GALVES (*UNICAMP*),  
Jairo NUNES (*UNICAMP*) & Eduardo RAPOSO (*UCSB*)

Os organizadores deste volume especial se juntam aos colaboradores e aos editores da *D.E.L.T.A.* para homenagear a Prof.<sup>a</sup> Dra.<sup>a</sup> Mary Aizawa Kato, que há trinta anos, com a obtenção de seu título de Mestre em Letras na Universidade de São Paulo, dava início a uma carreira acadêmica que marcaria profundamente o cenário da lingüística no Brasil.

Escrever uma apresentação da Mary Kato num volume em que se pretende homenagear a sua carreira de investigadora e de professora não é, entretanto, uma tarefa fácil. No caso da Mary, reduzi-la aos usuais critérios de quantidade e qualidade com que se costumam avaliar carreiras seria cometer uma forte injustiça. Não é que estes faltem numa carreira que está ainda, felizmente, em franco desenvolvimento. Pelo contrário. Tanto pelo número expressivo de publicações e orientações (22 teses de doutorado e 41 dissertações de mestrado!), como pelas importantes contribuições que fez em diferentes áreas tanto da lingüística teórica quanto da lingüística aplicada, a carreira da Mary é *impecável*, como fica claro na panorâmica breve que apresentamos abaixo e como se pode inferir facilmente de uma observação mesmo rápida do seu surpreendente *curriculum vitae*. Mas o que faz a carreira da Mary verdadeiramente *excepcional* é, para além de tudo isso, a maneira como ela *abriu a sua vida* generosamente ao seu campo de escolha, a lingüística, e como ela *tocou*, no sentido mais fundo da palavra, a vida profissional e pessoal daqueles que tiveram a fortuna de ver o seu percurso cruzar-se com o dela.

Falar sobre isso é bem mais difícil, porque é falar sobre fatos, acontecimentos e ações que não podem ser individualizados numa linha cronológica como aquela que costuma organizar os itens de uma bibliografia. É difícil, também, porque significa falar daquele acréscimo de subjetividade que dá um outro alcance às contribuições objetivas que um

indivíduo pode dar a um determinado campo, multiplicando os seus efeitos. No caso da Mary Kato, seria falar, por exemplo, de como a UNICAMP se tornou num dos centros mais efervescentes da teoria lingüística contemporânea, por onde lingüistas de todo o mundo passaram e continuam a passar, sentindo-se em casa: seria falar da rede internacional de lingüistas que a Mary soube montar em todo o mundo com profissionalismo e carinho, em benefício da lingüística brasileira; seria falar da sua imensa dedicação profissional e pessoal, sem egoísmos “curriculares”; seria falar de como a Mary soube reunir num mesmo círculo lingüistas brasileiros e portugueses, transcendendo pequenas ou grandes querelas nacionalistas; seria falar de como a Mary é muito mais do que uma simples orientadora para os seus alunos: mas *formadora* e *inspiradora*, no melhor sentido destas palavras: seria falar de como esses mesmos alunos freqüentemente a procuram para orientação ou apoio, muito depois de o terem deixado de ser; seria falar do enorme empenho e energia que continua a imprimir a todo o seu trabalho, e que contagiam aqueles que se encontram ao seu redor: seria falar, finalmente, da sua *generosidade inesgotável*, que nos tocou a todos, e que não poucas vezes ajudou alguns de nós a superar momentos difíceis da nossa vida.

Uma vez que essa enorme lacuna não tem mesmo como ser sanada, passemos então a uma apresentação da carreira da Mary, que será estoicamente sucinta.

Mary Kato concluiu seu doutorado em 1972 na Pontifícia Universidade Católica de São Paulo, com a tese *A Representação Semântica do Artigo Definido*, tendo posteriormente feito vários pós-doutorados em universidades americanas (Harvard University, University of Maryland, University of California at Los Angeles e University of Southern California). Tornou-se Professora Titular do Departamento de Lingüística da PUCSP em 1982 e do Departamento de Lingüística da UNICAMP em 1987. Desde sua aposentadoria em 1996, continua mais ativa do que nunca como Professor Titular Visitante deste último departamento.

Uma das principais pessoas responsáveis pelo surgimento e êxito da revista *D.E.L.T.A.*, Mary foi sua editora de 1985 a 1996, quando passou a fazer parte do Conselho Executivo. Além de sua presença marcante em *D.E.L.T.A.*, Mary é também membro do conselho editorial de outras importantes revistas nacionais e estrangeiras (*Bilingualism*, *Estudos Anglo-Americanos*, *Probus*, *Revista Alfa*, *Revista da ALFAL*, *Revista de Educação da USP*, *The ESpecialist – PUCSP*).

A seriedade, serenidade e bom senso da Mary também se destacaram nas funções administrativas que exerceu (Vice-Diretora da Faculdade de Comunicação e Filosofia da PUCSP de 1981-1985, Coordenadora do Programa de Pós-Graduação em Lingüística Aplicada da PUCSP de 1985 a 1988 e Coordenadora do Programa de Pós-Graduação em Lingüística da UNICAMP de 1992 a 1993). Sua reconhecida competência e imparcialidade têm levado Mary a freqüentemente ser requisitada para prestar assessoria a importantes entidades de fomento à pesquisa e educação (CAPES – presidente de área de 1985 a 1986 –, CFE, CNPq, Comissão de Ensino e Pesquisa da PUCSP, FAPEAL, FAPERJ, FAPESP, FULBRIGHT, INEP, Núcleo de Alfabetização da USP, Núcleo de Pesquisas em Ensino de Língua Portuguesa da UFCE, Secretaria de Educação de São Paulo, Sociedade Brasileira de Cultura Japonesa, SBPC, UFACre, UFAlagoas, UFSCar, Universidade de Londrina).

Desde sua ida para a UNICAMP em 1987, Mary tem concentrado suas pesquisas em teoria sintática. Além de suas inestimáveis contribuições para teoria sintática e para a sintaxe do português, em particular, Mary se notabilizou por patrocinar ativamente vários “casamentos” teóricos. Como grande navegadora de diversas áreas da lingüística e com a sua incrível capacidade de organizar grupos de pesquisa, Mary entusiasmamente começou a desenvolver e orientar pesquisas em trabalhos de interface: com a sociolingüística e com a lingüística histórica (em parceria com o professor Fernando Tarallo), com aquisição de primeira língua (interagindo com o grupo de pesquisa coordenado pela professora Cláudia de Lemos) e com aquisição de segunda língua (interagindo com o professor Jürgen Meisel, da Universidade de Hamburgo). Desses casamentos resultaram inúmeras publicações, teses e dissertações que contribuíram de forma incisiva para o estabelecimento de uma interação não-dogmática entre diferentes áreas e perspectivas teóricas no cenário nacional. Tiveram inspiração intelectual nessa querida madrinha casamenteira toda uma geração de promissores jovens talentos em lingüística.

Não pode ainda deixar de ser mencionado, como parte da incrível capacidade da Mary de trabalhar em conjunto com outras pessoas, sua expressiva participação no projeto NURC, coordenado pelo professor Ataliba de Castilho, em que atuou como coordenadora do grupo de Sintaxe II e organizou o volume V da *Gramática do Português Falado*. Atualmente, Mary coordena juntamente com o professor João Peres o

projeto *Português Europeu e Português Brasileiro: Unidade e Diversidade na Passagem no Milênio (PEPB2000)*, que tem como objetivo produzir uma coletânea de estudos sobre variação sintática e semântica entre os dois dialetos.

Os autores das contribuições para este volume são alguns dos pesquisadores em teoria gramatical com quem a Mary tem interagido mais de perto tanto no Brasil (Lobato e Negrão), quanto no exterior (Aoun, Hornstein, Lightfoot, Meisel, Peres, Roberts e Uriagereka). Os autores compartilham com os organizadores deste volume especial a profunda admiração à figura da Mary tanto enquanto lingüista, como enquanto pessoa. Mas a lista de admiradores evidentemente não está nem no começo e esperamos que novas e justas homenagens venham a se juntar a este volume. Até porque a proverbial energia da Mary faz com que todos os adjetivos usados acima já vão parecer esmaecidos quando este volume chegar às mãos do leitor. Entre outras coisas porque a produção bibliográfica da Mary, que segue resumida abaixo, já vai estar desatualizada!

## MARY AIZAWA KATO

### 1. Livros

*Brazilian Portuguese and the Null Subject Parameter* (organizado com E. Negrão). Vervuert, Frankfurt am Main. 2000.

*Aspectos Psicolingüísticos e Sócio-Lingüísticos da Alfabetização* (organizado com F. Tarallo e N. Moreira). Pontes, Campinas. 1998.

*Gramática do Português Falado* vol. V (org.). Editora da UNICAMP/FAPESP, Campinas. 1996.

*Português Brasileiro: Uma Viagem Diacrônica* (organizado com I. Roberts). Editora da UNICAMP. 1993.

*A Concepção da Escrita pela Criança* (org.). Pontes, Campinas. 1988.

*No Mundo da Escrita*. Editora Atica, São Paulo. 1986.

*Estudos de Semântica Aplicada ao Ensino do Português* (org.). Boletim do Curso de Pós-Graduação em Linguística e Língua Portuguesa, ano 1, nº. 1. UNESP, Araraquara. 1985.

*O Aprendizado da Leitura*. Editora Martins Fontes, Campinas. 1985.

*Seminário Multidisciplinar de Alfabetização Linguística: Aquisição de Linguagem* (org.). EDUC, Cadernos PUC 9, São Paulo. 1982.

*Linguística: Aquisição de Linguagem* (org.). EDUC, Cadernos PUC 9, São Paulo. 1982.

*A Semântica Gerativa e o Artigo Definido*. Editora Atica, São Paulo. 1974.

## 2. Capítulos de livros

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**RESUMPTION AND LAST RESORT\***  
(Pronomes Resumptivos e a Condição de Último Recurso)

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*ABSTRACT: This paper discusses the derivation of definite and indefinite relative clauses in Lebanese Arabic. The two types of relative clause are similar in that they require resumptive pronouns and do not exhibit island effects. Based on reconstruction effects, I however argue that definite relatives may be either base-generated or derived by movement, whereas indefinite relatives can only be base-generated.*

*KEY WORDS: Relative Clauses, Resumptive Pronouns, Reconstruction, Lebanese Arabic*

*RESUMO: Este trabalho discute a derivação de orações relativas definidas e indefinidas em árabe libanês. Os dois tipos de relativas são semelhantes na medida em que ambos requerem pronomes resumptivos e não exibem efeitos de ilha. Com base em efeitos de reconstrução, eu argumento no entanto que as relativas definidas podem ser tanto geradas na base, quanto derivadas por movimento, enquanto as relativas indefinidas só podem ser geradas na base.*  
*PALAVRAS-CHAVE: Orações Relativas, Pronomes Resumptivos, Reconstrução, Árabe Libanês*

## Introduction

The goal in this paper is to investigate some properties of resumptive pronouns in Lebanese Arabic (henceforth, LA).<sup>1</sup> I will do so by investigating the behavior of restrictive relative constructions in LA, which are generated with the resumptive strategy.<sup>2</sup>

\* For his comments on an earlier draft of this paper, I wish to thank Jairo Nunes. This paper was completed in the spring of 1996.

<sup>1</sup> Issues related to weak crossover and resumption are discussed in Aoun and Choueiri (1996). Different analyses of weak crossover with constructions involving resumptives can be found in Demirdache (1991, to appear), Georgopoulos (1989), McCloskey (1990), Safir (to appear), and Sells (1984).

<sup>2</sup> The topic of resumption has been the center of numerous studies. I have mainly relied on work done by the following authors: Borer (1983), Cinque (1990), Demirdache (1991), Doron (1983), Eid (1977, 1983), Engdhal (1986), McCloskey (1990), Sells (1984), Shlonsky (1992), and Zaenen, Engdahl, and Maling (1981).

LA distinguishes between definite relative constructions and indefinite ones. Definite relatives are generated with a definite complementizer; indefinite relatives are generated with no complementizer.

- (1) a. Definite relativized DP<sub>i</sub> .. definite complementizer .. resumptive element<sub>i</sub>  
 b. Indefinite relativized DP<sub>i</sub> ..... resumptive element<sub>i</sub>

The following generalizations hold true in LA:

- (2) a. all relative constructions may violate islands.  
 b. definite relatives display reconstruction only when the resumptive clitic does not occur within an island.  
 c. indefinite relatives do not display reconstruction.

Simplifying the features of the analysis, the behavior of the two types of relatives is accounted for under the following assumptions:

- (3) a. movement is available in definite relatives  
 b. movement is not available in indefinite relatives

In definite relatives, movement is triggered to check features of the complementizer. In indefinite relatives, on the other hand, there is no complementizer and nothing forces movement to occur. As such, it does not occur. In other words, in LA, movement in relative constructions appears to be a last resort strategy.

The theoretical implications of the analysis of resumptives will be discussed in the conclusion.

## 1. Restrictive relatives in Lebanese Arabic

As stated in the introduction, restrictive relative clauses in LA fall into two categories: restrictive relatives with a definite relativized DP (definite relatives) and restrictive relatives with an indefinite relativized DP (indefinite relatives).<sup>3</sup>

<sup>3</sup> In this paper, I restrict our investigation of relative constructions in LA to restrictive relatives. I will henceforth refer to them using the general term *relative clause*.



Definite relatives always occur with the complementizer *yalli*:<sup>4</sup>

- (4) a. l-kteeb \*(yalli) f tarayto mbeerifi Daaʃ  
 the-book that bought.1S-it yesterday is-lost.3SM  
 'The book that I bought yesterday is lost.'
- b. tʔaaSaS l-walad \*(yalli) (huwwe) xazzaʔ l-kteeb  
 punished.3SM the-boy that (he) tore.3SM the-book  
 'The boy that tore up the book was punished.'

Indefinite relatives on the other hand cannot occur with *yalli*; as a matter of fact, indefinite relatives have no complementizer:

- (5) a. ʃam fattif ʃa kteeb (\*yalli) Dayyaʃt-o l-yom  
 Asp look.1S for book that lost.1S-it today  
 'I am looking for a book that I lost today.'
- b. ʃam fattif ʃa walad (\*yalli) Darab kariim  
 Asp look.1S for boy that hit.3SM K.  
 'I am looking for a boy that Zeina said hit Karim.'

What unifies these two types of relatives is that they are both formed with the resumptive strategy: in constructions with definite relatives and indefinite relatives, the relativized DP is generally related to a resumptive element that occurs within the relative clause. In non-subject positions,<sup>5</sup>

<sup>4</sup> *yalli* is specific to relative constructions. Sentential complements in LA are introduced by *ʔauno*, as illustrated below:

- (i) l-bənt yalli/\*ʔənoo veebit min SSaff mbeerifi rəʒʃit  
 the-girl that was-absent.3SF from-the-class yesterday returned.3SF  
 'The girl that was absent from class yesterday returned.'
- (ii) ʔaalit l-mʃallme ʔənoo/\*yalli l-bənt l-veeybe rəʒʃit  
 said.3SF the-teacher that the-girl the-absent returned.3SF  
 'The teacher said that the absent girl returned.'

<sup>5</sup> These non-subject positions include complements of verbs as well as complements of prepositions (i) or nouns (ii).

- (i) a. ʃarayt l-kteeb yalli fkiite ʃanno mbeerifi  
 bought.1S the-book that talked.2S about-it yesterday  
 'I bought the book that you talked about yesterday.'
- b. ʃam fattif ʃa-kteeb ʃabbir tlemuiz ʃanno  
 Asp look.1S for-book tell.1S the-students about-it  
 'I am looking for a book to talk to the students about.'

the resumptive element is always realized as a clitic (or weak pronoun) (4a-5a). In subject position, the resumptive element may be realized as a tonic (or strong) pronoun (4b).<sup>6</sup> In what follows, I investigate the resumptive strategy in both definite relatives and indefinite relatives.

### 1.1. Resumption in definite relatives

In definite relatives, gaps are prohibited in all non-subject positions. Instead, the relativized position is occupied by a resumptive clitic (6-7).

- (6) [əfna l-hənt yalli hannət-\*(a) l-mʕallme  
saw.1P the-girl that congratulated.3SF-\*(her) the-teacher  
'We saw the girl that the teacher congratulated.'
- (7) [əfna l-hənt yalli ʔaal kariim ʔanno hannət-\*(a)  
saw.1P the-girl that said.3SM K. that congratulated.3SF-\*(her)  
l-mʕallme  
the-teacher  
'We saw the girl that Karim said that the teacher congratulated.'

As (6) and (7) show, the resumptive clitic is required both in the higher object position and the embedded object position within the definite relative.

- 
- (ii) a. ftarayt l-kteeb yalli btaʕrfe keetbo  
bought.1S the-book that know.2SF writer-his  
'I bought the book that you know its writer.'
- b. fiDrna masrafiyye btaʕrif muxriʒa laila  
saw.1P play know.1SF director-its L.  
'We saw a play that laila knows its director.'

I will illustrate our generalizations using constructions with accusative resumptive clitics. However, these generalizations hold true of resumptive clitics which are complements of prepositions and nouns as well.

<sup>6</sup> In this paper I will deal only with restrictive relatives involving resumptive clitics. The reader is referred to Aoun and Choueiri 1996 and Aoun, Choueiri, and Hornstein 1998 for a discussion of strong pronouns that occur as resumptive elements. For an evaluation of the difference between strong and weak pronouns in null subject languages, the reader is referred to Kato (1999). It should be noted that the analysis argued for in this paper may be extended to cases of resumption involving strong pronouns.

In subject position, a gap generally occurs:

- (8) a. l-mʕallme ʔaaSaSit l-walad yalli Darab laila  
 the-teacher punished.3SF the-boy that hit.3SM L.  
 'The teacher punished the boy that hit Laila.'
- b. l-mʕallme ʔaaSaSit l-walad yalli laila ʔaalit  
 the-teacher punished.3SF the-boy that L. said.3SF  
 ʒazzaʔ l-kteeb  
 tore-up. 3SM the-book  
 'The teacher punished the boy that Laila said tore up the book.'

The gap in subject can be identified as an instance of pro-drop, which is generally available in LA. It can occur, for instance, in the following contexts, which block movement:

- (9) a. **Adjunct Island**  
 laila btaʕrif l-walad yalli l-mʕallme fallit ʔabl ma  
 L. knows.3SF the-boy that the-teacher left.3SF before  
 ʒallaS  
 finished.3SM  
 'Laila knows the boy that the teacher left before he finished.'
- b. **Wh-Island**  
 l-mʕallme ʔaaSaSit l-walad yalli laila btaʕrif miin  
 the-teacher punished.3SF the-boy that L. know.3SF who  
 Darab  
 hit.3SM  
 'The teacher punished the boy that Laila knows whom he hit.'
- c. **Complex-NP Island**  
 ʕarrafna ʕala l-muʕriʒ yalli laila ʕeefit l-masrafiyye yalli  
 met.1P on the-director that L. saw.3SF the-play that  
 ʔaʒraʒa  
 directed.3SM-it  
 'We met the director that Laila saw the play that he directed.'

In (9a-c), a gap can occur in subject position within islands, which are known to disallow wh-extraction.

Similarly, the relation between a resumptive clitic and the definite relativized DP is not sensitive to islands, as illustrated in (10-12).

(10) **Adjunct Island**

fikiina maɗ l-muxri3 yalli fallit laila ʔabl ma (ʃuuf-\*(o)  
 talked.IP with the-director that left L. before see.3SF-\*(him)  
 'We talked to the director that Laila left before she saw him.'

(11) **Wh-Island**

tʃarrafna ʃala muxri3 l-masrafiyye yalli saʔalit laila ʔəza  
 met.IP on director the-play that asked L. whether  
 ʃabbaynee-\*(ha)  
 liked.IP-\*(it)  
 'We met the director of the play that Laila asked whether we liked it.'

(12) **Complex-NP Island**

ʃiɗina l-masrafiyye yalli tʃarrafna ʃala l-muxri3 yalli ʔaxra3-\*(a)  
 saw.IP the-play that met.IP on the-director that directed.3SM-\*(it)  
 'We saw the play that we met the director that directed it.'

Adopting the standard assumption that the absence of island effects indicates the absence of movement, it is possible to account for the distribution of resumptive pronouns within definite relatives in LA by generating those constructions without movement. In what follows, I examine how this account extends to indefinite relatives.

### 1.2. Resumption in indefinite relatives

Like definite relatives, indefinite relatives also require a resumptive clitic in all non-subject positions (13-14).

- (13) ʃam ʔfattiʃ l-mʃallme ʃa kteeb ma ʔəryu-\*(u) ttlemiiz  
 Asp look.3SF the-teacher for book not read.3P-\*(it) the-students  
 'The teacher is looking for a book that the students haven't read.'
- (14) ʃam ʔfattiʃ l-mʃallme ʃa kteeb ʔaalit l-mudiira ʔanno ma  
 Asp look.3SF the-teacher for book said the-principal that not  
 ʔəryu-\*(u) ttlemiiz  
 read.3P-\*(it) the-students  
 'The teacher is looking for a book which the principal said that the students haven't read.'

In subject positions (15a-b) instead, a gap occurs:

- (15) a.  $\zeta$ am tfattij l-m $\zeta$ allme  $\zeta$ a-walad bi $\zeta$ ebb yə $\zeta$ ra  
 Asp look.3SF the-teacher for-boy like.3SM read.3SM  
 'The teacher is looking for a boy who likes to read.'
- b.  $\zeta$ am tfattij l-m $\zeta$ allme  $\zeta$ a-walad badda l-mudiira  
 Asp look.3SF the-teacher for-boy want.3SF the-principal  
 y $\zeta$ ebb yə $\zeta$ ra  
 like.3SM read.3SM  
 'The teacher is looking for a boy that the principal wants him to  
 like to read.'

As can be observed in (16-18) below, the relation between the indefinite relativized DP and the resumptive element is not sensitive to islands:

(16) **Adjunct Island**

- a.  $\zeta$ ikiina ma $\zeta$  mu $\zeta$ ri $\zeta$  fallit laila  $\zeta$ abl ma t $\zeta$ uuf-\*(o)  
 talked.1P with director left.3SF L. before see.3SF-\*(him)  
 'We talked to a director that Laila left before she saw.'
- b. t $\zeta$ arrafna  $\zeta$ ala mu $\zeta$ ri $\zeta$  fallit laila  $\zeta$ abl ma yif $\zeta$ e ma $\zeta$ a  
 met.1P on director left.3SF L. before talk.3SM with-her  
 'We met a director that Laila left before he talked to her.'

(17) **Wh-Island**

- a. t $\zeta$ arrafna  $\zeta$ ala mu $\zeta$ ri $\zeta$  ma mna $\zeta$ ri $\zeta$   $\zeta$ əza nnə $\zeta$ ? $\zeta$ aad byə $\zeta$ itərmu-\*(u)  
 met.1P on director not know.1P whether the-critics respect.3P-\*(him)  
 'We met a director that we don't know whether the critics respect him.'
- b. t $\zeta$ arrafna  $\zeta$ ala mu $\zeta$ ri $\zeta$  ma mna $\zeta$ ri $\zeta$   $\zeta$ əza byə $\zeta$ itərim  
 met.1P on director not know.1P whether respect.3SM  
 nnə $\zeta$ ? $\zeta$ aad  
 the-critics  
 'We met a director that we don't know whether he respects the  
 critics.'

(18) **Complex-NP Island**

- a.  $\zeta$ iDəma masra $\zeta$ iyye t $\zeta$ arrafna  $\zeta$ ala l-mu $\zeta$ ri $\zeta$  yalli  $\zeta$ a $\zeta$ ra $\zeta$ -\*(a)  
 saw.1P play met.1P on the-director that directed.3SM-\*(it)  
 'We saw the play that we met the director that directed it.'
- b. t $\zeta$ arrafna  $\zeta$ ala mu $\zeta$ ri $\zeta$   $\zeta$ iDəma l-masra $\zeta$ iyye yalli  $\zeta$ a $\zeta$ ra $\zeta$ a  
 met.1P on director saw.1P the-play that directed.3SM-it  
 'We met a director that we saw the play that he directed.'

Since it occurs in island contexts (16b-18b), the gap in subject position within the indefinite relatives can be identified as the null pronominal element *pro*. Indefinite relatives pattern together with definite relatives in allowing resumptive pronouns to occur within islands. It is therefore possible to generalize the analysis put forward for definite relatives to include indefinite relatives; in this case, indefinite relatives would also be generated without involving movement.

## 2. Restrictive relatives in LA and movement: reconstruction effects

The discussion in the previous section highlighted the absence of island effects within restrictive relatives in LA. It was suggested that this characteristic indicates that the derivation of restrictive relatives does not involve movement. This being the case, we expect relative clauses in LA not to display any effects of movement. In what follows, I show that this expectation is not always fulfilled and that movement may be involved in the generation of relative clauses in LA.

### 2.1. Reconstruction within definite relatives

In Chomsky 1993, it is argued that reconstruction is a property of chains generated by (non-L-related or A'-) movement. In view of the non-movement analysis suggested above, we expect definite relatives in LA never to display reconstruction effects; as can be observed below, this expectation is not always fulfilled.

- (19) a. [əft [SSuura tabaŋ ʔəbn-a]<sub>i</sub> yalli [kəll mwazzafe]<sub>i</sub>  
saw.1s [the-picture of son-her] that [every employee.f]  
ʔaalit ʔənno badda tʃalləʔ-a<sub>i</sub> bi-maktab-a  
said.3sf that want.3sf hang.3sf-it in-office-her  
'I saw the picture of her son that every employee said she wants to hang in her office.'
- b. [əft [SSuura tabaŋ ʔəbn-a]<sub>i</sub> yalli [kəll mwazzafe]<sub>i</sub>  
saw.1s [the-picture of son-her] that [every employee.f]  
badda tʃalləʔ-a<sub>i</sub> bi-maktab-a  
want.3sf hang.3sf-it in-office-her  
'I saw the picture of her son that every employee wants to hang in her office.'

- c.  $\int\text{əft}$  [SSuura taba $\int$   $\text{ʔəbn-a}_j$ ] yalli  $\text{ʔəito}$   $\text{ʔəнно}$  [kəll mwazzafe]<sub>i</sub>  
 saw.1s [the-picture of son-her] that said.3p that [every employee.f]  
 badda [ʃallə $\text{ʔ-a}_j$  bi-maktab-a  
 want.3sf hang.3sf-it in-office-her  
 'I saw the picture of her son that you said that every employee  
 wants to hang in her office.'

The sentences in (19) can be represented as in (20a-b) (irrelevant details omitted):

- (20) a. .... [<sub>DRel-DP</sub> ..... pron<sub>i</sub> .....], ..... [*yalli* ..... QP<sub>i</sub> ..... ([<sub>CP</sub>] ..... RP<sub>j</sub> ...)] .....  
 b. .... [<sub>DRel-DP</sub> ..... pron<sub>i</sub> .....], ..... [*yalli* ..... [<sub>CP</sub> ..... QP<sub>i</sub> ..... RP<sub>j</sub> ...]] .....

In (19), the pronoun contained within the definite relativized DP *SSuura taba $\int$  ʔəbn-a* 'the picture of her son' can be bound from within the relative clause by the QP *kəll mwazzafe* 'every employee'. This bound reading is represented in (20a-b) by coindexing the pronoun with the QP. The availability of the bound pronoun reading may be taken to indicate that the pronoun within the definite relativized DP is interpreted from the position of the resumptive pronoun, a position which is c-commanded by the QP *kəll mwazzafe* 'every employee'.<sup>7</sup>

However, reconstruction is not always available: for instance, reconstruction is not available when the definite relativized DP and the resumptive element to which it is related are separated by an island (21-23).

(21) **Complex-NP Islands**

- \*  $\int\text{əft}$  [SSuura taba $\int$   $\text{ʔəbn-a}_j$ ] yalli  $\text{ʃriito}$  l-kadr yalli  
 saw.1s [the-picture of son-her] that bought.2p the-frame that  
 [kəll mwazzafe]<sub>i</sub>  $\text{fiTTət-a}_j$  fi-i  
 [every employee.f] put.3sf-it in-it  
 'I saw the picture of her son that you saw the frame that every  
 employee put it in.'

<sup>7</sup> I will discuss how this interpretation obtains in section 3.3.2.

(22) **Adjunct Islands**

\* [əft [SSuura tabaʃ ʔəbn-a]<sub>j</sub>] yalli zʃəto laʔanno [kəll  
 saw.1s [the-picture of son-her] that upset.2p because [every  
 mwazzafe]<sub>i</sub> badda tʃalləʔ-a<sub>j</sub> bi-l-maktab  
 employee.f] want.3sf hang.3sf-it in-the-office  
 'I saw the picture of her son that you were upset because every  
 employee wants to hang it in the office.'

(23) **Wh-Islands**

\* [əft [SSuura tabaʃ ʔəbn-a]<sub>j</sub>] yalli baddkun taʃrfo ween  
 saw.1s [the-picture of son-her] that want.2p know.2p where  
 [kəll mwazzafe]<sub>i</sub> badda tʃalləʔ-a<sub>j</sub>  
 [every employee.f] want.3sf hang.3sf-it  
 'I saw the picture of her son that you want to know where every  
 employee wants to hang it.'

The sentences in (21-23) have the representation in (24) (irrelevant details omitted):

(24) \* ..... [<sub>DPrel-DP</sub> ..... pron<sub>i</sub> ..... ] ..... yalli ..... [<sub>Island</sub> ... QP<sub>i</sub> ..... RP<sub>j</sub> .....]

As indicated by the ungrammaticality of (24), the bound reading of the pronoun contained within the definite relativized DP *SSuura tabaʃ ʔəbn-a* 'the picture of her son' cannot obtain. That is, at LF the relativized DP containing the pronoun cannot reconstruct to a position c-commanded by the QP *kəll mwazzafe* 'every employee'.

Summarizing, we have observed in this section, that reconstruction is available in definite relatives when the resumptive element does not occur within an island. This generalization is illustrated in (25):

(25) a. **Reconstruction available**

..... Relativized DP<sub>i</sub> ..... yalli ..... RP<sub>j</sub> .....

b. **No reconstruction available**

..... Relativized DP<sub>i</sub> ..... yalli ..... [<sub>Island</sub> ... RP<sub>j</sub> ... ] .....

At this point, it is possible to assume that reconstruction is only tied to islands: that is, reconstruction effects occur when no island intervenes between the relativized DP and the RP to which it is related. Alternatively, one may assume, as I have done so far, that reconstruction is tied to movement. Under the latter assumption, the selective availability



of reconstruction in definite relatives in LA indicates that movement is available for the generation of those constructions only when the island constraints are not violated: a derivation involving movement is thus available for the representation in (25a), but not for the one in (25b).

The working of reconstruction in indefinite relatives will provide motivation for the second assumption. It will appear that with indefinite relatives corresponding to the representation in (26), reconstruction is not available:

(26) ..... Indefinite Relativized DP<sub>i</sub> ..... RP<sub>i</sub> .....

The contrast between (25a) and (25b) shows that the absence of islands is a necessary condition for the availability of reconstruction. The unavailability of reconstruction in (26) will indicate that this condition is not sufficient to account for the cases in which reconstruction does in fact occur. Hence the assumption that reconstruction is tied to movement.

## 2.2. Reconstruction within indefinite relatives

As stated in the preceding paragraph, indefinite relatives do not display any reconstruction effects. That is, a pronoun contained within an indefinite relativized DP can never be bound by a QP in the indefinite relative clause. This generalization is illustrated below:

- (27) a.\* [ʔəft [Suura la-ʔəbn-a]<sub>i</sub>] [kəll mwazzafe]<sub>i</sub> ʔaalit ʔənnə  
 saw.1s [picture of-son-her]<sub>i</sub> [every employee.f] said.3sf that  
 badda tʃalləʔ-a<sub>i</sub> bi-maktab-a  
 want.3sf hang.3sf-it in-office-her  
 'I saw a picture of her son every employee said she wants to hang  
 in her office.'
- b.\* [ʔəft [Suura la-ʔəbn-a]<sub>i</sub>] [kəll mwazzafe]<sub>i</sub> badda  
 saw.1s [picture of-son-her]<sub>i</sub> [every employee.f] want.3sf  
 tʃalləʔ-a<sub>i</sub> bi-maktab-a  
 hang.3sf-it in-office-her  
 'I saw a picture of her son every employee wants to hang in her  
 office.'

- c.\* [əft [Suura la-ʔəbn-a]<sub>i</sub>] ʔəltə ʔənnə [kəll mwazzafə]<sub>i</sub>  
 saw.1s [picture of-son-her] said.3p that [every employee.f]  
 badda tʃalləʔ-a<sub>j</sub> bi-maktab-a  
 said.3sf that want.3sf hang.3sf-it in-office-her  
 'I saw a picture of her son you said that every employee wants to  
 hang in her office.'

The facts illustrated in (27) may be represented as in (28a-b) respectively:

- (28) a.\* ..... [<sub>IRel-DP</sub> ..... pron<sub>i</sub> ...], ..... QP<sub>i</sub> ..... ([<sub>CP</sub>] ... RP<sub>j</sub> .....  
 b.\* ..... [<sub>IRel-DP</sub> ..... pron<sub>i</sub> ...], ..... [<sub>CP</sub> ... QP<sub>i</sub> ..... V+RP<sub>j</sub> ... ] .....

Even when the RP does not occur in an island, the indefinite relativized DP with which this RP is coindexed cannot reconstruct below the QP in the indefinite relative (27). The contrast between definite relatives and indefinite relatives with respect to reconstruction, i.e. the contrast between (28) and (25), is accounted for under the assumptions that reconstruction is tied to movement and that no movement is involved in the generation of indefinite relatives.<sup>8</sup> In what follows, I offer an

<sup>8</sup> Obviously, indefinite relatives do not display reconstruction effects when the indefinite relativized DP is related to a resumptive element which occurs in an island:

(i)\* **Adjunct Island**

[əft [Suura la-ʔəbn-a]<sub>i</sub>] zʃəltə laʔanno [kəll mwazzafə]<sub>i</sub>  
 saw.1s [picture of-son-her] upset.2p because [every employee.f]  
 badda tʃalləʔ-a<sub>j</sub> bi-l-maktab  
 want.3sf hang.3sf-it in-the-office  
 'I saw a picture of her son you were upset because every employee wants to  
 hang it in the office.'

(ii)\* **Wh-Island**

[əft [Suura la-ʔəbn-a]<sub>i</sub>] baddkuntaʃrfo ween [kəll mwazzafə]<sub>i</sub>  
 saw.1s [picture of-son-her] want.2p know.2p where [every employee.f]  
 badda tʃalləʔ-a<sub>j</sub>  
 want.3sf hang.3sf-it  
 'I saw a picture of her son you want to know where every employee wants to  
 hang it.'

(iii)\* **Complex-NP Island**

[əft [Suura la-ʔəbn-a]<sub>i</sub>] [ʃriito l-kadr yalli  
 saw.1s [picture of-son-her] bought.2p the-frame that  
 [kəll mwazzafə]<sub>i</sub> ʃiATTət-a<sub>j</sub> fi-i  
 [every employee.f] put.3sf-it in-it  
 'I saw a picture of her son you bought the frame that every employee put it in.'

account for the discrepancy between definite relatives and indefinite relatives with respect to the availability of movement.

### 3. Generation of definite relatives

An analysis which allows movement in the derivation of definite relatives but not indefinite relatives raises the following questions:

- (29) a. What triggers movement in definite relatives?  
 b. What is the nature of the moving element?  
 c. What is the landing site of this movement?  
 d. Why isn't movement available for indefinite relatives?

I start by investigating the properties of definite relatives.

#### 3.1. Morphosyntactic properties of *yalli*

As noted earlier, the morpheme *yalli* occurs only in restrictive relatives that are definite, and is itself definite, as the following discussion indicates.<sup>9</sup>

In LA, nouns and their modifiers agree in definiteness (30).

- (30) a. l-kteeb l-3diid wəSil ʕal-maktabe  
 the-book the-new arrived at-the-bookstore  
 'The new book arrived at the bookstore.'  
 b. ʕam fattif ʕa kteeb 3diid ʔəʔraa  
 Asp look.1S for book new read.1S-it  
 'I am looking for a new book to read.'

<sup>9</sup> The counterpart of *yalli* in Standard Arabic (*ʔa*)*lla* *ǝ* occurs in definite relatives but not in indefinite relatives. It is morphologically definite: it is introduced by the definite article *al-*.

- (i) qaraʔtu l-kitaaba \*(llaǝi) ʕtaraytu-hu l-baarifha  
 read.1S the-book that bought.1S-it yesterday  
 'I read the book that I bought yesterday.'  
 (ii) qaraʔtu kitaaban \*(llaǝi) ʕtaraytu-hu l-baarifha  
 read.1S book that bought.1S-it yesterday  
 'I read a book that I bought yesterday.'

In (30a-b), the adjectives agree in definiteness with the nouns they modify: In (30a), the adjective *l-ɣdiid* (the new) occurs with the definite article *al-* since the noun it modifies, i.e. *l-kteeb* (the book), is definite. On the other hand, the adjective *ɣdiid* (new) in (30b) does not occur with the definite article *al-* since the noun it modifies, i.e. *kteeb* (book), is indefinite.

Furthermore, a sentence where the noun and its modifier do not show agreement in definiteness would be ungrammatical:

- (31) a.\* *l-kteeb ɣdiid wəSil ʕal-maktabe*  
 the-book newarrived at-the-bookstore  
 'The new book arrived at the bookstore.'
- b.\* *ʕam fattij ʕa kteeb l-ɣdiid ʔəʔraa*  
 Asp. look.1S for book the-new read.1S-it  
 'I am looking for a new book to read.'

Consider now the following sentences involving relative clauses:

- (32) a. *l-kteeb yalli Talabtii wəSil ʕal-maktabe*  
 the-book that ordered.2SF-it arrived.3SM at-the-bookstore  
 'The book that you ordered arrived at the bookstore.'
- b.\* *badde kteeb yalli yəʔdro l-wleed yəʔruu*  
 want.1S book that can.3P the-children read.3P-it  
 'I want a book that the children can read.'

The contrast between (32a) and (32b) indicates that *yalli* is definite: *yalli* can only occur when the relativized DP is definite. Assuming *yalli* to be a complementizer generated in the head C of the relative clause, this would mean that the relative clause *yalli Talabtii* (that you ordered) in (32a) is definite, thus matching the relativized DP *l-kteeb* (the book). The ungrammaticality of (32b) is the result of the clash between the definiteness of the relative clause and the indefiniteness of the relativized DP.

In addition to being [+definite], *yalli* also bears  $\phi$ -features. Generally, null subjects in LA occur in the context of overt agreement, as illustrated below:

- (33) a. raafit  
left.3FS  
'She left.'
- b.\* bəl-beet  
in-the-house  
'He/She is in the house.'
- c. ʔaal kariim ʔənnu/\*ʔəno bəl-beet  
said.3SM K. that.3SF in-the-house  
'Karim said that she is in the house.'

In (33a), a null subject occurs with a verbal predicate inflected for person, number, and gender ( $\phi$ -features). In (33b), a null subject is prohibited; the prepositional predicate doesn't have  $\phi$ -features. (33c) is well-formed only when the complementizer *ʔəno* has  $\phi$ -features that identify the embedded null subject.

Turning back to definite relatives, the grammaticality of (34) below indicates that *yalli*, like *ʔəno*, bears the necessary  $\phi$ -features in the context of which null subjects occur.<sup>10</sup>

- (34) l-bənt yalli bəl-beet  
the-girl that in-the-house  
'The girl that is in the house.'

The prepositional predicate *bəl-beet* ('in the house') obviously does not show overt subject agreement. If *yalli* did not agree with the null subject of this predicate, we would expect this sentence to be non-well-formed on a par with (33b), which is contrary to fact.

<sup>10</sup> Cross-linguistic data from Standard Arabic provide motivation for the assumption that the definite relative complementizer bears  $\phi$ -features, and a case feature as well:

- (i) a. raʔaytu l-waladayni llaʕayni tufibbuhuma l-muʕallima  
saw.1S the-boy.Dual.Acc that.3F.Dual.Acc like.3SF.them(dual) the-teacher  
'I saw the two boys that the teacher likes.'
- b. ʕaaʔat l-fataataani llaʕaani tufibbuhuma l-muʕallima  
came.3SF the-girl.Dual.Nom that.3FD.Nom like.3SF.them(dual) the-teacher  
'The two girls that the teachers likes came.'

As can be observed in (i) above, the Standard Arabic counterpart of *yalli* is inflected for person, gender, and number. In addition, the sentences in (i) illustrate that the relative clause complementizer *ʔallaʕ* displays overt agreement in case with the relativized DP.

Assume then that the features borne by *yalli*, i.e., its [+definite] feature and  $\phi$ -features, need to be checked in the course of the derivation.<sup>11</sup>

Summarizing, in this section I have discussed the morphosyntactic properties of *yalli*, the element which introduces definite relatives in LA. I have assumed that:

- (35) a. *yalli* is a complementizer  
 b. *yalli* bears the features [+definite], [ $\alpha$  person], [ $\beta$  number], and [ $\gamma$  gender]  
 c. The features of *yalli* need to be checked.

It is (35c) that provides the motivation for movement in definite relatives in LA: the necessity to check the features of the complementizer triggers movement in those constructions.

### 3.2. *Yalli and the nature of the moving element*

This movement can be characterized as covert, i.e. it doesn't involve pied-piping of a category. The moving element is then a bundle of formal features, which include the feature [+definite] and the relevant  $\phi$ -features. Within the minimalist theory of Move  $\alpha$ , the movement of these features will involve adjunction to the complementizer *yalli*, which heads the relative clause.

The formal features of *yalli* can only be checked by those of a DP, PPs being obviously not specified for definiteness, as well as  $\phi$ -features. This DP cannot be the counterpart of a wh-element: wh-elements are not definite in LA, as shown below.

<sup>11</sup> Although the complementizer *yalli* matches the relativized DP in definiteness, it cannot be said that *yalli* checks this feature against that of the relativized DP, since *yalli* can occur in headless relatives, unless headless relatives occur with a non-overt pronominal:

- (i) a. bħəbb yalli bəħfiibii  
 like.1S that like.3SF-it  
 'I like whatever you like.'  
 b. kariim byilʕab maʕ yalli byilʕab maʕo  
 K. play.3SM with that play.3SM with-him  
 'Karim plays with whoever plays with him.'

- (36) a.\* ʔayya təlmiiz l-ʒdiid ʃəft  
 which student the-new saw.2SM  
 ‘Which new student did you see?’  
 b. ʔayya təlmiiz ʒdiid ʃəft  
 which student newsaw.2SM  
 ‘Which new student did you see?’

Recall that in LA, the adjective and the DP it modifies agree in definiteness. The contrast between (36a) and (36b) indicates that the adjective modifying the wh-phrase *ʔayya təlmiiz* (which student) cannot be introduced by the definite article. This contrast shows that wh-phrases are indeed indefinite. Our conclusion is further confirmed by relative clauses modifying wh-phrases. These relative clauses cannot be introduced by *yalli*:

- (37) a.\* ʔayya walad yalli ʃəfto raah mbeerifh rəʒiʃ  
 which boy that saw.2SM-him left.3SM yesterday returned.3SM  
 l-yom  
 today  
 ‘Which boy that you saw leave yesterday returned today?’  
 b. ʔayya walad ʃəfto raah mbeerifh rəʒiʃ l-yom  
 which boy saw.2SM-him left.3SM yesterday returned.3SM today  
 ‘Which boy that you saw leave yesterday returned today?’

Since relative clauses introduced by *yalli* can only modify definite relativized DPs, the ungrammaticality of (37a) confirms the non-definite nature of wh-elements in LA.

In brief, the element that checks the features of *yalli* can be identified as a set of formal features. This set comprises the features [+definite],  $\phi$ -features, and case. I identify this set with the null pronominal element *pro*. *pro*, like all pronouns in LA, is related to an argument position: in LA, there are no pronouns corresponding to adjuncts. As a consequence, we expect an adjunct not to be relativized in LA, as illustrated by the ungrammaticality of (38).<sup>12</sup>

<sup>12</sup> In the well-formed phrases (i-iii), the relativized DP corresponds to a prepositional complement within the relative clause.

- (38) a.\* ssabab yalli rəfıt-o .....  
 the-reason that left.IS-it  
 'The reason why I left...'

### 3.3. The working of movement and reconstruction in definite relatives

The discussion so far has provided answers to three of the four questions in (29). Movement in definite relatives was motivated by the need for the relative complementizer to check its [+definite] feature and  $\phi$ -features (question (29a)) against those of an element which adjoins to it (question (29c)). The element that checks these features was identified as *pro* (question (29b)), characterized here as a set of formal features. In the light of this analysis, I examine the working of movement and reconstruction within definite relatives.

#### 3.3.1. Movement and minimality in definite relatives

*Yalli*, which occurs in all definite relatives in LA, bears features which need to be checked by *pro*. Furthermore, we have noted that in definite relatives, the selective availability of reconstruction indicates that movement is available in those constructions. In other words, to satisfy the morphological requirements of the complementizer *yalli* the following two scenarios are possible: (i) either *pro* is moved to COMP (39a) or (ii) *pro* is directly generated in COMP (39b).

- (39) a. ... Definite Relativized DP<sub>i</sub> ..... [*pro*<sub>i</sub>-*yalli* ..... RP<sub>i</sub> ..... t<sub>i</sub> ..... ] .....  
 b. ... Definite Relativized DP<sub>i</sub> ... [*pro*<sub>i</sub>-*yalli* ..... RP<sub>i</sub> ..... *pro*<sub>i</sub> ..... ] .....

- 
- (i) ssabab yalli řafeeno falleet...  
 the-reason that because-of-it left.2SM  
 'The reason why you left...'
- (ii) l-maTrař yalli tlaařayna fi-i  
 the-place that met.IP in-it  
 'The place where we met...'
- (iii) TTariiřa yalli řikiitne fiya  
 the-manner that talked.2SM-me(dat.) with-it  
 'The manner in which you talked to me...'



In the sentences where the relativized DP is separated from the resumptive pronoun by an island, only the representation in (39b) is available. Since movement of *pro* is not possible from within an island, the features of *yalli* can only be checked by generating a null *pro* directly in COMP. In that case, *pro* is coindexed with another null pronominal in an argument position within the relative clause. On the other hand, when no island intervenes between the relativized DP and the resumptive pronoun, the representation in (39a) is available for definite relatives, in addition to the representation which doesn't involve movement (39b). In (39a) movement of *pro* has occurred from within the relative clause to COMP.<sup>13</sup>

The movement analysis sketched in (39a) above raises a question with respect to minimality. Consider constructions such as (40) below, in which the movement of the object *pro* to *yalli* crosses the subject *pro*:

- (40) [əfna [SSuura tabaʃ təlmiiz-[a]<sub>i</sub>]<sub>j</sub> yalli kəll mʃallmee<sub>i</sub> ʔaalit  
 saw.1P [the-picture of student.m-her that every teacher said.3sf  
 ʔanno pro baddo yʃalləʔ-[a]<sub>j</sub>  
 that want.3sm hang.3sm-it  
 'We saw the picture of her student that every teacher said that he wants  
 to hang it.'

In (40) movement of the object *pro* to *yalli* should violate minimality. Indeed, there is a shorter derivation which involves moving the subject *pro* of the embedded clause instead. In other words, we are led to expect sentence (40) to be non well-formed, which is not the case.

However, under a theory which takes minimality to be sensitive to the feature being checked (see Chomsky 1995), we can account for the well-formedness of (40). *yalli* bears the same  $\phi$ -features as the relativized DP *SSuura tabaʃ təlmiiza* 'the picture of her student'. The *pro* subject of the embedded verb in (40), being masculine, cannot check those features. Raising *pro* from the embedded object position to *yalli* crosses

<sup>13</sup> The existence of the two representations in (42) for definite relatives does not raise a question of economy. Assuming that in evaluating derivations for economy, only convergent alternatives with the same numeration are considered, neither (42a) nor (42b) could have a blocking effect on the other, since they don't involve the same numeration: in (42b), *pro* is selected twice for the initial array whereas, in (42a), it is selected only once (see Aoun and Benmamoun (1998)).

the embedded subject *pro* but doesn't violate minimality, since the *pro* being crossed doesn't bear the relevant  $\phi$ -features that need to be checked in *yalli*.

### 3.3.2. *pro* and reconstruction of definite relativized DPs

As stated in the previous section, the generation of definite relatives may involve movement (39a) or not (39b). Following Chomsky (1977), Williams (1980) and Borer (1984), I assume that the relative clause and the relativized DP form a predication structure: the relative clause constitutes a complex predicate coindexed with the relativized DP, the subject of this predication. The complex predicate must contain an open position which functions as the predicate variable (the trace in (39a) and *pro* in (39b) within the definite relative).

We are now in a position to discuss how reconstruction operates within definite relatives. Consider the following English facts (Barss 1986, Hornstein 1984):

- (41) His last poem is what every Englishman prefers.

Although the c-command requirement on bound pronouns fails to apply in (41), the pronoun *his* can still be bound by the QP *every Englishman*. In (41), the DP *his last poem* is coindexed with the relative clause via predication. *What*, which bears the same index as *his last poem*, can be interpreted as a 'copy' of this DP. Informally, at LF, *his last poem*, *what*, and the trace of *what* within the relative clause, form an extended chain. Hence, the availability of the bound pronoun reading in (41).

Turning to the representations in (39), the relativized DP, the relative clause, and the *pro* in COMP are all coindexed. However, reconstruction is available in (39a) but not in (39b). In (39a), the *pro* and its trace form a chain generated by movement. In (39b), the two distinct *pro* do not form a movement chain. Since reconstruction occurs only with chains generated by movement, reconstruction will be available in (39a) but not in (39b) (see the conclusion for further discussion).

## 3.3.3. Reconstruction of definite relativized DPs

Having examined how reconstruction of the relativized DP obtains, I now identify the position to which the fronted *pro* reconstructs.

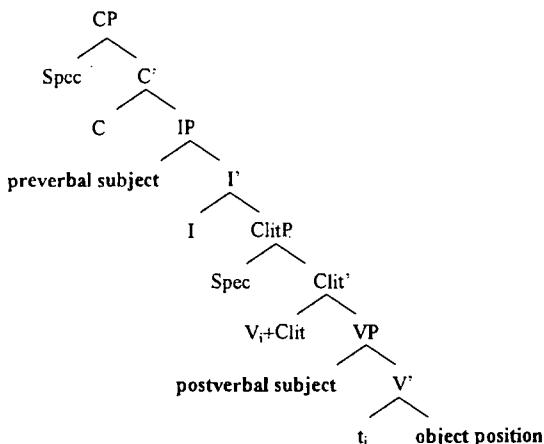
A close look at definite relatives in LA reveals an asymmetry between preverbal and postverbal subjects with respect to reconstruction.<sup>14</sup> That is, a pronoun contained in a definite relativized DP can be bound by a preverbal subject QP (42a-43a), but not by a postverbal subject QP (42b-43b).

- (42) a. [əft [SSuura tabaŋ ʔəbn-a]<sub>i</sub>] yalli [kəll mwazzafe]<sub>i</sub>  
 saw.1s [the-picture of son-her] that [every employee.f]  
 badda tʃalləʔ-a<sub>j</sub> bi-maktab-a  
 want.3sf hang.3sf-it in-office-her  
 'I saw the picture of her son that every employee wants  
 to hang in her office.'
- b.\* [əft [SSuura tabaŋ ʔəbn-a]<sub>i</sub>] yalli badda tʃalləʔ-a<sub>j</sub>  
 saw.1s [the-picture of son-her] that want.3sf hang.3sf-it  
 [kəll mwazzafe]<sub>i</sub> bi-maktab-a  
 [every employee.f] in-office-her  
 'I saw the picture of her son that every employee wants to hang in  
 her office.'
- (43) a. [əft [SSuura tabaŋ ʔəbn-a]<sub>i</sub>] yalli ʔəltə ʔənnə [kəll mwazzafe]<sub>i</sub>  
 saw.1s [the-picture of son-her] that said.3p that [every employee.f]  
 badda tʃalləʔ-a<sub>j</sub> bi-maktab-a  
 want.3sf hang.3sf-it in-office-her  
 'I saw the picture of her son that you said that every employee  
 wants to hang in her office.'
- b.\* [əft [SSuura tabaŋ ʔəbn-a]<sub>i</sub>] yalli ʔəltə ʔənnə badda  
 saw.1s [the-picture of son-her] that said.3p that want.3sf  
 tʃalləʔ-a<sub>j</sub> [kəll mwazzafe]<sub>i</sub> bi-maktab-a  
 hang.3sf-it [every employee.f] in-office-her  
 'I saw the picture of her son that you said that every employee  
 wants to hang in her office.'

<sup>14</sup> A contrast similar to the one illustrated in (42-43) was first pointed out for Spanish Left Dislocation constructions by Zubizarreta 1993.

The contrast between (42a) and (43a) on one hand, and (42b) and (43b) on the other, indicates that reconstruction is to a position lower than the preverbal subject, but higher than the postverbal subject position. I assume, along with Aoun and Benmamoun (1998), that this position is within the clitic projection (ClitP) (Sportiche 1992), as in (44) below:

(44)



Given the structure in (44), it is clear that reconstruction cannot be to the object position, for instance. If this were the case, this position being c-commanded by both the preverbal and the postverbal subjects, the contrast observed in (42) and (43) would not arise.

The structure in (44) also leads us to expect that, in case the QP subject is generated in a clause higher than the one containing the RP, the preverbal/postverbal subject asymmetry will disappear. This expectation is fulfilled:

- (45) a. [əft [SSuura tabaʔ ʔəbn-a<sub>i</sub>], yalli [kəll mwazzafe]<sub>i</sub>  
 saw.1s [the-picture of son-her] that [every employee.f]  
 ʔaalit ʔənnə badda tʃalləʔ-a<sub>j</sub> bi-maktab-a  
 said.3sf that want.3sf hang.3sf-it in-office-her  
 'I saw the picture of her son that every employee said she wants to  
 hang in her office.'

- b. [ʃəft [SSuura tabaʃ ʔəbn-a<sub>i</sub>] yalli ʔaalit [kəll mwazzafe]<sub>i</sub>  
 saw.1s [the-picture of son-her] that said.3sf [every employee.f]  
 ʔanno badda [ʃalləʔ-a<sub>i</sub>] bi-maktab-a  
 that want.3sf hang.3sf-it in-office-her  
 'I saw the picture of her son that every employee said she wants to  
 hang in her office.'

In (45), the bound reading is available: these sentences do not display any preverbal/postverbal subject asymmetry. In both (45a) and (45b), the QP *kəll mwazzafe* 'every employee' and the resumptive clitic occur in different clauses. If *pro* in (45) reconstructs to the clitic projection, it will end up in a position c-commanded by both the preverbal subject QP (45a) and the postverbal subject QP (45b) of the higher clause.

More generally, in other contexts too *pro* does not seem to reconstruct below the clitic:

- (46) a. ʔəmm kariim<sub>i</sub> bəʃfiibb-[o]<sub>i</sub>  
 mother K. love.3SF-him  
 'Karim's mother loves him'  
 b.\* bəʃfiibb-[o]<sub>i</sub> ʔəmm kariim<sub>i</sub>  
 love.3SF-him mother K.  
 'Karim's mother loves him.'

In (46a) but not in (46b), the object and the name *Karim* contained within the preverbal subject can be coreferential. In (46b), coindexing *Karim* with the object yields a violation of binding principle C. If the object *pro* were to reconstruct to the argument position, the sentence in (46b) would be well-formed, like (46a). This is contrary to fact.

We can conclude from the discussion so far that the *pro* related to an accusative resumptive pronoun cannot reconstruct below the ClitP. Assuming that *pro* is originally generated in the argument position and that it undergoes A-movement to ClitP, its behavior with respect to reconstruction may be accounted for along the following lines: According to Chomsky (1993), reconstruction is only a property of A'-chains; *pro* then will only reconstruct to the clitic projection and never below. Alternatively, one may assume that *pro*, being definite, needs to be interpreted within the clitic projection, outside the VP shell (see Diesing

(1992), Beghelli and Stowell (1995)). Therefore, at LF, it will not reconstruct to its original position.<sup>15</sup>

#### 4. Generation of indefinite relatives

Earlier it was observed that indefinite relatives did not display reconstruction effects (see section 2.2.). Using reconstruction as a diagnostic for the availability of movement, the absence of reconstruction effects in indefinite relatives was interpreted as indicating the absence of movement in those constructions. In other words, restrictive relatives are not systematically generated via movement in LA. Movement is only available for definite relatives. It is motivated by the need to check the definiteness feature and  $\phi$ -features of the complementizer *yalli*. Indefinite relatives lack a complementizer; the motivation for movement is absent in those constructions. Move  $\alpha$  being a Last Resort operation (see Chomsky 1995), the generation of indefinite relatives will not involve movement. This accounts for the discrepancy between definite relatives and indefinite relatives with respect to reconstruction effects (question (29d)).<sup>16</sup>

Two possible representations are consistent with the absence of movement in indefinite relatives: either (i) there is a *pro* directly generated in COMP within the indefinite relative, coindexed with another *pro* within the relative clause (47a), or (ii) there is no *pro* in the indefinite COMP and *pro* occurs only within the indefinite relative (47b).<sup>17</sup>

<sup>15</sup> When a subject gap occurs, I take definite relatives to be represented as follows: (ia) but not (ib) is generated by movement.

- (i) a. .... Definite Relativized DP<sub>i</sub> .....[*pro*<sub>i</sub> *yalli* ..... x<sub>i</sub>.....]......  
 b. .... Definite Relativized DP<sub>i</sub>..... [*pro*<sub>i</sub> *yalli* ..... *pro*<sub>i</sub> .....] .....

<sup>16</sup> For a different analysis assuming that movement is involved in resumption within restrictive relatives, see the important work of Demirdache (1991). In her analysis, restrictive relatives involving resumptive pronouns are all generated by LF-movement of a null operator- identified as *pro*, to the Spec of Comp. This LF movement, she assumes, does not obey island constraints. An analysis along these lines does not account for the contrast observed between indefinite relatives and definite relatives in LA with respect to reconstruction. Moreover, I have shown that reconstruction, and therefore movement, in definite relatives is indeed sensitive to islands.

<sup>17</sup> Recall that the resumptive *pro* within the indefinite relative provides the predicate variable which is coindexed with the subject of predication, i.e. the relativized DP. Since *pro* can only be related to an argument position in LA, I can account for the fact that adjuncts cannot 'head' indefinite relatives, as illustrated below:

- (47) a. .... Indefinite Relativized DP<sub>i</sub> ..... [<sub>rel</sub> *pro*<sub>i</sub> ..... *pro*<sub>i</sub> .....] .....  
 b. .... Indefinite Relativized DP<sub>i</sub> ..... [<sub>rel</sub> ..... *pro*<sub>i</sub> .....] .....

## 5. Conclusion: the raising analysis revisited

In this paper, we have examined the properties of restrictive relatives in LA. We have found the following generalizations to hold true of these constructions:

- (48) a. Restrictive relatives in LA are always generated with a resumptive element.  
 b. Restrictive relatives in LA are not sensitive to island constraints.  
 c. Definite restrictive relatives may display reconstruction effects only when the resumptive element does not occur within an island.  
 d. Indefinite restrictive relatives do not display reconstruction effects.  
 e. The definite relative COMP is always in a checking relation with *pro*.  
 f. *pro* cannot occur in the COMP of indefinite relatives.  
 g. Adjuncts cannot be relativized in LA.

To account for the above generalizations, I have argued for the following analysis:

- (49) a. In definite relatives, the features of the complementizer *yalli*, { [+definite],  $\phi$ -features, case } enter into a checking relation with *pro*.  
 b. This *pro* can be directly generated in relative COMP, or (covertly) moved to COMP in definite relatives.  
 c. In indefinite relatives, *pro* need not and therefore does not move to COMP (Last Resort).

- 
- (i)\*  $\Upsilon$ Tiine    sabab    rəfto  
 give.3S-me    reason    left.2SM-it  
 'Give me a reason why you left.'

The sentence in (i) contrasts with the one in (ii) below where the head of the relative is related to the complement position of a preposition:

- (ii)  $\Upsilon$ Tiine    ?əsm    maʃall    nənto    fi-i  
 give.2SM    name    place    slept.1P    in-it  
 'Give me the name of a place where you slept.'

In both indefinite and definite relatives, the relativized DP ends up coindexed with a *pro*. Since *pro* can only be related to argument positions, it follows that adjuncts in LA cannot be relativized, as seen in (41), repeated here for convenience, and in (50):

- (41) \* ssabab        yalli    rəft-o .....  
       the-reason    that    left.IS-it  
       ‘The reason why I left...’
- (50) \* sabab        rəft-o .....  
       the-reason    left.IS-it  
       ‘A reason why I left...’

As a final discussion, I would like to reconsider the working of reconstruction in definite relatives generated by movement. To account for the fact that a definite relativized DP can be interpreted with respect to a position within the relative clause, I assumed that this relativized DP, the fronted *pro* and its trace, form an extended chain (see section 3.3.2.). One might suggest an alternative analysis which can account for the reconstruction effects observed in definite relatives, in a more straightforward fashion; that is, the raising analysis argued for in Vergnaud 1974, 1985 and more recently in Kayne 1994. If the relativized DP is itself fronted from within the relative clause to check the necessary features of *yalli*, the reconstruction effects are to be expected. In the cases where movement cannot be involved, i.e. when the relativized site occurs within an island or when it corresponds to an indefinite DP, the relativized DP is directly generated in its surface position, coindexed with a null *pro* in the relativized site and no reconstruction occurs.

Under a raising analysis of the relativized DP, the obligatoriness of resumptive elements within definite relatives remains unaccounted for: if it is the relativized DP, and not *pro*, that raises to COMP, why is it necessary for the relative clause to contain a resumptive clitic in object positions? Why can't a gap occur in these positions?

As can be seen in the following examples, a DP in LA can be topicalized (51a) or clitic-left dislocated (51b). In (51a), the DP is coindexed with a gap, and in (51b), with a resumptive clitic:



- (51) a. l-walad zzyiir fəft mbeerif  
 the-boy the-little saw.1S yesterday  
 'The little boy. I saw yesterday.'
- b. l-walad zzyiir fəft-o mbeerif  
 the-boy the-little saw.1S-him yesterday  
 'The little boy. I saw him yesterday.'

Similarly, *wh*-elements in LA may be fronted and coindexed with a gap (52a) or a resumptive clitic (52b):

- (52) a. ʔayya walad fəft e mbeerif  
 which boy saw.2SF yesterday  
 'Which boy did you see yesterday?'
- b. ʔayya walad fəft i-i mbeerif  
 which boy saw.2SF-him yesterday  
 'Which boy did you see yesterday?'

Why is it then that definite relativized DPs cannot be coindexed with a gap? Under an analysis which considers that the relativized DP itself raises in definite relatives, the answer is not obvious. However, under an account that assumes *pro* raising to COMP, the answer is rather straightforward: *pro* in object positions is always generated with a clitic, as evidenced by the ungrammaticality of (53b), below:

- (53) a. fəft i-i  
 saw.2SF-him  
 'You saw him.'
- b.\* fəft e  
 saw.2SF-pro  
 'You saw him/her.'

The ungrammaticality of a definite relativized DP coindexed with a gap in LA (54) reduces to the ungrammaticality of (53b):

- (54)\* l-walad yalli fəfte .....  
 the-boy that saw.2SF  
 'The boy that you saw ....'

In the same vein, I argued that adjuncts in LA cannot be relativized because *pro* cannot be generated in non-argument positions. Once again, under a raising analysis of the relativized DP, it is not clear how to exclude sentence (41) in LA, given the well-formedness of the English sentence in (55):

(55) I know the reason why John left.

Thus, (definite) relatives in English differ from the definite relatives in LA, in at least the following respects:

- (56) a. Relatives in English can involve movement of a relative element which can be a bare wh-element (ia) or a wh-element embedded within a prepositional phrase (ib):
- (i) a. The book which Bill wrote
  - b. The table under which he is hiding
  - b. An adjunct in English can be relativized:
    - (ii) a. the reason why John left
    - b. The place where we met

In LA, wh-elements are indefinite (see section 3.2.) and thus cannot co-occur in COMP with the definite relative complementizer *yalli*. Since only *pro* can be fronted to COMP in definite relatives, only arguments can be relativized.

In brief, the difference between relative clauses in English and Lebanese Arabic may be accounted for in case Vergnaud's raising analysis is adopted for English and the *pro* raising analysis is adopted for LA.<sup>18</sup>

<sup>18</sup> A similar proposal is put forward by Denirdache (to appear) to account for the following facts; Doron (1982) and Sells (1984), indicate that restrictive relatives with gaps (as in English) differ from restrictive relatives with RPs (as in Hebrew) in their interpretation:

- (i) a. Dani yimca    ?et ha-?iʃa,    ʃe hu mexapes t,  
 D.    will-find Acc the-woman that he seeks  
 'Dani will find the woman that he seeks.'
- b. Dani yimca    ?et ha-?iʃa,    ʃe hu mexapes ota,  
 D.    will-find Acc the-woman that he seeks her  
 'Dani will find the woman that he seeks.'

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(ia) is ambiguous between a specific and a non-specific reading of the relativized head *ha-ʔifa* ('the woman'). That is, (ia) can be used in a context where Dani is looking for a woman who has a certain property, but he doesn't know her identity yet (non-specific reading). (ia) can also be used in a context where Dani is looking for a specific woman, say, Rina. (ib), on the other hand, is unambiguous; it has only the interpretation where the relativized head

*ha-ʔifa* ('the woman') refers to a specific person, whose identity is known to Dani. Demirdache (to appear) accounts for the contrast between (ia) and (ib) by assuming Vergnaud's raising analysis for (ia) and *pro* raising for (ib).

However, the facts described above for Hebrew do not carry over to LA: in LA, the equivalent of (ib), below, is ambiguous. It is interpreted as (ia).

- (ii) *saami rafi yleeʔe l-mara yalli ʕam ynabbif ʕalaya*  
 S. will find the-woman that Asp look.3SM for-her  
 'Sami will find the woman that he is looking for.'

LA, unlike Hebrew, does not allow gaps in object position within headed restrictive relatives. In LA, a contrast similar to the one in (i) seems to exist only when an alternation between gaps and resumptive pronouns is available. This is the case with *wh*-interrogatives:

- (iii) a. *ʔayya walad rafi tʔaaSiS l-mʕallme*  
 which boy fut. punish the-teacher  
 'Which boy will the teacher punish?'  
 b. *ʔayya walad rafi tʔaaSiS-o l-mʕallme*  
 which boy fut. punish-him the-teacher  
 'Which boy will the teacher punish?'

(iii)a) is ambiguous but not (iii)b). (iii)b) admits only a specific reading.

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**EXISTENTIALS, A-CHAINS AND RECONSTRUCTION**  
(Construções Existenciais, Cadeias-A e Reconstrução)

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*ABSTRACT: This paper provides a new minimalist analysis of existential constructions that reconciles two salient properties: (i) the fact that the local relation between there and its associate mimics the locality involved in movement relations and (ii) the fact that the associate is interpreted where it sits. Assuming that A-chains can only have one visible link at LF, I argue that, due to certain properties of there, reconstruction into the foot of the associate chain is the only option that yields interpretable results.*

*KEY WORDS: Existential Constructions, A-Chains, Expletives, Reconstruction*

*RESUMO: Este trabalho propõe uma nova análise minimalista de construções existenciais que reconcilia duas propriedades salientes: (i) o fato de a relação local entre o expletivo e seu associado reproduzir a localidade encontrada em relações de movimento e (ii) o fato de o associado ser interpretado na posição em que se encontra. Assumindo que cadeias-A só podem ter um elo visível em LF, eu argumento que, devido a certas propriedades do expletivo, a reconstrução na cauda da cadeia do associado é a única opção que produz resultados interpretáveis.*

*PALAVRAS-CHAVE: Construções Existenciais, Cadeias-A, Expletivos, Reconstrução*

## 1. The Problem

Every time syntactic theory changes, existential constructions (ECs) are reanalyzed. The turn towards minimalism is no exception. *there*-constructions have been the center of repeated theoretical speculation. Chomsky has developed no less than three different proposals (1995: chap. 2, 3 and 4). Lasnik has contributed two (Lasnik 1992, 1995). Yet another is offered here. Why are ECs so interesting? In my opinion, it is because these constructions must satisfy a pair of competing requirements that appear to pull in opposite directions. In particular, a successful account must reconcile two salient properties of ECs: the fact that there is a local relation between the expletive and its indefinite associate and

the fact that the associate's scope is determined by its overt position. There is a clear tension between these two properties. The former suggests that the associate moves to the position of the expletive at LF, the latter that the associate stays in place.

Different kinds of data motivate each half of the puzzle: Moving the associate to the expletive finds strong support in the locality facts extensively described in Chomsky 1986. These data show that the distance between *there* and its associate is identical to the span of an A-chain link, i.e. the distance between two successive members of a licit A-chain. This follows, Chomsky 1986 persuasively argues, if the associate A-moves to the expletive at LF. It accounts for the following kinds of data.

- (1) a. \*There is the man in a room  
 b. \*There seems that a man is in the room  
 c. \*There is the picture of a man in the room  
 d. \*There seems that Bill saw a man

Chomsky 1986 assimilates the unacceptability of the sentences in (1) to that in (2), in which the indefinite has overtly moved to Spec IP.

- (2) a. \*A room is the man in t  
 b. \*A man seems that t is in the room  
 c. \*A man is the picture of t in the room  
 d. \*A man seems that Bill saw t

The locality violations that render the overt movements ungrammatical in (2) are covertly present in (1) on the assumption that the indefinite associate A-moves to the neighborhood of the expletive at LF. If these locality restrictions are respected an acceptable sentence results.

- (3) a. There is a man in the room  
 b. A man is in the room

Current approaches to ECs maintain Chomsky's 1986 movement story though the details differ in important ways. I outline some of the current technology below.

The second salient property of ECs appears to clash with an LF movement story. The rub comes with the observation that the scope properties of the associate coincide with its overt position rather than its putative LF site. The data below illustrate this.<sup>1</sup>

- (4) a. There aren't many men in the room  
b. Many men aren't in the room
- (5) a. There must be someone in John's house  
b. Someone must be in John's house
- (6) a. There wasn't anyone in the room  
b. \*Anyone wasn't in the room
- (7) a. Some applicants seem to each other to be eligible for the scholarship  
b. \*There seem to each other to be some applicants eligible for the scholarship
- (8) a. Someone<sub>i</sub> seems to his<sub>i</sub> mother to be in the room  
b. \*There seems to his<sub>i</sub> mother to be someone<sub>i</sub> in the room
- (9) a. There might only be one man in the room  
b. \*One man might only be in the room
- (10) a. John expected no one that I did to be elected  
b. \*John expected there to be no one that I did elected

The contrasts in (4)-(10) all point in the same direction; the associate's interpretive properties diverge from those of its overt paraphrase and the contrasts are all accommodated if the associate is interpreted from its overt position.

Consider for example the contrast in (4). (4a) requires that *many men* be interpreted within the scope of negation. This contrasts with (4b) where scoping *many men* over Neg provides the preferred reading. The fixed scope relation between *many men* and negation in (4a) follows if the former's scope is fixed by its overt position. In this structure, Neg asymmetrically c-commands *many men*. The observed scope relations follow as a matter of course. If, however, *many men* raises at LF, it is unclear why the Neg over *many* reading is the only one available. Shouldn't the covert LF movement of the associate to Spec IP permit it to scope over Neg just as the overt movement in (4b) does?

<sup>1</sup> These data have been culled from the literature. The Neg-scope data and modal data are first discussed in Milsark (1974). The binding data and focus data are presented in den Dikken (1995). The ACD contrasts are mentioned in Hornstein (1995).



Similar conundrums emerge from the other pairs of examples. Each indicates that the scope of the associate in an EC is quite different from the scope of the indefinite in the corresponding overt raising structure. This emerges in the restricted binding powers of the associate in examples like (7) and (8). It is what permits the associate to be licensed by focus and negation in (6) and (9) but forbids it to scope over the main verb in (10) (a requirement for a licit ACD reading) or the modal in (5). In each case the contrasts follow if the associate's scope coincides with its overt position. Taken together these data cast a long shadow on the Chomsky's (1986) assumption that the associate raises to the neighborhood of the expletive at LF.<sup>2</sup>

To summarize, Chomsky 1986 has shown that the associate and the expletive in ECs must be near each other. This locality condition follows on the assumption that the associate covertly A-moves at LF to the expletive or some spot nearby. We have also seen that there exists considerable evidence that the scope position of the associate is fixed by its overt syntactic position rather than the LF position it would occupy were it to move at LF to the neighborhood of Spec IP. This follows if the associate does *not* move at LF. The problem is to reconcile these contradictory conclusions in a non-*ad hoc* manner.

In the context of MP the problem is spicier still. In a GB style account it is always possible to stipulate that scope in ECs is sensitive to S-Structure (SS) position while LF movement is required as well.<sup>3</sup> This option permits one to reconcile the antagonistic data, albeit in a less than elegant fashion. However, this descriptive option is not readily available for the minimalistically inclined as SS does not exist in MP. Consequently, this brute force reconciliation of the conflicting data is not a viable option. The theoretical problem that ECs pose stands out clearly once this GB option is set aside. The aim of this paper is to outline a set of assumptions consistent with the spirit of MP that allows us to have our cake and eat it; to show that the associates move at LF to the expletive as Chomsky 1986 argues but that at LF this raised expression must delete thereby leaving only the copy in the launching

<sup>2</sup> This is essentially the conclusion in den Dikken (1995).

<sup>3</sup> This is one way of reading Diesing's (1990) proposal. She provides an analysis of definiteness effects within ECs. However, she says nothing about the locality effects noted by Chomsky. In contrast, Chomsky has an account of the locality effects but has little to say about the interpretive data. This paper shows how to unite these two stories consistently.

site for interpretation. The next section outlines the assumptions required to arrive at this happy conclusion.

## 2. Some Assumptions

Several key assumptions drive MP analyses of ECs. Consider them in turn.

- (11) Expletives satisfy the EPP

In minimalist terms, (11) requires *there* and *it* to check the D-features of Infl projections, *viz.* the strong D-feature on  $T^0$  or  $Agrs^0$ . (11) is not novel with MP though the formulation in terms of checking D-features is. This implementation is intended to code the standard assumption that expletives occupy subject positions (in overt syntax) in a technically congenial fashion.

- (12)  $Agr/Tns/V$  have features like Case and agreement that must be checked.

These features must be checked for the derivation to converge.<sup>4</sup> In other words, for a derivation to be licit, the features on the verb must be checked against an appropriate expression by LF. I assume here that these features are weak and so are checked covertly. These contrast with D-features, which are strong and so must be checked in overt syntax.<sup>5</sup>

- (13) *It* and *there* have features

The various approaches to ECs mentioned above differ in how they treat the feature composition of these expressions. Chomsky (1995: chap.

<sup>4</sup> Case features on T and V are universally non-interpretable. Agreement features are interpretable on D/NPs and Chomsky (1995:chap. 4) seems to assume that they are interpretable on T and V as well. This need not be so, however. It is quite possible that  $\phi$ -features on nominals are interpretable while being uninterpretable on T and V. This is true, for example, for D-features which are interpretable on nominals but must be checked on Infl and, perhaps, expletives like *there*.

<sup>5</sup> But see the conclusion for some discussion of weak D-features.

3) assumes that there only differs from it in being unspecified for  $\phi$ -features. He assumes (following Belletti (1988) and Lasnik (1992)), that the associate bears partitive Case. This means that there checks both the Case and D-features of Infl. The  $\phi$ -features of V are checked against the features of the associate at LF by raising the latter to Infl.

In later work, Chomsky (1995: chap. 4) drops the assumption that there has Case features. He supposes that  $\phi$ -features are interpretable and so need not be checked. Case, in contrast, is the canonical uninterpretable feature. Consequently, the associate must be raising at LF to check its Case. As a by-product, the Case and  $\phi$ -features of Infl also get checked.

In sum, the standard assumption is that there is somehow less featurally endowed than it. It has a complete feature specification: D-feature, Case and  $\phi$ -features. In contrast, there has a D-feature but may be bereft of either Case or  $\phi$ -features or both. In Chomsky's various accounts, movement of the associate compensates for the reduced feature checking capabilities of *there*. Lasnik (1992, 1995) forces movement in a very different manner (see below).

- (14) Greed, interpreted as "enlightened self-interest", governs movement

This version of Greed is proposed in Lasnik (1995). Chomsky (1995: chap. 4) adopts it in its essentials. The restriction on movement, on this interpretation, is that it is only licit if it results in a feature checking configuration in which some uninterpretable features are checked. What distinguishes this from earlier stricter versions of Greed is that it need not be features of the moved expression that get checked. For example, in an ECM embedded clause – *John expects [Bill to leave]* – the D-feature of the embedded Infl is checked by raising a nominal (*Bill*) to its Spec position. No feature of the raised expression is checked that needs to be. However, the movement is licit because the D-feature of the embedded Infl is checked as a result of this operation.

- (15) *There* is an LF affix

(15) has been assumed in various guises since Chomsky (1989)<sup>6</sup>. Chomsky (1995: chap. 4) uses it to force the associate to adjoin to *there* at LF.<sup>7</sup> This is how Chomsky explains the definiteness effect.<sup>8</sup> Chomsky operationalizes this proposal by assuming that *there* has a weak affixal N-feature that must be checked.<sup>9</sup> Raising the associate to *there* at LF suffices to check this feature.

Lasnik makes more fundamental use of (15). Following Belletti (1988), Lasnik proposes to account for the definiteness effect via partitive Case. Thus, the associate does not move for Case. It moves, rather, to check an LF affix feature on *there*. To make this account empirically viable, Lasnik (1995) proposes that the affix feature on *there* must be checked by a partitively Case marked nominal expression.<sup>10</sup>

There is something odd about assumption (15) that becomes apparent once one considers the ways that Chomsky and Lasnik exploit it. *There* is endowed with special properties: for both it is an affix, for Lasnik it is an affix that can only be checked by a especially Case marked associate. The problem is that the more idiosyncratic these characteristics are to *there* the less we have an explanatory account of ECs. In other words, if we track the properties of ECs by ascribing distinctive properties to *there* we are no longer explaining the properties of ECs by bringing them under general grammatical principles. This is why Chomsky wants to treat *there* as just another determiner. However, as is perfectly clear, it is not just like other determiners. It cannot overtly do what Chomsky and Lasnik propose it covertly does: combine with an NP or N' to yield a licit DP, e.g. *\*there a book*, *\*there dog*. Conversely, unlike standard determiners *there* need not have an overt nominal restriction, e.g. *I saw him in there/the (\*room)*. In Lasnik's account the properties of *there* are even more tailored to the observed data. The intimate relation between *there* and its associate is captured by restricting LF affix checking to

<sup>6</sup> Reprinted in Chomsky (1995:chap. 2).

<sup>7</sup> More specifically, it is the features of the associate that so move. Where it does not matter, I will talk of the category as a whole moving rather than its features. See fn. 11 for a discussion of category versus feature movement.

<sup>8</sup> Chomsky also uses this assumption to account for why in transitive expletive constructions in languages like Icelandic the word order is expletive-associate rather than the reverse. see Chomsky (1995:chap. 4).

<sup>9</sup> He follows Longobardi (1994) in assuming that this is so for all determiners.

<sup>10</sup> Observe that this appears to assume that partitive Case does not delete once checked.

partitive Case marked NPs. All things being equal, I assume that it is better not to encumber *there* with such special morphological properties.<sup>11</sup>

(16) *there* has no interpretation

(16) has a special significance since Chomsky (1986). It requires that *there* be rendered LF invisible for a fully acceptable EC to result. If *there* has not been “disappeared” by the LF interface uninterpretability results. Chomsky (1995) insists that this should not be confused with ungrammaticality. If *there* survives to the interface the derivation converges but gibberish results. The distinction between non-convergence and convergence as gibberish prevents the free deletion of *there* at LF. This accounts for why *there* is paired one to one with an associate.

Consider an example. Assume that *there* could be freely deleted. This would allow the derivation of \**There seems there to be a man here* as follows. The lower *there* checks the D-feature of the lower IP. The indefinite *a man* raises to check the Case and phi-features of the matrix Infl. We then delete the lower *there* and all should be well. However, the sentence is unacceptable. This follows if such a free deletion is not allowed.

What prevents freely deleting *there*? If derivations with *there* converge then freely deleting the expletive is *prohibited* by economy considerations. In sum, ECs with “too many” expletives converge but are unintelligible.<sup>12</sup> Derivations are driven by narrow mechanical requirements of feature checking only, not by a “search for intelligibility” or the like. Free deletion of “excess” expletives is thereby prohibited by economy considerations, *viz.* deletion is an operation with a cost which cannot be incurred unless required for convergence.

<sup>11</sup> A similar problem affects the account of ECs in Groat 1995. This paper assumes that in ECs the syntactic features of the associate move at LF to check features of Infl but that the semantic features stay in place. This is how he accounts for the observation that the associate is interpreted from its SS position. The problem with this account is that it appears that only ECs involve the kind of feature movement postulated, i.e. one in which syntactic features move but semantic features remain in place. If this is so, then the account turns on postulating special *ad hoc* properties to associates in ECs. It appears that nothing else moves in quite this way.

<sup>12</sup> See Chomsky 1995:201. Here Chomsky argues that convergent derivations might still result in unintelligible sentences. As he puts it, “Derivations are driven by narrow mechanical requirements of feature checking only, not by a ‘search for intelegibility’ or the like”.

I have beaten this horse silly for two reasons. First, it is the sole method within MP of prohibiting the free deletion of “meaningless” expressions like *there*. Second, this reasoning appears to be incompatible with treating *there* as an LF affix. Recall that technically this means that *there* has a feature that needs checking. By assumption, if this feature is not checked, the derivation crashes. However, this implies that deleting *there* does not violate economy as it permits the derivation to converge. In short, if *there* has an affixal feature that needs checking then the economy account barring free deletion is inadequate.<sup>13</sup> A *man* should be able to check the affixal N-feature in the embedded *there* and then raise to check the affixal N-feature in the matrix *there*. This should be possible as, being interpretable, the N-feature of the associate does not delete on checking the affixal feature. In short, unless the feature of the associate relevant for checking the affixal feature of the expletive deletes (after checking the affix), we lose the fact that *there* expletives are biuniquely related to associates. To accommodate this fact, it would be necessary to further assume that the associate cannot “excorporate” from the expletive even after it has checked the affixal feature of *there*.

(17) The associate in ECs is assigned partitive Case

Chomsky (1995: chap. 3) uses (17) to account for the definiteness restriction observed in ECs. As noted, he abandons this assumption in chap. 4. Lasnik crucially assumes (16) in his accounts. In English, partitive Case is assigned by *be*, unaccusatives, and passives.

(11)-(17) are exploited by Chomsky and Lasnik in their various treatments of ECs. Of these, (15) and (17) are, in my opinion, the most problematic. Treating *there* as an LF affix is quite *ad hoc*. In addition, it raises technical problems that are better avoided. Partitive Case is also problematic. Lasnik (1995) does heroic work in domesticating the idea in minimalistically acceptable terms. However, the core idea that the definiteness effect is explained in terms of partitive Case is a real stretch.

<sup>13</sup> There is actually a second technical problem with the assumption that *there* has a feature that needs checking, especially if this feature is an N-feature as Chomsky (1995:chap. 4) proposes. Categorical features like N-features are interpretable. Hence they do not delete. But this means that a single associate should be able to check multiple N-features that require checking. Consider (i).

(i) \*There seems there to be a man in the room

This is not so much an explanation as a redescription. What partitive Case has to do with indefiniteness has never been made clear.<sup>14</sup> In what follows I drop these two assumptions.

In their place I substitute two others.

(18) An A-chain has one and only one visible link at LF

(18) requires that links delete in order for the derivation to converge. The simplest assumption is that deletion is essentially free. This amounts to allowing reconstruction in A-chains, analogous to what Chomsky (1995: chap. 3) proposes for A'-chains. Hornstein (1995, 1996) argues in favor of (18) and parries the arguments in Chomsky (1995: chap. 4) against A-chain reconstruction. Technically, following Chomsky (1995: chap. 3), I assume that reconstruction is a function of the fact that movement is actually copy-plus-deletion. Trace positions are copies of moved expressions. Reconstruction amounts to deleting the moved expression and retaining the original at LF. (18) assumes that in an A-chain one and only one member of the chain is visible at the CI interface. This requires deleting all copies but one. (18) treats such deletion as a convergence requirement, i.e. multi-membered A-chains violate full interpretation.<sup>15</sup> The requirement that A-chains must delete all but one member and that either the head or the tail can delete to satisfy this requirement is central to the proposed analysis of ECs below.

(19) The mapping hypothesis proposed in Diesing (1992) is essentially correct

Diesing (1992), building on work by Heim and Kratzer, proposes a mapping hypothesis that relates LF phrase markers to post LF propositional structures. In particular, she proposes that nominals within the VP (lexical) shell are mapped into the nuclear scope of a proposition while nominals outside it are mapped into the restrictive clause. The effect of this is to provide a structural account of definites. Nominals

<sup>14</sup> There are also a slew of problems revolving around the inherent nature of partitive Case. Vikner (1990) has an illuminating discussion of this as it relates to Icelandic. Lasnik's discussion is also illuminating. Suffice it to say, that this idea needs a lot of careful technicalia to make it work. For that reason alone it is suspect.

<sup>15</sup> A and A' should be cast in terms of movement to L-related and non-L-related positions. I dispense with the technical niceties here. See Chomsky (1995: chap. 3) for discussion.

are interpreted as indefinite if and only if they are inside the lexical shell at LF. As Diesing (1990) argues, this provides an elegant handle on the definiteness effect observed in ECs given the VP internal position of associates. In what follows, I assume that this is basically the correct approach to the definiteness effect and I show how to exploit Diesing's suggestion and still allow LF movement of the associate out of the lexical shell.<sup>16</sup>

One last point. A key virtue of Diesing's approach, in my opinion, is that it relates the definiteness effect to the new/old information structure of the proposition. Nominals in the restrictive clause are presupposed and hence definite. If we assume that presupposed information is old from the point of view of the hearer and that definites are generally presupposed (See Diesing, 1992, Enç 1991, Heim 1982) then we get a pretty fair description of what is permitted in the associate position in ECs. See below for further discussion.

To sum up this section. I have reviewed the assumptions deployed in two influential minimalist accounts of ECs. In so doing I have cast aspersions on two; the notion that *there* is an LF affix and the idea of partitive Case as an account of the definiteness effect. I adopt the other assumptions. In particular, I assume that *there* only contains a D-feature, that movement is driven by enlightened Greed, that *there* is uninterpretable and yields unacceptability unless deleted, and that features on Infl must be checked. I further assume (18) and (19). Section 3 outlines how these assumptions suffice to derive the scope and locality data sketched in section 1.

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<sup>16</sup> For present purposes it does not matter whether Diesing's specific proposal is right. I mean two things by this. First, that the VP shell internal/external cutoff may not be the right one empirically. What is important is that some form of the principle be correct and that associates are interpreted as indefinite because of their being mapped into the nuclear scope. Second, nothing that I say below requires accepting Diesing's account of the mapping facts. In effect, Diesing provides a descriptive generalization and an explanation of this generalization. The description is that elements inside the VP/lexical shell at LF are interpreted as indefinite while expressions outside the shell are interpreted as definite or specific. The explanation of this fact is the hypothesis that expressions inside the lexical shell are mapped into the nuclear scope while expressions outside the shell are mapped into the restrictive clause. Being in one or the other position accounts for an expression's in/definite properties. For my purposes here, all I require is that the descriptive generalization is correct. Whether it is best explained in Diesing's terms is a separate issue, albeit an interesting one.



### 3. The Basic Analysis

I've observed that a successful account of ECs must reconcile two salient properties; the fact that there exists a local relation between the expletive and the associate and the fact that the associate's scope is determined by its overt position. The former suggests LF movement while the latter argues that the associate remains in situ at LF. The theoretical problem is to retain a movement relation between the expletive and the associate (and thereby to account for the locality facts) and show why this movement nonetheless requires that the associate be interpreted in its overt position at LF. These twin goals are achieved if reconstruction *must* follow the movement of the associate.<sup>17</sup> This is required given (16), (18) and (19) above. Consider a sample derivation.

- (20) a. [there is a man in the room]  
 b. [<sub>IP</sub> [[a man]+there] is [<sub>sc</sub> [a man] in the room]]  
 c. [<sub>IP</sub> ([[a man]+there)] is [<sub>sc</sub> [a man] in the room]]

(20a) is the phrase marker in overt syntax. At LF, the associate raises and adjoins to the expletive, as shown in (20b). Given the standard definition of checking domain (see Chomsky 1995, chap. 3: 177ff) this move puts the associate in the checking domain of the finite Infl. Here the associate checks its Case and the Case features of the finite Tns as well as the agreement features that coincide with finite tense. The movement of the associate to *there* creates the two-link chain – (*a man+there, a man*). In order to converge, (18) requires that one of these links delete. If the foot of the chain deletes, then *there* survives to the CI interface and yields a deviant output albeit one that converges (see (16)), i.e. a grammatical but uninterpretable sentence. If, on the other hand, the head of the chain deletes then (20c) results. The structure is well formed and all of the expressions that make it up are interpretable. Consequently, a fully acceptable sentence results.

<sup>17</sup> Recall, that the movement should not be encumbered with idiosyncratic properties manifest only in ECs. The problem, then, is to explain why this application of A-movement, *in contrast to other applications of the same operation at LF*, requires that the moved expression, the associate, be interpreted from its launching site. For relevant contrasts see Hornstein (1995:chap. 8) where the movement of quantified DPs is discussed.

Observe that (18) is crucial in allowing the expletive to delete. Recall that economy considerations prevent the free deletion of offending expletives. As chain link deletion is a convergence requirement, it trumps economy considerations. The upshot is that expletive deletion is permitted just in Case the expletive is part of a multi-membered chain. In this circumstance, given (18), deletion finesses economy. Adjoining the associate to the expletive creates a chain that includes the offending expletive. This affords the option of deleting *there* while respecting economy.

This analysis reconciles the tension highlighted above. The account crucially requires that the associate adjoin to the expletive. This movement accounts for the locality effects reported in (1). Furthermore, the only fully acceptable output is the one in which the associate chain at LF contains only the link corresponding to the overt syntactic position of the associate. In effect, only the structure that has undergone obligatory reconstruction yields a fully acceptable LF as only in such an LF phrase marker has *there* been licitly deleted. Note that the reconstructed associate is back in its “S-Structure” position. This is just where we want it in order to explain the interpretive data reviewed in (4) through (10). Coupled with (19), this further provides an account of the definiteness effect in terms of the mapping hypothesis. The reconstructed position is inside the lexical small clause and this position can only be filled by expressions with indefinite interpretations.

To illustrate the mechanisms more fully consider once again the locality and binding data reviewed in section 1. The locality data follow straightforwardly. The present account adopts the basic story developed in Chomsky (1995: chap. 3 and 4). Consider (1b) for illustration, repeated here in (21).

(21) \*There seems that a man is in the room

Its unacceptability derives as follows. To be fully acceptable, the associate must raise, check its Case features, those of the matrix Infl and “disappear” the expletive. However, raising fails to check the Case features of the matrix finite Infl. Prior to movement (21) has the structure (22).

(22) [there [[<sub>0</sub>+finite] seems [that [[a man] [<sub>0</sub>+finite] is [[a man] in the room]]]]]

In the embedded clause *a man* is in the Spec of a finite IP. Here both its Case and the Case of the embedded Infl are checked. Once checked, the Case features of *a man* are no longer available for further checking. Consequently, raising *a man* to the expletive at LF will leave the Case features of the matrix Infl unchecked. These unchecked features cause the derivation to crash, hence the unacceptability of (21).<sup>18</sup>

The other locality violations succumb to the same sort of account. In the three remaining examples in (1), repeated below in (23), the indefinite is in a Case checking configuration. In (23a,b) it is inside a PP and in (23c) it is the object of a Case marking verb. If the indefinite were to raise at LF to the associate, therefore, the Case of the matrix Infl would remain unchecked and the derivation would crash.

- (23) a. \*There is the man in a room  
 b. \*There is the picture of a man in the room  
 c. \*There seems that Bill saw a man

There is another derivation to consider for the Cases in (23); to raise *the man*, *the picture of a man* and *Bill* at LF. This option runs afoul of the definiteness effect. The present account treats this as a violation of the mapping principle (19); the assumption that an D/NP can be interpreted as definite if and only if it is outside the VP shell at the CI interface. The contemplated derivations leave the definite inside the VP shell. Consider (23a) for example. The relevant LF after raising the associate is (24).

- (24) [ [[the man]+there] [<sub>VP</sub> is [<sub>SC</sub> [the man] in the room]] ]

Note that raising *the man* to *there*, checks the relevant features of Infl. However, for the derivation to converge, one of the two links of the chain formed by adjoining *the man* to *there* must delete (cf. (18)). To comport with the mapping principle (cf. (19)), the lower link must delete.

<sup>18</sup> Lasnik (1995) provides a rather different account of the unacceptability of (1b), which is not compatible with the present account. He relies on the assumption that *there* is an LF affix whose affixal feature is checked by a partitively marked associate that raises to check this feature at LF. The present analysis eschews the assumption that *there* is an LF affix and dispenses with partitive Case. Therefore, a derivation along these lines is unavailable.

This, however, leaves *there* visible at the CI interface and yields uninterpretability given (16). If, on the other hand, the upper link deletes, *the man* is inside the VP and so cannot receive the right interpretation (cf. (19)). In other words, (19) and (16) cannot both be satisfied.

In the Case of an acceptable EC like (20a), the associate that reconstructs to inside the lexical shell is indefinite and so this problem does not arise. (20c) is syntactically well formed, the uninterpretable *there*, has been deleted and the reconstructed indefinite associate is inside the small clause where it can be interpreted while respecting the mapping principle.

This approach to the definiteness effect (DE) has some advantages over the one advocated in Chomsky (1995). The latter ties the DE to checking an affixal N-feature carried by *there*. NPs can check this feature as they carry the N-feature. DPs cannot. If one assumes that definites are DPs then this accounts for the observed DE. However, as Chomsky (among others) has observed, definites are not strictly prohibited from being associates. Rather, they carry special interpretations if they are. This observation raises a difficulty for Chomsky's proposal. It is unclear how ECs with indefinite associates can carry any interpretation on this account of the DE. If *there* has an N-feature that must be checked in order for the derivation to converge and N-features cannot be checked by DPs then structures with definite associates should be strictly ungrammatical.

The present account does not face analogous problems. The DE is accounted for in terms of the mapping principle, not via checking morphological features of LF affixes. No grammatical requirement prohibits associates from being definite. The restriction is interpretive. Assume, for example, that definites typically involve presupposed – “old” – information. If we take this to mean information that is new/old with respect to the hearer, then we expect to find some ECs with definite associates. The literature is filled with these. Consider the following discourse.

- (25) a. – Who will we get to play Hamlet?  
 b. – Well *there's* (always) *Bill/the guy with the red hair*

Here *Bill/the guy with red hair* is novel information for the questioner in the sense that s/he does not presuppose it to be true. Note that (25b) is not a felicitous answer to (26).

(26) Which of these guys can we get to play Hamlet

The reason is that to ask (26) felicitously the potential relevant Hamlet candidates must be presupposed in the discourse.

Of course, things are more complex than this. However, what even these cursory observations suggest is that the DE does not hinge on the morphological structure of *there* or the feature composition of DPs and NPs. Rather, it reflects how an LF is interpreted. (19) provides a plausible mechanism. Being an associate imposes certain interpretive requirements on an expression. If a definite can carry the requisite interpretation it can be an associate. Typically this is not the interpretation that definites have. However, in certain contexts it is and in those contexts definites are permitted. In sum, the present account delivers what is required: not an absolute prohibition against definite associates but one that ties their admissibility to the interpretation available (see Chomsky 1995: 384, n. 44).

Obligatory reconstruction of the associate also provides a clean account of the scope, binding and focus data reviewed in (4)-(10). Recall that the data are explained if the associate is interpreted at LF as if it resides in the position it occupies in overt syntax. If reconstruction is obligatory, as proposed here, this is precisely where the associate must be. Consider some examples in detail.

In (4) and (5), repeated below in (27) and (28), *many men* must be interpreted as within the scope of negation in (27) and *someone* is obligatorily interpreted as within the scope of the modal *must* in (28). This follows if reconstruction is required to get fully interpretable ECs. Consider the derivation of (28) in (29).

(27) There aren't many men in the room

(28) There must be someone in John's house

- (29) a. [[someone+there] must be [someone in John's house]]  
 b. [[(someone+there)] must be [someone in John's house]]

At LF, the associate *someone* raises to check its Case and the features of the finite Infl. To converge with a coherent interpretation the head of the chain deletes leaving a copy inside the small clause asymmetrically

c-commanded by the modal. If scope tracks c-command, this requires that the associate be interpreted as within the scope of the modal. A parallel derivation obtains for (27) with similar interpretive effects.

The account for the remaining data is analogous. Binding of the reciprocal by the associate is forbidden in (7) because the associate does not c-command the reciprocal at LF. Pronoun binding in (8) is similarly prohibited. In fact, all the data fall into place given the fact that the associate occupies its overt position at LF despite having raised to *there* to check features. Nothing additional need be said. Chomsky (1995) notes some data that suggests that the associate binds from its raised position. He observes that control into adjuncts is possible in sentences such as (30) though this is not generally possible from object position.

- (30) There arrived three men without PRO identifying themselves  
 (31) \*I met three men without PRO identifying themselves

Citing Cardinaletti, he observes that the possibility for control into adjuncts appears to correlate with whether or not the main verb agrees with the associate or not. In French, for example, where ECs do not show agreement with the associate, such control is impossible.

- (32) \*Il est entré trois hommes sans PRO s'annoncer  
 expl is entered three men without announcing-themselves  
 'There entered three men without announcing themselves'

Chomsky and Cardinaletti suggest that the PRO in the adjunct is bindable just in Case the associate raises at LP and induces agreement. The theoretical presupposition seems to be that unless this raising takes place, the object is not in the right c-command position to control the PRO.

This presupposition, however, has an empirical difficulty. It seems to only really hold for unaccusative constructions. Consider for example cases such as (33) and (34).

- (33) a. Several men were believed to be in the room after PRO betraying themselves by sneezing  
 b. \*There were believed to be many men in the room after PRO betraying themselves by sneezing

- (34) a. No one was taken to be on staff until PRO interviewed by Sill  
 b. \*There was taken to be no one on staff until PRO interviewed by Bill

In these examples, the favored reading has the adjunct modifying the matrix clause. Thus, in (33a) the believing follows the sneezing and in (34b) the *until* phrase modifies the matrix and is licensed by the negative *no one*. The ECs in (33b) and (34b) are both unacceptable with these readings. The only readings available have the unnatural interpretation in which the adjunct modifies the embedded clause. This is quite unexpected given the Chomsky-Cardinaletti analysis. Note that the matrix Infls agree with the associates in these cases. Consequently, the associate must move to this region at LF. Nonetheless, the indicated binding of PRO is impossible.

What this suggests is that the Control possibilities noted here have little to do with the associate c-commanding the PRO at LF. Note that this conclusion must be right on independent grounds. Objects are able to bind into adjuncts quite freely. Thus, at LF there must be structures in which they c-command the adjunct.

- (35) John met no one<sub>i</sub> before Bill introduced him<sub>i</sub>

However, they cannot control PRO. This indicates that the problem with (31) is independent of whether objects c-command adjuncts at LF. In short, there is as yet no argument against the reconstruction of associates. Note, furthermore, that controlled a PRO in an adjunct does not correlate with having wide scope in other respects.

- (36) There didn't enter many men (last night) without PRO introducing themselves

Neg scopes over *many men*, in (36) despite the latter's ability to control PRO. This is a problem on the Chomsky-Cardinaletti proposal for presumably if the associate is high enough to bind the PRO it should be high enough to scope over the neg.

I conclude from this that the correlation noted above between control and agreement has little if anything to do with the LF position of the

associate. For an analysis of these constructions consistent with this conclusion see Hornstein (1995a). The present account forgoes the assumption that *there* is an LF affix in any sense other than that adjunction to *there* is permitted. The movement is driven by Case theory, as in Chomsky's account. The interpretive features of ECs are related to the fact that the associate is in its *in situ* position at LF. The mechanism that makes this story possible is A-chain reconstruction. What forces it to take place in ECs is the uninterpretability of *there*. What allows it to occur and still respect economy is the requirement that (A-)chains have only a single visible link at LF.

#### 4. Some More Data

Lasnik (1992, 1995) argues against one key assumption made above. Modifying Belletti (1988), Lasnik presents empirical arguments against the position that the associate in ECs has its Case checked by raising at LF, the "transmission hypothesis". These arguments provide strong support for the dual contention that associates carry partitive Case and that *there* is an LF affix. In fact, the latter assumption is almost inescapable if one takes associates to have partitive Case. The reason is that without it Greed as a condition on movement is at risk. More specifically, if the associate bears partitive Case then it need not raise to Infl to check Case. Further, if phi-features are interpretable the associate need not raise to check them either. Thus, there seems to be no grammatical motivation for raising the associate at LF at all. However, as Lasnik agrees, there is strong empirical evidence that associates do raise at LF. Why do they move? To check the affix features of *there*, Lasnik suggests. In short, the path from partitive Case on associates to *there* as an LF affix is a short and steep one. This section argues that the evidence Lasnik provides does not tell against the transmission hypothesis. In sum, the assumption that associates move at LF to check Case is empirically well founded.

##### 4.1. Adjacency Effects

Lasnik (1992) notes that accusative objects must be adjacent to the verbs that check their accusative Case. Since Stowell (1981), this has



been regularly diagnosed as a fact about Case, *viz.* in licensing accusative Case, the licensor and licensee must be linearly adjacent. Stowell (1981) proposes that accusative Case be assigned under government *plus* adjacency. Lasnik (1992) is less specific as his point does not rely on the details of Case licensing. He merely observes that adjacency and Case licensing are closely related. He uses this correlation to argue that the associate is assigned partitive Case by unaccusatives and *be* in English. Lasnik highlights the following contrasts.

- (37) a. I believe there usually to be a solution (available)  
 b. \*I believe there to be usually a solution (available)
- (38) a. There usually arrives a bus (at this time)  
 b. \*There arrives usually a bus (at this time)

On the basis of these-data, Lasnik reasons as follows. If the associates Case is checked at LF via movement to the expletive then such Case checking cannot be subject to adjacency. If, however, the associates Case is checked locally by *be* or the unaccusative verb, then as with accusative Case, we might expect to see an adjacency restriction come into play, as in (37) and (38). The contrasts displayed here seem quite analogous to the one in (39) which, since Stowell (1981) have been attributed to the requirement that accusative Case be licensed under adjacency.

- (39) a. John usually eats peaches  
 b. \*John eats usually peaches

In sum, if Case adjacency accounts for (39), then the rather similar contrast in (37) and (38) should be attributed to the same restriction. This, in turn, argues against the transmission hypothesis and for Belletti's partitive Case hypothesis.

This line of reasoning, however, is inconclusive. First, theoretically, Case adjacency is hard to reconcile with a minimalist theory of Case (see Chomsky 1995: 329ff). 'Therefore, the observations in (37)-(39), though interesting, are unlikely to be linked to Case theory given minimalism. However, unless Case theory is implicated there is no reason to postulate a Case relation between the associate and *be* or *arrive*.

Second, there are empirical problems with attributing adjacency effects to Case regardless of one's theoretical commitments. Adjacency is required quite generally between a verb and its complement.

- (40) a. John usually looks into such matters  
 b. \*John looks usually into such matters
- (41) a. John usually looks over the resumes  
 b. \*John looks usually over the resumes

(40) and (41) involve selected PP complements. Here, the verb does not Case mark the PP yet an adjacency effect is evident. This suggests that adjacency effects do not involve Case.

Further support for this conclusion comes from the existence of similar effects inside DPs.

- (42) a. The student of physics with long hair  
 b. \*The student with long hair of physics
- (43) a. My interest in physics which has been rekindled  
 b. \*My interest which has been rekindled in physics

(42) and (43) involve nouns with the selected complements *of physics* and *in physics*. These do not permute with adjuncts despite the only relevant Case marking being between *physics*, and the preposition.

In sum, the data in (40)-(43) suggest that adjacency and Case should be divorced from one another. The relevant factor is not being Case marked by a verb but being the complement of a head. The generalization seems to be that internal arguments cannot be separated from their heads in English.

This perspective on adjacency suggests an alternative analysis of the requirement. Assume that there is some grammatically imposed relation between hierarchy and linear order (Kayne 1994, Chomsky 1995). All the current proposals keep elements in structural configurations like (44) together.

- (44) [x' X<sup>0</sup> YP ]

(44) is the  $X'$ -structure that relates complements to heads. On the (standard) assumption that non-complements are outside the immediate  $X'$  projection of the head, all linearization algorithms prevent separating  $X^0$  from YP. In other words, the two will be linearly adjacent. This is so regardless of YP's category. All that is relevant is that YP be the internal argument of  $X'$ . This puts YP in the complement domain of  $X'$ . Your favorite linearization algorithm does the rest.

Note that linearization only assures that  $X^0$  and YP are adjacent if they form a phrasal unit like (44) at the point that linearization applies. This is what obtains in (37)-(43). Consider (38a) for example, repeated here.

(45) There usually arrives a bus (at this time)

A *bus* is the internal argument of *arrive*. Consequently, it merges with *arrive* in a structure like (44). The adverb *usually* is outside this projection. As English verbs do not raise, *a bus* remains in the immediate  $X'$  projection of *arrives* at Spell Out. On the assumption that linearization applies at this point, *a bus* and *arrives*, must be adjacent to one another. It is impossible to interpose *usually* between the two and respect any of the current algorithms for linearization.

The same reasoning extends to examples that do not involve Case, as in (42) and (43). ECM constructions like (46) provide additional instances of adjacency without Case.

- (46) a. John strongly expects there to be a man in the room  
 b. \*John expects strongly there to be a man in the room

In (46), *there* is not Case marked by the ECM verb *expects* (see Chomsky 1995). The IP of which *there* is the Spec is the complement of *expects*. Consequently, *strongly* cannot intervene between the two expressions without violating linearity.

We have observed that linearity prevents an expression from intervening between a head and its complement if some linearization procedure exists. Note, however, that if the verb raises, then linearization won't prevent an adverb from intervening between the head and its complement. This is what happens in finite *be* ECs.

- (47) a. there is usually a bus on this corner  
 b. [there [<sub>Infl</sub> Present usually [be a bus on this corner] ] ]  
 c. [there [<sub>Infl</sub> be<sub>i</sub>+Present usually [t<sub>i</sub> [<sub>sc</sub> a bus on this corner] ] ] ]

(47b) displays the complement structure of the sentence. Lasnik (following Stowell 1981), assumes that *be* takes a small clause complement in ECs. If *be* is infinitival, then we observe adjacency effects, as in (37) above. If however, *be* is finite, then it raises to Infl and we get a structure like (47c) at Spell Out. When this is linearized, *is* is not in the same immediate X' projection with its complement and so standard linearization algorithms won't require them to be adjacent. The same analysis applies to verb raising languages like French in which adjacency does not hold between the finite verb and its complement.

I have proposed treating adjacency effects as the reflex of a linearization procedure that holds at Spell Out. For current purposes, the specific details of this procedure do not matter. All current proposals have the consequence that items in the same immediate X' projection at Spell Out will be adjacent. This suffices to derive the observed data. Moreover, the account proceeds without invoking Case in any way. If this analysis is roughly correct, adjacency effects in ECs are not evidence against the transmission hypothesis.

#### 4.2. *The Distribution of Associates*

Lasnik (1992, 1995) observes that associates in English are only found in close proximity to *be* and unaccusatives. When either of these is absent, we fail to get acceptable ECs. He points out that this is directly accounted for if associates are partitively Case marked by these verbs. Lasnik observes two particularly interesting gaps; there are no unergative ECs and no small clause existentials.

- (48) a. There arrived many men  
 b. \*There a man jumped  
 c. There is a man jumping
- (49) a. \*I expect there someone here at 2  
 b. I expect there to be someone here at 2

The contrast in (48) follows if unaccusatives and *be* can assign partitive Case and associates must be so Case marked. The problem with (48b), then, is that there is nothing to assign partitive Case to *a man*. As soon as this is remedied, as it is in (48c) with the addition of *is*, the sentence is well formed. (49a) presents an analogous problem. In contrast with (49b) no partitive Case marker/checker exists to license the associate *someone*.

The contrasts in (48) and (49) are easily accounted for if associates are assigned partitive Case by *be* and unaccusatives. They appear to be problematic, however, for an account that assumes a version of the transmission hypothesis. The problem is to pinpoint what makes these structures unacceptable if not Case.

Consider the examples in inverse order. Even apart from Case, (49a) is problematic. It is unclear what the structure of the small clause in (49a) is supposed to be. What phrase structure position does *there* occupy? If, like Lasnik, we follow Stowell (1981), the structure of a simple non EC small clause is (50).

(50) I expect [<sub>sc</sub> someone [here at 2]]

The small clause in (50) is headed by *here*. To add *there* to this structure requires licensing another Spec position. This, however, requires the addition of another head in English. The problem with (49a), then, is that there is no place for *there*. The embedded small clause has but a single Spec position. To get another, one must add another verbal element. This is what (49b) does with the addition of *be* to the array. This then allows the construction of a larger IP structure that permits a further Spec position for *there*, to occupy. In short, the contrast in (49) is not due to Case theory but is a reflection of the phrase structure of English; too many nominals and not enough slots to put them in!

(48) is more of a challenge. There is nothing obviously wrong with the structure underlying (48b).

(51) [there [Past [<sub>vp</sub> a man [jumped]]]]

(51) licenses a Spec IP position in virtue of being finite. Hence, two Spec positions are available; Spec IP to house *there*, and Spec VP

for *a man*. So the problem cannot be too many D/NPs but too few positions to house them.

There is, however, another way to rule out (48b) given current minimalist assumptions. For concreteness assume the version of MP outlined in Chomsky (1995, chap. 3) plus the theory of bare phrase structure. Observe, first, that (51) cannot underlie (48b). The reason is that unergatives, as such, cannot exist given the theory of bare phrase structure. Unergatives are actually transitives (see Chomsky (1995: 399), following Hale and Keyser (1993)). Assume that this is correct. Then the actual structure of (48b) has a non-phonetic object in complement position.

(52) [<sub>IP</sub> there [Past [<sub>VP</sub> a man [jumped OBJECT ]]]]

Assume, furthermore, that the null object in (52) must be Case checked, just like any other object. At LF, prior to raising *a man* to the expletive, OBJECT moves to Spec Agro. If English verbs move no higher than Agro at LF, then the presence of this object in Spec Agro at LF freezes the associate in place and prevents it from moving. The relevant structure is (53).

(53) [<sub>IP</sub> there [Past [<sub>AgroP</sub> OBJECT [jumped [<sub>VP</sub> a man [jumped OBJECT]]]]]]]

The associate is frozen in place as moving violates minimality. *There* and OBJECT are not in the same minimal domain. Consequently, the associate cannot raise over OBJECT to adjoin to *there*. However, if the associate cannot move, its Case and that of the finite Infl cannot be checked and the derivation crashes. Consider now the acceptability of (48c). We can account for this by generalizing a suggestion that Lasnik (1995) makes for examples like (54).

- (54) a. There was a man arrested  
 b. \*There was arrested a man

He accounts for the contrast in (54) by arguing that the passive participle marking *led*, heads a small clause with a strong D-feature in English. This requires *a man* to raise overtly to check this feature.

(55) [there [Past [be [[a man]<sub>i</sub> [-ed [ arrest t<sub>i</sub> ]]]]]]

If we make a similar assumption for the *ing*-feature, then the structure of (48c) in overt syntax is (56a). Note that the associate has raised out of the embedded small clause to the Spec of the *ing*-projection. This allows it to stay outside the reach of the object of the unergative at LF and so raise to *there*, as shown in (56b). I assume that the LF of (48c) is (56b).

(56) a [there [was<sub>i</sub> [t<sub>i</sub> [[a man] [-ing [[a man] jump OBJ ]]]]]]  
 b. [([[a man]+there)] [was<sub>i</sub> [t<sub>i</sub> [a man] [-ing OBJ [[a man] jump OBJ ]]]]]

In sum, the two gaps identified by Lasnik in the EC paradigm in English can be filled without assuming that *be* and unaccusatives assign partitive Case. This then allows us to retain the assumption that it is Case that drives the LF movement of the associate and to dispense with the assumption that *there* has affixal features that need to be checked.

### 5.3. The Belletti Data

Before concluding, we must consider one more very influential gap in the EC paradigm from Italian. The problem noted is originally due to Belletti (1988) and it has proven to be very influential. The relevant data have recently been reanalyzed by Lasnik and domesticated to fit in with minimalist assumptions. The Cases concern the contrast in (57).

(57) a. Alcuni studenti<sub>i</sub> sono considerati [t<sub>i</sub> intelligenti]  
 many students are considered intelligent  
 b. \*pro sono considerati [alcuni studenti intelligenti]  
 expl are considered many students intelligent

Belletti argues that the contrast in (57) poses a problem for the transmission hypothesis. The problem is to explain why it is that one can overtly move to Spec IP as in (57a) but that covert movement to the expletive *pro* is forbidden in (57b). This problem is resolved, Belletti argues, if one assumes that associates are licensed by partitive Case.

Actually, a little more is required. She argues that partitive Case is inherent and cannot be assigned across a small clause boundary. What prevents examples like (57b), then, is that *alcuni studenti*, cannot be partitively Case marked because a small clause intervenes nor can it be exceptionally Case marked as the verb has been passivized.

Lasnik (1995) adopts Belletti's basic analysis but adds touches of his own. First, he rejects the view that partitive Case cannot in general be assigned across a clause since he accepts Stowell's (1981) small clause analysis of ECs. He then provides a parametric technology that can account for the differences between English and Italian partitive Case marking.. It is safe to say, that whatever its virtues, the theory of partitive Case does not provide a frictionless account of the contrast in (57). Nonetheless, the contrast is puzzling and raises questions about the transmission thesis.

A possible alternative analysis starts with observing that the contrast in (57) obtains in English as well.

- (58) a. Students are considered intelligent  
 b. \*There are considered students intelligent  
 c. \*There are considered to be students intelligent  
 d. I consider students (to be) intelligent

The contrast between (58a) and (58b) duplicates the one found in Italian. Note, however, that English, according to Lasnik (1995), in contrast to Italian, allows partitive Case to be assigned across a clause. To account for the unacceptability of (58b), therefore, Lasnik develops an interesting theory of inherent Case marking whose virtues, however, are empirically restricted to Cases such as the one at issue. In short, the account though ingenious is *ad hoc*. Moreover, it doesn't account for the unacceptability of (58c). Here *be* should be able to license the associate and all should be well. However, the sentence is no more acceptable than (58b). This suggests that something other than Case is at stake.

One possibility exploits observations made in Milsark (1974). He observes that ECs do not permit individual level predicates.

- (59) a. There were people available/dancing/burping  
 b. \*There were people smart/tall/heavy



As is well known, *consider* requires that its embedded propositions involve individual level predication. Contrast (58a,d) with (60).

(60) \*I consider students singing/yelling

Note furthermore that (58d) only permits a generic (i.e. non-indefinite) reading to the bare plural *students*. All this suggests that perhaps the unacceptability of (58b,c) is due to the conflicting interpretive requirements of ECs and verbs like *consider*. Let's explore this possibility.

One way of implementing this intuition modifies a proposal in Diesing (1992) which in turn builds on Kratzer (1989). Diesing proposes that individual level predicates have PRO subjects in Spec VP which are controlled by subjects base generated in Spec IP, as shown in (61a). This contrasts with stage level predicates in which the subject is base generated in Spec VP and raised to Spec IP, as shown in (61b).

- (61) a.  $NP_i \text{ Infl } [_{VP} \text{ PRO}_i \text{ predicate } \dots ]$   
 b.  $NP_i \text{ Infl } [_{VP} t_i \text{ predicate } \dots ]$

In effect, Diesing treats the Infl positions of individual level predicates as  $\phi$ -positions. The upshot of this is to prevent the NP in Spec IP from reconstructing into the VP shell at LP. This, in turn, forces a (non-indefinite) generic interpretation onto bare plurals.

In what follows, I follow Diesing (1992) part way. Let's assume that an  $[_{sc} NP \text{ predicate } ]$  small clause cannot be interpreted as having an individual level predication unless the NP is outside the lexical shell at LF. However, pace Diesing, assume that this is not grammatically implemented via a control relation or via  $\phi$ -marking Infs. This is not a significant departure from Diesing's main idea as she provides little motivation for the technical implementation in (61) except for the observed difference in interpretation between stage and individual level predications. I here adopt her main proposal (i.e. that at LF the subjects of individual level predicates must be outside the lexical small clause) but dissent on the structural implementation in terms of distinguishing raising from control inflections. Instead, let's simply assume that an individual level predication cannot be realized in an LF like (62).

(62)  $[_{sc} NP_i \text{ predicate } \dots ]$

The proposed prohibition against individual level predication in structures like (62) suffices to accommodate Milsark's observation about the lack of individual level predicates in ECs. If associates are obligatorily re-constructed, as argued for in section 3, the LF phrase marker of an EC is (63).

(63) [ (NP<sub>i</sub>+there) Infl [<sub>sc</sub> NP<sub>i</sub> predicate ... ]]

These assumptions also suffice to account for the data in (57) and (58). Verbs like *consider* must have IP complements if it is correct that individual level predication requires the subject to be outside the lexical small clause at LF. The derivation of a sentence like (58b) proceeds as follows. First, we form the VP small clause *students intelligent*. We then add Infl. Like all Infls, this has a strong D-feature to reflect the EPP requirements of clauses. The numeration and derivation at this point looks like (64).

(64) N={was, considered, there}  
[ I<sup>0</sup> [<sub>sc</sub> students intelligent] ]

*I* has a strong D-feature that must be checked. The options are to merge *there* or raise *students*. The latter option violates procrastinate. Thus, *there* is merged. After *was* and *considered*, are added to the phrase marker, *there* raises again to check the D-feature of the matrix Infl. At LF, *students* adjoins to *there* and checks its Case features and those of the matrix Infl. The expletive is deleted and the copy of *students* in the small clause is retained.

(65) [ (students+there<sub>i</sub>) Infl be considered [<sub>sc</sub> t<sub>i</sub> students intelligent]]

The phrase marker (65) is grammatical. All relevant features have been checked, the derivation converges and there is no more economical derivation. The problem is interpretive. The matrix verb *consider* semantically requires that the embedded proposition be stage level.<sup>19</sup> To derive a fully acceptable EC, *there* must disappear, i.e. the associate

<sup>19</sup> Note, I am not assuming that this is a selection requirement. Rather, it is a fact about the meaning of *consider* in semantic combination with its propositional complement.

must be reconstructed. The problem is that this yields a structure in which individual level predication cannot be expressed given (62). In short, the interpretive requirements of ECs and *consider* don't mix.

Essentially the same account extends to (58c), with *there* inserted into Spec IP of the embedded infinitival in preference to raising the associate from the small clause.

(66) [ ([students]<sub>i</sub>+there<sub>i</sub>) Infl be considered [t<sub>i</sub> be students intelligent]]

These derivations mimic the one in Chomsky (1995: chap. 4) that accounts for (67).

(67) \*There seems that a man was in the room

(67) violates economy as *a man* raises to check the D-feature of the embedded clause. It is cheaper to merge *there* to check this feature as this does not violate PROcrastinate. The same holds for the derivation in (64), (65) and (66). Note, that this argument crucially assumes that PRO is not part of the numeration (pace Diesing (1992)). If it were, *students* would directly merge in the embedded clause and *there* in the matrix IP. With this overt syntax, LF movement of *students*, should yield a grammatical derivation and a perfectly interpretable LF, as shown in (68).

(68) a. [there was considered [<sub>IP</sub> students Infl [PRO intelligent]]  
 b. [(students+there) was considered [<sub>IP</sub> students Infl [PRO intelligent]]]

All problems dissipate if *there* is absent from the numeration. Without *there* in the numeration, the only way to converge is to raise *students* to the embedded Infl and then raise it again to the matrix position. This accounts for the acceptability of (57a) and (58a,d). Without the expletive, reconstruction is not required and the derivation permits an LF structure consistent with an individual level predication, i.e. one in which the subject of the small clause is outside the lexical small clause.

(69) a. [students were considered [<sub>IP</sub> students [ Infl (to be) [ students intelligent]]  
 b. [students were considered [<sub>IP</sub> (students) [ Infl (to be) [(students) intelligent]]]

In sum, the apparent movement asymmetry analyzed by Belletti/Lasnik in *consider* constructions can be reanalyzed as an interpretive problem exploiting prior insights by Milsark combined with ideas from Kratzer and Diesing on how to relate syntax and interpretation – (62).<sup>20</sup> To deliver the goods, I have had to rely on the assumption that ECs obligatorily reconstruct the associate to its overt position after raising it at LF. I have also relied on Chomsky's proposal that procrastinate is an economy condition that regulates derivations. This apparatus, all of which has independent motivation, suffices to accommodate the examples in (57) and (58) without adverting to partitive Case and its various distinctive properties. Happily, this also leaves the assumption that Case drives movement of the associate in ECs intact.

This section has reanalyzed the data that motivated Lasnik's reworking of the partitive Case approach to ECs first broached by Belletti. I have argued that the data that Lasnik and Belletti used to argue against the transmission hypothesis is otherwise explicable. This allows us to abandon the assumption that ECs involve partitive Case and the companion assumption that 9 expletives are LF affixes with features that require checking. In fact, if the combination of standard minimalist technology together with Diesing's proposals plus a dash of reconstruction suffices to derive the full range of data characteristic of ECs.

## 5. Conclusion

This paper starts from the observation that ECs manifest two apparently conflicting sets of data. The first, due to Chomsky (1986), indicate that the associate adjoins to the expletive at LF. This assumption accounts both for the locality facts reviewed in section 1.<sup>21</sup> The second are interpretive data that indicate that associates must be interpreted as if in their overt positions. This argues against an analysis in which the

<sup>20</sup> Raposo and Uriagereka (1990) consider a further set of cases but these do not involve the theory of partitive Case as the relevant NPs that head the small clauses they consider can be definite.

<sup>21</sup> Chomsky (1995:chap. 4) argues that this also accounts for the fact that the order of NPs in languages like Icelandic which manifest transitive expletive constructions, the order of NPs is expletive-associate and never the reverse. Chomsky's account can be duplicated here as the associate adjoins to the expletive at LF.

associate raises to the expletive at LF. I have suggested that this apparent conflict can be reconciled if associates necessarily reconstruct after raising. I have further provided a set of assumptions from which such reconstruction results. The assumptions have independent motivation and have empirical support in data other than ECs.<sup>22</sup> They conspire together to implement Chomsky's original take on ECs: *there* must be deleted for ECs to be fully interpretable.

I close with a discussion of one remaining problem. Consider (70).

(70) We expect there to be a man in the room

(70) is an ECM construction. Chomsky (1995) takes *there* to be in Spec IP in overt syntax. But (70) should be unacceptable given this assumption. The problem is to explain how *there* disappears. Observe that in contrast to cases in which *there* resides in the Spec of a finite IP, there is no reason to adjoin the associate to *there* given the present analysis. In fact, so moving violates even the weak version of Greed adopted here. There is no problem moving the associate to Spec Agro of the matrix verb (or to adjoin to its outer Spec) at LF and so check the relevant features. The problem is how to eliminate *there*.

One possibility is to assume that Agro, like Agrs, has a D-feature that needs checking. This D-feature, however, is weak not strong as in EPP contexts. The presence of this D-feature in Agro (or on the accusative Case checking verb in an Agrless theory) attracts *there* at LF. The associate is subsequently adjoined to *there* to check Case and  $\phi$ -features.

(71) [<sub>we</sub> [<sub>AgroP</sub> ([a man]+*there*) [<sub>Agro</sub>+expect [<sub>t<sub>i</sub></sub> expect [<sub>t<sub>i</sub></sub> to be  
[[a man] in the room]]]]]]

<sup>22</sup> Lasnik (1995b) proposes to reconcile the tension noted here by only moving features at LF. The problem is that he combines this with overt movement of NPs in every other construction. This makes feature movement unique to ECs and so deprives it of independent empirical motivation. The problem is not, in my opinion, to find a technology that resolves the problem, but to propose a theoretical framework in which ECs are not particularly special. The mechanism of reconstruction proposed here has independent motivation; see Chomsky (1993:chap. 3), Hornstein (1995:chap. 7 and 8) and Hornstein (1996).

In other words, the weak D-feature attracts there at LF and then the derivation proceeds exactly as in derivations where a strong D-feature is checked. The intuition embedded in this view is that Case on heads (e.g. T and Vs) comes packaged with D-features. Case and D-features are inseparable.<sup>23</sup> Bundling features together has recently been proposed for agreement and nominative Case. Nominative always comes wrapped up together with agreement.<sup>24</sup> The suggestion about D-features and Case is similar.<sup>25</sup>

One of the most powerful arguments in favor of abstract LF movement comes from considering the locality effects manifest in ECs. The force of the argument, however, has been blunted by the apparently contradictory interpretive data. The aim of this paper has been to outline a set of “minimal” assumptions able to reconcile these conflicting data. To the degree that this analysis has succeeded it provides additional empirical support for and refinement of the core assumptions of the Minimalist Program.

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<sup>23</sup> This does not mean that D-features cannot stand alone. All that is required here is that Case cannot be a feature of a head without a D-feature there as well. One might interpret this as proposing that Case is actually a property of D-features, features that nominals have inherently but that Tns and verbs do not.

<sup>24</sup> As with D-features and Case, one may find agreement without nominative Case.

<sup>25</sup> Lasnik (19995) adopts a suggestion by Koizumi (1993) to check Case overtly. This requires raising objects to Case checking positions in overt syntax. If we assume that overt raising of objects coincides with strong D-features then the proposal in here and the one in the text coincide.

However, in the context of the present account, there is a problem with the general Koizumi-Lasnik proposal. The present analysis relies on the assumption that some Case is only checked at LF, viz. the Case of the associate. Lasnik (1995) assumes that *there* carries Case as he assumes that LF movement is driven by the requirement that the affixal features of *there* be checked. The present analysis has argued against this proposal.

There is a further potential problem with the Koizumi-Lasnik thesis. The cost of checking accusative Case in English by overt movement is a richer functional structure above the VP than is standardly assumed. If Case is checked by overtly moving the accusative D/NP, then the verb must also raise to locate itself to the left of the accusative in overt syntax. This movement must be to some functional position below TP given the differences between English and French verb-raising. As always, the postulation of more functional structure than meets the eye must be strongly motivated. I fail to see that it is well motivated in the particular instance at hand. For these reasons, my preference is for the weak D-feature/covert raising approach. However, the second serves current purposes just as well.

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## HOW LONG WAS THE NINETEENTH CENTURY? \* (Quanto Tempo Durou o Século XIX?)

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*ABSTRACT: This paper reviews the problems of the deterministic and predictive view of language change initiated by nineteenth century linguists and shows that such a view is still present in many analyses proposed by twentieth century linguists. As an alternative to such a view, the paper discusses an approach along the lines of Niyogi and Berwick (1997), which takes the explanation for long-term tendencies to be a function of the architecture of UG and the learning procedure and of the way in which populations of speakers behave.*

*KEY WORDS: Historical Linguistics, Langue Change, Language Acquisition, Dynamic Systems*

*RESUMO: Este artigo faz uma revisão dos problemas da concepção determinista e preditiva da mudança lingüística iniciada pelos lingüistas do séc. XIX e mostra que tal visão ainda está presente em muitas análises propostas por lingüistas do séc. Como alternativa a essa concepção, o artigo discute uma abordagem nos moldes de Niyogi e Berwick (1997), de acordo com a qual tendências de longo prazo surgem em função da arquitetura da GU e do procedimento de aquisição, e da maneira como as populações de falantes se comportam.*

*PALAVRAS-CHAVE: Lingüística Histórica, Mudança Lingüística, Aquisição de Linguagem, Sistemas Dinâmicos*

### 1. Introduction

Darwin and Marx had a view of history which has been very influential in the past century, a view which involves *principles of history*. Contemporary linguists were intimately involved in this distinctive, nineteenth-century approach, influencing Darwin and Marx and being influenced by them. And those linguists have shaped the way in which

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\* This paper deals with the history of ideas on the connection between language change and language acquisition, two matters of central importance in Mary Kato's work. The ideas are discussed in more detail in Lightfoot 1998.

the rest of us have thought about language change. Here I want to trace the history of some of these ideas and to show how they continue to influence our work.

The ideas I identify were real, although the reality is more complex than I will describe. Anna Davies (1998) noted that “starting with the nineteenth century there is ... a chasm between what linguists do and what they say they do. The method and achievements of the historical and comparative linguists of the nineteenth century must be extracted from their technical work rather than from their prefaces or their rhetoric” (p. xxvi). The same is true of modern work.

The study of language goes back to the early Arabic grammarians, to Greece and India in the pre-Christian era. Not much of the early work on language dealt with change and historical relationships, but some did. Sir William Jones is the marker. His famous after-dinner speech, postulating a common source for Greek, Latin and Sanskrit, which may no longer exist, initiated an unbroken tradition and eventually gave birth to an independent discipline which we now call linguistics.

Linguistics began as a *historical science*. There are many questions one can ask about language: how it is acquired by children, how it is used by poets, how it varies sociologically, and so on. The central research question for the nineteenth century was: how did a language get to be the way that it is? As far as Hermann Paul was concerned, this was the only possible question: “It has been objected that there is another view of language possible besides the historical. I must contradict this” (1891: xlvi).

As the nineteenth century progressed, historical “laws” were formulated with ever greater precision. In 1822 Jacob Grimm revised his *Deutsche Grammatik* by adding a 595 page account of the phonology of some fifteen different languages and stages of languages. He postulated his famous law dealing with consonant shifts in Germanic. There were several exceptions, cases where Grimm’s correspondences did not hold. Others cleared them up in the course of 50 years. The coup de grace came in 1875. Grimm’s Law worked quite well for initial consonants, but there were still many apparent exceptions in medial position, and they were explained away by Karl Verner. Verner’s Law yielded a more or less complete understanding of the evolution of the Germanic consonantal system and led to the triumphant notion that this was the

way things always were: sound change was always regular, exceptionless, and phonetically conditioned.

The idea that sound change was regular and systematic was formulated in 1878 in the preface to Osthoff & Brugmann's somewhat spasmodic journal *Morphologische Untersuchungen*, and the people who held the idea of exceptionless regularity were the neogrammarians. They are generally characterized in terms of their ideas about regularity and phonetic conditioning of sound change, but the manifesto stressed two other points: that a language is not an object with a reality of its own independent of its speakers (contra the Hegelian, group-psychology notions of Schleicher and others), and that the psychological and physical aspects of language must always have been the same.

The field of linguistics first identified itself by claiming that language history was *law governed*, even if the notion of law was scarcely that of Boyle's Law or the law of gravity, which are timeless. The term referred to specific sound changes or "correspondences" affecting specific languages at specific times. Languages were supposed to change in systematic ways and historical linguists, perhaps more than other kinds of linguists, have always been concerned with issues of explanation. The question then arises of what kind of explanation could be offered for sound changes of this type.

## 2. Historical explanations

Work on language history at this time reflected the dominant models of what a science should be: Newtonian mechanics and Darwin's theory of evolution. Newton had all phenomena describable by deterministic laws of force and motion, in such a way that all future states were, in principle, predictable in a "straight-line", linear fashion from a complete knowledge of the present state. This inspired the notion of sound *laws* to describe the history of changes. Darwin was inspired by work on language history, and he in turn inspired the linguists to view languages as natural organisms, on a par with plants and animals. This influence was explicit in the writing of Schleicher, Bopp and Pott.

Nineteenth-century linguists knew that language reflected psychological properties. However, the psychological notions of the time

were problematic, partly because they were wrapped up in ideas of Hegel's. Grimm, for example, adopted a mystical belief in a Hegelian *Spracheist*, which had some existence above and beyond individuals. This group psychology was attacked by Paul (1891: xxxivf) as being incoherent :

All psychical processes come to their fulfilment in individual minds, and nowhere else. Neither the popular mind (*Volksgeist*), nor elements of it, such as art, religion, etc., have any concrete existence, and therefore nothing can come to pass in them and between them. Away, then, with these abstractions!

Whatever the problems, linguists separated this kind of psychologizing from their day-to-day work. Pedersen's (1931) survey of the nineteenth-century scarcely refers to the psychologists at all. Similarly Davies 1998 has little to say. Whitney put the demarcation clearly:

The human capacity to which the production of language is most directly due is ... by no means a unitary capacity; on the contrary, it is a highly composite and intricate one. But it does not belong to the linguistic student to unravel and explain ... it falls, rather, to the student of the human mind and its powers, to the psychologist. So with all the mental capacities involved in language ... (1875: 303).

And this was the general view of linguists through Bloomfield and the structuralists: leave psychology to the psychologists. So linguists generally did not appeal to psychology to explain historical changes. Instead, there were independent laws of history to be found.

The neogrammarians were the culmination of this research paradigm but they confronted two major problems. First, there were regularities of language change which could not be stated in purely phonetic terms, which suggested that it wasn't the language or the sounds which were changing but rather some kind of abstract system. This matter has been discussed by twentieth century generativists but it didn't bother nineteenth-century linguists, because they thought of language as a collection of words, with everything else due either to universal "logic" or individually variable "habits". So there wasn't anything to have a history of except words, their pronunciations, and their meanings.

There was much debate, however, about the second problem, the causes of sound change. Grimm's and Verner's Laws were not general laws like Boyle's Law and therefore they required a deeper explanation. Changes were taken to be *directional* ... as in biology, where the replacement of one species by another was taken to result from a mutation which yields an organism which is more successful in the struggle for survival in a particular environment. Rask 1818 held that languages became simpler. Darwin 1874 thought that words became shorter and easier to pronounce. Schleicher 1848 identified a progression from isolating to agglutinating to inflectional types, although this was said to hold for preliterate societies and Rask's drive to simplicity was relevant for postliterate societies. There was widespread agreement that language change followed fixed developmental laws and that there had to be a direction to change, but there was active disagreement about *which* direction that was. This was a matter of live dispute. By the end of the nineteenth century there was an enormous body of work on sound correspondences between historically related languages and vast compendia of changes which had taken place in many Indo-European languages. But alongside such compendia there were few ideas of why those changes had happened. Eventually the directionality view crumbled.

The notion that languages became simpler/more natural/easier to pronounce was, first, circular. "Simpler" etc is what languages change to and there was no independent definition in a framework dealing entirely with historical change. Linguists regarded their work as essentially concerned with language change, so they sealed it off from other concerns, and did not work on language acquisition in an integrated way. So they had no independent way to define their central notions.

There were occasional attempts to break out of the circle by invoking psychology, but the psychology was implausible. So Grimm (1848) explained his law of consonant shifts as

connected with the German's mighty progress and struggle for freedom ... the invincible German race was becoming ever more vividly aware of the unstoppable of its advance into all parts of Europe ... How could such a forceful mobilization of the race have failed to stir up its language at the same time, jolting it out of its traditional rut and exalting it? Does there not lie a certain courage and pride in the strengthening of voiced stop into voiceless stop and voiceless stop into fricative?

Explanations of this kind never had much going for them, but they were curiously resistant. One has to see them for what they are: psychological elements introduced into essentially a historicist and a-psychological theory as an attempt to break out of a narrow circle and reach some level of explanation.

By the early twentieth century the data of linguistics were an inventory of sound-changes occurring for no good reason and tending in no particular direction. The historical approach had not brought a scientific, Newtonian-style analysis of language, of the kind that had been hoped for, and there was no predictability about changes. So the program was not viable: no sustainable explanations were available for the observed phenomena, i.e. historical changes, and there was no science of history which met nineteenth-century demands.

The historicist paradigm was largely abandoned in the 1920s, i.e. the notion that there are principles of history to be discovered, which would account for a language's development. In fact, there was a virulent anti-historicism in the writing of structuralists like Boas, Bloomfield, and Sapir. They worked on language change to their deaths, showing that the traditional methods were as applicable to the unwritten, indigenous languages of North America as they were to Indo-European. However, they abandoned *historicism*; they abandoned the earlier program of seeking to explain how it was that languages came to be the way they are. The perceived problems related to the circularity of invoking historical principles and to the psychological claims. Sapir (1929) wrote that the psychological interpretation of language change was "desirable and even necessary" but the existing psychological explanations were unhelpful and "do not immediately tie up with what we actually know about the historical behavior of language." Bloomfield (1933: 17) complained about the circularity of Paul's psychologizing, saying that there was no independent evidence for the mental processes other than the linguistic processes they were supposed to explain.

### 3. Determinist views of history

The deterministic view of history that the linguists articulated, the idea that there are laws which determine the way that history proceeds, is a hallmark of the nineteenth century. Biologists, political historians,

and linguists expected to find principles of history, as if there were a Newtonian-style, deterministic, predictive theory of change to be found. Give us a full, detailed, and accurate description of an organism or a political organization or a language, and, armed with our theory of change, we will give you a prediction of what that organism, society, or language will be like in two hundred years.

Darwin was too much of a Victorian not to appeal to notions of progress, but he was critical of the notion and modulated in his appeals to it. Marx too had an interesting theory of change in which ideas are socially embedded and are amended through conflict, through the clash of theses and antitheses. Marx's view of social change and revolution, in which small insults to the system build up until the system itself breaks, is quite a sensible approach. However, Marx was very much a nineteenth-century thinker in that he was caught up in notions of predestiny and determinism, particularly in theories of history, developing historical laws prescribing that a feudal society must necessarily develop into a mercantilist society, a mercantilist into a capitalist society, capitalism into socialism, and socialism into communism. For Marx, the real task of economics was to explain how society evolved over time. At his funeral, Engels eulogized him in a way that he would have liked: "Just as Darwin discovered the law of evolution in organic nature, so Marx discovered the law of evolution in human history."

Marx's approach to political history grew out of the idea that there is a political *science*, a science of the relationships of human beings to each other and to their environment, meeting usual scientific standards. This was an idea of Hobbes and Spinoza and their followers, and it became more and more powerful in the eighteenth and nineteenth centuries, as the natural sciences flourished and as the view emerged that anything not reducible to a natural science could not properly be called knowledge at all. The scientifically minded philosophers of the eighteenth century believed passionately in such laws, in a science of society.

It has been argued that not only is there a single, coherent, evolutionary process to history, but that it even has an end-point. Hegel and Marx believed that there was an end-point: the evolution of human societies would end when mankind achieved a form of society that satisfied its deepest and most fundamental longings. For Hegel, this was the liberal state, and for Marx the end-point was communism. Francis

Fukuyama (1992) has argued that the natural end-point is a liberal democracy and that this end-point has now been reached, at least as an *ideal*.

The “inexorable laws of history” never rose above the level of truisms (“most revolutions are followed by reaction”). As a system of knowledge, their fundamental problem is that they are too principled and exclude contingency, accidental, unpredictable factors which may have large consequences. That is where proponents of a determinist science of history are quite wrong.

This problem is aggravated by another nineteenth-century problem: formulating the primitives in terms of gross categories like classes and types of society (mercantile, capitalist, etc). In contrast, Jon Elster has emphasized the need to approach political psychology through what he calls methodological individualism: one must “study the individual human action as the basic building block of aggregate social phenomena” (1993: 8). This is a self-consciously reductionist account, seeking to explain the complex by the simple. When we talk about linguistic change, we face a precisely analogous problem in deciding the scale of our categories. I echo Elster’s view and argue that we understand “language change” best as an aggregate of changes in individual grammars.

At the other extreme, many scientists do not consider history a part of science, because it deals with particulars and contingency. But this too is not right and reflects a false taxonomy: history may resist straightforward analysis because it is so contingent, but that does not necessarily make it not part of science, rather just a different type of science, as Stephen Jay Gould has argued in many places.

History is chaotic in a technical sense. Systems are sensitive to slight variations in initial conditions. The fact that deterministic rules are indeed at work does not mean that there is predictability – they do not govern sequences of events. The deterministic laws governing weather formation are unstable and miniscule changes at one location may percolate through the system to cause major effects elsewhere – sensitive dependence on initial conditions. There is no hope of making long-term weather forecasts taking such elements into account. But there is order behind chaos; deterministic rules are at work, and there may be simple causes for complex and unpredictable effects.



If language or social history works like this, through what we may think of as scientific chaos, we shall never predict the future development of German grammars or of the US political system, but we may be able to understand local effects, why some change took place in the way and at the time that it did, if we can identify the source of the perturbation. Given a good theory of grammar or society or a species, then we may be able to predict that if the initial conditions are changed a little, then a particular grammar, society or species may change in some fashion; but in general, one cannot predict changes in initial conditions. Change is explainable but not predictable. This means that if one wants to know what will happen with the weather, the stock market, US political history, or the development of German, the best one can do is turn the system on and just watch it unfold.

#### 4. Historicism

This kind of approach, however, is not generally adopted. The nineteenth century has lasted a long time and much modern work is more historicist and perpetuates that special, nineteenth-century, predictive view of history. There has been a revival in work on change over the last few decades, but the nineteenth century teleological ideas about history have re-appeared in the modern work.

In the 1970s much work focused on the notion of “drift”, originally due to Sapir 1921: ch.7. Unlike Sapir, the typologists invoked a drift as an *explanans*, not the *explanandum*. The typologists remained faithful to the methods of the nineteenth century. They retained the same kind of determinism and they dealt with the products of the language capacity rather than with the capacity itself. Other modern approaches have been less conservative. They have formulated change in terms of changes in grammars, abstract systems of some kind, but they have retained the commitment to a nineteenth-century, deterministic, predictive history.

These approaches compare the grammars of various stages of a language and identify tendencies at the grammatical level. So some people have argued that grammars tend to simplify over the course of time. Bauer (1995) offers a historicist approach in a novel guise, appealing to biological factors which predict historical developments. Bauer adapted the work of the typologists and avoided postulating

principles of history. Like the typologists, Bauer was interested in big, comprehensive changes. Bauer argued that Latin was a thorough-going left-branching language, which changed into a thorough-going right-branching system in French.

There are several differences in branching structure between the grammars of Latin and French. In fact, right-branching structures emerged at very different times and at different rates. Those details, if they could be established, might enable us to track changes in primary linguistic data, which is what we need for an explanation of the grammatical shifts.

That, however, is not Bauer's view of how the changes should be explained. Rather, she views the change in direction of branching as irreversible and adopts an "evolutionary concept of language change", under which general and irreversible language changes are viewed as analogous to evolutionary change at the phylogenetic level. Under this view, general, linear, irreversible, and unidirectional changes are due to a natural selection process. She speaks repeatedly of the "advantages" of a right-branching system and she implements these ideas by arguing that the switch from left branching to right branching represents evolutionary progression because left-branching languages, at least as manifested in Latin, were hard for children to acquire. Hence the progression to right-branching structures:

Latin must have been a difficult language to master, and one understands why this type of language represents a temporary stage in linguistic development (p. 188).

So, she explains her diachronic change not in a mysterious theory of history but rather in terms of human biology: our brains work in such a way that complex structures in left-branching languages are hard to acquire. This is more sophisticated but, of course, it immediately raises the question of why early Latin would have been left branching:

If left-branching structures are less recursive and are acquired with greater difficulty, it is indeed legitimate to wonder why languages, in an early period, exhibit this kind of structure (p. 216).

She concludes that this "still remains to be explained".

## 5. Diachronic reanalyses

Consider now a very different kind of work, more mainstream generative, but of a kind which also appeals to deterministic views of history. A grammar emerges in a child on exposure to some particular trigger experience (PLD): a different grammar may emerge if a child is exposed to a different trigger (1).

- (1)  $PLD_1 (UG \rightarrow \text{grammar}_1)$   
 $PLD_2 (UG \rightarrow \text{grammar}_2)$

It is a fallacy to think that there could be formal operations that relate sets of PLD.

- (2)  $PLD_1 \rightarrow PLD_2$

The notion of a formal device operating on sets of PLD has reappeared in the recent work of Ian Roberts and it is linked to an attempt to explain some changes entirely through UG, independently of changes in trigger experiences. Roberts 1993 invokes a new technical device, what he calls a Diachronic Reanalysis, taking place in the sixteenth century. Modal verbs and periphrastic *do* originally moved to a Tense position (T) (3a) but came to be base-generated there (3b). A Diachronic Reanalysis maps one analysis into another.

- (3) a.  $NP_i [_{T} \text{do}/M_j T^1] e_j [e_i \text{VP}] \Rightarrow$   
 b.  $NP [_{T} \text{did}/M] \text{VP}$

Diachronic Reanalyses, we are told, are provoked by principles of acquisition, often by the “Least Effort Strategy;” the Least Effort Strategy led children to reanalyzè (3a) as (3b) in the sixteenth century. So early grammars had structures like (3a) and later grammars had simpler structures like (3b). The learning strategy is: when faced with highly ambiguous PLD, children acquire a grammar with covert movement rather than one with overt movement. They “follow a least effort strategy in that they try to assign the simplest possible parse to the input string” (Clark & Roberts 1993: 335).

In reality, Roberts' Diachronic Reanalyses and parametric shifts more generally are NOT provoked by the Least Effort Strategy. Specifically, the Least Effort Strategy cannot be "the sufficient condition for the move from one step to the next" (Roberts 1993: 159). If there were no change in trigger experiences, there would be no changes in grammars. If representations like (3a) disappeared, it was because they ceased to be triggered; the children who did not acquire them must have had different experiences from earlier generations – it wasn't because they were more sensitive to the demands of the Least Effort Strategy.

Hale (1998: 13) discusses this nicely. He points out that in the model that Roberts & Clark assume, parsing requires that a child posit a numeration, find that that numeration can converge at LF when run through the computational operations, and posit the relevant features on the functional heads to allow convergence at PF. "As Chomsky has pointed out (Chomsky 1995: 227), Economy of Derivation is relevant only to the evaluation of derivations involving the same numeration. It cannot, therefore, be invoked to choose between these two competing hypotheses since they involve different numerations" (p. 14).

Paul Kiparsky is also impressed by what he takes to be long-term tendencies and by asymmetries like the prevalence of object-verb systems changing to verb-object and the rarity of the reverse development (Kiparsky 1996, 1997). Like Roberts, he also appeals to internal causal factors, forces of "endogenous optimization." However, Kiparsky's position is different. He does not rely exclusively on these endogenous forces and he requires other, external factors to interact with the internal forces. He argues that the "enabling cause" of the change to verb-object syntax in certain languages was verb-fronting in embedded clauses. Yiddish, for example, shows general verb-fronting, where verb-fronting appears in all types of embedded clauses, while German grammars show verb-fronting only in matrix clauses and in embedded clauses with no complementizer. Yiddish has sentences like those of (4a), where the equivalent in German would be (4b).

- (4) a. A jidisch mejdl hot sech barimt, as efscher hundert mol **hot** men si schojn gebetn, as si sol chassene hobn.  
'A Jewish girl bragged that she had already been asked perhaps a hundred times to marry.'
- b. Ein jüdisches Mädel hat sich gerühmt, dass man sie vielleicht hundert Mal schon gebeten **hat**, dass sie heiraten **soll**.

The generalization is that “the shift from a head-final base to a head-initial base took place in exactly those languages which developed general verb-fronting in embedded clauses” (p. 155).

So Kiparsky’s claim is that the change to verb-object order is an endogenous optimization, but one which is “enabled” by certain verb-movement operations. Crucially for our discussion here, Kiparsky recognizes that grammaticalization or, more generally, any optimization account does not by itself explain why a change takes place in one language but not in another closely related one. His account motivates the change to verb-object syntax through environmental factors, and to that extent the account is not historicist, even though he invokes optimization effects. Such a story raises empirical questions about how much influence the internal and external factors each had, but it does not raise the methodological problems of historicism.

## 6. Trajectories

So far I have been sceptical of work offering principled explanations for purported, long-term historical tendencies, because the historicist principles strike me as implausible. I do not see how an historical law can be anything other than an epiphenomenon, an effect of other aspects of reality.

Niyogi and Berwick (1997) (NB) have developed an interesting computer simulation of language change, which suggests more sympathetic ways of thinking of these long-term tendencies, which do not fall into the traps I have discussed.

As is natural, NB take the problem of grammar acquisition at the individual level as leading logically to the problem of language change at the group or population level. If it is possible that children might not converge on the target grammars, then, over several generations, this could drive language change. If one has an adequate grammatical theory, then there are two means by which the linguistic composition of the population might change over time.

First, the primary data presented to the child might change in some critical way. This might happen because of contact with another speech community, presence of foreigners, or just because the speech community

has taken to using some construction more or less frequently than in a previous generation or with a special kind of pragmatic force.

Second, even if the PLD comes from a single target grammar, the actual data presented to any learner are truncated, some finite subset of what the grammar is capable of generating. After a finite (truncated) sample sequence, children may arrive at a grammar different from that of their parents. This can again lead to a differing linguistic composition in succeeding generations. Once one child converges on some new grammar, then the linguistic composition of the population will change, because the child with the new grammar produces different structures: this may have domino effects.

In short, the diachronic model is this: individual children attempt to attain their caretaker grammar. After a finite number of examples, some are successful, but others may misconverge. The next generation will therefore no longer be linguistically homogeneous. The third generation of children will hear sentences produced by the second – a different distribution – and they, in turn, will attain a different set of grammars. Over successive generations, the linguistic composition evolves as a dynamical system (p. 2).

NB develop a computer simulation which contains precise assumptions about a set of relevant parameters, a learning algorithm, and the primary linguistic data. If these three items are specified appropriately, then the system computes the linguistic composition for the next generation. By repeating the process, NB compute the evolving composition of the population from generation to generation and arrive at a dynamical system.

First, they assume Gibson & Wexler's Trigger Learning Algorithm and they consider the case of a homogeneous population, with no noise or confounding factors like foreign target languages. "Some small proportion of the children misconverge; the goal is to see whether this small proportion can drive language change – and if so, in what direction". (5) shows the language mix after thirty generations. +V2 refers to a grammar with the verb-second property, and the grammars may or may not be Specifier-final or Complement-final, so there are eight language-types.

(5) Initial Language	After 30 generations
1 (-V2)	2 (0.85), 6 (0.1)
2 (+V2)	2 (0.98): stable
3 (-V2)	6 (0.48), 8 (0.38)
4 (+V2)	4 (0.86): stable
5 (-V2)	2 (0.97)
6 (+V2)	6 (0.92): stable
7 (-V2)	2 (0.54), 4 (0.35)
8 (+V2)	8 (0.97): stable

This model generates some striking patterns. First, homogeneous populations may split into different groups and they may split at different rates. For example, a population of Language 7 speakers splits over 5-6 generations to one with 54% speaking Language 2 and 35% speaking Language 4 and remains that way with no further shifts through thirty generations. On the other hand, Language 1 eventually gravitates to Language 2, but very little happens over the first six or seven generations, and then the population changes at a much faster rate.

Second, all the verb-second languages are relatively stable and their linguistic composition did not vary significantly over thirty generations. On the other hand, the non-verb-second languages all drift to verb-second. So a population of Language 1 speakers winds up speaking mostly Language 2.

NB have a model which generates diachronic trajectories; there are long-term tendencies for certain language types to change to certain other types. Some of the actual tendencies that they find in their initial model are not realistic. Their model shows verb-second languages to be quite stable, but we know that both English and French lost their verb-second properties, an observation that needs to be explained.

Immediately then, we see that our dynamical system does not evolve in the expected manner. The reason could be due to any of the assumptions behind the model: the parameter space, the learning algorithm, the initial conditions, or the distributional assumptions about sentences presented to learners. Exactly which is in error remains to be seen, but nonetheless our example shows concretely how assumptions about a grammatical theory and learning theory can make evolutionary, diachronic predictions – in this case, incorrect predictions that falsify the assumptions (p. 6).

NB then proceed to change assumptions and to derive different trajectories. For example, they drop one particular constraint from Gibson & Wexler's Trigger Learning Algorithm (the Single Valued Constraint) and the dynamical system yields very different results, shown in (6).

(6) Initial Language	After 30 generations
1 (-V2)	2 (0.41), 4 (0.19), 6 (0.18), 8 (0.13)
2 (+V2)	2 (0.42), 4 (0.19), 6 (0.17), 8 (0.12)
3 (-V2)	2 (0.40), 4 (0.19), 6 (0.18), 8 (0.13)
4 (+V2)	2 (0.41), 4 (0.19), 6 (0.18), 8 (0.13)
5 (-V2)	2 (0.40), 4 (0.19), 6 (0.18), 8 (0.13)
6 (+V2)	2 (0.40), 4 (0.19), 6 (0.18), 8 (0.13)
7 (-V2)	2 (0.40), 4 (0.19), 6 (0.18), 8 (0.13)
8 (+V2)	2 (0.40), 4 (0.19), 6 (0.18), 8 (0.13)

Under this scenario, all initially homogeneous populations eventually drift towards the same composition mix after thirty generations, 2, 4, 6, 8. As under the earlier scenario, all populations drift to a population mix of only verb-second languages, and again there is a tendency to gain verb-second systems, contrary to the facts of the history of French and English.

This work opens up the possibility of revising the model in such a way that the diachronic trajectories generated correspond most closely to those that are actually attested, and this introduces a new criterion for the success of a grammatical model. So one seeks a class of grammars and learning theories which yields a dynamical system which matches that of the true evolution of human languages.

That, in turn, suggests that maybe one day work on long-term tendencies may not be as mysterious as I have suggested here, that one can find explanations for long-term diachronic tendencies in terms of the shape of the learning mechanism. To be sure, progress will come only through the precise kind of work that NB exemplify and not through the anecdotal generalizations which have typified some of the work discussed earlier. There will be no historicist principles nor any primitive principles of change. Rather, the explanation for the long-term tendencies, if they emerge, would be a function of the architecture of UG and the learning procedure and of the way in which populations of



speakers behave. In that way the historical tendencies would be epiphenomena, derived in an interesting fashion and not stipulated by brute force.

## 7. Conclusion

Work on language change has been dominated by nineteenth-century thought. It has been too ambitious, too principled, and has sought to explain too much. Where does that leave us? Language change is fascinating because it represents an interaction between chance oscillations in the trigger experience and the biological necessities of the human language acquisition device. Change may be chaotic and flukey, but it is explainable to a degree. To explain language change, one needs: (a) an account of how trigger experiences shifted and (b) a theory of language acquisition that matches PLD with grammars in a deterministic way. A minor perturbation in a child's trigger experience may entail a new grammar which, in turn, yields dramatically different phenomena. This "sensitive dependence on initial conditions" accounts for why languages change in fits and starts. So we strike a chord with work on "punctuated equilibrium" in biological change and with the work of historians seeking to account for revolutionary political change as the product of minor shifts. But we do so without invoking deterministic, nineteenth-century style laws of history.

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**FORMAL FEATURES AND PARAMETER SETTING:  
A VIEW FROM PORTUGUESE PAST PARTICIPLES  
AND ROMANCE FUTURE TENSES\***

(Traços formais e fixação de parâmetro: uma perspectiva a partir dos participípios passados do português e do futuro românico.)

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*ABSTRACT: This paper examines the morphophonological shape of Portuguese past participles, including the stress placement change that took place from Latin to Portuguese in these forms, and argues for a concept of formal feature more abstract than the concept of morphosyntactic feature. Parameter setting is treated as relating to the location in which the configuration of formal features relevant to grammatical semantic interpretation is visible to the PF system. The stages in the development of the Romance future tenses are claimed to follow from a shift in the visibility of the sentential functional heads.*

*KEY WORDS: Portuguese past participles, Romance future tenses, Stress, Formal feature, Grammaticalization, Parameter setting, Language change.*

*RESUMO: Este artigo examina a forma morfofonológica dos participípios passados do português, incluindo a mudança na colocação do acento ocorrida na evolução do latim para o português, e argumenta a favor de um conceito de traço formal mais abstrato do que o de traço morfosintático. A fixação paramétrica é tratada como uma questão da localização onde a configuração de traços formais relevante para a interpretação semântica gramatical é visível para o sistema PF. Os estágios no desenvolvimento do futuro românico são analisados como decorrentes de uma mudança na visibilidade dos núcleos funcionais sentenciais.*

*PALAVRAS-CHAVE: Participípio passado português, Futuro românico, Acento, Traço formal, Gramaticalização, Fixação de parâmetro, Mudança lingüística.*

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\* It is a great pleasure to dedicate this paper to Mary Kato, who has been responsible for the training of more than one generation of Brazilian linguists in the investigation of language variation and change, and who gave to Brazilian linguistics a characteristic profile, beyond the barriers among theories. The paper, written in 1998 and revised in June 2000, presents part of the results of a research project on formal features funded by a grant from CNPq. I express my deepest gratitude to Ken Hale for his comments on a preliminary draft of the first version. I would also like to thank Anna Roussou for kindly giving me access to unpublished and as yet undistributed work, Mark Ridd for editorial collaboration, and Orlene Carvalho, Acrísio Pires, Cilene Rodrigues and John Schmitz for logistic and bibliographical help.

## Introduction

The central thesis of this paper is that each generation by the computational system produces a “formal” structure that is analyzed from two different perspectives – phonological and semantic. This structure is formal because it is built with formal features and formal features do not have phonological or semantic “substance”. And it is possible to have two different levels of analysis for a single structure because formal features are simultaneously visible to phonological and semantic interpretation. Formal features are also defined as the only features in language actively involved in the building of structures. This means that the abstract features that drive linguistic construction and interpretation are formal features.

The paper argues that this approach sheds some light on language change. It is proposed that language change has to do with the PF visibility of the configurations that formal features project (so parametric settings are a matter of location). It is claimed that the manifestation of the Romance future tenses is either morphological or periphrastic, depending on the visibility of the relevant syntactic chain of functional heads –T(ense), Asp(ect). A basic claim in this argumentation is that the derivation may start either from the relevant formal feature of the lexical category involved in the derivation (V, in the derivation of future tenses), or from the formal feature of the higher functional head involved in the derivation (T, in the case of the future tenses). The derivation of the Portuguese periphrastic futures belong to the former type, while the derivation of the morphological futures belong to the latter.

The paper is organized as follows. Section 1 identifies three morphological types of past participles in Portuguese. Section 2 argues that the same configuration of abstract features underlies these three types of formation and leads to participial interpretation. It is claimed that the facts examined favor a theoretical framework that incorporates the thesis of isomorphism between expression and content at an abstract level where the notion of formal feature does not match the notion of morphosyntactic feature. Section 3 analyzes the diachronic stages in the development of the Romance future tenses and indicates how this development may be accounted for. The paper closes with a brief summary of the proposal, in Section 4.

## 1. The derivation of Portuguese past participles

### 1.1. Long and short forms

Portuguese morphology derives non-root-stressed and root-stressed past participles. The non-root-stressed form is longer than the root-stressed form, because it overtly manifests a verb theme vowel (TV), which necessarily bears the main word-stress.<sup>1</sup> Some verbs only allow one of these forms, others allow both, as illustrated in (1):

- (1) a. *Long Form Only:*
- |                     |            |
|---------------------|------------|
| começ-a-r ('begin') | começ-a-do |
| mov-e-r ('move')    | mov-i-do   |
| sorr-i-r ('smile')  | sorr-i-do  |
- b. *Short Form Only:*
- |                    |          |
|--------------------|----------|
| diz-e-r ('say')    | di-to    |
| escrever ('write') | escri-to |
| cobr-i-r ('cover') | cober-to |
| v-i-r ('come')     | vin-do   |
- c. *Long and Short Forms:*
- |                                   |              |   |            |
|-----------------------------------|--------------|---|------------|
| expuls-a-r ('expel')              | expuls-a-do  | / | expuls-o   |
| peg-a-r ('catch')                 | peg-a-do     | / | peg-o      |
| prend-e-r ('bind', 'arrest')      | prend-i-do   | / | pre-so     |
| suspend-e-r ('hang', 'interrupt') | suspend-i-do | / | suspen-so  |
| exprim-i-r ('express')            | exprim-i-do  | / | expre-ss-o |
| imprim-i-r ('imprint')            | imprim-i-do  | / | impre-ss-o |

The double participles in (1c) are of the kind called 'near doublets' by Kroch (1994). They are 'near' doublets because the two members of each pair differ in meaning (see next section) and in grammatical

<sup>1</sup> This generalization holds without exception for forms of 1st conjugation. For 2nd and 3rd conjugations, there is sometimes no means of comparison, given the morphological unavailability of the long form (*pôr*: *posto* 'put'; *ver*: *visto* 'seen'; *vir*: *vindo* 'come'). For other verbs, with a "regular" form that is ungrammatical in standard Portuguese but grammatical in some dialects (cf. fn. 3), both forms have the same number of syllables and phonemes (*abrir*: *aberto* / *abrido* 'opened'; *cobrir*: *coberto* / *cobrido* 'covered').

properties.<sup>2</sup> This paper will not try to investigate the theoretical status of the doublets. Our main concern will be the following issue: How is it possible for a language to arrive at the same grammatical semantic interpretation, namely, participial interpretation, from different types of morphological formation? There are in fact three different types of past participle morphological formation in Portuguese, as I will try to show in section 1.3, after having demonstrated, in the next section, that the long and short forms have autonomous derivations.

### 1.2. Autonomous derivations

There is empirical evidence that the two types of participles (long and short) have independent derivations, so the short-form participles (traditionally called irregular) cannot be analyzed as reduced forms of the corresponding long participles (traditionally called regular). First of all, in the case of the near doublets, the two forms have specialized meanings, so, even when the long form is older and the short form more recent, they cannot have a derivational relationship. This seems to be the case for *elegido* / *eleito* ('elected'). As noted by Said Ali (1966), *elegido* seems to have been the common form in archaic Portuguese, and was supplanted by the Latinate form *eleito*, which entered into the language as an erudite creation of Renaissance writers, though both coexisted in Renaissance literary language. In present-day Portuguese, *elegido* has a very specific use as a verbal participle, restricted to contexts in which the agentivity of the process is emphasized, as in (2b), while *eleito* preserves its verbal interpretation in a wide variety of contexts and has an interpretation that has to do only with the result of the process, as in (2a):

<sup>2</sup> In oral Brazilian Portuguese there is a tendency to the formation of innovative short forms, leading to genuine doublets, as in *comprado* / *compro* (*Ele já tinha comprado / compro o carro* 'he had already bought the car'). These short forms are highly marked, sometimes causing laughter. It is as if their production were a slip of the tongue – a formation intended as a past participle by the speaker, but not interpreted as such by the hearer. These formations do not arise through dialect and language contact, but are confined to register variation. Among the stable doublets, the pairs *aceitado* / *aceito* (accepted), *entregado* / *entregue* ('delivered'), *ganhado* / *ganho* ('won'), *gastado* / *gasto* ('spent'), *pagado* / *pago* ('paid'), *pegado* / *pego* ('caught') are very close to genuine doublets. However, the preference for the use of the short form with passives shows that in fact they are not genuine doublets. On the theoretical issues related to doublets, see Kroch (1994).

- (2) a. Ter sido eleito foi bom.  
 (to) have been elected was good: Having been elected was good  
 (=without implicit agent)
- b. Ter sido elegido foi bom.  
 (to) have been elected was good: Having been elected was good  
 (=with implicit agent)

Second, as Said Ali pointed out, the hypothesis that one of these forms is derived from the other is invalidated by the fact that in some cases the short form is older and the long form more recent. I will refer to this as the ‘linked derivation’ hypothesis, in opposition to the ‘autonomous derivation’ hypothesis. In his examples (3), the short form either became obsolete or underwent a category shift, so that the long form is currently the only usual verb form of the participle:

- (3) absolver (‘absorb’): absolvido (V) / absoluto (A. N)  
 cingir (‘encircle’): cingido (V) / cinto (N)  
 spender (‘spend’): spendido (V. A) / despeso (arch.)  
 defender (‘defend’): defendido (V) / defeso (A)  
 dividir (‘divide’): dividido (V. A) / diviso (A)  
 resolver (‘solve’): resolvido (V. A) / resolutivo (A).

Furthermore, as also indicated by Said Ali, for some verbs Latin only generates the short form, while Portuguese only generates the long form:

- |                                    |                                    |
|------------------------------------|------------------------------------|
| (4) <i>Latin: Only short forms</i> | <i>Portuguese: Only long forms</i> |
| motum (‘moved’)                    | movido                             |
| missum (‘put’)                     | metudo (arch.), metido             |
| receptum (‘received’)              | recebudo (arch.), recebido         |
| victum (‘beaten’)                  | vençudo, vivido (arch.), vencido   |

It seems then that Portuguese does not license the short forms for these roots, while Latin fails to license the long forms, which strongly favors the autonomous-derivation hypothesis. Finally, the linked-derivation hypothesis is untenable because in some cases the short form entered the language before the creation of the verb, and only subsequently became a part of the verb inflection. Said Ali cites the

case of *entregue* ('delivered') first used as an adjective (*ser entregue de alguma coisa*: lit.: 'be delivered of something'), from which the verb *entregar* ('deliver') was created, the adjective then being used as a past participle as well.

### 1.3. Three types of past participle formation

The morphophonological shape of Portuguese past participles shows that there are three types of past participle formation in Portuguese, one for long forms and common to all conjugations, and two for short forms, one specific to first conjugation and the other specific to second and third conjugations. These three types share the presence of the string [stressedV + C + V], but differ with respect to its location within the word:

- (5) Location of the string [stressedV + C + V] in Portuguese PPs:
- (A) [[...]Root + [stressedV + C + V]] (all conjugations)
- (B) [[... [stressedV (...)]Syllable ]Root + C + V] (2nd and 3rd conjugations)
- (C) [[...[stressedV (...)]Syllable + C]Root + V] (1st conjugation)

For the sake of simplicity, I will illustrate these three patterns with participles that allow a verbal interpretation. The pattern (5B) is only licensed by verbs of 2nd and 3rd conjugations, whether these verbs only take short forms, as in (6), or not, as in (7). The pattern (5C) is licensed by verbs of 1st conjugation, and is illustrated in the list of 1st conjugation participles in (7). The pattern (5A), typical of the majority of the verbs of the language, is illustrated by the long forms in (7), from all the three conjugations.<sup>3</sup> The other verbs of the language only take long forms for the formation of verbal past participial.

<sup>3</sup> Morphological derivations enlarge the inventory of these classes: *descrever*: *descrito*, *refazer*: *refeito*, *transportar*: *transportado*, *prever*: *previsto*, *satisfazer*: *satisfeito*; *encobrir*: *encoberto*; etc. The inventory presented in this paper corresponds to my own judgment about these forms; see Lobato (1999) for an explanation of the criteria used for the classification of these participles as verbs. Besides the diachronic variation already noted by Said Ali and mentioned above, there is variation in the use of the near doublets in contemporary language as well. C. Rodrigues (p.c.) has brought to my notice the normal use of the long forms *abrido* (V) and *escrivido* (V, A) in Minas Gerais, in the region of Matutina, São Gotardo and Tiros



## (6) Single Irregular Participles:

2nd conjugation:

dizer ('say'): dito, escrever ('write'): escrito, fazer ('do'): feito, pôr ('put'):

posto, ver ('see'): visto.

3rd conjugation:

abrir ('open'): aberto, cobrir ('cover'): coberto, vir ('come'): vindo.

## (7) Double Participles:

1st conjugation:

aceitar ('accept'): aceitado / aceito, cegar ('blind'): cegado / cego, circuncidar ('circumcise'): circuncidado / circunciso, descalçar ('take off the shoes'): descalçado / descalço, despertar ('wake up'): despertado / desperto, dispersar ('disperse'): dispersado / disperso, entregar ('deliver'): entregado / entregue, enxugar ('dry'): enxugado / enxuto, expressar ('express'): expressado / expresso, expulsar ('expel'): expulsado / expulso, findar ('end'): findado / findo, fixar ('fix'): fixado / fixo, fritar ('fry'): fritado / frito, ganhar ('win'): ganhado / ganho, gastar ('spend'): gastado / gasto, isentar ('exempt'): isentado / isento, juntar ('join'): juntado / junto, libertar ('liberate'): libertado / liberto, limpar ('clean'): limpad / limpo, livrar ('free'): livrado / livre, manifestar ('manifest'): manifestado / manifesto, matar ('kill'): matado / morto, murchar ('wilt'): murchado / murchado, ocultar ('conceal'): ocultado / oculto, pagar ('pay'): pagado / pago, pegar ('catch'): pegado / pego, quitar (pay off (a debt), release (from an obligation)): quitado / quite, salvar ('save'): salvado / salvo, secar ('dry'): secado / seco, segurar ('hold'): segurado / seguro, sepultar ('bury'): sepultado / sepulto, soltar ('loosen', 'set free'): soltado / solto, sujeitar ('subject'): sujeitado / sujeito, suspeitar ('suspect'): suspeitado / suspeito, vagar ('roam'): vagado / vago.

2nd conjugation:

acender ('light'): acendido / aceso, benzer ('bless'): benzido / bento, eger ('elect'): elegido / eleito, envolver ('envelop', 'wrap'): envolvido / envolto, incorrer ('incur'): incorrido / incurso, morrer ('die'): morrido / morto, prender ('arrest'): prendido / preso, revolver ('revolve'): revolvido / revolto, suspender ('hang', 'interrupt'): suspenso / suspenso.

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(Triângulo Mineiro), where the short form *aberto* also exists, but as an adjectival form (*porta aberta* 'open door'). The long form is also the current form for *eleger* in this region. She specifies that this variety of the language was heavily influenced by the variety of Portuguese spoken in Madeira Island, and is idiosyncratic also in the use of 'b' for 'v' (e.g. *bassoura* 'broom' and *trabesseiro* 'pillow' for *vassoura* and *travesseiro*). Besides the geographic variation, in oral Brazilian Portuguese the occurrence of short forms is attested for verbs that in the standard language only take the long form (comprar: comprado / comprado, falar: falado / falo, etc.), as pointed out in fn. 2.

3rd conjugation:

emergir ('emerge'): emergido / emerso, exprimir ('express'): exprimido / expreso, extinguir ('extinguish'): extinguido / extinto, frigr ('fry'): frigido / frito, imergir ('immerse'): imergido / imerso, imprimir ('print', 'imprint'): imprimido / impresso, incluir ('include'): incluido / incluso, inserir ('insert'): inserido / inserto, restringir ('restrict'): restringido / restrito, submergir ('submerge'): submergido / submerso, tingir ('dye'): tingido / tinto.

In (8)-(9) the three patterns in (6) are presented with specific information about the nature of the segments of the string [stressedV + C + V]:

(i) In the long forms, each of these segments performs a grammatical function within the word: they are interpreted as [stressed verbal T(heme) V(owel) + suffixal -d + nominal TV]. This sequence of grammatical elements is realized to the right of the verb root, as in (8).

(ii) As for the short forms, what makes them different from the long forms is that now some of the segments of that string are necessarily found within the root, which means that they are integrated in the lexical part of the word. In all the short forms the stressed V is a part of the root: it is precisely the vowel of the rightmost syllable of the root, and, in the case of two contiguous nonconsonants in that syllable, it is the one to the left (e.g.: a-cei-to, e-lei-to). A consonant may follow this vowel (e.g.: desperto, gasto). The difference between the two types of short forms concerns the role played by C:

(iia) In the short forms of second and third conjugations the two segments [C + V] perform a grammatical function: [suffixal -t or -s + nominal TV]. This sequence of two grammatical elements is realized to the right of the root, as in (9a).

(iib) In the short forms of first conjugation both the stressed V and C are found within the root. Thus, in this case it is only the last V, interpreted as the [nominal TV], that has a grammatical function. It is then only this V that is realized to the right of the root, as in (9b).

(8) Long-form participial formation:

(1st, 2nd and 3rd conjugations)

[Root] + [stressed verbal TV] + [-d] + [nominal TV].

## (9) Short-form participial formations:

## a. 2nd and 3rd conjugations:

[[...[Stressed V(...)]<sub>Syllable</sub>]<sub>Root</sub> + [-*t* or -*s*] + [nominal TV]] or  
 [[...[Stressed V + Nasal]<sub>Syllable</sub>]<sub>Root</sub> + [-*d*] + [nominal TV]]

b. 1st conjugation:<sup>4</sup>

[[...[Stressed V (...)]<sub>Syllable</sub> + C different from *d'*]<sub>Root</sub> + [nominal TV]] or  
 [[...[Stressed V + Nasal]<sub>Syllable</sub> + *d*]<sub>Root</sub> + [nominal TV]].

Two facts make it clear that the string [stressedV + C + V], shared by the three types of formation, is the manifestation of underlying information crucial for participial interpretation. One of them is the grammatical status of the three segments of this string in the long-forms – verbal TV, suffix and nominal TV: since these three parts of the string bear grammatical information, they bear information necessary to the building of the participial interpretation. Therefore, this information has to occur with any lexical item with past participial interpretation. The other fact is the occurrence of this same string in the short forms, even though at different locations of the morphological structure: since in the long forms the three segments of the string bear information necessary to the building of the participial interpretation, participial interpretation requires the presence of well-defined information: given this requirement,

<sup>4</sup> There are two 'exceptions' to the generalization that in the case of short forms of first conjugation the element C corresponds to the last consonant of the root: *circuncid-ar*: *circunciso*, *enxug-ar*: *enxuto*. In the theory of grammar we are suggesting, the C in these two examples is the form of underlying information, and the underlying information is projected according to the intended semantic interpretation, due to the isomorphism principle. Thus, these examples are not exceptions. The fact that the consonant is then overtly realized as 's' (phonetically [z]) and 't', which are the forms of C in the participial suffixes of second and third conjugations is a piece of evidence in favor of this proposal, although we do not provide a full explanation. Concerning *circunciso*, notice that it is a general fact for short forms that the segment C is only realized as 'd' when immediately preceded by a nasal (e.g. *findar*: *findo*). Given that generalization, the non-generation of the form *circuncido* does not seem fortuitous (differently from *findar*) in *circuncidar*, although the root also ends in *-d*, there is no nasal immediately preceding it. In the framework I am suggesting, phonemes are built with formal features. We may then suppose that in the derivation of the long forms the underlying position of segment C has a configuration of features that matches, at least in part, the configuration of the segment 'd'. In the short forms, in contrast, the underlying configuration of features would only match this configuration when immediately preceded by nasality. And this is because nasality would likewise be the form of a certain underlying configuration involving formal features. Thus, in the short forms the conjunction of these two configurations (nasality + C) is a necessary condition for the overt manifestation of C as 'd'. This would explain why the consonant 'd' is not realized in those short forms: the necessary configuration is not present.

it is reasonable to think that the occurrence of the same string in the short forms manifests the presence of the same type of underlying information.

## 2. A feature-based proposal

### *2.1. In favor of feature-based morphological derivations*

The facts presented in Section 1 and summarized in (5), (8)-(9) show that interpretation of a given Portuguese lexical item as a past participle depends on information underlying the segments of the string [stressedV + C + V]. Thus, the difference between the three observed types of formation relates to the location, within the morphological configuration of the word, of the information relevant to participial interpretation. It is important to keep in mind that this paper is concerned with what these forms have in common and accounts for their sharing of the same type of grammatical semantic interpretation – namely past participle interpretation – in spite of their morphophonological differences. There are also semantic differences among the different types of morphological formation (categorical and thematic differences, for instance), which must be accounted for, but they are beyond the scope of this paper.<sup>5</sup>

It is very reasonable to think that the information that is relevant to participial interpretation is expressed in the form of features and, more precisely, as a configuration of features. First of all, as I have already pointed out, the fact that the long form is derived with the manifestation of three segments of the grammatical type is evidence that these three segments are the manifestation of information necessary for past participle interpretation. These segments correlate here with a string of three grammatical constituents, and there with a string of either one or two grammatical constituents, the other segment(s) being found within the root. However, the same string occurs in the three types of formation. This means that the grammatical semantic information required for past participle interpretation is found in different constituents of the word, depending on the type of formation involved. Since the same type of

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<sup>5</sup> On the categorical and thematic issues related to past participles, see Pires (1996).

information, needed for past participle interpretation, is found in different constituents of the word, these different constituents obviously bear the same type of information. Thus, for instance, the information that underlies the stressed root vowel in the short formations – and which is relevant for the interpretation involved – underlies the verb TV in the long formation. This conclusion is reinforced by the hypothesis of this paper that a certain configuration of formal features underlies the occurrence of stress: if that is so, the stressed root vowel of the short formations and the TV of the long formation share the same type of formal feature, relevant for stress realization. Therefore, the root of the short formation and the TV of the long formation bear the same underlying information, crucial for past participle interpretation. In turn, since different constituents bear the same type of information, it must be true that this information is expressed as discrete elementary units.

The same has to be true with respect to the relevant consonant: in the long forms, and in the short forms of the 2nd and 3rd conjugations, this consonant is a suffix, while in the short forms of the 1st conjugation it is within the root. Since a given piece of information, needed for participial interpretation, is found in the relevant consonant (here a suffix, there a part of the root), it is necessarily true that the suffix and the relevant roots bear the same type of information in the underlying, abstract structure. Since the same type of information underlies the relevant suffix and roots, this information has the form of discrete, elementary units.

These units are then discrete in the sense that they have their individuality, so that on the one hand they are not continuous and on the other hand they are autonomous, in some sense, of particular morphemes or morphs. In linguistic literature, the notion of features applies to basic, discrete units used in the construction of higher objects; e.g. the properties [Consonant] and [Coronal]; [Human] and [Animate]. Accepting this type of characterization, it seems appropriate to say that the information required for the interpretation of past participle is expressed in the form of features.

Furthermore, there is empirical evidence that these elementary units have the theoretical status of features. One such piece of evidence is found in the change in stress placement that took place in the verb system, from Latin to Portuguese. For instance, Latin, but not Portuguese, licenses root-stressed participles with overt verb TV. In Portuguese, if the past

participle bears a verbal TV, the TV must bear main stress. Those forms were licensed in Latin for the 2nd and 3rd conjugations, as illustrated in (10), where the underlining marks the stressed syllable:

- (10) Stress Differences between Latin and Portuguese Past Participles:  
2nd Conj.: debitu > devido; 3rd Conj.: molitum > moído

However, as (11)-(12) show, the change relating to the requirement of stress in the verb TV was not restricted to past participles. In fact, this change evolved in all infinitive and past forms, but only in the past forms among the inflected forms.<sup>6</sup> In Latin, in the past tense inflected forms (whether perfective or imperfective), word stress is borne by the verb TV or by a mood-tense suffix, this suffix being the rightmost mood-tense suffix of the word in the linear order (or the higher one in terms of the c-command relations among the morphological constituents of the word), in the case of two suffixes of this kind within the word:

- (11) Latin Verb Past Forms. 1st Conjugation. 1P
- | Imperfect                         | Past Perfect     | Pluperfect          |
|-----------------------------------|------------------|---------------------|
| Mood-TenseSuffix                  | VTV              | Mood-TenseSuffix    |
| <u>am</u> abamus <u>am</u> aremus | <u>am</u> avimus | amaver <u>am</u> us |

In Portuguese, in the past inflected forms stress is always borne by the TV, for all persons and all conjugations, so the Latin data in (11) correspond to the Portuguese data in (12):

- (12) Portuguese Verb Past Forms. 1st Conjugation. 1P
- | Imperfect                          | Past Perfect   | Pluperfect       |
|------------------------------------|----------------|------------------|
| VTV                                | VTV            | VTV              |
| <u>am</u> ávamos <u>am</u> ássemos | <u>am</u> amos | <u>am</u> áramos |

Therefore, where the stress was borne by the verb TV in Latin past forms, its placement did not change in Portuguese; but where it was placed elsewhere, it did, resulting in a new pattern of stress assignment in Portuguese past tenses. As mentioned above, this type of change only took place in past tenses, among the inflected forms. For present tenses,

<sup>6</sup> See Wohlmutth 's (1978) work on the irregular past participle in Hispano-Romance.

Portuguese maintains the general Latin stress pattern of distinguishing the stressed segment according to grammatical person – with stress on the TV for first and second plural and on the root for all other persons. Thus, Latin: amo, amas, amat, amamus, amatis, amant, and Portuguese: amo, amas, ama, amamos, amais, amam. As for the future tense, a totally new pattern was created, with stress on a suffix: amarei, amaras, amara, amaramos, amareis, amarão.<sup>7</sup> This contrasts with the Latin pattern of stressing either the verb TV or the root for the imperfect future – amagbo, amagis, amagit, amagimus, amagitis, amagunt; audiam, audies, audiet, audimus, auditis, audient. Of course, I am not implying that there was a stress shift here, since the Portuguese future represents a new type of morphological formation, created in the recent history of Romance. What I am saying is that this new creation as well as the loss of the Latin-type future formation are anyway related to stress. It is as if, in the inflected forms, the change were oriented by the presence / absence of the properties underlying past tense interpretation, but in a mutual relationship between expression and content (i.e., between the phonological and semantic representations): past tense interpretation requires the presence of these properties, and the presence of these properties compulsorily attracts stress: the change consisted in the visibility of these properties solely in the position of the verb TV, with the result that stress became necessary in this position, for this interpretation; in turn, the presence of stress in the verb TV of inflected forms is an overt sign of the underlying presence of these properties: thus, where this interpretation does not pertain, these properties are absent, as is stress, hence the loss of the Latin type of future formation. Given this analysis, I conclude that in the present tense forms with stress in the verb TV (1st and 2nd person plural), the person interpretation creates a configuration of features in which those properties, necessary for past tense interpretation, are again visible for phonological interpretation in the position of the verb TV. Similarly, the infinitive forms are presumably underpinned by those properties that attract stress to the verb TV.

<sup>7</sup> Bisol (1994) analyzes Portuguese future forms as having two main stresses assigned in the course of the derivation, one on the VTV and the other on the suffix; only the stress on the VTV remains at the end of the derivation, due to Portuguese restriction on sequences of two main stresses, Mateus (1990: 358) argues that the stress occurs in the first vowel of “the morphal morpheme” (*re* / *ra* for the present future and *ria* for the preterit future).

The fact, then, is that at some underlying level there is a crucial linguistic difference between past, present and future, in the case of the inflected forms, and this difference is relevant for stress placement in Portuguese. Let us assume that tense is represented by means of features. I have just observed that there is a relationship between the location of stress and the temporal information. It therefore follows that the location of stress is related to the location of certain features, namely features associated with temporal information. Furthermore, accepting that a given piece of information borne by the root in short forms is also borne by the verb TV in long forms, then that also is information represented in the form of features. Finally, the fact that in the architecture of the mind / brain there is mutual interaction among the different conceptual and sensorimotor systems also requires the overall use of the same type of basic unit: thus, given these legibility conditions (or bare output conditions: see Chomsky 1995, 1998), the information underlying each segment of the string [stressedV + C + V], and not only the information underlying the stressed V, takes the form of features.

Finally, the configurational status of the relevant features is clear in the fact that participial interpretation depends on a string of segments – [stressedV + C + V] – rather than on a unitary segment. This linear string corresponds to a given hierarchical configuration in structural terms, which is the mirror image of the linear order (Baker's Mirror Principle). It is easy to see how this works in the phonological representation, because of the overt phonetic manifestation of this string in Portuguese. How do I know that there is a hierarchical configuration also at the formal level? I know it, first of all, because this string corresponds to part of the syllabic structure. The facts examined in Section 1.3 support this conclusion. The three types of participial formation show that the phonological content of the segments of the string [stressedV + C + V] does not matter, since the segments get varied forms. What matters is their C,V nature. Thus, these segments relate to the C,V structure of the word. Therefore, since the C,V structure is the syllabic structure, they relate to the syllabic structure. Thus they belong to the non-segmental PF structure and are then assigned a structural interpretation. The requirement of stress on the leftmost vowel of the string supports this conclusion: the segments of that string indeed seem to belong to PF structure. However, as mentioned, this information has a semantic correlate of the grammatical type. I take this to indicate that there is isomorphism between expression and content at an abstract



structural level: assuming that there is this type of isomorphism, the configuration related to the string [stressedV + C + V] likewise belongs to the level of grammatical semantic structure. In syntax, this configuration drives grammatical semantic interpretation. We now have a possible explanation for the fact that Portuguese past participles have the right information in an underlying location that parallels its location in the phonological representation: this information is information of syntactic structure that is visible to phonology in Portuguese, driving spelling out.

Of course, I have not accounted for stress placement, nor for the relationship between the hierarchical configuration and the stress placement. This is a complex issue that I treat elsewhere (Lobato 1998, In preparation).

## 2.2. *Towards a theory of grammar with formal features*

The analysis above suggests that the formula [stressedV + C + V] is a 'template' that can be associated with a past participle by aligning the rightmost vowel of the formula with the final vowel of the participle; the consonant with the suffix or the last consonant of the root, and the leftmost vowel with the theme vowel or the vowel in the rightmost syllable of the root (or the vowel to the left in this syllable, in the case of two contiguous nonconsonants in this syllable); stress goes on the vowel that matches the position of the leftmost vowel of the formula, whether this is the TV or the root vowel. There is, therefore, a direct and necessary correlation between the morphophonological form of these lexical items and their grammatical interpretation. This correlation supports the view that the syllabic, prosodic and morphological configurations are built with the same kind of features, which are also accessible to grammatical interpretation and phonetic realization, as well as the hypothesis that the phonological and semantic representations are isomorphic at the grammatical semantic structure.

First, if it is possible to have a template associated with the participial interpretation, with a well defined variation in the location of this template within the word, it is because each segment of the template is the form for a very precise abstract type of information leading to the participial interpretation: and there is variation in the location of the template within

the word because these abstract pieces of information are found in different locations of the word, according to the root under consideration. This hypothesis is compatible with a compositional view of participial interpretation. However, the morphological structure is highly relevant to semantic interpretation because the presence of the formula is not a sufficient condition for participial interpretation: verb inflections such as *amo* '(I) love', which overtly manifests the phonological string [stressedV+C+V], but has the morphological structure [Root+Ø+Ø+o], with null phonological realization for the TV and tense-mood, and overt realization for person-number, are not interpreted as past participles (so *amo* allows the interpretation 'I love' but not the participial interpretation).<sup>8</sup> So, for the participial interpretation to obtain, there must be a correlation between this phonological string and the verbal morphological structure, according to the morphological types we have already pointed out. Thus, what is relevant for the participial interpretation is not exactly a string of three independent and isolated bits of information, but rather a given structure involving that information and defined in morphological terms. Furthermore, these bits of information activate semantic interpretation. Under the assumptions of this paper, this structure is built with formal features.

Further, we have already argued that the string [stressedV+C+V] is absolutely necessary for participial interpretation, and reflects part of the syllabic and prosodic structure of the word. This fact favors the hypothesis that the same feature configuration underlies the grammatical and phonological organizations, as well as the grammatical and phonological interpretations. On the one hand, the mutual, necessary correlation between the presence of this string and the participial interpretation supports the view that the configuration of features that the grammatical semantic interpretation reads is exactly the same configuration read by the syllabic and prosodic phonological interpretation. On the other hand, if this is possible, it is because the computational system generates a single formal structure that is analyzed from two different standpoints: the standpoints of non-segmental phonology and grammatical semantics.

<sup>8</sup> Notice that the short-form participles of the 1st conjugation are homonyms of the forms of 1st person singular of the indicative present: *pago* 'paid' or '(I) pay'. This is only a surface identity, since their morphological structures are different. See also fn. 2.

These conclusions are compatible with the claim that there is isomorphism between expression and content, in the sense that there is a formal structure common to the PF and semantic systems, but strictly at the non-substantive level: at the substantive level, due to the addition of substantive semantic and phonological information, there cannot be isomorphism.

In this discussion, it remains to be demonstrated that syntactic constructions use abstract features of the same type used in morphology, phonology and semantics. The morphological and syntactic nature of inflection provides a line of argumentation in favor of this proposal: (a) it is an empirical fact in Portuguese that the nominal TV is a necessary segment of the phonological template associated with participial interpretation: TVs, in general, are the phonetic realization of formal features: (b) it is also an empirical fact in Portuguese that the nominal TVs are inflectional marks in the passive participles, as in *Os livros foram lidos* (the books-MP were read-MP), *As revistas foram lidas* (the magazines-FP were read-FP), and it is a generality in languages that inflectional properties are elements of the syntactic construction, as in the subject-predicate agreement relation: (c) therefore, assuming that in the morphological construction the nominal TVs are the spelling out of formal features, given that nominal TVs are used as inflectional marks in the syntactic construction, then the features used in the morphological construction are of the same type as the features used in the syntactic construction – in both cases, they are formal features.<sup>9</sup> A different line of argumentation may be constructed on the basis of the thematic information in the past participles. As noticed, there is a thematic difference between *eleito* and *elegido*, in the sense that the long form, but not the short form, expresses the existence of an implicit agent:

- (2) a. Ter sido eleito foi bom.  
 (to) have been elected was good; Having been elected was good  
 (=without implicit agent)
- b. Ter sido elegido foi bom.  
 (to) have been elected was good; Having been elected was good  
 (=with implicit agent)

<sup>9</sup> This is what one expects, under the assumption that the formation of lexical items “is subject to principles known to be operative in syntax”, as pointed out by Hale & Keyser (1993).

The same thematic difference is found regarding certain verbs of the 1st conjugation:

- (13) a. despertado / desperto  
 b. manifestado / manifesto

I have argued that each segment of the participial template is a form of specific underlying information, and have treated this abstract information as formal features. What distinguishes the long forms in (13) from the respective short forms is the manifestation of both the VTV and the suffixal consonant: *despert(ad)o*; *manifest(ad)o*. Therefore, given that the difference in the spelling out of formal features in the participial formation correlates to a difference in the thematic information of the participle, the natural conclusion is that formal features are also involved in the thematic information of the lexical item. Since thematic information is used in syntactic construction, the formal features must be basic elements of the syntactic construction as well.

### 3. Language change

The last section of this paper has already presented empirical evidence of the role of formal features in linguistic change: the case of change in stress placement in the verb system, from Latin to Portuguese. I have pointed out that this change was directional, in the sense that it selectively took place in the inflected forms according to tense distinctions: there was a change towards necessarily placing stress in the VTV in past tenses and necessarily avoiding it in this position in the future tense forms, so in the cases in which these two intended situations already existed, there was no change; in the present tenses there was not any change either. Thus, the change was "directional" in that it was guided by some abstract property underlying temporal interpretation. Accepting the view that present-past-future are conceptual constructs, compositionally derived from the use of features, I must conclude that the change in stress placement from Latin to Portuguese relates to some use of features. Since only formal features are simultaneously visible to phonological and semantic interpretation, from the fact that the information underlying tense is visible to stress I must also conclude that this information is encapsulated under the guise of formal features

(after all, it is information visible to the phonological component, as we know from the Portuguese facts concerning stress placement, and visible to the semantic component, as we know from the temporal interpretation). This conclusion is reinforced by the property of formal features of being the only features in language actively involved in the building of structures: if this is indeed so, then the properties underlying the building of past participles are formal features.

In this section I want to argue that language change has to do with the visibility to the PF system of the configurations of formal features relevant to grammatical semantic interpretation. Assuming that Spell-Out depends on the inherent information of functional heads, parametric setting relates to the PF role of the functional heads. Taking the information of the functional heads to be formal features, language change relates to the PF role of the formal features of the functional heads. Under the perspective of this paper, language change reduces to visibility to PF of the underlying configurations of formal features in the abstract grammatical semantic structure.

In some cases, it is clear that parametric setting is a matter of choosing either the lexical information or the information structure as the layer, or plane, where the formal features are visible to phonology. This is the type of change that seems to have occurred in the evolution from Latin to Portuguese, with respect to stress. In the Latin verbal system, the stress configuration is read by the PF system in the lexical network of features projected from the root for the building of the word grammatical semantic interpretation. Evidence of that is the relevance of the conjugation for stress placement in the past tenses in Latin: the pertinence to a specific conjugation is a totally idiosyncratic fact depending solely on the root. In Portuguese, the stress configuration is likewise read by the PF system at the formal level, but in the network of features provided by the word morphological structure. Evidence of that is the fact that only the verbal TV *position* matters for stress placement in Portuguese past tenses, so the information about the occupant of this position is totally irrelevant, for this purpose. Thus, the change from Latin to Portuguese, relating to stress, was a change concerning the location of the visibility to the PF system of a certain configuration of features: visibility in the lexical network, in Latin; visibility in the information structure, in Portuguese.

The proposal that parametric setting is a matter of the phonological role of the formal features of the functional heads is highly favored by the analysis of the diachronic evolution of the future tenses, from Latin to Portuguese. So far, the comparison between Latin and Portuguese in this paper has focused on the contrast in stress placement between Classical Latin and Portuguese. However, it is a well-known fact that there were intermediate stages in the diachronic development from Classical Latin to Romance languages, as illustrated by the evolution of the future tenses of most of the modern Romance languages (future and conditional, or present future and preterit future in the grammatical literature). According to the traditional view on this subject, the future endings of these Romance languages are the direct development of the Latin periphrastic construction Infinitive + *habere* ('have'), through specific stages. Roberts (2000) points out two stages: (A) *habere*, a full lexical verb in Classical Latin, was reanalyzed as a future auxiliary, comparable to *will* and *shall* in Modern English – it became athematic and was used to mark purely temporal content (*amare habeo* 'love have+1stSing'); (B) the auxiliary *habere*, an autonomous word, was reanalyzed as a syntactic affix. This development was then clearly a process of grammaticalization. I will try to show now that, although this is not so clear at *prima facie*, in this case too, the distinction between visibility in the lexical network and visibility in the information structure is likewise involved.

Grammaticalization has been commonly analyzed as a case of change from lexical to grammatical material. Roberts & Roussou (1999, Forthcoming) and Roberts (2000) try to account for this intuition in a generative framework. Roberts and Roussou claim that grammaticalization is a process of loss of movement from a lexical head to a functional head, and direct merge of the lexical material in that position. This is then a process of reanalysis of lexical heads as functional heads. They also hold that grammaticalization is a case of reanalysis involving structural simplification (see Roberts & Roussou 1999, for instance). Due to limitations of space, I will not give a detailed presentation of Roberts and Roussou's proposal on grammaticalization here. I remit the reader to the references above and now turn to the suggestion of an alternative analysis. This alternative analysis conforms to the basic idea of this paper that there is a formal structure generated by the computational system and shared by the non-substantive phonological and semantic components. In this alternative proposal,

every case of linguistic change is a case of change of the location, in the grammatical semantic structure, where a certain configuration of formal features – relevant for grammatical semantic interpretation – is visible to the modules dealing with linearization of the information (PF modules, taking them to include the module of word formation). Thus, in this new approach, language change relates to the visibility to the PF system, in the underlying formal structure, of the abstract configurations relevant to grammatical semantic interpretation. I will conclude that it is always the case that the change of location relates to a change in the plane where the formal features configurations are accessed – either L(exical) plane (plane of the lexical information), or G(rammatical) plane (plane of the lexical information structure). The argumentation here will be restricted to futurity interpretation.

Let us first clarify three fundamental assumptions of our proposal, concerning future tense interpretation. First, the auxiliary of the periphrastic construction does not represent the overt realization of futurity. This means that, in the Latin formation infinitive + *habere*, *habere* is not the manifestation of the notion of futurity, just as ‘ir’ (go) is not, in the Portuguese future construction ‘ir’ + infinitive. As clearly demonstrated by Benveniste (1965), the temporal interpretation of periphrastic constructions comes from different aspects of these constructions (lexical information of each verb, grammatical information of each verb, syntactic structure). Second, tense interpretation is a mental construct, compositionally derived with the use of abstract features. Thus, the notion of futurity correlates with a well defined configuration of formal features. This compositional property is what is expected, given that temporal interpretation comes from different parts of a construction. Third, since there is a well-defined configuration of formal features involved in the semantic interpretation of futurity, this configuration has to underlie the morphological future tense formations as well. I will come to this assertion below.

Accepting that the notion of futurity correlates with a certain configuration of formal features, the changes that effectively took place in the expression of futurity in Romance languages cannot be seen as having affected this structural configuration. Given the assumptions of the theory of grammar expounded in this paper, the only possible, natural explanation is in terms of visibility to the PF system of the formal

feature configuration related to futurity interpretation.<sup>10</sup> This eliminates the possibility of taking grammaticalization as a case of structural simplification.

Let us consider the stages of the change, borrowing from Roberts & Roussou's (Forthcoming) analysis and adding the facts about Classical Latin and present-day Portuguese. The addition of facts from Classical Latin gives us: (I) Classical Latin: morphological formation with use of special endings incorporated in the verb form; these endings inform about grammatical person, temporal distinctions, thematic function of the subject ('amabo'); (II) Imperial Latin: syntactic formation with use of an autonomous word (the auxiliary verb) that bears the inflectional marks ('amare habeo' 'love have+1stSing'); this formation gradually spread to different types of verbs, with loss of the active / deponent distinction; (III) Romance: morphological formation with use of a grammatical affix incorporated in the verb form; the verbal morphology does not overtly realize the thematic function of the subject ('amarei', in Portuguese). The subsequent (and much more recent) stage represents a return to a periphrastic construction. At this stage, French and Portuguese, for instance, use the auxiliaries 'aller' and 'ir' (go), respectively, as the autonomous word generated by the future tense formation. Keeping to the Portuguese case for the simplicity of the subsequent argumentation, we have: (IV): Modern Portuguese: syntactic formation with use of 'ir' as the autonomous word that bears the inflectional marks (vou amar 'go+1stSing love').<sup>11</sup> Thus, stage (IV) is similar to stage (II) in that both manifest the notion of futurity as a syntactic formation with inflection realized by an autonomous word. However, these two cases differ in two significant respects: choice of the auxiliary ('ir' vs *habere*) and word order (Aux+Inf vs. Inf+Aux). In current-day Brazilian Portuguese (IV) is clearly the only futurity

<sup>10</sup> Observe that in this approach it is not appropriate to say that the future surface construction of a given stage is the direct development of the future surface construction of the previous stage. For instance, we could not say that the Romance future endings are the direct development of the Latin verb *habere*; rather, both the Latin verb *habere* and the Romance future endings share common underlying properties.

<sup>11</sup> At present, it is not possible to date the beginning of stage (IV). It is clear, however, from Mattos e Silva's (1989) work, that at the end of the 14th century the 'ir + infinitive' periphrasis was not yet purely temporal. Mattos e Silva (see p. 459) found only 13 occurrences of this periphrasis in a total of 17,429 occurrences of verb forms in her corpora from that century and in every case these few occurrences have a modal interpretation (intention).



formation triggered by the PLD; since the morphological formation in (II) is acquired through teaching in schools. Therefore, since the more grammaticalized form of futurity is succeeded by the less grammaticalized, if it is true that language change follows some 'direction', the cause of this direction is not grammaticalization, so some different type of motivation must be found.

In the approach suggested in this paper, grammaticalization, like every other type of language change, has to do with the PF side of the grammatical semantic structures generated by the computational system. However, we have to take 'PF side' as including the word formation module. That is, we have there a PF system and not just phonology. Indeed, in each case of grammaticalization, a given construction relevant for grammatical semantic interpretation is involved and the issue is the morphological realization of this construction: in the case of the expression of futurity in Romance, realization either as an autonomous word, or as an affix. The change, in this case, then, relates to morphological manifestation of the underlying configuration of formal features – either as a one-word construction, or as a two-word construction. I have already observed that the same configuration of formal features underlying the periphrastic future formation must underlie the morphological formation, according to the approach propounded in this paper. An empirical piece of evidence in favor of this claim is found in the morphic structure of Portuguese morphological futures: they have a morphic structure that precisely mirrors the underlying syntactic formation. To demonstrate this, I will first consider the syntactic structure of the periphrastic formations and some related facts.

I have observed earlier in this paper that the change in stress placement, from Latin to Portuguese, took place in the inflected forms according to tense distinctions. I have also observed that the infinitive and past participle forms underwent the same type of change that evolved in the inflected past forms: change towards stress on the VTV. Let us take the traditional view on the distinction between inflected / uninflected verbal forms, and say that uninflected forms are aspectual forms and inflected forms are temporal forms. Under this view, the facts about stress placement that I have examined support the conclusion that tense and aspect are crucial functional heads for the occurrence of stress. Let us assume the strongest hypothesis about sentential structure: that up to

TP this structure has only two functional heads – T(ense) and Asp(ect). Thus, a complete sentential structure of a verbal nature implies the following hierarchical organization: (TP, AspP, VP). The facts I have examined concerning the development of future tenses in Romance add a new consideration to the conclusion that T and Asp are crucial functional heads for the occurrence of stress: the importance of tense and aspect for word formation, with respect to verbs. Indeed, on the one hand, the morphological and periphrastic formations seem related to the tense / aspect distinction: in the morphological formation, there is word formation at the level of tense, and only at this level; in the periphrastic formation, there is word formation at both levels, tense (for the auxiliary) and aspect (for the main verb). On the other hand, main stress is clearly related to word formation. Stress is, in fact, crucial information for word formation. Leaving aside all the complexities, let us say that at the non-segmental PF system there is a module of word formation that makes a strict correlation between the stress underlying configuration, on the one hand, and word formation, on the other hand, such that the visibility of the main stress configuration defines the word at this level: one stress configuration visible to this module in a given location, one word; two stress configurations visible to this module in two different locations, two words. It seems clear that these locations are the positions of the functional heads T and Asp. Following the traditional approach, I will take morphology to be the module of word formation. Assuming that the visibility of the stress structure forces morphological interpretation, this configuration is read by the non-segmental PF system the very moment it is visible. A temporal affix of futurity is derived in the case of the stress structure being visible only in the higher position of the functional chain (T, Asp) – T.<sup>12</sup> An auxiliary is derived in the case of the stress structure being visible in both positions of this chain – T, Asp. Visibility in Asp generates the infinitive: visibility in T generates the auxiliary. It is clear that in this case V is not in T at PF.

Let us now turn to the morphic structure of Portuguese morphological futures. The constituents of the present future of ‘amar’ (love), are shown in (14):

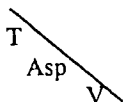
<sup>12</sup> This proposal may be compatible with the analyses in both Bisol (1994) and Mateus (1990) (see Fn.7), due to the conception of PF as a set of modules.

(14) am-a-r-e-i	(1sg)	'I will love'
am-a-r-á-s	(2sg)	'you-Sg will love'
am-a-r-á-Ø	(3sg)	'he will love'
am-a-r-e-mos	(1pl)	'we will love'
am-a-r-e-is	(2pl)	'you-Pl will love'
am-a-r-ã-o	(3pl)	'they will love'

These constituents are: [Root + VTV + Aspect + Tense / Mood + Person / Number], respectively. The preterit future forms share the same morphic structure. We thus have here the exact mirror image of the syntactic functional heads that I have postulated – (Asp, T). The presence of these constituents in the morphic structure of the morphological futures gives additional support to the proposal that Asp and T are the functional heads of futurity in syntax. On the other hand, the fact that the morphological and periphrastic futures share the same chain of functional heads supports the claim that grammaticalization relates to the PF visibility of the functional heads. This being true, the difference between the morphological and periphrastic formations may simply be a matter of visibility to the PF system of the formal features of the functional heads, at a point of the derivation where these formal features are relevant to word formation: visibility at T, and T only, for the morphological formation, and at both Asp and T for the periphrastic formation. In the case of the Portuguese future formations, the morphological form is more 'grammaticalized' than the periphrastic formation. The notion of grammaticalization applies then, in this case, to the formation where only T was visible for word formation. Where does this intuition about grammaticalization come from?

I consider that there is a precise source for the intuition about grammaticalization: in the case of grammaticalization, the derivation starts from the information structure (thus, the G plane), rather than from the lexical information (the L plane), in contrast with the less grammaticalized form, which has a derivation that starts from the relevant formal feature of the lexical head (the L plane). More clearly, I am suggesting that there are two possible ways of starting the derivation of the future interpretation from the heads (T, Asp, V). In Romance, these heads are as in (15):

(15)



One possible way is to start from T. The other possible way is to start from V. If the derivation starts from T, a single word is derived, because T c-commands the whole structure, so the whole structure (including T) is immediately visible to the PF system. If the derivation starts from V, two words are derived, because Asp is visible to the PF system before T, forcing the spelling-out of its c-commanded domain (Asp included). In this derivation, T is only visible to the PF system after Asp was made visible, and, when visible, forces the spelling-out of its c-commanded domain. However, this c-commanded domain does not contain Asp, V anymore, since Asp, V were already "stripped-away from the derivation" (to use Chomsky's terminology). If the derivation starts from V, we have a derivation that starts from the L plane. If the derivation starts from T, we have a derivation that starts from the G plane. There is intuition about there being grammaticalization precisely when the derivation starts from the G plane.

Summarizing, the morphological futures of stage (III) are the result of derivations that start from T, while the periphrastic futures of stage (IV) result from derivations that start from V. Of course, I know that this is not yet full explanation of the facts. Full explanation requires explanation of the process of word formation, including the exact role of stress and involvement of formal features in this process.<sup>13</sup> I will leave this question open.

<sup>13</sup> I also know that this is a new proposal, with far-reaching consequences for different theoretical issues, such as the representational or derivational status of the theory and the proper account of word order variation, crosslinguistically as well as concerning the different stages of the same language. For instance, it gives support to a representational theory of grammar, in the sense of a theory that accounts for the property of 'displacement' without the concept of movement in syntax. The theory I am suggesting is indeed, in some aspects, very similar to Brody's (1995) representational framework. For instance, both postulate the concept of chain rather than the concept of Move, and so eliminate syntactic derivations through movement. These two proposals are yet very different in other respects. For instance, they differ in the formal feature based character of the theory in this paper. This theory is also

Another important issue related to the Romance futures concerns the causes of the change from a morphological to a periphrastic formation, and vice versa. Roberts & Roussou (in preparation) and Roberts (2000), citing Fleischmann (1982), consider that the reanalysis of the auxiliary *habere* as a syntactic affix may be a direct reflex of the reanalysis of the full lexical verb *habere* as an auxiliary verb. This is a reasonable hypothesis for this morphological formation, although it is not yet full explanation. We still need to know why stage (III) is a 'direct reflex' of stage (II). Moreover, what does it mean to be a 'direct reflex', in this context? In any case, this hypothesis implies that the reason of this change is internal to the properties of the futurity configuration. With respect to the periphrastic constructions (stages (II) and (IV) above), it is reasonable to suppose that the development results from independent diachronic changes in the language, affecting the visibility to PF of the formal features of the functional heads T, Asp. This hypothesis is plausible, due to the loss of the active / deponent distinction during the Imperial period of Latin. Obviously, this loss altered the network of formal features for each verb of the language. It may be the case that, as a consequence of this change, the structure driving stress in the underlying structure expressing futurity was made visible at T and Asp, as opposed to visibility in only one position (T) in the previous stage. It is also possible that this independent change was the consequence of a more abstract change with cascade effects. What about the periphrastic construction of contemporary Portuguese? Which was, then, the change that took place and licensed it? Accepting the hypothesis that the periphrasis is motivated by changes that took place outside the domain of the futurity configuration, this change may be the shift that caused the distinction between Classical Portuguese and Modern Portuguese. A piece of evidence in favor of this hypothesis is the empirical observation that at the end of the 14th century the 'ir' + infinitive

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similar but yet different from Bobaljik's (1995) copy theory. Bobaljik's framework postulates syntactic movement and explains the distinction between overt and covert movement as a phonological phenomenon relating to the spelling out of either the head or the tail of the syntactic chain produced by movement. The idea is that "in the event that there is more than one copy of a single element in a given syntactic representation, only one copy is pronounced in the general case" (Bobaljik 1995: 350). The similarity is the appeal to phonology to explain the effect of movement. One of the differences is that there is no syntactic movement chain in our framework. Since the syntactic chains are not produced by syntactic movement, there is no copy of elements either. A detailed explanation of the framework of this paper is presented in Lobato (in preparation).

periphrasis was not yet purely temporal, as we have pointed out in Fn. 11. This hypothesis squares with the fact that the Modern period of the language began in 1500. If this analysis is correct, the cause of the emergence of the periphrastic constructions is outside the futurity configuration, in contrast with the emergence of the morphological construction. However, this is a highly tentative hypothesis, which has to be confronted with additional empirical information. Anyway, according to the proposal in this paper, the two types of change (towards morphological formation and towards periphrastic formation) relate to the use of formal features.

#### 4. Final comments

This paper examined the derivation of the morphophonological shape of Portuguese past participles, including the change in stress placement that took place from Latin to Portuguese in these forms, and argued that the observed facts favor a theory of grammar in which linguistic construction and interpretation involve the same kind of element – formal features. The ‘formal’ property of these features derives from their absence of ‘substantive’ information, of the type found in features such as [Nasal], [Coronal]: [Human], [Animate]. It was assumed that grammatical semantic structure is the formal structure generated by the computational system. It consists of structural configurations of formal features. These configurations are semantic, but they drive phonological interpretation. Therefore, there is isomorphism between expression and content at this grammatical level. The formal features are then more abstract than the morphosyntactic features, such as gender and number.

The paper also argued that this kind of theory may straightforwardly account for diachronic change, and the case of the development of Romance future tense was examined. Parametric settings were said to relate to the location in which the configurations of formal features relevant to semantic interpretation are visible to the PF system. It was claimed that this proposal explains the change in stress placement that took place in past participles in the evolution from Latin to Portuguese. The development of future tense formations in Romance was claimed to be a consequence of a change in the visibility of the functional heads.. In the case of the Imperial Latin and Romance periphrastic formations, it

was assumed that independent changes in the PF system may have altered the visibility of the functional heads. In the case of the Romance morphological formation, it was assumed with Roberts & Roussou (in preparation) that it may be a reflex of the previous stage, which reanalyzed the full lexical verb *habere* as an auxiliary verb.

The proposal that there is a well-defined kind of feature shared by the different levels of construction and interpretation is indeed what one expects, if the design of the language faculty and the derivations in languages are in fact radically minimalist: the same type of feature is used to perform all types of operations in language (assembling of phonetic segments, generation of morphological and syntactic objects, and carrying out of phonological and semantic interpretations). How this occurs is a question for further investigation.

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REVISITING UNIVERSAL GRAMMAR\*  
(Rediscutindo a Gramática Universal)

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*ABSTRACT: This paper sketches various specific scenarios within the Principles and Parameter Theory under which the question of whether Universal Grammar remains accessible to second language learners should be addressed. It also discusses some implications of several approaches to this issue and offers some speculation as to how the question is to be reformulated in the context of the Minimalist Program.*

*KEY WORDS: L2 Acquisition, Partial Access to UG, Principles and Parameters*

*RESUMO: Este artigo delinea vários cenários dentro da Teoria de Princípios e Parâmetros dentro dos quais se deveria abordar a questão de a Gramática Universal permanecer ou não acessível na aquisição de segunda língua. O trabalho discute implicações de várias abordagens e faz algumas especulações sobre como o problema deve ser reformulado no contexto do Programa Minimalista.*

*PALAVRAS-CHAVE: Aquisição de L2, Acesso Parcial a GU, Princípios e Parâmetros*

## 1. The original access question

The theory of Universal Grammar (UG), especially in its Principles and Parameters (PPT) version, has exerted a very significant influence on research investigating second language acquisition (L2A) over the past 15 years. Yet whereas UG-based research on first language development almost unanimously agrees in viewing UG principles as

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\* This is a slightly revised version of a paper presented at the 18th Second Language Research Forum, University of Hawaii, October 1998, as part of a colloquium on "UG Access in L2 Acquisition: Reassessing the Question" organized by Lynn Eubank (U North Texas). The other participants in this colloquium were Robert Bley-Vroman (U Hawaii), Susanne E. Carroll (U Potsdam), Kevin Gregg (St. Andrew U), Bonnie D. Schwartz (U Durham), and Lydia White (McGill U). I want to thank all of them as well as the audience of the colloquium for the stimulating discussion.

constraining properties of children's grammars in essential ways, related issues are much more controversial in L2 studies. The question of whether or not UG continues to be accessible to second language learners has, in fact, been answered differently ever since it was first asked and has subsequently provoked a substantial number of conflicting suggestions and hypotheses. Approximately ten years ago, the state-of-the-art in this area of research was summarized by Eubank (1991a) and by the other contributions to Eubank (1991b). It may, thus, be useful to take another look at this controversy, to ask what we have learned from it and whether it might make sense to pursue it further.

The access question is, of course, understood and answered differently as our understanding of the concept of UG changes. It has, furthermore, become obvious over the past decade that possible answers are not confined to a binary choice between all or nothing. "No access", as it appears to be understood by many, i.e., as claiming that L2 knowledge does not and cannot comprise concepts defined by UG (e.g. grammatical categories, etc.) is probably the least plausible assumption. But as far as I can see, no one is defending it, nor has anybody ever defended such a claim. Clahsen & Muysken (1986), to whom this position is frequently attributed, rather seem to be saying that L2 learners do make use of abstract grammatical categories and relations, although they also develop "rules" which do not conform to principles of UG. I doubt whether the "full access" hypothesis fares much better than "no access" in view of the many obvious and observable differences between first and second language acquisition. This is not to say that such differences must necessarily be attributed to the unavailability of UG in L2 acquisition, but they certainly ask for meaningful explanations which stand up to scrutiny. Non-specific reference to performance factors clearly does not qualify as such. "Partial access" thus seems to offer a more attractive solution to the UG paradox (Clahsen & Muysken, 1989). The term is, however, used ambiguously. Under one understanding, it is claimed that UG shapes L2 knowledge via the linguistic competence acquired in the course of L1 development: this, however, might better be referred to as "indirect access". "Partial access" then implies that some but not all principles of UG can be accessed directly, i.e. not via the L1 grammar, during L2 acquisition.

If we take these considerations into account, the issues which the original access question aimed to resolve can still be crucial ones for research on L2 acquisition. In other words, we have to reinterpret this

question in the light of a different and, hopefully, better understanding of UG and of L2 acquisition, and we should consider the possibilities of indirect and partial access. In what follows I propose a number of different scenarios varying along these lines, in an attempt to avoid terminological and conceptual confusion and in order to be able to spell out explicitly some implications and consequences of the various approaches to the “access to UG” issue.

## 2. Reformulating the access question within Principles and Parameter Theory

As a first approximation, let us maintain the distinction between A) Full Access to UG, B) Partial Access to UG, and C) No Access to UG. Whereas (A) and (C) appear to be self-explanatory, it should be obvious without further elaboration that the intermediate position (B) needs to be further specified in a theoretically satisfactory way with respect to what kind of knowledge is accessible and what is not, in order to be able to make principled predictions which go beyond *ad hoc* claims. The distinction suggested by the Principles and Parameters Theory (PPT) between parameterized and non-parameterized principles indeed allows us to phrase the question about access to UG in a more subtle way since the answer may well depend on the kind of principle one refers to. Moreover, UG access may or may not be mediated by previously acquired languages, most likely by the grammatical knowledge about the L1. In fact, once one considers the possibility that solutions for the “access to UG” problem depend on the relevance attributed to these distinctions, i.e. parameterized *versus* non-parameterized principles and direct *versus* indirect accessibility, it immediately becomes obvious that this is not only the case for option B, Partial Access.

A) Full Access necessarily implies that learners have access to all principles and parameterized options, at every point of acquisition. But although it formulates a categorical statement, the FA hypothesis still leaves room for variation, depending on what role is attributed to previously acquired knowledge. Learners may be expected either a) to normally draw on the knowledge provided by UG, unless there exist specific reasons to first explore the possibilities offered by the L1 grammar, or b) to rely initially on previously acquired knowledge and to resort to UG knowledge only if the former fails to provide the desired results.

C) No Access obviously means that L2 learners do not have direct access to the wealth of implicit knowledge provided by UG. But here too different conclusions can be drawn regarding the knowledge sources available in L2 acquisition. a) One possibility is to maintain that L2 learners have to rely exclusively on non-linguistic, i.e. non-domain-specific cognitive operations. As mentioned above, this is how Clahsen & Muysken (1986) have sometimes been understood: but since these authors made it clear in (1989) that this is not the position they want to propagate, this scenario is apparently not defended by anyone working within a framework which postulates UG as part of the L1 learner's language making capacity. b) Another option is to argue that principles instantiated in the L1 grammar can be used in L2 acquisition, although parameter values cannot be changed since the alternative parametric options are not available any more. Principles not activated in the L1 grammar (non-parameterized and parameterized ones) are, of course, also lost. In other words, given this scenario, "no access" means that UG principles are only indirectly accessible via the L1 grammar, much like in version (b) of the "partial access" hypothesis, see below, except for the fact that parameter values are assumed to be unchangeable. Consequently, the learners' knowledge about grammatical properties of the L2 target language may be expected to conform, in part at least, to the constraints imposed by UG on natural grammars.

B) Partial Access can focus on both the parameterized/non-parameterized distinction and the one between previously activated or not activated principles. This allows for a number of logical possibilities, though not all are of equal plausibility: (1) presents a schematized overview of the more likely ones.

**(1) Scenarios involving partial access to UG knowledge in L2 acquisition**

UG principles	not activated in L1	activated in L1
(a) parameterized	+	-
non-parameterized	+	-
(b) parameterized	-	+
non-parameterized	-	+
(c) parameterized	-	-
non-parameterized	+	+

(a) One imaginable scenario is that only principles not activated in the L1 grammar can still be accessed in L2A; parameterized principles would then need to be set on the appropriate target value. The idea behind such a claim is that UG principles become part of the native grammar as the L1 is acquired: in other words, UG gradually self-destructs in the course of L1 development. As for those principles activated in L1, they may or may not be available for L2 learners, but if they are, this happens via the L1 grammar. Parameter values cannot be reset since the necessary information to do so has been lost with the pristine state of UG. At any rate, this would not count as an instance of access to UG. (b) The second option is that only principles activated in the course of L1 development can also be accessed in L2 acquisition. With respect to parameterized principles this means that they can be fixed to a new value if the L2 target setting differs from the L1 setting. The rationale behind this idea is that knowledge not activated during the appropriate period of language acquisition degenerates, possibly as a result of neurological maturation. UG knowledge activated in L1, on the other hand, remains available, including the parameter values not chosen in the L1 grammar. How the latter could be possible is not obvious, however, neither from a psycholinguistic nor from a neurolinguistic point of view. (c) The third option is that only non-parameterized principles can be accessed in L2 acquisition: open parameters cannot be fixed and settings of parameter values cannot be altered. The explanation of this hypothesis relies on the assumption that only parameterized principles are subject to maturation and are thus not available any more during L2 acquisition: see Smith & Tsimpli (1995).

This schematic review of some possible scenarios shows quite clearly that the broader question of whether or not L2 learners can access UG knowledge can only be answered if a number of more specific issues are settled which are currently treated controversially in L2 research. The extent to which L1 knowledge is used in developing a L2 competence, however, is not one of them. The idea of "indirect access" via the L1 grammar is indeed compatible with all three types of approaches (A, B, C) and is thus not dependent on the access problem. The various scenarios confirm, on the other hand, that the question of whether the setting of parameter values can be changed is indeed a crucial one, although it is also important to determine whether inert UG principles can still be activated. In fact, to the extent that changing the settings of parameters requires the continued availability of information

provided by UG but not instantiated in the L1 grammar, the two questions focus on different aspects of the problem, but are closely related. The two issues thus highlighted are the following:

1) The restructuring issue, whether parameters can be (re-)set to different values once they have been fixed.

2) The inertia issue, whether UG knowledge not activated during L1 development can be accessed in the course of L2 acquisition.

Not all the logically possible positions captured by these scenarios are defended in published research on L2 acquisition. It is, in fact, frequently difficult to allocate individuals or publications to a specific scenario. Let me, nevertheless, try to summarize how the two issues discussed are treated by currently debated hypotheses on L2 acquisition. The Full Access hypothesis in its various forms relies on both issues, although Aa stresses 2 (e.g. Flynn, 1996), and Ab (e.g. Schwartz & Sprouse, 1996) makes heavier use of 1. Proponents of the No Access hypothesis reject both 1 and 2. Note that the Cb scenario (Bley-Vroman, 1989; Clahsen & Muysken, 1989) actually represents the indirect access hypothesis, via the L1 grammar, although critics of C tend not to acknowledge the possibility of this option, e.g. Flynn (1996). The Incompleteness Hypothesis, suggested by Schachter (1996: 170 ff.), can also be assigned to Cb. She rejects 2, arguing that UG is not available anymore to L2 learners, only a “language-specific instantiation of it will be”. She allows, however, for 1, parameter resetting, in L1 acquisition as well as in child L2 acquisition, postulating a “Window of Opportunity”, i.e. a period during which UG remains available: child L2 acquisition, in this case, is then like L1 development. Turning to the various versions of the Partial Access hypothesis, Ba strongly relies on 2, allowing for the setting of unset parameters while rejecting 1, i.e. resetting of fixed parameters: surprisingly, perhaps, this type of approach is apparently not explored in published work on L2 acquisition. I am not sure either about who would subscribe to Bb, supporting 1 but not 2, although this appears to be a plausible hypothesis, provided one accepts parameter resetting in L1 development. Bc, finally, rejects 1, but it accepts 2 in part, i.e. for non-parameterized principles; see Hawkins (1994) and Hawkins & Chan (1997).

### 3. Towards a shared research program

The purpose of this exercise, sketching a number of different scenarios and trying to identify the crucial issues on which they differ, has been to identify research questions which might be fruitfully pursued, even by advocates of contrary hypotheses. The significance of the restructuring and the inertia issues resides in the fact that most of the above mentioned scenarios can or rather must be discarded if the questions implied in (1) and (2) are answered negatively.

– If it can be shown that parameter resetting is not possible, Full Access (Aa as well as Ab) and Partial Access in the Bb version are out.

– If setting of inert parameters (not activated in L1) is not possible in L2 acquisition, A (a and b) and Ba are out.

Given that Ca appears to be implausible for principled reasons and is not defended in current research, this would leave us with Bc and Cb. But even these two need to be specified. If, for example, parameter resetting can be shown to be impossible in L1 acquisition, too, the Window of Opportunity approach (Cb) needs to be modified with respect to its implications for child L2 acquisition. Bc, on the other hand, is not entirely independent of the inertia issue since it maintains that non-parameterized principles remain available to child and adult L2 learners.

To conclude this part of the discussion, let me add some speculations on how plausible it is that we will indeed find that the most promising candidates are either a version of the No Access hypothesis, i.e. no direct but indirect access (Cb), or a version of the Partial Access hypothesis, i.e. continued access only to non-parameterized principles (Bc). There are indeed good reasons to believe that any scenario involving parameter resetting is doomed to fail. In the present context, it is obviously not possible to substantiate this claim in any detail (see Meisel, to appear), but a number of facts and arguments certainly speak in its favor. 1) The available empirical evidence for parameter resetting is not convincing, e.g. one finds neither clustering of grammatical phenomena related to a specific parameter nor abrupt changes in L2 acquisitional patterns. 2) From a psycholinguistic perspective, major restructuring of this sort is unlikely to happen since it would be very costly in terms of processability, as has been argued by Pienemann (1998), discussing the notion of generative entrenchment. 3) Setting parameters to different values does

not happen in L1 development (Clahsen 1991; Müller, 1994) and is thus highly implausible for L2 acquisition. All this is not to say, of course, that learning a second language is impossible; rather, inductive learning needs to be attributed a more important role, not only for lexical learning. As for the second issue, i.e. access to UG principles not activated during L1 development, it is more difficult to make an educated guess about how this will be solved by future research. The claim, however, that only non-parameterized principles, if anything, remain accessible to the L2 learner is theoretically plausible. It also squares with what we know about other types of genetically transmitted knowledge: i.e. sensitive periods typically relate to stimulus-dependent, externally triggered knowledge.

Let me add that both scenarios nevertheless suggest that UG shapes L2 grammatical knowledge, at least in part, i.e. learners are predicted to have indirect access to UG via the L1 grammar. Note also that L2 knowledge, following these approaches, refers, *inter alia*, to grammatical entities such as verbs and nouns: in this respect, it includes domain-specific cognitive representations and operations.

#### **4. Reformulating the question (tentatively) in terms of the Minimalist Program**

At the beginning of this discussion I stated that the question of access to UG in L2 acquisition is asked and answered differently, depending on one's understanding of the human language faculty in general and of UG in particular. The brief review of a number of approaches offered by PPT has led me to conclude that the distinction between parameterized and non-parameterized principles is a crucial one, in this respect, in that only the latter might possibly still be accessible in the successive acquisition of two or more languages. In view of recent developments in linguistic theory, the question arises, of course, whether the notion of UG, as it is developed by the Minimalist Program (Chomsky, 1995), offers different and new perspectives on the access to UG question. I believe that what has been said so far is not in conflict with these ideas, but new insights can be derived from this program which might indeed enhance our understanding of L2 acquisition. Let me therefore, in place of a more general conclusion, add some thoughts on this topic.



In terms of the Minimalist Program, the language faculty is understood as a mental organ interacting with other systems of the mind/brain which impose “legibility conditions” that i-language must satisfy in order to be usable: i.e. linguistic expressions are “read” by these external systems which must be able to use them as “instructions” at the two interface levels, one related to meaning, the other to sound. The idea of explanatory adequacy, accounting for the fact that a particular language is derived from a uniform initial state, is based on the assumption that most of language structure is invariant. Structural change, however, must explore just these “marginal” possibilities of variation. A crucial source of variation is located in the inflectional systems and thus depend on the featural composition of lexical items.

“Legibility conditions impose a three-way division among the features assembled into lexical items:

- (1) semantic features, interpreted at the semantic interface
- (2) phonetic features, interpreted at the phonetic interface
- (3) features that are not interpreted at either interface.

Independently, features are subdivided into the “formal features” that are used by the syntactic operations and others that are not. A natural principle that would sharply restrict language variation is that only inflectional properties are formal features. That seems to be correct, .... In a perfectly designed language, each feature would be semantic or phonetic, not merely a device to create a position or to facilitate computation. If so, there are no uninterpretable formal features. That is too strong a requirement, it seems.” In other words, natural languages exhibit just this type of imperfection, i.e. grammars rely crucially on formal features. “In the syntactic computation, there seems to be a second and more dramatic imperfection in language design, at least an apparent one: the “displacement property” that is a pervasive aspect of language. ... We now have two “imperfections”: uninterpretable formal features, and the displacement property. On the assumption of optimal design, we would expect them to be related, and that seems to be the case: uninterpretable formal features are the mechanism that implements the displacement property.” (Chomsky, 1997a: 12f)

Returning now to the accessibility question, one might ask which of these properties of language characterize L2 interlanguage

expressions. My suggestion is that L2 learners operate on the interface levels. In other words, L2 knowledge cannot interpret uninterpretable formal features: see also Hawkins & Chan (1997). Consequently, inflectional properties may be expected to be ignored or to be assigned semantic or pragmatic values in L2. This seems to be supported by findings like those by Beck (1998) according to which functional categories in adult L2 acquisition are completely unspecified, and permanently so. Displacement properties, too, tend to be interpreted at the interface, attributing to them, for example, non-grammatical, configuration-specific interpretations, e.g. topic-comment, specificity, new and old information, agentive force, etc. Phenomena which cannot be interpreted in this way, e.g. verb second placement, are therefore notoriously difficult for L2 learners but not for children acquiring their L1s, as is well-documented by research on L2 German: see Meisel (1997a,b).

More research is, of course, needed in order to decide whether predictions of this sort can be corroborated. What should be emphasized, however, is that we find substantial differences between first and second language development which a satisfactory theory of second language acquisition needs to account for in a theoretically insightful fashion, rather than treating them as contingent phenomena. The suggestions made here might help to achieve this. Let me add that the principal force behind the development of the notion of Universal Grammar shares the view that L2 acquisition is radically different from L1 development:

“What about second language? That’s harder. Like other kinds of growth, language acquisition happens easily at a certain age, but not later. There comes a time when the system doesn’t work anymore. There are individual differences ... but for most people, after adolescence, it becomes very hard. The system is just not working for some reason, so, you have to teach the language as something strange.” (Chomsky, 1997b: 128)

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**WH-EXTRACTIONS AND RELATIVE CLAUSES IN BRAZILIAN PORTUGUESE**  
(Extrações-WH e Orações Relativas no Português Brasileiro)

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*ABSTRACT: This paper aims to describe and explain WH-extraction patterns out of island contexts in Brazilian Portuguese (BP), by means of the principles established by Generative Theory. I claim that BP uses a strategy for the extraction of subjects which involves a special case of Agreement. Extractions out of relative islands are possible when the extracted WH-phrase ends up in the specifier position of the higher CP and from there it behaves as the subject of the predication. The subject-predicate relationship established under agreement makes Comp a proper governor for traces in subject position under its scope. The analysis proposed makes a distinction between two processes of relative clause formation. One in which que is an operator that transforms sentences into predicates and sits in the Comp position of a CP whose specifier can be occupied by a QP functioning as the subject of the predication. The other, in which que is a WH-word, traditionally treated as a pronoun, occupying the specifier position of a QP and introducing relative clauses as we know them.*

*KEY WORDS: WH-Extractions, Relative Clauses, Predication.*

*RESUMO: Este artigo tem por objetivo descrever e buscar uma explicação para o comportamento de extrações de sintagmas QU- do português brasileiro (PB) para fora de contextos que ficaram conhecidos pelo nome de ilhas, utilizando os princípios da Teoria Gerativa que explicam as propriedades dessas construções nesta e em outras línguas. Argumenta-se que o PB utiliza uma versão da estratégia de concordância em CP, estabelecida por meio de uma relação de predicação, para a extração de sintagmas QU- a partir da posição de sujeito. A análise proposta para essas extrações evidenciou a existência de dois processos sintáticos diferenciados na construção das orações relativas. Um em que que é um operador que transforma sentenças em predicados, ocupa a posição de núcleo de CP em cujo especificador pode-se encontrar o sintagma que funciona como sujeito da predicação. Outro em que que é um elemento QU-, o que tradicionalmente poderia ser caracterizado como um pronome, que ocupa a posição de especificador de um sintagma quantificado, e introduz as relativas tal como tradicionalmente descritas.*

*PALAVRAS-CHAVE: Extrações de Sintagmas-QU, Sentenças Relativas, Predicação.*

## 1. Introduction

One of the ways by which Generative Theory captures the observation that a certain phrase may exhibit behavior compatible with two different syntactic positions is by assuming movement of the phrase in question from one to the other position involved. WH-questions and relative clauses are two examples of such constructions. Treating them in this way made it possible to observe general patterns and asymmetries in the distribution of those phrases within the same language and across languages, which, in turn, are explained by general principles of the grammar.

This paper aims to offer an account of the behavior of WH-question and relative clause extractions in Brazilian Portuguese (BP) by means of the principles already established by the theory as playing a role in explaining the properties of these constructions in other languages. In doing so, the paper will also offer an analysis for relative clauses in this language, showing that work being done on relative clauses in BP have treated two different constructions under the same analysis: restrictive relative clauses and another construction that I will call pseudo-relative clauses, following McCawley (1988).

A theory explaining the behavior of WH-questions and relative clauses through movement has to take into consideration the following questions:

- 1) what elements move from where?
- 2) to where do they move?
- 3) how do they move?
- 4) why do they move?

In this paper I will address the two first questions.

## 2. The BP facts

In pursuing an answer for the first question, we have to examine the paradigms involving the structural position occupied at D-structure by the WH-phrase within its clause, plus the kind of clause itself. The

paradigm in (1) shows the behavior of WH-phrases when extracted from subject (1a), object (1b) and adjunct (1c) positions out of *that*-complement clauses:

- (1) a. Quem<sub>1</sub> você acha [que t<sub>1</sub> saiu da festa mais cedo]?  
 who<sub>1</sub> do you think that t<sub>1</sub> left the party earlier  
 b. Quem<sub>1</sub> você acha [que a Maria encontrou t<sub>1</sub> na festa]?  
 Who<sub>1</sub> do you think that Maria met t<sub>1</sub> at the party  
 c. Por que<sub>1</sub> você acha [que a Maria foi até a casa do Pedro t<sub>1</sub>]?  
 why<sub>1</sub> do you think that Mary went to Pedro's house t<sub>1</sub>

With *that*-complement clauses, extraction from the three positions is grammatical in BP. Unlike English, extraction from the subject can be done even though the complementizer is always overtly present (Complementizer deletion is not allowed in BP).

However, an asymmetry shows up when we try to extract out of WH-complement clauses:

- (2) a. Quem<sub>1</sub> a secretária não sabe [se t<sub>1</sub> já deixou o currículo para ser analisado]?  
 who<sub>1</sub> the secretary do not know whether t<sub>1</sub> left the curriculum to be analysed  
 b. O que<sub>1</sub> a secretária não sabe [se a Cristina deixou t<sub>1</sub> no departamento]?  
 who<sub>1</sub> the secretary do not know whether Cristina left t<sub>1</sub> in the Department  
 c. \*Por que<sub>1</sub> a secretária não sabe [se a Cristina deixou estes documentos no Departamento t<sub>1</sub>]?  
 why<sub>1</sub> the secretary do not know whether Cristina left this documents at the Department t<sub>1</sub>
- (3) a. Que aluno<sub>1</sub> você não sabe [quando<sub>2</sub> t<sub>1</sub> vai entregar o trabalho t<sub>2</sub>]?  
 Which student<sub>1</sub> you do not know when<sub>2</sub> t<sub>1</sub> is going to hand in the paper t<sub>2</sub>  
 b. Que aluno<sub>1</sub> você não sabe [quando<sub>2</sub> o professor vai entrevistar t<sub>1</sub> t<sub>2</sub>]?  
 which student<sub>1</sub> you do not know when<sub>2</sub> the professor is going to interview t<sub>1</sub> t<sub>2</sub>  
 c. \*Por que<sub>2</sub> você não sabe [que trabalho<sub>1</sub> o aluno não entregou t<sub>1</sub> t<sub>2</sub>]?  
 why<sub>2</sub> you do not know which paper<sub>1</sub> the student did not hand in t<sub>1</sub> t<sub>2</sub>

Sentences (2) and (3) show that extraction out of indirect question clauses (WH-islands) is fine when the extracted WH-phrase occupies either the subject ((2a) and (3a)) or the object position ((2b) and (3b)). However, if it is sitting on an adjunct position, extraction creates an ungrammatical sentence ((2c) and (3c)).

The pattern exhibited by extraction out of complement clauses contrasts with the one exhibited by extraction out of adjunct clauses in (4) and subject clauses in (5):

- (4) a. \*Quem<sub>1</sub> o João saiu da festa antes que t<sub>1</sub> tivesse entregado o presente?  
Who<sub>1</sub> John left of the party before that t<sub>1</sub> had given the present
- b. \*Quem<sub>1</sub> o João saiu da festa antes que a Maria tivesse encontrado t<sub>1</sub>?  
who<sub>1</sub> John left of the party before that Mary had met t<sub>1</sub>
- c. \*De que modo<sub>1</sub> o João saiu da festa antes que a Maria tivesse tratado o Pedro t<sub>1</sub>?  
in which manner<sub>1</sub> John left of the party before that Mary had treated Peter t<sub>1</sub>
- (5) a. ?\*Quem<sub>1</sub> [que t<sub>1</sub> recebeu doações ilegais para sua campanha] é óbvio?  
Who<sub>1</sub> that t<sub>1</sub> received illegal contributions to his campaign is obvious
- b. ?\*Quem<sub>1</sub> [que a Academia vai escolher t<sub>1</sub> para ganhar o prêmio] é óbvio?  
Who<sub>1</sub> that the Academy will choose t<sub>1</sub> to win the prize is obvious
- c. \*Quando<sub>1</sub> [que a Maria vai entregar o trabalho t<sub>1</sub>] é óbvio?  
When<sub>1</sub> that Maria will hand in the paper t<sub>1</sub> is obvious

The sentences in (4) and (5) show that in adjunct and subject clauses, the syntactic position occupied by the WH-phrase does not matter. Extraction out of them produces ungrammatical sentences. Nonetheless, if the subject clause is extraposed, the ungrammaticality disappears. WH-extractions out of extraposed subject clauses patterns WH-extractions out of *that*-complement clauses:

- (6) a. Quem<sub>1</sub> é óbvio [que t<sub>1</sub> recebeu doações ilegais para sua campanha]?  
Who<sub>1</sub> it is obvious that t<sub>1</sub> received illegal contributions to his campaign
- b. Quem<sub>1</sub> é óbvio [que a Academia vai escolher t<sub>1</sub> para ganhar o prêmio]?  
Who<sub>1</sub> it is obvious that the Academy will choose t<sub>1</sub> to win the prize
- c. Quando<sub>1</sub> é óbvio [que a Maria vai entregar o trabalho t<sub>1</sub>]?  
When<sub>1</sub> it is obvious that Maria will hand in the paper t<sub>1</sub>



It seems interesting to observe the pattern of extractions out of complement and adjunct clauses of nouns, that is, the set of data which in traditional terms was captured by the *Complex Noun Phrase Constraint*. The sentences in (7) show the pattern of extraction out of sentences which are complements of nouns. The asymmetry "argument versus adjunct" ((7a-b) vs. (7c)) is also found in the extraction out of a clause which is the complement of a noun:

- (7) a. ?Quem<sub>1</sub> o Pedro ficou chocado com a notícia de que t<sub>1</sub> está namorando a Maria?  
Who<sub>1</sub> Peter got shocked with the news of that t<sub>1</sub> is dating Mary
- b. ?Quem<sub>1</sub> o Pedro ficou chocado com a notícia de que a Maria está namorando t<sub>1</sub>?  
who<sub>1</sub> Peter got shocked with the news of that Mary is dating t<sub>1</sub>
- c. \*Como<sub>1</sub> o Pedro ficou chocado com a notícia de que o Pedro tinha morrido t<sub>1</sub>?  
how<sub>1</sub> Peter got shocked with the news of that Peter have died t<sub>1</sub>

In the case of extraction out of relative clauses, a different pattern is observed:

- (8) a. Que animais<sub>1</sub> o diretor do Zoo disse que a comida<sub>2</sub> que t<sub>1</sub> comeram t<sub>2</sub> estava estragada?<sup>1</sup>  
which animals<sub>1</sub> the director of the Zoo said that the food<sub>2</sub> that t<sub>1</sub> ate t<sub>2</sub> was deteriorated
- b. ???Que animais<sub>2</sub> o diretor do Zoo disse que a comida<sub>1</sub> que t<sub>1</sub> matou t<sub>2</sub> estava estragada?  
Which animals<sub>2</sub> the director of the Zoo said that the food<sub>1</sub> that t<sub>1</sub> killed t<sub>2</sub> was deteriorated
- c. \*De que modo<sub>2</sub> o diretor do Zoo disse que os animais<sub>1</sub> que as crianças alimentaram t<sub>1</sub> t<sub>2</sub> eram da África?  
In which way<sub>2</sub> the director of the Zoo said that the animals<sub>1</sub> that the children fed t<sub>1</sub> t<sub>2</sub> were from Africa
- d. \*De que modo<sub>1</sub> o diretor do Zoo disse que as crianças<sub>2</sub> que t<sub>2</sub> trataram dos animais t<sub>1</sub> estavam muito felizes  
in which way<sub>1</sub> the director of the Zoo said that the children<sub>2</sub> that t<sub>2</sub> treated the animals t<sub>1</sub> were very happy

<sup>1</sup> This sentence corresponds to example (30a) used by Lobato (1986:419) to show that extractions out of some islands are possible in BP.

Extraction of a subject over a relativized object (8a) is perfect, but extraction of an object over a relativized subject (8b) is not as good. Extraction of an adjunct either over a relativized subject (8d) or a relativized object (8c) results in ungrammaticality. However, some further remarks about the data in regard to extraction out of relative clauses is in order.

First, building up the data for testing the extraction possibilities has to be done very carefully in order to control for other variables that may be playing a role in the results obtained. For example, sentences (9) exhibit a different pattern:

- (9) a. ??Que comida<sub>1</sub> o diretor do Zoo disse que os animais<sub>2</sub> que t<sub>2</sub> comeram t<sub>1</sub> foram dormir?<sup>2</sup>  
Which food<sub>1</sub> the director of the Zoo said that the animals<sub>2</sub> that t<sub>2</sub> ate t<sub>1</sub> went to sleep
- b. Que comida<sub>1</sub> o diretor do Zoo disse que os animais<sub>2</sub> que t<sub>2</sub> comeram t<sub>1</sub> passaram mal?  
Which food<sub>1</sub> the director of the Zoo said that the animals<sub>2</sub> that t<sub>2</sub> ate t<sub>1</sub> got sick

Sentence (9b) may sound perfect at first, because we can attribute it a different analysis. In as much as anteposed PPs may have their preposition deleted in BP, the WH-phrase *que comida* 'which food' may be interpreted as an argument of the predicate *passaram mal* 'got sick', meaning that 'the animals got sick with the food', in which case it has been extracted out of a *that*-complement clause.

Second, extractions out of relative clauses whose head is in subject position are much better than the ones in which the head is in object position. This claim can be attested if we compare the sentences in (8a-b) with the sentences in (10a-b):

- (10) a. ?\*Que animais<sub>1</sub> o diretor do Zoo disse que a televisão mostrou as crianças<sub>2</sub> que t<sub>1</sub> atacaram t<sub>2</sub>?  
Which animals<sub>1</sub> the director of the Zoo said that the television showed the children<sub>2</sub> that t<sub>1</sub> attacked t<sub>2</sub>

<sup>2</sup> The strangeness of sentence (9a), in which the subject is extracted over a relativized object, will be explained later in the text.

- b. ?\*Que animais<sub>1</sub> o diretor do Zoo disse que a televisão mostrou as crianças<sub>2</sub> que t<sub>2</sub> alimentaram t<sub>1</sub>?  
Which animals<sub>1</sub> the director of the Zoo said that the television showed the children<sub>2</sub> t<sub>2</sub> fed t<sub>1</sub>

In sentences (10) the relative clause is attached to the DP *as crianças* 'the children' which occupies that object position of the complement clause, whereas in sentences (8) the relative clause is attached to the DP *a comida* 'the food', which is in subject position.

If the grammatical results achieved with extraction from the subject out of *that*-complement clauses and WH-islands is something that requires some adjustment in the principles of the theory, nevertheless it is a fact already accounted for in regard to a variety of languages. What is unexpected, as far as I know, is the asymmetry between subject extraction versus object extraction out of relative clauses. In this paper, I will propose that the same strategy of subject extraction used by BP to escape *that*-complement clauses and WH-islands plays a role in subject extraction out of relative clauses.

### 3. The Literature and the BP Data

Asymmetries of the kind observed in the BP data have been dealt with under the Generative framework by a principle that regulates the licensing of empty categories resulting from movement (traces), namely, the Empty Category Principle (ECP). This principle has received different formulations along the history of the theory. Rizzi (1990) revises Chomsky's (1986) proposal, postulating that ECP could be formulated as comprising a formal licensing and an identification requirement. He ends up proposing that ECP is a principle dealing only with the formal licensing of traces:

- (11) ECP: a nonpronominal empty category must be properly head-governed (p. 87).

The identification requirement of traces is subsumed either under binding of referential chains, where by referential he means a chain formed by segments sharing a referential index attributed at D-structure

under theta-role assignment: or under government, a more local relationship. This system gives the possibility of accounting for the two kinds of asymmetries: 1) in terms of its formal licensing, subject traces pattern with adjunct traces against object traces, in as much as for a trace to be properly head governed, it must be governed by a head within its immediate projection, namely,  $X'$ ; 2) in terms of its identification, subject traces pattern with object traces against adjunct traces, because subjects and objects receive a referential index, in as much as they may receive a referential theta-role, whereas that is not the case with adjuncts.

The extraction pattern showed in the BP data described above may be explained in part by the principles of the theory as already proposed for other languages. For example, the ungrammaticality in the case of subject, object and adjunct extractions out of subject and adjunct clauses fall under the generalization expressed by the *Condition on Extraction Domains* (CED) proposed by Huang (1982), and as such can be captured by any of the proposals dealing with this kind of extraction in the literature (see Chomsky, 1986). Other than that, the analysis proposed for the Italian data in regard to the grammaticality of object and adjunct extractions out of *that*-complement clauses, in regard to the grammaticality of object extractions out of WH-islands, and in regard to the ungrammaticality of adjunct extractions out of WH-islands may account for equivalent BP sentences (see Rizzi, 1990). The problem seems to be the grammaticality of subject extractions out of *that*-complement clauses and WH-islands. This kind of extraction is also possible in Italian, but the attempt to extending to the BP data, the analysis proposed for this kind of extraction in the former language, requires discussion.

Rizzi discusses the symmetrical behavior in regard to subject and object extractions out of *that*-complement clauses and WH-islands, and claims that languages of the world seem to diverge in terms of the strategies they use to license subject traces. He describes three major strategies used in order to license subject traces. The first consists of transforming  $C^{\circ}$  into a proper governor when it hosts Agreement features. This is the case of English, which deletes the complementizer to avoid incompatibility with Agr features raised to  $C^{\circ}$ . This is also the case in French, a language in which the form of the complementizer changes, showing its agreement features. This is also true of V2 languages, in which the inflected verb moves to Comp. The second strategy is to

eliminate the gap through the insertion of a resumptive pronoun. This is true of languages such as Swedish and Vata. The third strategy, used by Null Subject languages, consists of extracting the subject from a post verbal position, a strategy available because those languages allow for free inversion of the subject. This is the case of Italian.

In the literature about BP we can find defenders of all three strategies. Moreira da Silva (1983) and more recently Vitral (1992) argued that the empty category is in fact a null resumptive pronoun. Figueiredo Silva (1994), who argues that empty categories in BP result from movement, claims that this language adopts the Agreement strategy, in the case of epistemic verbs, and the extraction from post-verbal position, in other cases. This latter conclusion is a little surprising, because in her dissertation she argues convincingly for the fact that BP, having lost the possibility of assigning Case under government, does not allow free inversion of the subject, a fact also recognized by other Brazilian linguists.

Based on the claims already made in the literature that BP has lost the possibility of free inversion of the subject, therefore it does not count on a post verbal position for subject extractions: and that empty categories in extraction contexts are traces of movement, in as much as they obey islands, as shown in this paper with adjunct islands: in the next section, I will develop the claim that BP uses a strategy for the extraction of subjects which involves a special case of Agreement.

#### 4. The strategy of Subject Extraction in BP

Rizzi (1990) explains the asymmetries in subject traces when extraction took place out of complement clauses in regard to extraction out of subject relative clauses in English by establishing a typology of complementizers in terms of the features [+/- wh] and [+/- pred(icative)]:

- (12) a. +wh -pred: (I wonder) what 0 [ you saw t ]  
 b. +wh +pred: The thing which 0 [ you saw t ]  
 c. -wh +pred: The thing Op that [ you saw t ]  
 d. -wh -pred: (I know) that [ you saw t ] (p. 68)

*That* in English is incompatible with COMPs having a [+wh] feature. Therefore, if a trace of a WH-moved phrase has to be in COMP, *that*

must be deleted. Relative clauses have the feature [+pred], because they establish with their heads a predication relationship. *That* can cooccur with the null operator, which has a [+pred] feature, because Op is underspecified for the [+/- wh] features. Therefore, *that*-deletion does not have to take place in relative clauses.

To account for the data regarding subject extractions in BP two assumptions have to be made. First, the complementizer *que* is underspecified for the [+/- wh] features. As a consequence, *que* can occupy the head of a [+wh] COMP which has a WH-phrase in its specifier position. An evidence for this claim is the fact that in sentence (13), a grammatical sentence, *que* cooccurs with the WH-phrase *quem* under the same functional projection:

- (13) Quem<sub>i</sub> que João espera encontrar t<sub>i</sub> na festa?  
Who<sub>i</sub> that John expects to meet t<sub>i</sub> at the party

Second, *que* functions as an operator which transforms the clause under its scope into a predicate of the phrase occupying its specifier position, which in turn will function as the subject of the predication. Having established this predication relation under Spec-Head agreement, Comp becomes a proper governor for the trace in subject position.

The use of the agreement in Comp strategy to explain the grammaticality of subject extractions in Portuguese is not new. Zubizarreta (1982) claimed that agreement in Comp is the process taking place in European Portuguese when subject WH-phrases are extracted out of non-factive complement clauses. She says: "We may assume that Portuguese has recourse to the same strategy as French. The only difference is that in Portuguese the result of the morphological rule is phonologically identical to the complementizer. That is, the complementizer *que* and the trace of the nominative WH-morpheme in Comp rewrite as *que*." (p. 85-86).

The analysis proposed in this paper differs from Zubizarreta's proposal not only in its motivation and implementation, but also in its scope. The agreement in Comp strategy takes place in a large number of extraction contexts, even in the case of extractions out of relative clauses.

#### 4.1. Agreement in Comp: The Relative Clause Cases

Let's start by investigating two observations already presented: 1) extraction of a subject over a relativized object produced better results than extraction of an object over a relativized subject; 2) extraction out of relative clauses whose head is in subject position are much better than the one in which the head is in object position. The paradigm in (14)-(17) was built to test those observations. Each sentence exemplifies one of the two observations combined. Sentence (14) is an example in which the head of the relative clause is in the subject position of the matrix clause and is anaphorically<sup>3</sup> related to the object of the relative clause. In this case WH-movement extracts the subject of the relative:

- (14) [Que animais<sub>1</sub>] [a comida<sub>2</sub> [que t<sub>1</sub> comeram t<sub>2</sub>] estava estragada]?<sup>4</sup>  
 [which animals<sub>1</sub>] [the food<sub>2</sub> [ that t<sub>1</sub> ate t<sub>2</sub> ] was deteriorated ]  
 (14') A comida<sub>2</sub> [que que animais<sub>1</sub> comeram t<sub>2</sub>] estava estragada ?  
 the food<sub>2</sub> [that which animals<sub>1</sub> ate t<sub>2</sub>] was deteriorated

Sentence (15) is an example in which the head of the relative clause is in the subject position of the matrix clause and is anaphorically related to the subject of the relative clause. In this case WH-movement extracts the object of the relative:

- (15) \*?[Que animais<sub>2</sub>] [a comida<sub>1</sub> [que t<sub>1</sub> matou t<sub>2</sub>] estava estragada]?  
 [which animals<sub>2</sub>] [ the food<sub>1</sub> [ that t<sub>1</sub> killed t<sub>2</sub> ] was deteriorated ]  
 (15') A comida<sub>1</sub> [que t<sub>1</sub> matou que animais<sub>2</sub> ] estava estragada ?  
 the food<sub>1</sub> [ that t<sub>1</sub> killed which animals<sub>2</sub> ] was deteriorated

Sentence (16) is an example in which the head of the relative clause is in the object position of the matrix clause and is anaphorically related to the object of the relative clause. In this case WH-movement extracts the subject of the relative:

<sup>3</sup> The word *anaphorically* is taken in a very generic sense to express the relationship with an antecedent.

<sup>4</sup> Sentence (14') represents sentence (14) before WH-extraction. This observation is also valid for all the examples having a (x') counterpart.

- (16) \*?[Que animais<sub>1</sub>] [a televisão mostrou as crianças<sub>2</sub> [que t<sub>1</sub> atacaram t<sub>2</sub>]]?  
 [which animals<sub>1</sub>] [the television showed the children<sub>2</sub> [that t<sub>1</sub> attacked t<sub>2</sub>]]  
 (16') A televisão mostrou as crianças<sub>2</sub> [que que animais<sub>1</sub> atacaram t<sub>2</sub>] ?  
 the television showed the children<sub>2</sub> [that which animals<sub>1</sub> attacked t<sub>2</sub>]

Sentence (17) is an example in which the head of the relative clause is in the object position of the matrix clause and it is anaphorically related to the subject of the relative clause. In this case WH-movement extracts the object of the relative:

- (17) \*?[Que animais<sub>2</sub>] [a televisão mostrou as crianças<sub>1</sub> [que t<sub>1</sub> alimentaram t<sub>2</sub>]]?  
 [which animals<sub>2</sub>] [the television showed the children<sub>1</sub> [that t<sub>1</sub> fed t<sub>2</sub> ]]  
 (17') A televisão mostrou as crianças<sub>1</sub> [que t<sub>1</sub> alimentaram que animais<sub>2</sub>]?  
 the television showed the children<sub>1</sub> [that t<sub>1</sub> fed which animals<sub>2</sub>]

The pattern showed in the paradigm (14 – 17) leads to the conclusion that extraction of a subject WH-phrase out of a relative clause whose head is the subject of the matrix clause generates the best results. Nevertheless, this generalization does not seem to hold in sentences (18):

- (18) a. \*?[Que menina<sub>1</sub>] [os alunos<sub>2</sub> [que t<sub>1</sub> convidou t<sub>2</sub> para a festa] se arrependeram]?  
 [Which girl<sub>1</sub>] [the students<sub>2</sub> [ that t<sub>1</sub> invited t<sub>2</sub> to the party] felt sorry]  
 a'. Os alunos<sub>2</sub> [que que menina<sub>1</sub> convidou t<sub>2</sub> para a festa] se arrependeram ?  
 the students<sub>2</sub> [that which girl<sub>1</sub> invited t<sub>2</sub> to the party] felt sorry  
 b. ??[Que menina<sub>2</sub>] [os alunos<sub>1</sub> [que t<sub>1</sub> convidaram t<sub>2</sub> para a festa] se arrependeram]?  
 [which girl<sub>2</sub>] [the students<sub>1</sub> [that t<sub>1</sub> invited t<sub>2</sub> to the party] felt sorry]  
 b'. Os alunos<sub>1</sub> [que t<sub>1</sub> convidaram que menina<sub>2</sub> para a festa] se arrependeram  
 the students<sub>1</sub> [that t<sub>1</sub> invited which girl<sub>2</sub> to the party] felt sorry

The judgments in sentences (18) are reversed if compared with sentences (14 – 17). Subject extraction out of a relative clause whose head is the subject of the matrix clause (18a) generates an unacceptable sentence whereas object extraction out of a relative whose head is the



subject of the matrix clause (18b) is more acceptable. Things get even more complicated in sentences (19):

- (19) a. ??[Que rapaz<sub>1</sub>][a comissão premiou o conto<sub>2</sub> [ que t<sub>1</sub> escreveu t<sub>2</sub> ]]?  
 a'. A comissão premiou o conto<sub>2</sub> [que que rapaz<sub>1</sub> escreveu t<sub>2</sub> ]  
 b. \*[Que comissão<sub>1</sub>][o rapaz escreveu o conto<sub>2</sub> [que t<sub>1</sub> premiou t<sub>2</sub> ]]?  
 b'. O rapaz escreveu o conto<sub>2</sub> [que que comissão<sub>1</sub> premiou t<sub>2</sub> ]

In both sentence (19a) and (19b) the WH-phrase is extracted from the subject position out of a relative clause whose head is the object of the matrix clause. Although both are not perfect, (19a) is acceptable whereas (19b) is ungrammatical. At this point, an observation is in order. Although the resumptive pronoun strategy may be used to improve those sentences, their acceptability varies in the same direction than the acceptability of the sentences in (18) and (19):

- (20) a. ?[Que menina<sub>1</sub>][os alunos<sub>2</sub> [que ela<sub>1</sub> convidou t<sub>2</sub> para a festa] se arrependeram]?  
 [which girl<sub>1</sub>] [the students<sub>2</sub> [that she<sub>1</sub> invited t<sub>2</sub> to the party] felt sorry] ?  
 b. [Que menina<sub>1</sub>][os alunos<sub>1</sub> [que t<sub>1</sub> convidaram ela<sub>2</sub> para a festa] se arrependeram]?  
 [which girl<sub>1</sub>] [the students<sub>1</sub> [that t<sub>1</sub> invited her<sub>2</sub> to the party] felt sorry] ?  
 c. [Que rapaz<sub>1</sub>][a comissão premiou o conto<sub>2</sub> [ que ele<sub>1</sub> escreveu t<sub>2</sub> ]]?  
 [which boy<sub>1</sub>] [the committee gave a prize to the short story<sub>2</sub> [that he<sub>1</sub> wrote t<sub>2</sub> ]]?  
 d. ???[Que comissão<sub>1</sub>][o rapaz escreveu o conto<sub>2</sub> [que ela<sub>1</sub> premiou t<sub>2</sub> ]]?  
 [which committee<sub>1</sub>] [the boy wrote the short story<sub>2</sub> [that she<sub>1</sub> gave a prize t<sub>2</sub> ]]?

The sentences in (20) are evidence for the fact that although BP can build interrogative sentences with resumptive pronouns, this is not the strategy being used for WH-extractions in this language. Moreover, the data presented so far is counterevidence for analyses that claim that it is because BP has resumptive empty pronouns that islands can be violated in this language. If overt or empty resumptive pronouns were the strategy adopted by BP to avoid island effects, all those sentences would have to be equally acceptable.

The examination of all the cases involving subject and object extraction out of relative clauses leads to the conclusion that, in order for the sentence to be acceptable, some kind of relation must hold between the eventualities expressed by the two sentences related through relativization. If we build an example in which the two sentences express independent eventualities, even if we relate both sentences through relativization, WH-extraction will produce an ungrammatical sentence. The comparison between the sentences in (21) with the sentences in (22) and (23), both cases in which the extraction is of a subject WH-phrase out of a relative clause whose head is the subject of the matrix clause, shows a contrast in acceptability:

- (21) a. Meu aluno encontrou alguns amigos  
my student met some friends  
b. Os amigos sabem falar inglês  
the friends know to speak English  
c. Os amigos<sub>1</sub> [que meu aluno<sub>2</sub> encontrou t<sub>1</sub>] sabem falar inglês  
the friends<sub>1</sub> [the my student<sub>2</sub> met t<sub>1</sub>] know to speak English  
d. \*[Que aluno<sub>2</sub>][os amigos<sub>1</sub> [que t<sub>2</sub> encontrou t<sub>1</sub>] sabem falar inglês]]?  
[which student<sub>2</sub>][ the friends<sub>1</sub> [that t<sub>2</sub> met t<sub>1</sub>] know to speak English]]?
- (22) a. Um motorista desconhecido atropelou uma criança  
an unknown driver run over a child  
b. A criança ficou muito machucada  
the child got very hurt  
c. A criança<sub>1</sub> [que um motorista desconhecido<sub>2</sub> atropelou t<sub>1</sub>] ficou muito machucada  
the child<sub>1</sub> [that an unknown driver<sub>2</sub> run over t<sub>1</sub>] got very hurt  
d. ?[Que motorista<sub>2</sub>][a criança<sub>1</sub> [que t<sub>2</sub> atropelou t<sub>1</sub>] ficou muito machucada]]?  
[which driver<sub>2</sub>][ the driver [ that t<sub>2</sub> run over t<sub>1</sub> ] got very hurt]]?
- (23) a. A cozinheira fez a comida  
the cook made the food  
b. A comida matou os animais  
the food killed the animals  
c. A comida<sub>1</sub> [que a cozinheira<sub>2</sub> fez t<sub>1</sub>] matou os animais  
the food<sub>1</sub> [that the cook<sub>2</sub> made t<sub>1</sub>] killed the animals  
d. [Que cozinheira<sub>2</sub>][a comida<sub>1</sub> que t<sub>2</sub> fez t<sub>1</sub>] matou os animais]]?  
[which cook<sub>2</sub>][the food<sub>1</sub> that t<sub>2</sub> made t<sub>1</sub>.killed the animals]]?

The contrast between (21), on one hand, and (22) and (23), on the other, is evidence that the relationship between the two eventualities expressed in the matrix and the relative clause makes WH-extractions more acceptable. Moreover, although it is very hard to characterize this relationship between eventualities, it does not seem unsound to claim that in order for this greater acceptability to take place, the eventuality expressed by the VP of the relative clause takes place in a time prior to the time of the event corresponding to the matrix clause. In other cases, it is an antecedent-consequence relationship that seems to hold, in other words, the eventuality expressed by the main clause seems to close the sequence of eventualities.

An observation has to be made in regard to the sentences in (22). Sentence (22d) is ambiguous: it has a first interpretation in which the child run over the driver and end up hurt. Even though pragmatically odd, this interpretation seems to be the preferable one. If we build the structures corresponding to this interpretation, we will see that it is a case of object WH-extraction out of a relative clause whose head is the subject of the matrix clause. The second interpretation corresponds to the structure presented in (22d), which is also possible. If so, why is sentence (15), repeated here as (24), much worst?

- (24) \*?[Que animais<sub>1</sub>] [a comida<sub>1</sub> [que t<sub>1</sub> matou t<sub>2</sub>] estava estragada]?  
 [which animals<sub>2</sub>] [ the food<sub>1</sub> [ that t<sub>1</sub> killed t<sub>2</sub> ] was deteriorated]

Being a sentence in which an object extraction out of a relative clause whose head is the subject of the matrix clause was performed is not the problem with (24). The unacceptability of (24) comes from the fact that the relationship between eventualities is reversed: *killing the animals* should be the eventuality closing the sequence. Sentence (25) in contrast with (26) support the claim even further:

- (25) a. Os ladrões roubaram o banco  
 the robbers robbed the bank  
 b. Os ladrões foram presos  
 the robbers were put in jail  
 c. Os ladrões<sub>1</sub> [que t<sub>1</sub> roubaram o banco<sub>2</sub>] foram presos  
 the robbers<sub>1</sub> [that t<sub>1</sub> robbed the bank<sub>2</sub>] were put in jail  
 d. [Que banco<sub>2</sub>] [os ladrões<sub>1</sub> [que t<sub>1</sub> roubaram t<sub>2</sub>] foram presos]?  
 [which bank<sub>2</sub>] [the robbers<sub>1</sub> [that t<sub>1</sub> robbed t<sub>2</sub>] were put in jail]]?

- (26) a. Os ladrões roubaram o banco  
the robbers robbed the bank
- b. Os ladrões moram em outra cidade  
the robbers live in another city
- c. Os ladrões<sub>1</sub> [que t<sub>1</sub> roubaram o banco<sub>2</sub>] moram em outra cidade  
the robbers<sub>1</sub> [that t<sub>1</sub> robbed the bank<sub>2</sub>] live in another city
- d. ?\*[Que banco<sub>2</sub>] [os ladrões<sub>1</sub> [que t<sub>1</sub> roubaram t<sub>2</sub>] moram em outra cidade]]?  
[which bank<sub>2</sub>] [the robbers<sub>1</sub> [that t<sub>1</sub> robbed t<sub>2</sub>] live in another city]]?

Sentence (26d), in which the eventualities *to rob the bank* and *to live in another city* are independent, is ungrammatical, whereas sentence (25d), in which the eventualities *to rob the bank* and *to be put in jail* express a cause-effect relationship is perfect.

The contrast between (19a) and (19b), a case of extraction out of relatives whose head is in object position, also reinforce this conclusion. *To write a short story* and *to win a prize* are related eventualities and the eventuality closing the sequence is the eventuality expressed by the main clause.

The claim that the unacceptability of (24) comes from the fact that the eventuality corresponding to the consequence should be the closing sequence eventuality is related to one of the properties proposed by McCawley (1988) to characterize a third type of relative clauses: "There is a class of cases in which what appears at first glance to be restrictive relative clauses behave more like the cleft clauses than the restrictive relatives (...) The apparent relative clauses, which I will henceforth refer to as pseudo-relative clauses, also differ from ordinary restrictive relatives with regard to a constraint ( the Complex NP constraint) that excludes extraction of material from a relative clause construction"(p. 428).

According to McCawley, pseudo-relatives usually occur in the coda of existential sentences. The sentences in (27), McCawley's (34a) and (35a), show the contrast between extraction out of a restrictive relative (27a) and extraction out of a pseudo-relative (27b):

- (27) a. \*Which books did John praise the person who wrote  $\phi$ ?  
b. ?Which persons do you think there are many Americans who distrust  $\phi$ ?

Other properties associated with pseudo-relatives are: they appear in final position in regard to the sentence in which they are embedded and they can be paraphrased by an existential clause in which the relative is the main clause. Moreover, based on work done by Prince (apud McCawley), he points out that pseudo-relatives are the only relatives that allow deletion of the subject relative pronoun, that is, they allow *that*-deletion, which is the strategy used by English to render grammatical sentences containing extraction out of a subject position in *that*-complement clauses. The sentences in (28), his examples (3), are examples of *that*-deletion in relative clauses:

- (28) a. I have a friend  $\phi$  called me yesterday  
 b. We got a lot of fancy cadillacs  $\phi$  don't tip

McCawley also points out that those sentences correspond to a class of Chinese sentences, studied by Huang (apud McCawley, 1988), which shows up in the coda of existential sentences, in final position, and are not introduced by any particle. They differ from Chinese relative clauses, which precede their heads and are closed by the *-de* particle.

Brazilian Portuguese has a very used strategy for giving indefinite quantifier phrases wide scope, which correspond to existential sentences having a relative clause in its coda. Following McCawley, we can characterize them as pseudo-relatives. The interesting is that the acceptable sentences exhibiting extraction out of relative clauses may have an existential version, as shown in (29):

- (29) a. Tem uns animais que a comida que comeram estava estragada.  
 there are some animal that the food that they ate was deteriorated  
 b. Tem uma cozinheira que a comida que fez matou os animais  
 there is a cook that the food that cooked killed the animals  
 c. Tem um rapaz que a comissão premiou o conto que escreveu  
 there is a boy that the committee gave a prize to the short story wrote

The proposal developed is that extraction out of relative islands is possible when the extracted WH-phrase ends up in the specifier position of the higher CP and from there it behaves as the subject of the predication. The subject-predicate relationship established under

agreement makes Comp a proper governor for traces in subject position under its scope. Independent eventualities do not allow the establishment of a predication relationship. Consequently, in cases like that, Comp will not be a proper governor for traces in subject position and ECP will be violated. The possibility of having an overt complementizer *que* 'that' occupying the head position of CP renders visible the predication relationship between the WH-phrase and the sentence transformed into a predicate by the operation of *que*:

- (30) a. [<sub>CP</sub> [Que animais<sub>1</sub>] [<sub>C</sub> que a comida<sub>2</sub> [que t<sub>1</sub> comeram t<sub>2</sub>] estava estragada] ?  
[which animals<sub>1</sub>] [ that the food<sub>2</sub> [ that t<sub>1</sub> ate t<sub>2</sub> ] was deteriorated ]
- b. [<sub>CP</sub> [Que rapaz<sub>1</sub>] [<sub>C</sub> que a comissão premiou o conto<sub>2</sub> [ que t<sub>1</sub> escreveu t<sub>2</sub>] ] ?  
[which boy<sub>1</sub>] [that the committee gave a prize to the short story<sub>2</sub> [that t<sub>1</sub> wrote t<sub>2</sub>] ] ?

The analysis proposed in this article in regard to the strategy used in BP for extracting WH- phrases out of islands gives motivation to one of the properties raised by McCawley to characterize pseudo-relatives, namely, the property of appearing in final position in regard to the sentence in which they are embedded. This property is recaptured in terms of the need to establish a predication relationship.

The proposal that subject extraction out of relatives is licensed by a predication relationship which establishes agreement in Comp and renders C° a proper governor has to be extended for the subject extraction cases out of *that*-complement clauses and WH-islands.

#### 4.2. The Analysis Implementation

Subject WH-Extractions out of *that*-complement clauses, as in the case of sentence (1a) repeated here as (31) is derived in the following way:

- (31) Quem<sub>1</sub> você acha [que t<sub>1</sub> saiu da festa mais cedo] ?  
who<sub>1</sub> do you think that t<sub>1</sub> left the party earlier

The WH-phrase *quem* moves to the specifier position of the embedded CP, whose head is occupied by the operator *que*. A predication relation takes place and consequently, agreement between the referential features of the WH-phrase renders Comp a proper governor for the trace. From there, the WH-phrase moves to Comp in the matrix sentence. The licensing conditions are fulfilled and the interpretation of the empty category is given by binding by an antecedent.

Let's see now the cases of subject extraction out of WH-islands, as in (3a), repeated as (32a) and (32b):

- (32) a. *Que aluno<sub>1</sub> você não sabe [quando<sub>2</sub> t<sub>1</sub> vai entregar o trabalho t<sub>2</sub> ]?*  
Which student<sub>1</sub> you do not know when<sub>2</sub> t<sub>1</sub> is going to hand in the paper t<sub>2</sub>
- b. *Que moça<sub>1</sub> você não sabe [com que rapaz<sub>2</sub> t<sub>1</sub> saiu t<sub>2</sub> ontem ]?*  
Which girl<sub>1</sub> you do not know with which boy<sub>2</sub> t<sub>1</sub> went out t<sub>2</sub> last night

In (32a) the WH-phrase *que aluno* moves first to the specifier (Spec) position of the embedded COMP, leaves a trace there and goes on to the Spec of the matrix COMP. Being in a Spec-Head relation, Agreement takes place and the embedded COMP receives the referential index from the subject trace left there. Consequently, COMP becomes a proper head licenser for the trace in the subject position.

It is time now to address the second question raised in the introduction of this paper, namely, where do WH-Phrases move to? We assumed that WH-phrases are the 'subject' of a predicate created by the operator *que*. Therefore, they move to the specifier position of the phrase headed by *que*. However, in (32a-b) we are dealing with two moved WH-phrases. BP is a language that does not allow for two WH-phrases to surface in the same COMP system, as shown in (33):

- (33) a. *\*Que aluno<sub>1</sub> quando<sub>2</sub> você não sabe [t<sub>1</sub> vai entregar o trabalho t<sub>2</sub> ]?*  
Which student<sub>1</sub> when<sub>2</sub> you do not know t<sub>1</sub> is going to hand in the paper t<sub>2</sub>
- b. *\*Que moça<sub>1</sub> com que rapaz<sub>2</sub> você não sabe [t<sub>1</sub> saiu t<sub>2</sub> ontem ]?*  
Which girl<sub>1</sub> with which boy<sub>2</sub> you do not know t<sub>1</sub> went out t<sub>2</sub> last night

I do not want to discuss in this paper the internal structure of the CP-system. Since we need two specifier positions available for phrases to move to, either we propose that we have CP adjunction or COMP has two specifier positions. So, in sentence (32a) the WH-phrase *quando* also moves to a specifier position within the COMP system. The trace of the adjunct phrase is locally antecedent governed by the WH-phrase *quando*, which stays in the embedded COMP, otherwise ungrammaticality would result. The WH-phrase *quando* being an adjunct, is not theta-marked and does not have a referential index. Although *quando* is in a specifier position of the embedded COMP system, it does not have a referential index to turn COMP into a proper governor for traces. It is the trace of the subject moved WH-phrase that gives its index to COMP turning it into a proper governor for the trace in subject position. This explains the grammaticality of (32a).

However, things may be more complicated with sentence (32b). In (32b) the object WH-phrase surfaces in the Spec position of the embedded COMP and the subject WH-phrase is in the Spec of the main clause. Since the object WH-phrase is referentially theta-marked, it has a referential index which, in turn, can be transferred to the embedded COMP. The question is how is the trace in subject position properly head governed? Probably we could say that the subject extraction in this sentence is a case of long distance binding (in terms of its interpretation) and having a referential index suffices for a COMP to be a proper head licenser of the trace. Rizzi argues against this hypothesis, requiring that Agreement in COMP takes place when the AGR features of COMP are the subject trace features.

However, another route may be pursued. We can keep the assumption that the subject WH-phrase has first moved to the Spec position of the embedded COMP, being in a Spec-Head agreement relation with *que* it gives it its index, and then moves on to the matrix COMP. The object WH-phrase also moves to another Spec position of the embedded COMP system, but cannot transfer its index to the whole COMP, because it already has the subject trace's index. At this point of the investigation I do not have evidence to decide which is the best alternative.

The last case to be treated is the subject extraction out of relative clauses, here represented by our very known sentence:



- (34) [Que animais<sub>1</sub>] [a comida<sub>2</sub> [que t<sub>1</sub> comeram t<sub>2</sub>] estava estragada]?  
 [which animals<sub>1</sub>] [the food<sub>2</sub> [ that t<sub>1</sub> ate t<sub>2</sub> ] was deteriorated ]

If we adopt the raising analysis of relative clauses proposed by Kayne (1994), following Vergnaud, we can say that the WH-phrase *que comida* 'which food' in the object position of the embedded clause, raises to one of the most-embedded specifier positions of Comp and the NP *comida* moves to a specifier position of the quantified phrase introduced by the WH-word *que*. Then the WH-phrase *que animais* moves to the higher specifier position, enters in a predication relation, renders C° a proper governor for the trace, and moves again to the specifier Comp position of the matrix clause. The same proposal made for the cases of extraction out of WH-islands applies here in regard to being the WH-phrase establishing the predication relation with the whole clause the one to transfer the referential features that renders the embedded clause a proper governor for traces. This is why only interrelated eventualities, which in turn allow the establishment of the predication relationship, that generate acceptable sentences.

## 5. WH-Extractions and Relative Clauses

The analysis proposed made a distinction between two processes of relative clause formation. One in which *que* is an operator that transforms sentences into predicates and sits in the Comp position of a CP whose specifier can be occupied by a QP functioning as the subject of the predication. It introduces the kind of clause called pseudo-relative, which appears in existential constructions, WH-extraction constructions and probably, cleft sentences. The pseudo-relative is the prototypical construction showing the strategy used by BP to extract subject WH-phrases, namely, agreement in Comp established through predication.

The other, in which *que* is a WH-word, traditionally treated as a pronoun, occupying the specifier position of a QP and introducing relative clauses as we know them.

Kato (1993) proposes an insightful analysis for relative clauses in BP. She starts reviewing Tarallo's (1983) classical proposal by which BP has three different strategies for relative clause formation: 1) the standard relative, in which the WH-phrase moves from its base position

to a specifier position in the CP system: 2) the resumptive pronoun strategy in which *que* is a complementizer occupying the head position of CP; 3) and the gap-leaving strategy, formed by the ellipsis of the constituent containing the resumptive pronoun. Kato shows that in the three cases raised by Tarallo, the *que* introducing the clause is a relative pronoun. For her, the difference among them resides in the fact that relativization takes place from the left-dislocated position occupied by the relative pronoun in the case of the last two strategies.

The analysis developed in this article shares Tarallo's idea that *que* in relative clauses may occupy either the specifier or the head position of CP. It also shares Kato's idea that a predication relation is established between the relative clause and its head, which is similar to the one observed in left-dislocation constructions.

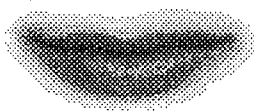
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ON THE NATURE AND LICENSING CONDITIONS  
OF *N*-PHRASES IN PORTUGUESE\*  
(Sobre a Natureza e as Condições de Licenciamento  
de Sintagmas-N em Português)

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*ABSTRACT: The present paper focuses on the syntactic and semantic nature of the expressions identified in the literature as n-words (preferably, n-phrases), and on their licensing conditions. Concerning their status, arguments will be given in favor of Ladusaw's 1992 thesis that these are existential (and non-specific, it is claimed) indefinites. In a brief excursus, it will be shown that other constructs engage in the process known as "negative concord". In the final part, an attempt will be made to offer a systematic picture of the intra- and cross-sentential licensing conditions of classical n-phrases. In this regard, the paramount importance of contextual decreasing monotonicity becomes apparent.*

*KEY WORDS: N-Words, Negative Concord, Indefinites, Decreasing Monotonicity*

*RESUMO: O presente artigo centra-se na questão do estatuto sintático e semântico das expressões que têm sido identificadas na literatura como n-words (preferivelmente, n-phrases, sintagmas-n) e nas condições do seu licenciamento. Sobre o seu estatuto, argumenta-se em favor da tese de Ladusaw 1992 de que se trata de expressões com valor indefinido existencial (e não-específico, acrescenta-se). Num breve excursus, mostra-se que outras estruturas além de sintagmas deste tipo se envolvem no processo conhecido como "concordância negativa". Na parte final, tenta-se sistematizar as condições de licenciamento intrafrásico e transfrásico dos sintagmas-n clássicos, ressaltando a importância das propriedades de monotonia decrescente dos contextos relevantes.*

*PALAVRAS-CHAVE: Palavras-N, Concordância Negativa, Indefinidos, Monotonia Decrescente*

\* Part of this paper was presented at the second Sinn und Bedeutung conference, in Berlin, December, 1997. Another part was contributed to the Colóquio de Sintaxe e Semântica, organized by Mary Kato and Rodolfo Ilari, in May 1998, at the Universidade de Campinas, Brazil. I am most indebted to the organizers of these meetings and to Anastasia Giannakidou, Frans Zwarts, John Robert Ross and Rui Chaves for their feedback on specific points. Of course, I am not willing to share with them the merit of my errors. The research was sponsored by the project PCSH/C/LIN/936/95, funded by the Fundação para a Ciência e a Tecnologia, Portugal.

### Basic features of “*n*-words / *n*-phrases”

The label *n*-word, coined by Laka 1990, identifies a class of expressions found in many languages and basically equivalent to Portuguese nominal and adverbial phrases like *ninguém* (*nobody*), *nenhuma pessoa* (*no person*), *nada* (*nothing*), *nenhuma coisa* (*no thing*), and *nunca* (*never*). In a brief picture, the more commonly acknowledged distinctive features of these expressions in a subgroup of languages that includes Italian, Portuguese and Spanish are revealed by the following data and representations (where, for the sake of perspicuity, restricted quantification is used, with “T” and “M” standing for the predicative restrictors “Turista/Tourist” and “Museu/Museum”, respectively).

- (1) a. Nenhum turista (\*nã)o protestou.  
       ‘no tourist (\*not) protested’  
       b. No tourist protested.  
       c.  $\neg\exists x_T$  [protest- (x)]
- (2) a. O Pedro \*(nã)o visitou nenhum museu.  
       ‘the Pedro \*(not) visited no museum’  
       b. Pedro visited no museum / didn’t visit any museum.  
       c.  $\neg\exists x_M$  [visit- (p. x)]
- (3) a. Nenhum turista (\*nã)o visitou nenhum museu.  
       ‘no tourist (\*not) visited no museum’  
       b. No tourist visited any museum.  
       c.  $\neg\exists x_T \exists y_M$  [visit- (x,y)]

The relevant facts in (1)-(3) and its congruent expansions can be summarized in the following terms: (i) when in pre-verbal position, an *n*-word cannot, on one side, co-occur with an overt (say, propositional) negation operator, and, on the other side, does induce *per se* a negative value (to be specified later) in the sentence, as is clearly visible in formulas (1c) and (3c); (ii) when in post-verbal position, an *n*-word requires the presence of an overt negation operator acting as a licenser, which consequently suggests that post-verbal *n*-words are negative polarity items (somehow misleadingly, since, as is well-known, *n*-words can be licensed in other kinds of contexts); (iii) as shown in (3a), the negative licenser of a post-verbal *n*-word can be a pre-verbal *n*-word, whose

presence precludes the emergence of an overt propositional negation. It should be noticed that the property in (ii) is nothing but the process of **negative concord**, as characterized in Labov 1972. Indeed, what is at issue there is the fact that an expression that in a pre-verbal position has the capacity to convey a negative value can also appear in a configuration where its negative force can be dispensed with, given the presence of the propositional negation operator. This peculiar arrangement raises the idea of “concord”: with some similarity with gender and number concord, where just one marker is vital, the others being mere redundant replicas, in the sequences with a negation operator licensing an *n*-word only one negation value is needed, and, much more importantly, only one can be computed.

In general, the domain of *n*-words is extended in the literature to languages like French, Greek, Hungarian or the Slavic languages, where the pre-verbal occurrences of the expressions at stake require, just like the post-verbal ones, the presence of an overt negation operator (which amounts to, *grosso modo*, saying that, in the structures corresponding to (1a) above, the asterisk would lie outside the parentheses). Obviously, the pertinence of such an extension crucially depends on a clear definition of *n*-word. Furthermore, it can also be argued – e.g. in Acquaviva 1993 (according to Giannakidou 1997) and in Peres 1995/1997 – that a unified cross-linguistic account of the relevant expressions can be formulated, which encompasses the standard English sort of languages.

In the next section, I will try to sketch a hopefully convincing concept of *n*-phrase. Before engaging in that, let me settle a minor, but not negligible, terminological matter: henceforth, I will use the label *n*-phrase instead of *n*-word, considering that the former seems much more in accordance than the latter with the internal composition of the expressions under scrutiny.

## 1. On the syntactic and semantic nature of *n*-phrases

### 1.1. The indefinite theory of *n*-phrases

The exact semantic and syntactic nature of *n*-phrases has been the object of considerable and divergent discussion in the literature. If we restrict ourselves only to the languages of the predominant Romance

type, the different theories on the categorial status of *n*-phrases can be divided in three groups. In a first cluster of theories (including, among others, those sketched in Rizzi 1982, Longobardi 1987 and Dowty 1994), *n*-words are treated as NQ's or indefinites, depending on syntactic properties in the first two authors and on a computation of semantic values of monotonicity in the latter. In a second group (see, e.g. Laka 1990, Ladusaw 1992, Zanuttini 1994, Suñer 1995, and Peres 1995/1997), *n*-words are systematically treated as indefinites, or, from another point of view, as existentially quantified expressions. Finally, in a third group (see e.g. Zanuttini 1991 or Haegeman and Zanuttini 1991), *n*-words are always treated as NQ's whose negative value can be canceled. In the present section, focusing on Portuguese data, I will aim at showing that the indefinite theory of *n*-phrases, besides offering a general and economic account of the constructions at stake, explains a larger number of facts than other theories.

Ladusaw's basic idea of an indefinite theory of *n*-phrases, which I will subsequently argue for, can be summarized in the following terms: "in a NC language in which only one of these expressions [- negative terms -] can express negation in a particular clause, the way is open for proposing that the negative phrases in fact never express negation. In fact, we could propose that they are univocally interpreted as NPI indefinites and that it is not necessary that any visible formative of S-structure actually express negation" (Ladusaw 1992: 251). The answer to the question "how does the negation get expressed and how are these polarity items licensed" is phrased as follows: "The answer (...) must be (...): a negation operator, preferably (anti-morphic) negation. (...) It need not be part of lexical meaning: it may be constructional, in the sense that it is associated with some structural feature not necessarily visible in the clause" (cf. *ib.*: 251-2). In Peres 1995/1997, I proposed an extension of this view of *n*-phrases to languages which, like standard English, only obey the first of the above mentioned two properties, that is, which lack negative concord.

### 1.2. Arguments in favor of the indefinite theory of *n*-phrases

In this subsection, I will bring together several arguments in favor of treatments of *n*-phrases that are basically equivalent to the indefinite theory. Some of these arguments appear in Klima 1964, some in Peres



1995/1977, Peres 1977 and Peres 1978, and a few others are, to my knowledge, now presented for the first time. As for the arguments contra an indefinite theory – or, for that matter, in favor of the (total or partial) negative quantifier approach –, having, I believe, shown the immateriality of the most important of them in my 1995/1997 paper, I should now only pay attention to the *absolutely* argument. However, for absolute time and space constraints, I will not address this issue, just suggesting the idea that a treatment of that sort of expressions as discourse operators on propositions – not on noun phrases – can lead to showing that also this argument fails to prove that *n*-phrases are not indefinites.<sup>1</sup>

### 1.2.1. The paradigms of indefinite *n*-phrases

The paradigmatic relations of *n*-phrases constitute a vantage point for understanding their nature, which, in my view, strongly favors the indefinite analysis, and which, to my knowledge, has not been paid noticeable attention. As we shall promptly see, the relevant paradigms are particularly rich and revealing in a language like Portuguese. To start with, let us observe a basic instance of an *n*-phrase involved in negative concord:

- (4) a. O Paulo não leu nenhum livro.  
       'the Paulo not read<sub>PAST</sub> no book'  
       Paulo [read no book] / [didn't read any book].  
       b.  $\neg \exists x[\textit{book}(x) \wedge \textit{read}(p, x)]$

For the sake of simplicity, I am ignoring the possible so-called (non-specific) “generic” (preferably, “kind”) interpretation of a sequence like (4a), where a natural kind is involved more directly than common individuals. Such reading is more easily available if certain factors are present, like a generic aspectual value or a contrastive context:

<sup>1</sup> The same sort of approach may well be able to overcome difficulties raised by the test with *virtually* that Jack Hoeksema proposed during the Salford conference on negation, in November 1998. I thank him for having subsequently given me further details on this test, that I will not be able to discuss here.

- (5) O Paulo não lê nenhum livro.  
 'the Paulo not reads no book'  
 Paulo doesn't read [a book] / [books].
- (6) O Paulo não leu nenhum livro, só (leu) revistas.  
 'the Paulo not read no book, only (read) magazines'  
 Paulo didn't read any book, just magazines.

If we skip this interpretation and stick to the existential one – which is entailed by the former –, we can say that the sentence in (7a) below, also in its existential interpretation (specific – in the sense of “somehow known to the speaker” – or not, it is now irrelevant), is the contradictory of the existential reading of (4a):

- (7) a. O Paulo leu um livro.  
 'the Paulo read<sub>PAST</sub> a book'  
 Paulo read a book.
- b.  $\exists x [book(x) \wedge read(p, x)]$

The interesting fact to be noticed at this point is that (4a) is only one of a bunch of (*grosso modo*) free variants that express – by intrasentential means; not by some logically legitimate expansion of formulas – the contradictory of (7a). In Portuguese, one of these variants is obtained by assigning the relevant constituent (in the case under scrutiny, an object NP) a prosodic focus, as symbolized by the capital letters in (8):

- (8) O Paulo não leu UM livro (que fosse).  
 'the Paulo not read a book (that be<sub>SUBJ</sub>)'  
 Paulo didn't read any book (whatsoever).

As for the prosodically more neutral form, represented in (9) below, it only has a wide scope (that is, specific, in another sense) reading for the quantifier, and therefore isn't a variant of either (4a) or (8):

- (9) a. O Paulo não leu um livro.  
 b.  $\exists x [book(x) \wedge \neg read(p, x)]$

An alternative version of (8), which preserves the indefinite determiner is

- (10) O Paulo não leu um só / único livro (que fosse).  
Paulo didn't read a single book.

At this point, it becomes evident that *n*-phrases are (possibly among other things) one of the means for expressing the contradiction of existence, in combination with a sentential negation operator. This is what clearly shows the following paradigm of free variants:

- (11) a. O Paulo não leu [um livro] (que fosse).  
b. O Paulo não leu [um só / único livro] (que fosse).  
c. O Paulo não leu [nem um (só / único) livro] (que fosse).  
'the Paulo not read<sub>PAST</sub> [neither a (single) book]'  
d. O Paulo não leu [nenhum livro] (que fosse).  
'the Paulo not read<sub>PAST</sub> [no book]'  
e. O Paulo não leu [livro nenhum].  
'the Paulo not read<sub>PAST</sub> [book no]'

Interestingly – not to say surprisingly – enough, the paradigm in (11) is still incomplete. In fact, the formula in (4b) can be expressed by all the sentences below (possibly with some discourse, not semantic proper, subtleties involved), where the subjunctive appendix is now ignored, for the sake of simplicity:

- (12) a. O Paulo não leu [um livro].  
b. O Paulo não leu [um só / único livro].  
c. O Paulo não leu [nem um (só / único) livro].  
d. O Paulo não leu [nenhum livro].  
e. O Paulo não leu [livro nenhum].  
f. O Paulo não leu [livro algum].  
'the Paulo not read<sub>PAST</sub> book some'  
g. O Paulo não leu [qualquer livro].  
'the Paulo not read<sub>PAST</sub> any book'

If we take into account the crucial correspondence between, on one side, sentence (7a) and its meaning translation in formula (7b),

$\exists x [\textit{book}(x) \wedge \textit{read}(p, x)]$ , and, on the other side, all the sentences in (12) and their meaning translation in formula (4b),  $\neg \exists x [\textit{book}(x) \wedge \textit{read}(p, x)]$ , the contradictory of (7b), the conclusion can be drawn that, in a language like Portuguese and at least in the syntactic context at issue (namely, under the scope of sentential negation), **non-specific existential indefinites** (NSEIs, for short) must be subject to a particular (prosodic or morpho-syntactic) formatting – which results in the distinguished forms (12a-f) – and are in free variation with a *qualquer(any)*-type NP, as shown in (12g). In this perspective, and if only the context presented above is considered, the *n*-phrases under discussion can be taken as just one of the forms that non-specific existential indefinites can assume in such context.

The behavior of existential indefinites I have been describing in connection with a post-verbal object position under the (syntactic and semantic) scope of negation *grosso modo* generalizes to every post-verbal position. In other words, in languages like Portuguese, paradigms like the one given in (12) above are in general valid for any post-verbal argument or adverbial position within a given syntactic domain (and, under certain conditions, not to be specified now, even across sentential boundaries). Nevertheless, in some cases there may arise some blocking effects that prevent all the expressions at stake to be free variants of one another, even in the negative configurations we have been considering. For the sake of simplicity, I will ignore here such possible effects. As for pre-verbal positions, the relevant facts are disclosed by the following pattern, where the asterisks mark either ungrammaticality or the unavailability of the intended meaning of negation of existence:

- (13) a. \*[UM estudante] não respondeu à pergunta.  
 'a student not answered the question'  
 b. \*[Um só / único estudante] não respondeu à pergunta.  
 c. [Nem um (só) estudante] (?não) respondeu à pergunta.  
 d. [Nenhum estudante] (\*não) respondeu à pergunta.  
 e. [Estudante nenhum] (\*não) respondeu à pergunta.<sup>2</sup>

<sup>2</sup> My initial reaction to this sentence was not very favorable. However, I came to recognize that it is acceptable, although undoubtedly much more so if some discourse factors intervene, for example in a rather emphatic statement like

(i) Estudante nenhum responderia a essa pergunta!

No student would (ever be able / want to) answer that question!

- f. [Estudante algum] (\*nã) respondeu à pergunta.
- g. \*[Qualquer estudante] não respondeu à pergunta.

The operative paradigm is hence now reduced to four possibilities:

- (14) a. Nem um estudante respondeu à pergunta.
- b. Nenhum estudante respondeu à pergunta.
- c. Estudante nenhum respondeu à pergunta.
- d. Estudante algum respondeu à pergunta.

Regardless of the idiosyncratic wealth of variants that (14) reveals, the general syntactic pattern, namely as exhibited in (14b), is a well-known one, which consists of a typical form of *n*-phrase occurring pre-verbally. It is common to the traditionally called “negative concord languages”, but also to languages like standard English, usually classified as “double negation languages”. We will return to this distinction in section 2.3 below.

In my view, this is the right ambiance for an initial approach to the *n*-phrases we are now dealing with, insofar as the observed paradigms constitute strong evidence that such expressions belong in the class of non-specific existential indefinites. In the next subsection, further evidence in this direction will be adduced.

### 1.2.2. Klima’s (1964) tests

The first battery of tests that suggests itself as a tool for evaluating the negative character of sentences with pre-verbal *n*-phrases is devised in Klima (1964). All such tests involve subtypes of elliptic constructions that seem to require the presence, in the first member of the dyadic structure, of some sort of negative operator. Putting things in these terms, I am implying not only the old and trivial claim that different sorts of negation are available in a language, but also that, as should be expected, the differences in attachment and scope of negative operators bear upon the viability of certain constructions. However, contrary to my previous contentions on this matter (cf. Peres 1995/1997), I now believe that, while Klima’s tests indicate that some kind of negative value (possibly just decreasing monotonicity in one or more cases) is at work in the

structure, it is not yet clear which. I will not further elaborate on this point, merely suggesting for now, with data presented in (15)-(17), that different levels of negative values may license the constructions at stake:

- (15) a. Os estudantes leram o livro. \*(nã) leram?  
The students read the book. did\*(n't) they?
- b. Os estudantes não leram o livro. (\*nã) leram / pois não (ERR. PORT.)?  
The students didn't read the book. did(\*n't) they?
- c. Nenhum estudante leu o livro. (\*nã) leu / pois não (ERR. PORT.)?  
No student read the book. did(\*n't) they?
- d. Não poucos estudantes leram o livro. (\*nã) leram /  
pois não (ERR. PORT.)?  
'not few students read the book. did they?'
- (16) a. Os estudantes leram o livro. \**nem mesmo* o Rui.  
The students read the book, \*not even Rui.
- b. Os estudantes *não* leram o livro. *nem mesmo* o Rui.  
The students didn't read the book. not even Rui.
- c. *Nenhum estudante* leu o livro. *nem mesmo* o Rui.  
No student read the book. not even Rui.
- d. Poucos estudantes leram o livro. *nem mesmo* o Rui?(leu).  
Few students read the book, not even Rui?(did).
- (17) a. A Vera leu o livro e o Rui também (\*nã).  
'the Vera read the book and the Rui also (\*not)'  
Vera read the book. and so did(\*n't) Rui.
- b. A Vera *não* leu o livro e o Rui também \*(nã).  
'the Vera not read the book and the Rui also \*(not)'  
Vera didn't read the book, neither did Rui.  
Vera didn't read the book, and Rui didn't either.
- c. *Nenhum estudante* leu o livro e o Rui também \*(nã).  
'no student read the book and the Rui also \*(not)'  
No student read the book, neither did Rui / and Rui didn't either.
- d. Poucos estudantes leram o livro e o professor também não?(leu).  
Few students read the book, and the teacher didn't?(read it) either.

### 1.2.3. Arguments in favor of the indefinite Further theory of *n*-phrases

The arguments in favor of the indefinite theory of *n*-phrases that will now be put forward can be divided into two groups: those that

strengthen the idea that a constructional (arguably, contradictory) negation is available in the relevant structures, which are grouped below under (i)-(iii), and those that confirm the (existential) indefinite character of *n*-phrases, which constitute the group (iv)-(vi). I will keep on skipping the indispensable syntactic characterization of the relevant negation operator that is at issue in the present discussion, which, in very vague terms, I conceive of as lying between constituent negation and a fully external propositional negation: “a mode of predication, a recipe for combining subject (...) and predicate (...) to form a proposition or sentence (...), rather than an operation on a fully formed proposition or sentence” (cf. Horn 1989: 469).

(i) Other cases of sensitivity to negation

The constructions given in (18)-(20) point in a more distinct direction than Klima’s tests. Since they will not be described in detail, suffice it to say that they all can be considered as composites of negative propositional components.

- (18) a. Durante este depoimento, os fotógrafos **NÃO** poderão ficar na sala  
NEM as câmaras de televisão poderão filmar.  
During this deposition, the photographers will be allowed to  
remain in the room nor will the TV cameras be allowed to shoot.
- b. Durante este depoimento, **NENHUM** fotógrafo poderá ficar na sala  
NEM as câmaras de televisão poderão filmar.  
[cf. Peres 1995/1997: 292]  
During this deposition, no photographer will be allowed to remain  
in the room nor will the TV cameras be allowed to shoot.
- (19) a. Espero que você **NÃO** se sinta aqui mal, mas **SIM** como na sua  
própria casa.  
'(I) hope that you not yourself feel here badly, but yes as in-the  
your own home'  
I hope you won't feel uncomfortable here, but instead as if you  
were at home.
- b. Espero que **NINGUÉM** se sinta aqui mal, mas **SIM** como na sua  
própria casa.  
'(I) hope that nobody not himself feel here badly, but yes as in-the  
his own home'  
I hope nobody feels uncomfortable here, but instead as if being at  
home.

- (20) a. Eu NÃO disse que estava a chover, (mas) APENAS que podia chover.  
 '(I) not said that was at rain, (but) just that could rain'  
 I didn't say that it was raining, just that it could rain.
- b. NINGUÉM disse que estava a chover, (mas) APENAS que podia chover.  
 'nobody said that was at rain, (but) just that could rain'  
 Nobody said that it was raining, just that it could rain.

(ii) Anaphoric *do the same* constructions

The third source of evidence in favor of the postulation of a negative operator concerns the anaphoric *do the same* construction (cf. Peres 1995/1997: 292), whose interpretation in the relevant cases must include a negation operator.

- (21) Nenhum dos meus colegas aceitou o convite e eu vou *fazer o mesmo*.  
 'none-of-the my colleagues accepted the invitation and I go do the same'  
 None of my colleagues has accepted the invitation, and I will do the same.

(iii) Structures with pre-verbal *n*-phrases licensed by an overt negation operator

The next piece of evidence (again, extracted from Peres 1995/1997) concerns the occurrence of *n*-phrases in pre-verbal position even when immediately preceded by an overt sentential negation operator (*sem / without*) – cf. (22a) below –, which also licenses post-verbal realizations of the same items – cf. (22b). Furthermore, it is shown that when such an operator is neutralized by genuine (adjacent) double negation, yielding a positive context, the licensing is blocked – cf. (23b).

- (22) a. A Ana saiu SEM NINGUÉM a ver.  
 'Ana left without nobody her see'  
 Ana left without anyone seeing her.
- b. A Ana saiu SEM falar com NINGUÉM.  
 'Ana left without talk with nobody'  
 Ana left without talking to anyone.



- (23) a. A Ana saiu. NÃO SEM falar com o Pedro.  
 'Ana left not without talk with the Pedro'  
 Ana left, not without talking to Pedro.
- b. \*Ana saiu. NÃO SEM falar com NINGUÉM.  
 'Ana left not without talk with nobody'

We now move to the evidence in favor of the analysis of *n*-phrases as indefinites.

(iv) Anaphoras of *n*-phrases

The data in this item show that empty constituents or even pronouns whose interpretation is dependent on *n*-phrases can only receive an existential reading (cf. Peres 1997: 2).

- (24) Nenhuma lei proíbe o que estamos a fazer e se [*ec*] proibisse [*ec*] devia ser revogada.  
 'no law forbids the-what [we] are at do and if [*ec*] did [*ec*] should be revoked'  
 No law forbids what we are doing, and if (?it/one) did it should be revoked.
- (25) Não te peço nenhum livro emprestado, porque não sei quando to poderia devolver.  
 '[I] not you ask no book borrowed, because [I] not know when you-it could return'  
 I won't borrow any book from you, because I don't know when I would be able to return it.

(v) *Else*-constructions

The present construction (with, e.g., English *else*, French *plus*, or Portuguese *mais*) is preferably or even exclusively – possibly varying across languages – used with non-universal noun phrases (cf. Peres 1995/1997: 292):

- (26) a. Mais ninguém falou.  
 "more nobody spoke"
- b. Plus personne n'a parlé.  
 "more nobody not has spoken"
- c. No one else spoke.

(vi) *in particular*-constructions

This construction has *grosso modo* the same restrictions as the previous one (cf. Peres 1997: 2). In fact, the adjunct *in particular* can only be computed in combination with an expression conveying an existential value.

(27) They chat about nothing in particular.

(28) Are you looking for someone in particular? No, I am looking for no one in particular.

From all these facts together, the reasonable conclusion to be drawn seems to be that, in languages like Portuguese, both pre- and post-verbal *n*-phrases are, at least in the kind of contexts examined so far, nothing but NSEIs under the scope of (not necessarily visible) negation. When pre-verbal, they are negation inducers which, in an abstract syntax, may be considered to have undergone a process of agreement with a negative head towards which they act as specifiers (cf., e.g., Zanuttini 1991). When post-verbal, they enter a negative concord process with a visible negation operator, which amounts to saying that they keep the same form as when they are negation inducers, but in reality do not perform such role, but only that of NSEIs.

I believe that it was abundantly evidenced that only under the categorial assignment and constructional postulation just discussed can the right semantics be obtained in all the relevant cases, namely in the distinguished structures that were presented above as a diagnostic for the existentiality and indefiniteness of *n*-phrases. In its general features, this is a view that was proposed in Ladusaw 1992 for so-called negative concord languages and generalized to so-called double negation languages, like standard English, in Peres 1995/1997.

### 1.3. A concept of *n*-phrase and some typological matters

From the evidence presented so far, the notion of *n*-phrase, as applying to the Portuguese expressions we have been considering, can be associated with the following features: (i) the marked expression of non-specific existential indefiniteness in special contexts, namely in those

that can be characterized as involving the “mode of predication” sort of negation: (ii) the capacity these marked NSEIs have, when located in at least one distinguished syntactic position (in the language under consideration, a pre-verbal position) and in the absence of any visible sentential negation operator, to make their clausal domain exhibit certain properties that are exclusive of the subtype of negative constructions mentioned in the previous item; (iii) the restraint to the mere role of NSEIs – without any conveyance of a negation value –, when in the appropriate dependency vis-à-vis another appropriate conveyor of negation (in short, the engagement in what Labov termed “negative concord”).

Now, it is just a terminological matter to decide whether or not for an expression to be classified as an *n*-phrase all the above features are required. My position is that the notion will gain in generality and expressive power if only the first two properties are summoned into its definition. In fact, given such a comprehensive outlining, not only languages like Italian, Portuguese and Spanish but also languages like English are endowed with *n*-phrases, a claim that allows a unified treatment of the pre-verbal behavior of the relevant expressions. Clearly, this was (*avant la lettre* in what concerns the labels *n*-word/*n*-phrase and related concepts) the view adopted in Klima 1964, where such expressions were derived in English by means of a rule of “incorporation” of (an abstract) negation (constituent) – “*neg*-incorporation”. Sticking to Klima’s terminology, all the languages in the wider group just mentioned are ***neg*-incorporation languages**. Without further speculation, it must be stressed that, under the definition of *n*-phrases that I have just adopted, languages like Greek, Hungarian and the Slavic languages – and even European French, although somehow more arguably, given that the omnipresent negation operator is a weak form (as opposed to *pas*) – appear not to be *neg*-incorporation languages. The obvious entailment is that the latter are not negative concord languages either. In fact, if the expression “negative concord” is to be taken at its face value, as it should, then an expression can be involved in negative concord if and only if it can also be by itself a conveyor of negation, which, in the cases under analysis, requires that it undergoes a process of the *neg*-incorporation type. From the above enumerated *neg*-incorporation languages, Italian, Portuguese and Spanish are genuine

**NC languages**, while English is not<sup>3</sup>. As for French, Greek, Hungarian and the Slavic languages, all that can rigorously be said is that they possibly have marked NSEIs, namely for negative contexts, but not that they admit negative concord.

From this standpoint, the typological question can be further extended to the following issue: is the markedness of NSEIs in distinguished negative contexts a universal feature of languages or not? According to Bernini and Ramat 1996 the answer is negative, considering the behavior of Basque, two Celtic languages – Irish and Gaelic –, and three from the Finno-Ugric family – Lapp, Finnish and Estonian (cf. p. 182). Furthermore, if the evidence in (29) – from Evenki (“one of the ten Manchu-Tungusic languages”, spoken in Siberia and Northern China see Nedyalkov 1994: 1) – truly corresponds to a general pattern, also this language belongs in the same group, the same being the case with Zazaki (“a language (or network of dialects) spoken in the region of the head waters of the Euphrates river in east-central Anatolia” see pp. 125 ff.), following the description found in Sandonato 1994.

- (29) a. | kun: da                    e-che            {-ra  
           anything: Encl    Neg-Past    become-Ffnlv  
           Nothing happened.
- b. | kun: da            {-ra-n  
           anything: Encl    become-NonFut-3Sg  
           Something happened.                    [cf. Nedyalkov 1994: 25]

## 2. Additional instances of *n*-phrases and/or Negative Concord

In general, the work on negative incorporation and negative concord – or, briefly, on *n*-phrases – has focused on *n*-indefinites, notwithstanding the fact that Klima (1964) had already stressed the parallel between what have come to be known as *n*-words and other kinds of phrases, namely the *neither... nor* sequences. In the present section, I will stress that the field where these processes take place is much wider than the

<sup>3</sup> I am departing from my previous terminological choice, in Peres (1995/1997), according to which the incorporation process establishes a minimal degree of negative concord, exhibited by a very large number of languages, standard English included.

domain of indefinites. Subsequently, I will briefly list what I think to be the types of Portuguese structures that involve such processes, without attempting to establish a rigorous characterization of the material. Pending further research, suffice it to say for the moment that the data below may contain cases of *n*-phrases of the type we have been considering so far – that is, supporting both negative incorporation and negative concord –, but also cases where, the “mode of predication” negation not being involved, negative incorporation has a different morpho-syntactic and semantic architecture, although negative concord may survive as the semantically void concord process we know from the foregoing. Besides Portuguese (PT), occasionally examples are given for some other Romance languages.

### 2.1. Simple phrases other than indefinites

- (30) a. NEM vinte estudantes foram à festa. [PT]  
 ‘not-even twenty students went to-the party’
- a’. NÃO foram à festa (NEM) vinte estudantes. [ambiguous without *nem*]  
 ‘not went to-the party not-even twenty students’
- b. Not even twenty students attended the party.

When definites are involved, the same facts occur, with an additional implicature value:

- (31) a. NEM ao Domingo descanso. [PT]  
 ‘not-even at-the Sunday (I) rest’
- a’. NÃO descanso NEM ao Domingo. [PT]  
 ‘not [I] rest not-even at-the Sunday’
- b. NEANCHE la domenica mi riposo. [IT]  
 ‘not-even the Sunday me rest’
- b’. NON mi riposo NEANCHE la domenica.  
 ‘not me rest not-even the Sunday’
- c. I can’t rest, even on Sundays.

2.2. *Coordinate structures (NP, AP, VP, AdvP, ...)*

## 2.2.1. (Arguably) NP coordination

- (32) a. NEM o Pedro NEM a Ana (\*NÃO) foram à reunião. [PT]  
 'neither the Pedro nor the Ana (\*not) went to-the meeting'  
 a'. \*(NÃO) foram à reunião (NEM) o Pedro NEM a Ana.  
 '\*\*(not) went to-the meeting (neither) the Pedro NOR the Ana'  
 b. Neither Pedro nor Ana attended the meeting.
- (33) a. O Paulo \*(NÃO) viu (NEM) o Pedro NEM a Ana. [PT]  
 'the Paulo \*(not) see (neither) the Pedro nor the Ana'  
 b. Paulo didn't see either Pedro or Ana. / Paulo saw neither Pedro nor Ana.
- (34) a. No conozco (NI) Paris NI Londres. [SP]  
 'not [I] know (neither) Paris nor Londres'  
 b. I don't know either Paris or London. / I know neither Paris nor London.
- (35) a. Vi-me SEM bagagem NEM dinheiro. [PT]  
 '[I] saw-myself without baggage nor money'  
 b. I found myself without baggage or money.

## 2.2.2. (Arguably) VP coordination

- (36) a. NON lo aprovo NÉ lo desaprovo. [IT]  
 a'. Né lo aprovo né lo desaprovo.  
 'not it [I] approve nor disapprove'  
 b. I don't approve or disapprove it.

2.3. *A subtype of conditionals*

- (37) a. NEM que me matem eu conto tudo o que sei.  
 [PT; ambiguous: a' vs. a'']  
 'neither that kill me I tell all the what [I] know'  
 a'. Eu conto tudo o que sei, NEM que me matem.  
 I will tell everything I know, even if they kill me.

- a". Eu NÃO conto tudo o que sei. NEM que me matem.  
I won't tell everything I know. even if they kill me.
- b. Ni aunque me maten contaré todo lo que se. [SP: same ambiguity]
- c. Neanche se mi ammazzano io racconterò tutto ciò che so.  
[IT: idem]

### 3. Back to *n*-indefinites and their licensing conditions

In many languages that have *n*-phrases, these, namely in the indefinite variety, can occur in contexts where a negation value can only be obtained via indirect computations (for instance, interrogatives, comparatives or the antecedent of conditionals). As I tried to show in Peres (1998), Portuguese, contrary to other Romance languages, is in this respect a very restrictive language, which almost exclusively allows indefinite *n*-phrases in contexts where a basic negation operator is appropriately available. In the present section, I will only take into account this kind of licensing, both in single sentential domains and cross-sententially.

#### 3.1. Intra-sentential anti-veridical licensing

All the occurrences of *n*-phrases discussed so far took place in a context where overt or covert sentential negation was present (in the second case, once the indefinite theory of *n*-phrases is assumed). Using the terms of Giannakidou (1997), this amounts to saying that up to now we have only considered the licensing of *n*-phrases in “averidical contexts” (or in the author’s 1998 revised terminology, “anti-veridical”), according to the following definition:

- (38) Let *OP* be a monadic sentential operator. The following statements hold:
- (i) *OP* is veridical just in case  $OP(p) \rightarrow p$  is logically valid. Otherwise, *OP* is nonveridical.
  - (ii) A nonveridical operator *OP* is anti-veridical just in case  $OP(p) \rightarrow \neg p$  is logically valid. [cf. Giannakidou 1998: 106]

Besides the common sentential negation operator, another operator that can be considered anti-veridical is *sem* (and its equivalents in other languages, like English *without*). Accordingly, such operator licenses *n*-phrases (that is, it gives rise to negative concord), as shown by the following data<sup>4</sup>:

- (39) a. O prisioneiro fugiu SEM [que [NEM UM polícia] o conseguisse deter].  
 'the prisoner escaped without that neither a policeman him could detain'  
 The prisoner escaped without any policeman being able to detain him.
- b. O prisioneiro fugiu SEM [que [NENHUM polícia] o conseguisse deter].
- c. O prisioneiro fugiu SEM [que [polícia NENHUM] o conseguisse deter].
- d. O prisioneiro fugiu SEM [que [polícia ALGUM] o conseguisse deter].

As shown by Zwarts (1995) and Giannakidou (1997), in some languages the anti-veridical version of operators like temporal *before* license *n*-phrases and other items that are sensitive to anti-veridicality. French and Spanish examples are:

- (40) Partez avant que personne vous voie. [Grevisse-Goosse 1993: 1461]  
 'leave before that nobody you see'  
 Leave before anyone sees you.
- (41) Antes de decir nada más, piénsatelo dos veces.<sup>5</sup>  
 'before of say nothing else, think-it two times'  
 Before saying anything else, think twice.

<sup>4</sup> In certain contexts, the sequences with *nem* are less easily accepted. This is presumably a syntactic fact, due to the somehow hybrid nature of *nem*, which, having not been collapsed with the determiner (as it is the case in *nenhum* and other forms), may have preserved some of its original character as a sentential operator and, as a consequence, be hardly combinable with *sem*. It is also possible that the reversed order as exhibited in (39c) is not always interchangeable with the other forms in *sem* contexts. I will skip these predominantly syntactic issues.

<sup>5</sup> I owe this sentence to León Acosta.



In Portuguese, this possibility is much less assured:

- (42) O avô morreu antes de conhecer ??*nenhum* / \**algum* / *qualquer* neto.  
'the grandfather died before of know ??no / \*some / any grandchild'  
The grandfather died before knowing any grandchild.
- (43) Sai. antes que ??*ninguém* / *alguém* / *qualquer pessoa* te veja.  
'leave. before that ??nobody / someone / any person you see'  
Leave. before anyone sees you.
- (44) Antes de dizeres \*(*mais*) *nada* / *algo mais* / *mais alguma coisa* /  
*qualquer coisa*, pensa duas vezes.  
'before of say \*(else) nothing / something else / more some thing / any  
thing. think two times'  
Before saying anything else, think twice.

### 3.2. Cross-sentential licensing

Developing the trend open by Ladusaw 1979, where decreasing monotone contexts are the crucial factor in the licensing of negative polarity items, Dowty 1994 propositions the idea that, in so-called negative concord languages, these items, including *n*-phrases, can only occur in positions that are subject to that sort of inferences, which they are aimed at marking. He re-elaborates the logic of monotonicity defined in Valencia (1991) as a tool for calculating monotonicity, and, as mentioned before, adopting an ambiguity (between NPI's and NQ's) treatment of *n*-phrases, he evaluated his theory in the domain of simple sentences. In Peres 1994 and 1995/1997, I tried to reach the following two goals with respect to the licensing of *n*-words: (i) showing that, in a language like Portuguese, Dowty's monotonicity effects were preserved across clause boundaries, that is, in cases where NPI's, *n*-words included, are located within an embedded clause, the licenser being in the matrix; (ii) proving that the indefinite categorization of *n*-words and the invisible negation approach I was adopting, while being strictly opposed to Dowty's ambiguity treatment of *n*-words, was fully compatible with his basic intuition concerning the role of monotonicity not only of negation in the licensing of *n*-words. In face of the limited amount of data I analyzed at the time, the results seemed to confirm Dowty's hypothesis rather straightforwardly. Crucial cases were sentences like

- (45) a. NÃO me surpreende que NINGUÉM tenha telefonado à Maria.  
 'not me surprises that nobody has called the Maria'  
 ≠ It doesn't surprise me that someone has called Maria.
- b. ??Não me surpreende que a Maria tenha telefonado a nenhum dos colegas.  
 'not me surprises that the Maria has called to none of-the colleagues'  
 It doesn't surprise me that Maria has called any of her colleagues.
- (46) a. O ministro NÃO exigiu que NINGUÉM fosse preso.  
 "the minister not required that nobody be arrested"  
 The minister did not require that nobody / anybody be arrested.
- b. O ministro NÃO exigiu que a Polícia prendesse NINGUÉM.  
 "the minister not required that the Police arrested nobody"  
 The minister did not require that the Police arrested anyone.

Laterally, it must be pointed out that the classification of a verb like *surpreender* (*surprise*) as downward entailing, which appears recurrently in the literature, is not accurate, at least if the verb is taken to be veridical (or, more strongly, factive). In fact, as Zwarts (1995) has proven, an operator cannot be both veridical and downward entailing with respect to the same argument position. At any rate, the more accurate characterization of the verb as nonmonotone with respect to its sentential argument does not preclude the final value obtained under negation to be increasing monotonicity, for which reason no ambiguity arises with the pre-verbal instance. Concerning (46), where the inference is decreasing – as a result of the neutralization of the increasing value of the verb by the decreasing value of negation –, such ambiguity surfaces. However, this neutralization does not take place in every case, namely when factivity is present, leading to the conclusion that the negation of an increasing monotone predicate of force does not preserve the direction of the inference, but does not necessarily reverse it. This is the case with the next example, which contains a higher verb that (arguably, leaving apart the vexing controversy on the closure of the complements of positive epistemic predicates under logical consequence) is increasing monotone regarding its complement, and where nonmonotonicity is obtained after the application of negation. In such context, *n*-phrases are not licensed cross-sententially.

- (47) a. *Eu NÃO SABIA que aqui NINGUÉM fala chinês.*  
 'I not knew that here nobody speaks Chinese'  
 ≠ I didn't know that somebody here speaks Chinese.
- b. *\*NÃO SABIA que vocês falavam NENHUMA língua asiática.*  
 'not (I) knew that you spoke no Asian language'

The hypothesis I raised at the time in order to account for the kind of discrepancies I then noticed was that at least one modal feature of verbs – EPISTEMIC – plays a crucial role in the reading selection. The general picture is given in Table 1 (from Peres 1994).

PROPERTIES OF THE PREDICATES	MON↓	MON↑ [- EPISTEMIC]	MON↑ [+ EPISTEMIC]	- MON
LICENSING OF <i>N</i> -PHRASES	no	yes	restricted	varying (w/ a tendency for blocking)

*Table 1. Cross-sentential licensing of n-phrases in Peres 1994*

In defiance of the apparent regularity of this general picture, some problems remained unsolved, namely cases, which I then overlooked, with MON↓ predicates, trivially yielding upward entailment under negation [(-, -) / +], but (at least to some degree) licensing *n*-phrases:

- (48) a. *Eu NUNCA EVITO que ninguém me critique.*  
 'I never avoid that nobody me criticizes'  
 ≡ I don't want to prevent anyone from criticizing me.
- b. *A Ana NÃO EVITA que os colegas leiam NENHUM dos seus poemas.*  
 'Ana not avoids that the colleagues read<sub>STB</sub> none of her poems'  
 ≡ Ana does not prevent her colleagues from reading any of her poems.
- (49) *O Pedro NÃO EVITA falar com NINGUÉM.*  
 'the Pedro not avoids talk with nobody'  
 Pedro doesn't avoid talking with anyone.

- (50) a. O presidente da sessão NÃO IMPEDIRÁ que NINGUÉM se pronuncie.  
 'the chairman of-the session not prevent<sub>prt</sub> that nobody himself  
 intervene'  
 ?≡ The chairman of the session will not prevent anyone from  
 intervening.
- b. ?O presidente da sessão NÃO IMPEDIRÁ que se fale sobre NENHUM  
 assunto.  
 'the chairman of-the session not prevent<sub>prt</sub> that one talk about no  
 subject'  
 ?≡ The chairman will not prevent any subject from being addressed.

Bearing in mind the fact that Giannakidou 1997 found a correspondence between nonveridicality and the licensing of (what she considers to be) *n*-phrases in Greek, the attempt to compute this value instead of decreasing monotonicity seems to be a reasonable trend of research. The *prima facie* evidence is that all the above cases are now solved in a simpler way (assuming a non-standard computation of monotonicity that I will not elaborate on):

in (45), the context is $\text{MON}\uparrow [(-, \emptyset)/+]$ and VERIDICAL	NO LICENSING
in (50), the context is $\text{MON}\uparrow [(-, -)/+]$ and NONVERIDICAL	LICENSING
in (46), the context is $\text{MON}\downarrow [(-, +)/-]$ and NONVERIDICAL	LICENSING
in (47), the context is $\neg\text{MON} [(-, [+ \text{FACTIVITY}])/ \emptyset]$ and VERIDICAL	NO LICENSING

However, a minute scrutiny of further data suggests that the domain resists simple solutions, more factors being involved than those considered both in Table 1 and in Giannakidou's approach. In fact, not only it cannot be said that embedded nonveridical contexts constitute a sufficient condition for the licensing of *n*-phrases, but also the veridical ones are not regular in respect to the expected anti-licensing effect. The first relation, between nonveridicality and licensing, can be abundantly illustrated. The following data portray embedded nonveridical contexts which are also decreasing monotone due to combinations of negation and upward entailing predicates in the relevant argument. It should be noticed that henceforth no mention will be made of the availability of the reading in which (within a framework that incorporates the indefinite approach) the pre-verbal *n*-phrases are being licensed in the lowest clausal domain, since it is irrelevant for the purpose of the argument.

- (51) a. O pianista NÃO gosta que NINGUÉM ocupe as filas da frente.  
'the pianist not likes that nobody occupies the front rows'  
≡ The pianist doesn't like that anybody occupies the front rows.
- b. O pianista NÃO gosta que lhe digam NADA antes dos recitais.<sup>6</sup>  
'the pianist not likes that him say nothing before of-the recitals'  
≡ The pianist doesn't like to be told anything before the recitals.
- (52) a. O Pedro NÃO pediu que NINGUÉM o ajudasse.  
Pedro not asked that nobody him helped.  
≡ Pedro didn't ask that anybody helped him.
- b. O Pedro NÃO PEDIU que a Ana ajudasse NINGUÉM.  
Pedro not asked that the Ana helped nobody'  
≡ Pedro didn't ask Ana to help anyone.
- (53) a. NÃO ACREDITO que NINGUÉM tenha dito isso.  
'(I) not believe that nobody has said that'  
≡ I don't believe that anybody has said that.
- b. NÃO ACREDITO que ele tenha dito isso a NENHUM colega.  
'(I) not believe that he has said that to no colleague'  
≡ I don't believe that he has said that to any of his colleagues.
- (54) a. NÃO ME LEMBRO de NINGUÉM me ter dito isso.  
'not me remember of nobody me have said that'  
≡ I don't remember anyone having said that to me.
- b. NÃO ME LEMBRO de ter escrito NADA sobre esse assunto.  
'not me remember of have written nothing about that subject'  
≡ I don't remember having written anything about that subject.

A clear weakening of the likelihood of licensing originates when nonveridicality combines with the nonmonotonicity of declarative verbs and their kin (for instance, *prove* or *demonstrate*) regarding their complement:

<sup>6</sup> Without further evolvment, let me just note that (non)veridicality cannot be computed only from predicates and negation. In fact, values like modality, tense and aspect can also play a crucial role. For instance, if in (51), with the erotetic verb *gostar* (*like*), the tense/aspect value is changed to 'past tense' (*grosso modo*, the Portuguese 'pretérito perfeito simples'), the cross-sentential licensing effect is blocked:

O pianista NÃO gostou que NINGUÉM ocupasse as filas da frente.  
'the pianist not liked that nobody occupied<sub>non</sub> the front rows'  
≠ The pianist didn't like that (??)anybody occupied the front rows.  
The pianist didn't like that nobody occupied the front rows.

- (55) a. A Maria NÃO VAI DIZER que NINGUÉM a ajudou.  
 'the Maria not goes say that nobody her helped'  
 ??≡ Maria is not going to say that someone helped her.
- b. ??A Maria NÃO VAI DIZER que ajudou NINGUÉM.  
 'the Maria not goes say that (she) helped nobody'  
 ??≡ Maria is not going to say that she helped someone.

The same, even more dramatically, is the case when nonveridicality is associated with increasing monotonicity, as was the case in (48) and (50), and, in addition, the higher verb can be considered epistemic (even though with some alethic flavor to it):

- (56) a. NÃO É IMPOSSÍVEL que NINGUÉM fale sobre a questão.  
 'not is impossible that nobody speaks about the issue'  
 ≠ It is not impossible that anyone speaks about the issue.
- b. \*NÃO É IMPOSSÍVEL que a Ana fale com NINGUÉM.  
 'not is impossible that the Ana speak<sub>SP1</sub> with nobody'
- (57) a. NÃO É IMPOSSÍVEL NINGUÉM falar sobre a questão.  
 'not is impossible nobody speak about the issue'  
 ≠ It is not impossible that anyone speaks about the issue.
- b. \*NÃO É IMPOSSÍVEL a Ana falar com NINGUÉM.  
 'not is impossible the Ana speak with nobody'

By the way, notice that in metalinguistic realizations of negative sentences, the licensing of *n*-phrases is almost invariably admitted. For instance, sentence (55a) above is perfectly sound with cross-sentential licensing – that is, with the meaning corresponding to the English translation in (55) – if it is used metalinguistically, as in the following dialogue:

- (58) Sp<sub>1</sub> A Ana vai dizer que alguém a ajudou.  
 'the Ana goes say that someone her helped'  
 Ana is going to say that someone helped her.
- Sp<sub>2</sub> A Ana NÃO dirá que NINGUÉM a ajudou: é demasiado orgulhosa para isso.  
 'Ana not say<sub>SP1</sub> that nobody her helped: (she) is too proud for that'  
 Ana will not say that someone helped her: she is too proud to do that.

The same goes for post-verbal positions:

(59) Sp<sub>1</sub> A Ana disse que tinha escrito um artigo.

'Ana said that (she) had written an article'

Sp<sub>2</sub> A Ana NÃO disse que tinha escrito NENHUM artigo, apenas disse que pensava escrever.

'Ana not said that (she) had written no article, just (she) said that (she) intended write'

Ana didn't say she had written any article, she just said she intended to.

What apparently we have here is a direct negative echo of the initial statement, the negative concord effect serving to stress the relation of contradiction. Given this circumstance, I have so far systematically ignored the licensing effects that depend on metalinguistic use, and will continue to do so.

If we now move to the domain of veridicality, where of necessity decreasing mono-tonicity cannot obtain (again, cf. Zwarts 1995), things are somewhat more subtle. My basic hypothesis is that nonveridicality, besides not being a sufficient condition for licensing, is not a necessary one either. In fact, as we will promptly see, the data permit the conclusion that the licensing can take place if the lack of a strong semantic condition as nonveridicality is, as it were, compensated by strong syntactic conditions, in particular the nonfiniteness of the embedded clause and the post-verbal position of the relevant indefinite. Additionally, again some accessory role may be played by further semantic properties of the matrix verb, namely the epistemic value.

The first cases of veridicality to be considered are of the kind of (47) concerning monotonicity, but now with a non-epistemic predicate:

(60) a. NÃO FOI FÁCIL NINGUÉM aceitar o convite.

'not was easy nobody accept the invitation'

≠ It wasn't easy for anyone to accept the invitation.

b. NÃO FOI FÁCIL subir a NENHUMA daquelas montanhas.

'not was easy climb at none-of-those mountains'

≡ It wasn't easy to climb any of those mountains.

In (61)-(62), we have an epistemic and a declarative predicate that can be considered decreasing monotone in its internal argument:

- (61) a. NÃO DUVIDO de que NINGUÉM votará a favor da proposta A.  
 'not (I) doubt of that nobody will-vote to favor of-the proposal A'  
 ≠ I don't doubt that someone will vote for proposal A.
- b. \*NÃO DUVIDO de que o Pedro contou NENHUM dos seus problemas à Ana.  
 'not (I) doubt of that the Pedro told none of-the his problems to-the Ana'
- (62) a. Ele NÃO NEGOU que NINGUÉM o tinha autorizado a entrar.  
 'he not denied that nobody him had authorized at get-in'  
 ≠ He didn't deny that someone had authorized him to get in.
- b. \*Ele NÃO NEGOU que tinha autorizado NINGUÉM a entrar.  
 'he not denied that had authorized nobody to get-in'  
 ≠ He didn't deny that he had authorized someone to get in.

The next data exemplifying veridicality, in (63)-(64), involve a verb that arguably is nonmonotone regarding its complement:

- (63) a. Ele NÃO SE ESQUECEU de que NINGUÉM o convidou.  
 'he not himself forgot of that nobody him invited'  
 ≠ He didn't forget that someone invited him.
- b. \*Ele NÃO SE ESQUECEU de que tinha de convidar NINGUÉM.  
 'he not himself forgot of that (he) had of invite nobody'
- (64) Ele NÃO SE ESQUECEU de convidar NINGUÉM.  
 'he not himself forgot of invite nobody'  
 ≡ He didn't forget to invite anyone.



	VERID.	MON.	EPIST. OR DECL.	NON- FINITE	POST- VERBAL	LICENSING	
						YES	NO
46a	-	↓	-	-	-	x	
46b	-	↓	-	-	+	x	
51a	-	↓	-	-	-	x	
51b	-	↓	-	-	+	x	
52a	-	↓	-	-	-	x	
52b	-	↓	-	-	+	x	
53a	-	↓	+	-	-	x	
53b	-	↓	+	-	+	x	
54a	-	↓	+	+	-	x	
54b	-	↓	+	+	+	x	
48a	-	↑	-	-	-	x	
48b	-	↑	-	-	+	x	
49	-	↑	-	+	+	x	
50a	-	↑	-	-	-	?	
50b	-	↑	-	-	+	?	
55a	-	NULL	+	-	-		??
55b	-	NULL	+	-	+		??
56a	-	↑	+	-	-		x
56b	-	↑	+	+	+		x
57a	-	↑	+	+	-		x
57b	-	↑	+	+	+		x
47a	+	NULL	+	-	-		x
47b	+	NULL	+	-	+		x
60a	+	NULL	-	+	-		x
60b	+	NULL	-	+	+	x	
45a	+	↑	+	-	-		x
45b	+	↑	+	-	+		x
61a	+	↑	+	-	-		x
61b	+	↑	+	-	+		x
62a	+	↑	+	-	-		x
62b	+	↑	+	-	+		x
63a	+	↑	+	-	-		x
63b	+	↑	+	-	+		x
64	+	↑	+	+	+	x	
65a	+	↑	-	-	-		x
65b	+	↑	-	-	+	?	
66	+	↑	-	+	+	x	

Table 2. Semantic and syntactic relevant values of the data

Finally, in (65)-(66), the predicate that matters – *lamentar* (*regret*) – is nonepistemic and nondeclarative. As for monotonicity, the situation is the same as with *surpreender* (*surprise*; cf. (45) above): the verb is nonmonotone in its complement, the inferential output under negation being increasing monotonicity:

- (65) a. A Maria NÃO LAMENTOU que NINGUÉM a tivesse ultrapassado.  
 ‘the Maria not regretted that nobody her had passed’  
 ≠ Maria didn’t regret that someone had passed her.
- b. A Maria NÃO LAMENTA que o Pedro tenha escrito NENHUM destes artigos.  
 ‘the Maria not regrets that the Pedro has written none of-these articles’  
 ?≡ Maria doesn’t regret that Pedro has written any of these articles.
- (66) A Maria NÃO LAMENTA ter escrito NENHUM destes artigos.  
 ‘the Maria not regrets have written none of-these articles’  
 ≡ Maria doesn’t regret having written any of these articles.

The whole array of features and values considered in the observed examples is given in Table 2, where the monotonicity values are the result of combining the downward monotonicity of the negation operator with the value the predicate exhibits and, additionally, considering factivity. The reading of Table 2 is quite linear, with three facts about Portuguese imposing themselves: (i) only decreasing monotonicity of the embedding position under negation is a sufficient (but not necessary) condition for the cross-sentential licensing of *n*-phrases; (ii) nonveridicality is neither a sufficient nor a necessary condition to that effect; (iii) *n*-phrases are not anti-licensed by veridicality; (iv) under increasing monotonicity, the licensing factors consist of rather restrained combinations of values in semantic and syntactic features (namely, in the latter domain, nonfiniteness of the embedded clause and the post-verbal position of the affected indefinite).

Table 3 constitutes one of several possible compressed views of the information contained in Table 2. The shaded areas express the apparent insignificance of the values at stake. It should be noticed that the cluster of values “OK/?” can easily be split, as Table 2 clearly shows.

	VERID.	MON.	EPIST.	NON-FINITE	POST-VERBAL	LICENSING
46, 51, 52, 53, 54		↓				OK
48, 49, 50	-		-			OK/?
60b, 65b, 66	+		-		+	OK/?
64	+		+	+	+	?

*Table 3. A condensed view of the semantic and syntactic relevant conditions for the licensing of n-phrases according to the examined data*

Let me try a brief and informal account of the findings we have been discussing in this section. In the first place, it seems to be the case that, in consonance with a basic idea found in Dowty 1994, which was later explored in my 1994 and 1995/1997 papers, in a language like Portuguese the computation of monotonicity values plays a prominent role in the cross-sentential licensing of *n*-phrases, for which decreasing monotonicity is a sufficient condition. In the second place, it seems that such kind of language is crucially sensitive to the modal character of the higher predicate, namely to whether it has an epistemic or declarative component. In a manner that demands explanation well beyond the limits of this study, these modal values tend to block the licensing (or, in other words, the concord) process. Interestingly, as Marques 1995 revealed, the same modal values appear to be anti-licensors of the subjunctive mood in languages like French, Italian, Portuguese and Spanish, contrary to what happens in languages like Greek, Hungarian and Rumanian, where the same mood value allegedly is anti-licensed by veridicality.

In merely intuitive terms, what appears to be the case in the licensing of *n*-phrases in languages like Portuguese is that, in the absence of a value of decreasing monotonicity in the embedding context, with which the indefinite would be prepared to engage in a process of (negative) concord (of downward monotonicities) – which ultimately is what the licensing at stake amounts to –, it is nonveridicality that, by inducing a virtual negative value in the lower sentence (say, to the extent that no assurance is given about its truth), as it were supplies a (semantic) concordant element for the negative concord process. However, this

operation can only take place given certain modal properties of the matrix predicate. On the other side, the matrix negation can by itself, even in the absence of nonveridicality in the complement clause, be responsible for the licensing effect, provided that the syntactic conditions already discussed above apply.

A second point that must be stressed is that the above dependencies reinforce another of Dowty's basic intuitions, namely that *n*-phrases are markers of downward entailing positions. Indeed, in all the data under scrutiny that involve *n*-phrase licensing, the position of the indefinite is invariably a downward entailing position. The crucial licensing examples to be checked are those where the monotonicity computation à la Dowty yields a positive value, which is the case in (48)-(50). I will exemplify this verification with one of the relevant predicates:

- (67) O Pedro não evita falar com nenhum colega.  
 'the Pedro not avoids talk with no colleague'  
 Pedro doesn't avoid talking with any colleague.  
 → O Pedro não evita falar com nenhum colega mais novo.  
 'the Pedro not avoids talk with no colleague more young'  
 Pedro doesn't avoid talking with any younger colleague.

What, in my view, this entailment reveals is that, regardless of the computation of mono-tonicity in terms of Valencia-Dowty's calculus, down to the position of clause embedding (resulting from combining the values of the negative operator and that of the higher predicate), the increasing monotonicity of the indefinite can be reversed by the matrix clause negation operator, if the (preferably post-verbal) position of the indefinite becomes transparent to that operator, by a sort of clause-union. Nevertheless, as we have seen, the possibility exists that a post-verbal indefinite in a finite clause be accessible to the negation in the matrix. Regarding this latter case, although one can intuitively perceive that it makes all sense that such access is allowed only under a non-epistemic verb in the matrix, as is the case in (65b), the reasons why this might be so will have to be the object of more extended and sophisticated investigation.

#### 4. Conclusion

The main focus of this paper was the nature of *n*-phrases and their licensing conditions in Portuguese. In the first two sections, I tried to reach a clear definition of this class of expressions and of the related notions of “incorporation of negation” and “negative concord”. The general purpose was to show that a theory like Ladusaw’s indefinite theory of *n*-phrases fits the facts about these expressions, namely in Portuguese, better than concurrent theories. Working in this direction, I discussed several tests for indefiniteness. Moreover, close attention was paid to the paradigmatic relations of *n*-phrases, which were taken as a major source of evidence for a categorial settlement. In section 3, data were presented that clearly show that the characteristics of *n*-phrases that have to do with negation are manifest in several constructions of Portuguese and other languages which cannot be considered as simple noun or adverbial phrases with indefinite value. In the final part of the paper, I aimed at showing that the cross-sentential licensing of *n*-phrases in Portuguese – and presumably in several other languages – is subject to a rather well-defined network of semantic and syntactic properties, where, in partial confirmation of Dowty’s 1994 hypothesis, the role of decreasing monotonicity is of paramount importance.

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**SOME REMARKS ON THE DIACHRONY OF FRENCH NEGATION**  
(Algumas Observações sobre a Diacronia  
da Negação em Francês)

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*ABSTRACT: Based on the history of negation in French, this paper proposes a parametrization of how languages may morphologically express the logical structure of negation provided by UG. It is argued that the change of French negation is driven by a general economy principle governing language acquisition, which favors shorter chains over longer ones. It is also claimed that this change correlates with a change in the determiner system which leads negative chains to develop from indefinite chains*

*KEY WORDS: French Negation, Syntactic Change, Determiner System, Economy*

*RESUMO: A partir da história da negação em francês, este artigo propõe uma parametrização da maneira como as línguas expressam morfologicamente a estrutura lógica da negação produzida pela Gramática Universal. Argumenta-se que a mudança na negação em francês é provocada por um princípio de economia regendo a aquisição da linguagem que favorece cadeias mais curtas em detrimento de cadeias mais longas. Propõe-se também que essa mudança está relacionada com uma mudança no sistema dos determinantes, que leva as cadeias negativas a se desenvolverem a partir das cadeias indefinidas.*

*PALAVRAS-CHAVE: Negação em Francês, Mudança Sintática, Sistema de Determinantes, Economia*

## 1. Introduction

The purpose of this paper is to draw attention to a fairly well-known aspect of the history of French, and attempt to account for it in terms of straightforward and independently-motivated assumptions about the relationship between logical representations and morphophonological expressions. The phenomenon concerns the development of certain aspects of the French negation system, essentially the fact that negation

in Modern French is typically marked by two elements, a preverbal clitic *ne* in combination with a postverbal substantive negation:<sup>1</sup>

- (1) a. Jean n'a pas vu Marie.  
 b. Jean n'a rien fait.  
 c. Jean n'a jamais dit cela.  
 d. Jean n'a aucun espoir de gagner.  
 e. Jean n'a vu personne.  
 f. Jean n'a plus d'argent.

The Old French (OF) system was quite different to this, as we shall see below. So, our investigation of the history of French must account in a natural way for the changes; in doing this, then, we are essentially proposing at least a partial account of the mode of cross-linguistic (and therefore diachronic) variation in the relation between the logical and the morphological.

The paper is organised as follows: in Section 2 we introduce our assumptions about the representation of negation in general, comparing English, French and Italian. The presentation adopts and adapts recent ideas of Déprez (1997), Haegeman (1995) and Zanuttini (1997). Section 3 presents the OF data and an analysis. The analysis relies on ideas about language change proposed elsewhere (Clark & Roberts (1993), Roberts (1993, to appear), Roberts & Roussou (to appear)).

## 2. Negation Synchronically

My approach follows that of Haegeman (1995) in relying extensively on the notion of negative-chain (although it differs from Haegeman's in various ways, as will become apparent below). I define chains as follows (see Manzini (1995)):

<sup>1</sup> The "substantive" negation is postverbal only in finite clauses. It precedes a (main-verb) infinitive. I attribute this to the operation of verb-movement in finite clauses – see Pollock (1989).

- (2) a.  $(\alpha, \beta)$  is a well-formed chain iff:
- i.  $\alpha$  asymmetrically c-commands  $\beta$ :
  - ii. There is some feature F such that  $\alpha$  and  $\beta$  share F:
  - iii. There is no  $\psi$  such that  $\psi$  asymmetrically c-commands  $\beta$  but not  $\alpha$ .
- b. If  $(\alpha_1 \dots \alpha_n)$  is a well-formed chain and  $(\beta_1 \dots \beta_m)$  is a well-formed chain and  $(\alpha_n, \beta_1)$  satisfies (2a) then  $(\alpha_1 \dots \beta_m)$  is a chain.

In a neg-chain, F is the feature [Neg]. For present purposes, I read (2a,iii) as a relativised condition, i.e. for  $\psi$  to break the chain,  $\psi$  must be specified as [Neg], and not a member of the chain.<sup>2</sup>

My approach differs from Haegeman's, however, in that I follow Déprez (who follows Ladusaw (1992)) in taking negative elements to be indefinites and in taking the cross-linguistic variation in the morphosyntactic realisation of negation to be connected to intrinsic properties of negative words and negative operators, rather than to the position and the structure of NegP.

Before presenting a brief sketch of an analysis of English, it is necessary to introduce some general assumptions. First, I define sentential negation as follows (cf. Acquaviva (1996: 298)):

- (3) Sentential negation = closure of the temporal variable by a negated existential.

The existential in question I take to be introduced as a facet of the temporal representation. I take a sentence like (4) to have the temporal representation in (5):

- (4) John left.  
 (5)  $\exists t[t > t_0]$  (AT (e,t) & leave (e,j))

<sup>2</sup> A number of technical points arise here, which I will gloss over. The intuition behind the locality condition in (2a,iii) is that like intervenes in chain-formation for like; this is of course the idea behind Relativised Minimality (see Rizzi (1990, Chapter 3)). However, one can classify features such that members of given class act as interveners for other members of the same class (again, Rizzi (1990) does this): a simple way to do this is to state that [F] is an intervener for both [+F] and [-F]. I have avoided this in the text presentation, partly for simplicity and partly because it plays no real role in the case of negation. Negation seems to be a monovalent feature: I know of no reason to posit [+Neg] and [-Neg]. (2a,iii) is of course the Minimal Link Condition of Chomsky (1995).

(5) states that there is a time  $t$ , which precedes the speech time  $t_0$ , and the event of John leaving took place at  $t$ . In neo-Reichenbachian terms we can take  $T$  to provide the Reference Time which gives the restriction on the temporal quantifier. A naive first approximation is thus that the Past feature of  $T$  is interpreted as  $[t > t_0]$  (cf. Stowell (1998)). The Speech Time is contextually given, presumably via  $C$ , as in Eng's (1987) theory of  $T$ -anchoring. The temporal quantifier is associated with  $C$ .  $AT$  may correspond to an aspectual head (giving a punctual reading here, as opposed to other aspectual operators one may imagine).  $AT$  relates the Event Time to the Speech Time and the Reference Time: the Event Time is an argument of the predicate, cf. Higginbotham (1985).

(5) relates to the structure of (4) as follows:

(6)  $\exists t[t > t_0]$  (AT (e,t) & leave (e,j))  
       C   T    Asp            V

The existential in  $C$  is thus what is negated, giving rise to sentential negation. This implies that sentential negation must be interpreted as having scope over  $C$ , a matter which has a number of implications which I will leave aside here. Since negation is not realised in  $C$  in all languages (although it might be in some), this implies that scope properties cannot be directly read off morphosyntax. This is of course an entirely standard observation.

The negation is realised by a negative morpheme associated with  $T$  or  $C$ .  $T$  and  $C$  together form a chain (this is necessary for the interpretation in (5) to be derived from the structure of (4)). Negated arguments are realised by a ( $T$ ,  $D$ ) chain, interpreted as "for no  $x \dots x$ ". The negation is given along the lines to be described directly. The variable is supplied by the negated argument; I take this to imply that negated arguments are indefinites, i.e. weak DPs in the sense of Milsark (1974). The quantification is supplied by the chain ( $C$ ,  $T$ ) (recall that the existential is structurally located in  $C$ ) and the quantifier-variable relationship is established by the ( $T$ ,  $D$ ) chain.

Languages differ along lines determined by the parametrisation operator, which assigns a diacritic (written \*) randomly to the functional features in the lexicon. The diacritic forces the morphophonological realisation of functional material (by Move or Merge). This idea is developed in full in Roberts & Roussou (1997).

Following a standard paradigm for work on language change in generative grammar initiated by Lightfoot (1979), I assume that change is initiated when (a population of) language acquirers converge on a grammatical system which differs in at least one parameter value from the system internalised by the speakers whose linguistic behaviour provides the input to those acquirers. As the younger generation replaces the older one, the change is carried through the speech community (subject to the vicissitudes of history). More specifically, Clark & Roberts (1993, – C&R henceforth) propose that the parameter-setting device (i.e. the language acquirer) is computationally conservative, in that there is a built-in preference for relatively simple representations. For present purposes, this implies that “shorter”, non-composed chains are preferred “longer”, composed ones. Put more technically,  $CH = (F, G)$  will be preferred, other things being equal, over  $CH = ((F,G), G,H)$ . This idea plays a central role in our discussion of the development of the French negative words illustrated in (1).

Finally, I propose the following recoverability condition on chains:

(7) Recoverability of chains:

$CH = (\alpha_1 \dots \alpha_n)$  with  $\alpha_i = F^*$  is a well-formed chain iff:

- (i)  $[\alpha_i F^*]$  which asymmetrically c-commands all  $[\alpha_{j>i} G^*]$ ;
- (ii)  $CH$  is interpreted as an F-chain;
- (iii)  $G^*$  is interpretatively identified with  $F^*$ .

(7iii) implies that if  $G^*$  is not interpretatively identified with  $F^*$ , then  $G^*$  heads its own chain, i.e. it breaks the F-chain, following (2a,iii). (7) requires all neg-chains to have an overt negative morpheme in the structurally highest phonologically realised position ((7i,ii)), and all other overt negative morphemes in the chain to be interpreted as part of a single negation ((7iii) = (2a,ii)). For the simple case of a single negated argument, the (T, D) chain required for negation then has three possible realisations:

- (8) a. (T\*,D)
- b. (T, D\*)
- c. (T\*, D\*)

I assume that the chain (T, D), i.e. where neither T nor D is realised by an overt negative morpheme, is ruled as a negative chain, since

negation, as an inherently marked property in relation to positive polarity, must be morphologically marked in order for the chain to be identified as negative. The other possibilities are instantiated parametrically in various languages, as I will now describe.<sup>3</sup>

### 2.1. English

In this section, I briefly describe salient points of English negation. The purpose of this is not to give anything like an exhaustive treatment of the situation in English, but rather to illustrate some of the mechanisms introduced in the previous section.

The basic paradigm regarding the expression of negation in English is summarised by the following examples:

- (9) a. I saw nothing.  
 b. I didn't see anything.  
 c. I didn't see nothing.  
 d. \*I saw anything.

In (9a), negation is not realised on T but on D – this is thus a case of (8b): (T, D\*). Note that we can fairly straightforwardly take *no* to be a D, with *thing* the NP complement. The impossibility of DPs like *\*no a man*, *\*no the man*, *\*no many linguists*, *\*no every man*, *\*no no man* supports treating *no* as a D (but see Acquaviva (1995) for a different interpretation of this fact). Note that the *no*-words of English are weak in Milsark's sense (and in Barwise & Cooper's (1981) sense, cf. (10b)):

<sup>3</sup> I understand the notion of markedness along the lines sketched by Cinque (who takes it from Jakobson (see Cinque (1997:214)) in regarding unmarked values as, in a sense, underspecified. What is needed is a feature hierarchy. Functional heads, as features F, G, H ..., can come with various further feature specifications f, g, h ... (I will write the subfeatures with lower case and potentially autonomous functional features with upper case). We can then treat unmarked values of functional heads as simply the autonomous functional feature F, while the marked value will have a further subfeature, giving F+f. In the case in point, T is the autonomous functional feature, and f=neg is a marked subfeature. We can then add to (7) the requirement that marked subfeatures must be identified. An unmarked feature (e.g. positive polarity) can be entirely absent from the representation, but will be "read in" at LF by convention. On the other hand, the marked feature has to be syntactically present and recoverable in the sense of (7) in order to be interpreted. See also Giorgi & Pianesi (1998) for similar ideas.

- (10) a. There is noone in the garden.  
 b. Nothing is a thing.

(9a) thus receives the interpretation “There is no  $x$ ,  $x$  a thing, such that I saw  $x$ ”. I take no view on how the scopal properties of this or other operators are determined (or on more complex and ambiguous cases like Klima (1964)’s *I will force you to marry noone* – cf. Kayne (1998) for a recent treatment).<sup>4</sup> The NP-denotation provides the restriction on the quantifier denoted by the (T, D) chain. So (9a) is interpreted as follows:

$$(9a') \quad \neg \exists x [\text{thing}(x)] \exists t [\text{PAST}(t)] \text{ (AT (e.t) \& see (e.l.x))}$$

D            NP            T-chain

Both existentials are introduced under closure: in fact we can assume that a single operator in C unselectively binds both variables, thanks to composition of the (T, D) chain and the T-chain). I will return to this point in the next section.

In (9b), clausal negation is overtly realised as *n't*.<sup>5</sup> This element combines with *anything* to form a single negation. So here we have the chain (T, D), where T is *n't* and D is *any*. Clearly, *n't* morphologically realises the negative property of the chain on T, in conformity with (7), i.e. this is an example of (8a): (T\*, D). *Any*, on the other hand, is not intrinsically negative, as its appearance in non-negative environments shows (e.g. *Did you see anyone? etc.*). It is clear that any-DPs are weak:

- (11) There isn't anyone in the garden.

The interpretation of (9b) is illustrated in (12):

<sup>4</sup> It is natural to contemplate a QR-like operation, but see Kayne (1998). Note that linking the interpretation of negative arguments to T-chains makes possible an account based on “restructuring” phenomena if these are seen as involving extended T-chains as in Roberts (1997). Kayne (1998) also notes a connection between scope of negation and clitic-climbing, cf. also Déprez (1997).

<sup>5</sup> A full consideration of the position and nature of *noth't*, negative contraction and the mechanism triggering *do*-support would go beyond the scope of this paper. See Roberts (1998) for some proposals.

- (12)  $\neg\exists x$  [thing(x)]  $\exists t$ [PAST(t)] (AT (e,t) & see (c.I.x))  
 n't DP/NP T-chain

*n't* combines with *anything* to form a single negation. So here we have the chain (T, D), where T is *n't* and D is *any*. *n't* morphologically realises the negative property of the chain on T, in conformity with (7). As with *nothing*, the NP *thing* gives the restriction, the D provides the variable, the (T, D) chain the quantifier-variable relation. And so (9b) is interpreted as "There is no x such that I saw x", like (9a).

(9c) can only be interpreted as double negation (in Standard English). This is because both *no* and *not/n't* introduce negation. Because of this, where *not/n't* is realised higher in the chain, *no* cannot be interpreted (see (2a,iii)/(7iii)). But *no* must be interpreted as negative, hence, by (2a,iii)/(7iii), it must head a separate neg-chain, hence double negation.<sup>6</sup>

Finally, (9d) is an example where the licensing condition on *any* is not met. *Any* must form a chain with a downward-entailing operator, such as negation ((9b)), Q, *if*, the restrictive clause of *every* or the comparative operator. If *any* does not form such a chain, it simply cannot be interpreted.

So, we see that English allows negation to be realised either on T or on D. In Standard English, the true negative D *no* is inherently negative, and as such incapable of forming a chain with a c-commanding negative element. For this reason, Standard English lacks negative concord. On the other hand, *any* requires an operator to license it, one of the possible licensors being clausal negation. We see how different lexical items instantiate the possibilities for realising negation listed in (8), and how these interact with the well-formedness conditions on chains given in (2) and (7).

<sup>6</sup> Varieties of English where (9c) is grammatical are those where *no* does not have to be interpreted as negative, and hence the *no*-DP can join the neg-chain and a single-negation interpretation results. In terms of the idea to be proposed in (15) below, we must treat such dialects as having the noun *nothing* rather the determiner *no* and the noun *thing*. It may be significant in this connection that the word for "nothing" in many non-standard varieties of English corresponds to *nought* (*nowt* /naʊt/ in Northern British English (cf. Yorkshire "Eat all and say nowt, sup all and pay nowt")).



## 2:2. Romance N-words and negative concord

As is well-known, the modern Romance languages show negative concord (NC). The Standard Italian paradigm is illustrated in (13):

- (13) a. Non ho visto nessuno.  
 b. Nessuno (\*non) mi ha visto.  
 c. Non mi ha visto nessuno.  
 d. \*Ho visto nessuno.  
 e. Non ho detto niente a nessuno.

In (13a), the N-word *nessuno* functions like polarity *any*.<sup>7</sup> It forms a chain with *non*, which is part of the T-chain, giving CH = (T\*, D\*), i.e. (8c). Again, the DP provides the variable, and the (T, D) chain provides both the quantifier and the quantifier-variable relation.

In (13b), *nessuno* identifies the chain as a neg-chain. Since it c-commands T (being in the subject position SpecAgrSP or SpecTP), *non* cannot appear. *Non* cannot appear for exactly the reason that *no* cannot appear in the English example (9c): it must be interpreted as introducing its own negation and cannot do so when it is c-commanded by a Neg\* element (the D of *nessuno*). *Nessuno*, on the other hand, as (13a) shows, is not obligatorily interpreted as introducing its own negation, although it must be so interpreted when it heads a chain, following (7i) (this is also why it is interpreted as negative in isolation – cf. Zanuttini (1991, 1997)).

In (13c), on the other hand, the “freely inverted” subject does not c-command T; instead, *non* appears as the head of the chain and *nessuno* appears lower in the chain, as in (13a).

Putting these observations together with what we saw in the previous section, we have:

<sup>7</sup> In fact, non-negative *nessuno* can appear in other polarity-licensing contexts, reinforcing the similarity with *any* (Rizzi (1982:122)):

- (i) Mi chiedo se Gianni abbia contattato nessuno.  
 (ii) Mi chiedo se nessuno abbia contattato Gianni.

As these examples show, there is no subject-object asymmetry here. It is clear that +Wh *se* licenses *nessuno*.

- (14) a. Items which obligatorily introduce negation: *not. non. no*+NP  
 b. Items which optionally introduce negation: Italian *n*-words.  
 c. Items which never introduce negation: *any*+NP.

It follows from (7) that the items in (14a) must head their own neg-chains, that the items in (14b) will be interpreted as negative exactly when they head their own neg-chains and not otherwise, and that the items in (14c) will never head their own neg-chains. Items like those in (14c) are not properly negative, as is of course the case with *any*. Naturally, we would like to find a correlate for the distinction between negative words like (14a) and those like (14b). One possibility is that morphemes whose entire content is negation are those of type (14a). To put it another way, if a morpheme expresses negation without expressing a restriction it is of type (14a). This is clearly true for *not*, *non* and English *no*. Now, if functional heads must express logical content like negation (see von Stechow (1995), Roberts & Roussou (to appear)), then (15) follows:

- (15) Negative functional categories obligatorily introduce negation: negative lexical categories optionally do so.

It is clear that *not*, *non* are clausal functional elements (perhaps of category Neg, obviously the negative functional category par excellence), and always compose with the T-chain as we saw in the previous section. *No* is of category D, as we saw. *Nessuno* must now be treated as being of category N, as in Déprez (1996) (although it forms a chain with D, and hence the composed chain ((T,D)(D,N)) has the properties we observed – cf. (2b)). Strictly speaking then, we never have (T\*, D\*) in a single chain, but rather (T\*, D\*, N\*) (where D\* indicates that D attracts N). (15) is relevant for the analysis of the development of French negation, as we shall see in Section 3.

What we have said so far allows (13d), on a par with English *I saw nobody*. So we need to add a further observation about Italian: if T heads the neg-chain, it must be realised by *non*. In other words, Italian has (T\*, D\*) and (D\*, T), but neither (T, D\*) nor (D\*, T\*).

(13e) illustrates the fact that branching neg-chains are allowed, as long as each can be interpreted as headed by *non*. The branching chains give a multiple-quantification interpretation “there is no person *x* and no thing *y* such that I said *x* to *y*”.

Turning now to Modern French, we find a situation which is substantially the same as that in Italian, with one important difference. Compare the following with (13):

- (16) a. Je n'ai vu personne.  
 b. Personne ne m'a vu.  
 c. Je n'ai pas vu Marie.  
 d. \*Je n'ai pas vu personne.  
 e. Personne ne m'a pas vu.

(16a) looks exactly like (13a): we can analyse *ne* as the instantiation of negation in the T-chain, and *personne*, like *nessuno*, as the realisation of the negated argument in the neg-chain. This implies that *personne* resembles *nessuno* in optionally introducing negation, and so, following the proposal in (15), we should treat it as a noun (note also that *personne* is like *nessuno* in containing both the negation and the restriction). We thus have the chain (T\*, D\*, N\*) here, as in (13a) (again D\* attracts N).

On the other hand, (16b) differs minimally from (13b) in that clausal negation *ne* is required.<sup>8</sup> This implies two things, given the above discussion: first, negation is always realised in the T-chain in French and, second, *ne* is a sentential negation of type (14b). The first conclusion is unproblematic; the second might appear to contradict (15) but in fact it does not, as the claim is that *ne* is either negative or expletive, but never has further content.

(16c) illustrates the well-known double expression of negation in French. This is allowed by our system; clausal negation simply has two realisations in one chain. (16d) is ungrammatical because no well-formed chain can contain all of *ne*, *pas* and *personne*. The chain (*ne*, *pas*, *personne*) cannot receive a well-formed interpretation as a single negation; since both *pas* and *personne* must introduce their own negation. For the same reason *pas* and *personne* cannot form a chain independently of *ne*. The chain (*ne*, *personne*) violates the locality condition on chains (2,a,iii).<sup>9</sup>

<sup>8</sup> *Ne* is most frequently omitted in spoken French and in informal written French. In the relevant registers, though, *ne* is required here.

<sup>9</sup> What is not clear why *personne* cannot head its own chain, giving rise to a double-negation reading. I leave this question aside here.

Finally, in (16e), which according to Déprez (1997: 114), is “very marginal” and “... always ha[s] a double negation reading”, *personne* heads its own chain, as does *ne*. It is unclear, however, what the difference is between this example and its ungrammatical Italian counterpart in (13b).

The above remarks, although they leave a number of questions open, are sufficient to illustrate the ideas and mechanisms that I will exploit in the account of the diachronic development of certain negative words in French. The central ideas are the definition of chains as in (2), the recoverability condition in (7), the possibilities of parametric variation see in (8) and the generalisation in (15) (which may follow from the characterisation of functional categories as elements which must express logical content).

### 3. The diachronic development and loss of Neg-dependencies

The basic observation about earlier stages of French that we are interested in here is summarised by the following quotation from (Foulet (1990: 244)):

“Si *ne* est la négation essentielle du vieux français et n’a besoin d’aucun secours étranger pour exprimer l’idée négative, il est vrai pourtant que depuis longtemps on aime à la renforcer par une série de mots dont l’emploi est parfois bien curieux. Ces mots, à une exception près, ... tiennent leur valeur négative uniquement de leur association avec *ne*, et il est impossible de les employer au sens négatif sans les faire précéder ou suivre de *ne*.”

[Although *ne* is the essential negation in Old French and needs no extra help to express the idea of negation, it is nevertheless the case that from an early stage there is a preference to reinforce it with a series of words whose usage is sometimes rather curious. These words, with one exception, ... take their negative value purely from their association with *ne*, and it is impossible to use them with a negative meaning without *ne* preceding or following them – my translation].

This phenomenon is illustrated by the following examples, which I have translated directly into Modern English on the basis of Foulet’s translations into Modern French and his comments:

(17) *Nul* (“any”):

- a. Cuidiez vous. se me disiez  
vostre conseil celement  
que jel deisse a **nule gent**. (*La Chastelaine de Vergi* 318-20; Foulet,  
p. 245)  
“Do you think, if you tell me your advice secretly that I would tell to  
(just) anyone”
- b. Estre morte o lui me fust mieus  
que vivre si que de mes ieus  
**ne** le veisse **nule foiz**. (*La Chastelaine de Vergi* 805-7; Foulet, p. 246)  
“To be dead or him(?) would be better for me than to live if I didn’t see  
him any time with my eyes.”

(18) *Aucun* (“some”):

- a. **Aucuns** se sont aati ... (le Bossu. *Le Jeu de la Feuillée* 438; Foulet, p. 246)  
“Some people have boasted...”
- b. k’il n’aient de vous **aucun bien** (*Le Jeu de la Feuillée* 671; Foulet, p. 247)  
“That they won’t have any good(s) from you”

(19) *Plus* (“more”):

je n’ai or **plus d’argent** (*Le Jeu de la Feuillée* 554; Foulet, p. 249)  
I haven’t got more money (vs. ModFr “I haven’t any money”)

(20) *Onques* (“(n)ever”):

- a. comment qu’il **onques** en aviegne (*Courtois d’Arras* 66; Foulet, p. 252)  
“how it might ever happen”
- b. Et dist li dus: “Ce n’avint **onques**: .. (*La Chastelaine de Vergi* 349;  
Foulet, 252)  
“And the duke said: ‘That didn’t ever happen’”

(21) *rien*, still could be a feminine noun:

- a. Douce **riens** por cui je chant (Muset, *Chansons* VIII, 44; Foulet, p. 273)  
“Sweet one for whom I sing”
- b. ... li feus,  
qu’il **ne** pooit por **riens** estaindre  
(Hyon le Roi, *Le Vair Palefroi* 204-5; Foulet 279)  
“.. the fire that he couldn’t put out for anything”

It seems clear that all the above words were indefinites, interpreted as having existential quantificational force (*plus* must have been a scalar quantifier of some kind). As indefinites, these elements interact scopally with negation, and as such are able to be interpreted in or out of the

scope of negation. It seems, then, that these elements were neither n-words nor negative quantifiers in Old French (OF). In fact, the only such element in OF appears to have been *niens* ("nothing"), which has subsequently disappeared.

Diachronically, these elements turn into n-words with the properties described and analysed in Section 2 (except *onques*, which disappears and is replaced by *jamaiz*, a compound of *ja* ("ever") and *mais* ("more"), both of which were like the items in (17-21) in OF). In terms of the general account of negation given in Section 2, this implies that these elements underwent a diachronic change such that forming a chain with the clausal negation became obligatory. They become, in the new chain, elements which introduce negative quantification and a restriction on that quantifier, e.g. *rien* now means "for no x, x a thing" while in OF it meant "for some x, x a thing" or, in terms of the Kamp/Heim approach to indefinites (see below) "x, x a thing". In terms of the typology in (14), the OF elements were of type (14c), and the Modern French ones are of type (14a), since they obligatorily introduce negation (note that the generalisation regarding the expression of a restriction given in the text below (14) is formulated as a one-way implication, and so it allows an element to express a restriction and still be of type (14a)).

There are various technical ways to think of this change: as the loss of independent quantificational force, as the loss of the ability to undergo QR at LF, as the loss of an interpretable  $\exists$ -feature (in terms of the feature system in Chomsky (1995, Chapter 4)), or, in terms of the theory of indefinites in Heim (1982), as the loss of the ability to be bound under existential closure by a non-negated existential. The account that I will now propose relies on this last idea.

Let us suppose, following Kamp (1981), Heim (1982) and much subsequent work (notably Diesing (1991)), that indefinites are free variables bound under existential closure. The system for negation sketched in Section 2 is one in which chains are the structural entities which receive an interpretation (whether or not this happens at a dedicated level of representation). As we saw there, an example like (9b), repeated here, has a logical form like (22):

(9b) I didn't see anything.

(22)  $\neg \exists x [\text{thing}(x)] \exists t[\text{PAST}(t)]$  (AT (e,t) & see (e,I,x))



French DPs (almost) always require an article (*Zinedine a marqué \*(des) buts*). Second, this was not the case in earlier French. The following is a 16th-century example cited by Déprez (1995: 53):

- (25) Tu as exemple de ce vice en infinis endroits  
 You have example(s?) of this vice infinite places

Déprez concludes “an attractive conjecture is that the use of bare *rien* and *personne* in environments from which bare NPs gradually disappeared, survived by ... undergoing incorporation into the obsolete empty indefinite determiners which preceded them” (p. 54). Given (15) and the fact that these items are obligatorily negative in Modern French, we conclude that they were reanalysed as members of D. This may also explain why these elements no longer bear N-features, if we think that such features are attributes of Nouns that derive from the interaction of N with the functional positions inside DP. Pure Ds thus lack such features.

In other words, French lost a class of null indefinite determiners; these were replaced by *un(e)*, *des* and, for generic plurals (corresponding to bare plurals in English and many other languages) the plural definite article *les*. Following the general approach in Longobardi (1994), we can suppose that French Ds developed the property of always having to be filled. In terms of Roberts & Roussou (1997), French developed D\*. Positive D satisfies this property by Merge; certain negative Ds by Move, i.e. the earlier bare indefinites, *rien*, *personne*, and those illustrated in (17-21), innovated N-to-D movement, as Déprez suggests.

There is also the null article in negative complements: *Jean n'a pas mangé [ e de pommes ]* (cf. Kayne (1984)). This is the only case of a null D in Modern French; significantly, it is negative. The development of this construction supports our approach. In OF, this construction did not exist (see the detailed discussion in Foulet (1990: 73ff.)). A simple negative indefinite lacked an article altogether (which is why, in our terms, *un(e)* did not become an n-word:

- (26) je ne nourriroie trahitor (Ch. 1223-4; Foulet (1990: 73))  
 “I would not feed [a] traitor”

Null indefinite articles could also appear in non-negative contexts in OF:



- (27) S'avions palefrois et seles (V.P. 527-8; Foulet (1990: 63))  
 If we had horses and saddles

So this D changed in the same way as those in (17-21), and, we presume, for the same reason.

We can tie the development of French negation to the development of the D-system more tightly in terms of the following conjecture: French lost ((C, T), (T, D)) for indefinites. Negative indefinites were reanalysed as (T\*, D\*) (*Jean n'a vu personne*) or (T\*, D) (*Jean n'a pas mangé de pommes*) and positive indefinites as (T, D\*). This took place presumably due to developments in the determiner system itself, combined – in the case of negation – with the existence of the more elegant shorter chain (T\*, D). Note that all of the new chains are identified by phonologically overt material (i.e. they contain F\*): this factor too may have played a role in the reanalysis if we assume that overt PF-identification of the properties is preferred by acquirers over purely abstract chains of the type (F, G). (This implies that there is a certain cost to the LF “reading in” of unmarked features alluded to in fn. 3).

#### 4. Conclusion

In this paper I have sketched a general account of negation, and attempted to describe how a number of indefinite nouns in OF became n-words in Modern French. The account relies on the idea that negative chains develop naturally from indefinite chains, in that (where clausal negation is morphologically realised on T) negative chains are more local than indefinite ones. The fact that in OF many indefinite chains had no overt exponent at all may also have played a role. Finally, as observed by Déprez (1995), this change interacts with a more general development in the French D-system, such that, with one (negative) exception, there are no empty Ds in Modern French. In the last analysis, then, as Déprez (1996) has observed, fully understanding the development of French negation involves understanding the development of the D-system, i.e. the general development of a requirement that D be phonologically realised.

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**SOME THOUGHTS ON ECONOMY WITHIN LINGUISTICS\***  
(Algumas Observações sobre Economia dentro da Lingüística)

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*ABSTRACT: One of the cornerstones of Chomsky's Minimalist Program is the role played by economy. This paper discusses different ways in which Chomsky's notion of economy in linguistics can be understood, given current views on dynamic systems and, in particular, on evolution in biological systems.*

*KEY WORDS: Economy in Linguistics, Minimalism, Exaptation, Dynamic Systems*

*RESUMO: Um dos pontos principais do Programa Minimalista de Chomsky é o papel desempenhado pela noção de economia. Este trabalho discute várias maneiras como essa noção de economia em lingüística pode ser entendida em face de recentes concepções sobre sistemas dinâmicos e, em particular, sobre evolução nos sistemas biológicos.*

*PALAVRAS-CHAVE: Economia em Lingüística, Minimalismo, Exaptação, Sistemas Dinâmicos*

### 1. Three (more or less) reasonable takes on Minimalism

I'll start with a quote by Stephen Jay Gould, who I take to have understood the significance of generative grammar when accounting for faculty psychology within the confines of evolution. "The traits," he writes in (1991: 59), "that Chomsky (1986) attributes to language – universality of the generative grammar, lack of ontogeny, . . . highly peculiar and decidedly non-optimal structure, formal analogy to other attributes, including our unique numerical faculty with its concept of discrete infinity – fit far more easily with an exaptive, rather than a adaptive, explanation." By an *exaptation* Gould means an individual feature that did not emerge adaptively for its current purpose, but was

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\* The research behind this note was partly funded by NSF grant SBR9601559. I am indebted to Elena Herburger, Jairo Nunes, Carlos Otero, and Phil Resnik for specific comments on an earlier draft.

co-opted by the individual; for example, for Gould brain size is not the consequence of “intelligence” related to language, rather the brain got big for whatever reason (e.g. circulatory benefits), which somehow caused linguistic competence.

Another thinker who eloquently presents a view like Gould’s is David Berlinski, who writes in (1986: 130): “mathematicians thought they might explain the constraints of grammar on such grounds as effectiveness or economy. . . In fact, the rules of English grammar appear to owe nothing to any principles of economy in design.” This line is well-known from the work of, in particular, Jerry Fodor, who has systematically built arguments against an unstructured mind, constructivism, connectionism, gradualism, or behaviorism, on the basis of clever, yet sub-optimal quirks of language that linguists have found.

Such comments emphasizing the “decidedly non-optimal structure” of language or “grammar ... ow[ing] nothing to ... economy in design” seem at odds with Chomsky’s Minimalist program. I can only see four possible routes one can take in light of that. The most reasonable one is that the Minimalist Program is just too good to be true; maybe we, linguists, have planted the elegance that we now harvest, unaware of our acts. Even Chomsky constantly admits that the program is partly a bold speculation, rather than a specific hypothesis. So it could just be all wrong.

A second possibility is that, right though the program may be, it is not meant to square its basic tenets with the Gouldian rhetoric behind them. Suppose that the computational system of Human language is in some interesting sense optimal; why this should be is the obvious question. Schoemaker (1991) analyzes optimality as a possible organizing principle of nature, instantiated in terms of least action in physics, entropy in chemistry, survival of the fittest in biology, and utility maximization in economics, among the more or less “natural” sciences. If we play in this key, one could try to argue that economy in linguistics should reduce to survival of the fittest.

Such a view is reasonably defended by important scientists, perhaps most popularly by Pinker in his 1994 best-seller:

Selection could have ratcheted up language abilities by favoring the speakers in each generation that the hearers could best decode, and the hearers who could best decode the speakers. . . Grammars of intermediate complexity. . .

could have symbols with a narrower range, rules that are less reliably applied, modules with fewer rules. . . . I suspect that evolving humans lived in a world in which language was woven into the intrigues of politics, economics, technology, family, sex, and friendship that played key roles in individual reproductive success. (p. 365-9)

Of course, this adaptation story still raises non-trivial questions about what is selective about, say, having thousands of languages, or a parser which doesn't decipher some simple grammatical sentences like *the mouse the cat the dog bit chased left*, or why parsable and sound sentences – like *\*who do you think that left* – are ungrammatical to begin with. Nonetheless, the view is very reasonable, and would take (generic) Minimalism as significant evidence: if you claim language has evolved adaptively, you expect it to show its success right up its sleeve.

A third possibility (for why Minimalism) could be that all this linguistic elegance is nothing but mathematical clout. In one sense, this is trivially acceptable: if interpreted in purely methodological terms. One can be interested in language for its value as a tool to better a logical system, an algebraic apparatus, a computer program, or some such thing. There is meaning to the notion of (often various ways of) optimizing a function, and it could just be that linguistic functions (whatever those turn out to be) happen to be an interesting sub-case of that. Notice, this isn't saying much about language as a natural object, but one doesn't have to. However, many linguists and philosophers do not take any of this as just methodological (cf. Richard Montague's famous (1974: 188) rejection of "the contention that an important theoretical difference exists between formal and natural languages"). That poses very different questions.

The most immediate issue is what *underlies* formal and natural languages, which from the evolutionary perspective we are now considering should be what has evolved, and furthermore optimally, by hypothesis. As Partee (1996: 26) notes, "[o]ne can easily understand Chomsky's negativity towards Montague's remark that he failed to see any interest in syntax other than as preliminary to semantics." But Montague's perspective is reasonable, particularly if as he thought syntax should be homomorphic with semantics; what would be the naturalistic (evolutionary) point in having *both* syntax and semantics evolve as separate systems that (presumably, then) get connected? It is more sound to expect one of these systems to piggy-back on the other, as the aerodynamical structure of a wing allegedly piggy-backs on its flying

function. To the extent that there is a function to language, it surely must have to do with such things as communicating truths or denoting referents – semantic stuff. Clearly, from this perspective, the third view reduces to the second: syntax is just the epiphenomenological form that semantic function has met in the course of evolution.

Less reasonably, I suppose, view number three stays distinct if one goes metaphysical, and claims that English as a formal language actually exists out there in some primitive sense. I don't understand anything about metaphysics, so I won't venture much beyond this point: but I guess this view is also entertained, and I imagine once one is in that world of logic, mathematics, or ideas, to find that they should be elegant a priori – Platonic, after all – is perhaps not that amazing.

Judging from his writings, none of these alternatives is what Chomsky is entertaining, which moves me to the fourth possibility I said I see – the most unreasonable of all.

## 2. A different take

Chomsky (1995) declares it “of considerable importance that we can at least formulate [minimalist] questions today, and even approach them in some areas with a degree of success” (p. 9). He furthermore takes the matter to be far reaching, daring to claim that if on track “a rich and exciting future lies ahead for the study of language and related disciplines.” This isn't just rhetoric: later on (p. 169) Chomsky admits that “[s]ome basic properties of language are unusual among biological systems, notably the property of discrete infinity, . . . that the language faculty is nonredundant, in that particular phenomena are not ‘overdetermined’ by principles of language, . . . [and] the role of ‘principles of economy.’” To the remark about biological oddity, he appends that the basic linguistic properties are “more like what one expects to find (for unexplained reasons) in the study of the inorganic world.” None of these ideas should be taken lightly. “There is,” Chomsky thinks, “good reason to believe that [such considerations] are fundamental to the design of language, if properly understood.”

The *design* of language? What could that possibly mean if not any of the things just discussed? This is the main question that will concern me here.

Of course, the emergence of language is not the only difficulty for standard optimistic stories in the theory of evolution. There to be explained are also incredible convergences in organ structures without any shared functions (e.g. Fibonacci patterns in cordate skins, mollusc shells, jellyfish tentacle arrangements, plant phyllotaxis, microtubules within cytoskeletons in every eukaryotic cell), the very concept of speciation (how do you go from a mutant to an array of them that stay close enough to matter for reproduction?), or individual (what makes a prokaryotic cell turn into an eukaryotic one, in the process subsuming nucleus, mitochondria, organelles, presumably through symbiosis with other micro-organisms, and then start aggregating to what we now see?), not to speak about the competence underlying the observable behavior of animals – including, in some, altruism.

To all those questions, the standard answer is the Neo-Darwinian synthesis (of Darwinism and neo-Mendelian genetics) which in its highlights speaks of selfish genes using individuals as their mere vehicle for survival and reproduction (Dawkins 1987). How one goes from a couple of (our) selfish genes to our exchanging thoughts this very minute, nobody knows.

These matters have a long history within biology, and have now been retaken by researchers dissatisfied with the party line. This is, for instance, what Goodwin (1994: xiii) has to say about the pioneering *On Growth and Form*, D'Arcy Thompson's 1917 classic:

[H]e single-handedly defines the problem of biological form in mathematical terms and re-establishes the organism as the dynamic vehicle of biological emergence. Once this is included in an extended view of the living process, the focus shifts from inheritance and natural selection to creative emergence as the central quality of the evolutionary process. And, since organisms are primary loci of this distinctive quality of life, they become again the fundamental units of life, as they were for Darwin. Inheritance and natural selection . . . become parts of a more comprehensive dynamical theory of life which is focused on the dynamics of emergent processes.

Of course, the devil is in the details, and one wants to know what is meant by *the dynamics of emergent processes*. I'll spare the reader the specifics, but I would like to give at least some sketch of the general picture from Stuart Kauffman's work (1995: 18):



[M]uch of the order seen in development arises almost without regard for how the networks of interacting genes are strung together. Such order is robust and emergent, a kind of collective crystallization of spontaneous structure. ... Here is spontaneous order that selection then goes on to mold. ... Examples that we shall explore include the origin of life as a collective emergent property of complex systems of chemicals, the development of the fertilized egg into the adult as an emergent property of complex networks of genes controlling one another's activities, and the behavior of coevolving species in ecosystems that generate small and large avalanches of extinction and speciation. ... [T]he order that emerges depends on robust and typical properties of the systems, not on the details of structure and function.

Kauffman aptly sums up this view: "Under a vast range of different conditions, the order can barely help but express itself."

What I have reported may sound like prestidigitation, but Kauffman's book seeks to show that it is not – I cannot go into that here, although see the fractal example below. My point is this: If any of this is independently argued for, or at least plausible, the question of language design doesn't have to reduce, in the course of evolution, to "the natural response of an organism looking for the most efficient way in which to transmit its thoughts", as Berlinski jokes in 1986: 130. It could be that linguistic order can barely help to express itself, in whatever sense other kinds of biological order do. If that is the case, we do expect it to "involve a bewildering pattern without much by way of obvious purpose," to again borrow from Berlinski's prose, and pace those who find a grand purpose to syntactic principles.

### 3. Arguments against the unreasonable view

Chomsky often appeals to the metaphor that Kauffman uses: crystallization. He seems to think that grammar could have emerged in roughly the way a crystal does, only at a more complex and arcane level of physics for, as he puts it, "[w]e have no idea, at present, how physical laws apply when  $10^{10}$  neurons are placed in an object the size of a basketball, under the special conditions that arose during human evolution." This passage (see also Chomsky 1993 and 1994) is cited by Pinker, who then adds (p. 363) "the possibility that there is an undiscovered corollary of the laws of physics that causes brains of human size and shape to develop the circuitry for Universal Grammar seems

unlikely for many reasons." He gives two. To start with, "what sets of physical laws could cause a surface molecule guiding an axon. . . to cooperate with millions of other such molecules to solder together just the kinds of circuits that would compute. . . grammatical language?"

The presuppositions of that question are curious. On the one hand, that neurons regulate mind functions (and not something more basic, as suggested for instance by Penrose (1994), or something else entirely) is just a hypothesis, even if this isn't always remembered or even admitted. On the other hand, if there ever is an answer to the rhetorical question Pinker poses, it will most likely arise not from a reduction of mind to whatever the known laws of physics happen to be when the answer is sought, but rather from a serious unification between the two (or more) empirical sciences involved. It is perfectly possible that physics will have to widen or deepen or strengthen (or whatever) its laws as understood at a given time, precisely in order to accommodate the phenomenon of mind – just as they had to be modified to accommodate the phenomenon of chemistry, and to some extent are being stretched when contemplating the phenomenon of life in an entropic universe. I realize I'm appealing to caution in the presence of ignorance – but that's shown better results than letting ignorance dictate.

In relation to Pinker's question to Chomsky, an intriguing instance that comes to mind and seems significant is the discovery by Barbara Shipman that von Friesch's arcane observations regarding bee-dances can be best described in terms of mapping objects existing in six-dimensional flag manifolds to a two-dimensional expression. This already interesting formal fact becomes fascinating when Shipman, a physicist and mathematician studying quarks – which also happen to be aptly described in terms of six-dimensional flag manifolds – speculates that bees might be sensitive to quantum fields. At some level, that they are is already known: given their orientation system (which is not object-driven, like ours, but geodesically grounded), bees apparently can be "fooled" by placing them in the presence of heavy magnetic fields. But what Shipman is exploring is in a sense extraordinary: that a creature may be able to use sensitivity to quantum fields as a system to communicate information. Now imagine we had posed Pinker's question to von Friesch instead of Chomsky, at a time when quantum physics was either not developed or even very well-known, in light of the bizarre behavior of bees. What physical laws could possibly cause molecules in

bee neurons to cooperate with millions of other such molecules to compute bee dances? Well, who knew: of course, who knows now as well, but at the very least the Shipman take on these matters puts things in perspective: perhaps the little creatures are sensing something on the basis of the laws of physics, crucially as presently understood.

Pinker's second problem (with Chomsky's crystallization metaphor, or more generally Gould's argument that large brains predate speaking humans) insists on a common-places: that large brains are, per se, maladaptive, and hence they could have emerged only as a result of some good associated function.

Suppose we grant that initial premise (in terms of metabolic cost, for instance). Still, the general reasoning misses the point I'm trying to establish, which Kauffman so poetically expressed: some order can barely help to express itself, maladaptive or not. Of course, if some expressed order turns out to be so maladaptive that you won't transmit your genes, then your kind dies out. That is tough to prove, though: play in animals, for instance, is somewhat maladaptive: you waste time and energy, you get injured, you expose yourself to predators... Does play kill you? Obviously not, or scores of species would have vanished; does that mean there is a tremendous alternative, systematic benefit in play, that so many species have it? If there is, it isn't obvious. And incidentally, in the case of language, if what one is seeking is a function as a way out of the puzzle of maladaptive brains, just about any function does the job (e.g. Gould's circulatory gains). You certainly don't need the whole "benefit" of language for that, which simply highlights the general problem with doing "reverse engineering": my reason is as good as yours, so nobody wins.

Those are the "many reasons" the "circuitry for Universal Grammar" should be blamed on Darwinian adaptationism. Perhaps, but the force of the argument is nowhere to be seen. Yet Pinker's reasoning does certainly go with the mainstream in biology, which seems ready to presuppose answers to these fascinating questions on form – on the basis of the dogma of function. Witness in this respect the critique that Givnish (1994) gives of the extremely interesting work by Roger Jean (1994), who attempts an analysis and explanation of Fibonacci patterns in plant phyllotaxis. After asserting that Jean's explanation for plants displaying geometrical patterns is not compelling, Givnish writes (p. 1591):

He raises no adaptive explanation for phyllotactic patterns... and fails to cite relevant papers on the adaptive value of specific leaf arrangements. Worse, the author espouses Lima-de-Faria's bizarre concept of autoevolution, arguing that phyllotaxis is nonadaptive and reflects a pattern of self-assembly based on prebiotic evolution of chemical and physical matter... recapitulating the natural philosophy of D'Arcy Thompson that led many biologists to abandon phyllotaxis as a subject of study... [N]othing in biology makes sense except in the light of evolution.

The last sentence is a famous prayer by Dobzhansky, which exhorts the listener to follow the (here useless) party line. But in this instance (perhaps even more so that in the case of language) no imaginable adaptive story could serve to explain the (mathematically) exact same form that arises all over the natural world (cf. for instance a viral coating vs. the feather display in a peacock's tail, both arrangements of the sort seen in plants).

#### 4. Basic minimalist properties and how unusual they are

I have argued elsewhere (e.g. (1995)) that Fibonacci patterns present all three of Chomsky's basic unusual properties among biological systems: discrete infinity, underdetermination, and economy. To demonstrate that this is not an isolated instance, I'd like to present another case, which as it turns out also exhibits the property of self-similarity, or fractality. Fractals are recursive structures (hence discretely infinite) of extreme elegance (the economy bit, which in fractals is easily expressible); as for their underdetermination, it usually shows up in the system not coding some of its overt properties, such as handedness or various details about systemic implementation.

The example I have in mind comes from a piece by West, Brown, and Enquist (1997), who analyzed the vertebrate cardiovascular system as a fractal, space filling network of branching tubes, under the economy assumption that the energy dissipated by this transportation system is minimized, and supposing the size of terminal tubes (reaching sub-tissue levels) does not significantly vary across species. In so doing, they deduce scaling laws (among vertebrates) that have been known to exist for quite some time, but hadn't been accounted for as of yet.

What was known already is that biological diversity – from metabolism to population dynamics – correlates with body size (itself varying over twenty one orders of magnitude). Allometric scaling laws typically relate some biological variable to body mass  $M$ , by elevating  $M$  to some exponent  $b$ , and multiplying that by a constant characteristic of a given organism. This leads one to thinking that  $b$  should be a multiple of  $1/3$ , so that the cubic root of an organism's mass relates to some of its internal functions in the way that a tank with 1000 cubic feet of water has multiples of 10 as a natural scale to play tricks with. Instead, what researchers have found is that  $b$  involves not cubic roots, but rather *quarter* roots, unexpectedly, at least if one is dealing with standard geometric constraints on volume. For example, the embryonic growth of an organism scales as  $M^{1/4}$ , or the quarter root of its mass (the larger the mass of the organism, the slower its embryonic growth, but as mass increases, embryonic growth differences decrease). These quarter-power scalings are apparently present all throughout the living kingdoms

I'll spare the reader most of the geometrical details of why a fractal network does involve quarter powers as the scaling factor. The gist can be seen by entertaining the exercise of systematically producing holes in a cylindrical, solid Manchego cheese. Suppose you isolate an outer layer from an inner core, with the intention of producing holes, first, in the outside. Call  $C$  the volume of the entire cheese and  $L$  the volume of the outer layer; obviously, the relation between  $C$  and  $L$  is cubic, corresponding to the three dimensions of length, width, and height. But now consider a further dimension: that by which we systematically produce holes in the outer layer. Call  $H$  the volume of  $L$  minus the holes. To express the relation between  $C$  and  $H$ , we need a more complex exponential function than the cubic one: we must add the contribution of the fourth dimension. More generally, if we continue producing layers inside the cheese, theoretically ad infinitum (if the holes get smaller and smaller, up to some limit), the basic dimensions won't have to change. That will create a fractal structure of holes in the cheese – a Swiss cheese.

The fractal model was cleverly used to describe the inner "guts" of an organism, where tubules of various sorts play the role of the holes, and of course the entire organism is the cheese. The model predicts facts with an incredible degree of accuracy: (where the  $P$ [redicted] and  $O$ [bserved] numbers express the scaling exponent, as is obvious a multiple of  $1/4$ ) aorta radius  $P_{3/8} = 375$ ,  $O = .36$ ; circulation time  $P_1/$

4=.25, O=.25; cardiac frequency  $P=-1/4=-.25$ , O=.25; metabolic rate  $P=3/4=.75$ ; O=.75. The list goes on. West, Brown, and Enquist observe that “the predicted scaling properties do not depend on most details of system design, including the exact branching pattern, provided it has a fractal structure” (p. 126).

That last sentence, apart from directly illustrating systemic underspecification, resonates directly with Kauffman’s contention that some kinds of order arise without regard for how underlying gene networks are put together – which is well, considering that we may be dealing with species that have few genes in common, particularly when extending these observations to other kingdoms.

In sum, a kind of biology is beginning to gain momentum: it is focused on systemic properties that arise via principles of reality which are more elementary than adaptations. Needless to say, the emergence of one of these core systems may well have been adaptive to an organism, but crucially not (at least not necessarily) for whatever it is eventually put to use.

I think this is relevant to Chomsky’s recent (or old) ideas in two respects. First, it directly shows, at least to my mind, that if Chomsky has gone supernova, he has together with a very exciting branch of biology. One may have biases against whatever is biological and non-adaptive or touches on weird physics, but there is no crisis here for standard linguistics as we know and love it, even if one is as skeptical about adaptative explanations of language as Gould, Berlinski, and Fodor all strung together. One doesn’t then have to turn to metaphysics, mathematics, functionalism, or deny the facts. Or to put it bluntly: Chomsky isn’t doing now what he hasn’t done before.

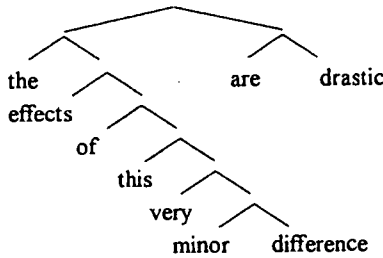
Second, if properly understood, the fact that fractals appear so central to organic nature may give us another argument for the autonomy of syntax. At first, this doesn’t seem so. After all, am I not saying the language faculty is, in relevant, according to Chomsky, “basic” respects, like the scaling system? It is, I think, in those basic properties (of discrete infinitude via recursivity, underspecified or gene-independent plasticity, and structural economy with no direct functional correlate): but it’s plainly the case that language has properties that, for example, Fibonacci patterns do not. For instance, some phyllotactic patterns, underspecified for handedness, branch rightwards or leftwards depending on whether

the previous branch was in some definable sense heavy or light (branching opposite to a heavy branch's direction: see Jean 1994: chapter 3); phrasal linguistic structures are also underspecified for handedness, and if Kayne is right in his (1994) proposal, they branch in the direction that codes command, or the history of their merging process, in Epstein's (1995) interpretation. As I now proceed to show, the effects of this minor difference are drastic.

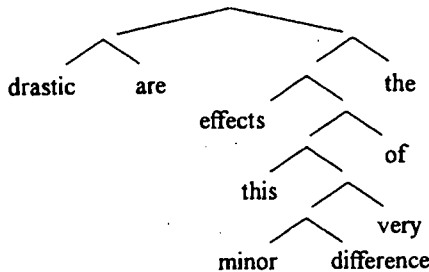
**5. From a minor change to a major consequence**

Imagine the last sentence in the previous paragraph had branched according to the Fibonacci display in vegetable trees, instead of Kayne's. That is, rather than (1), we would have (2), assuming the "heavy" branch is the one with more letter symbols and that the first heavy branch goes right:

(1)



(2)



This is an adequate way to linearize plants (indeed a more “balanced” way than the one seen in (1): here the right branches have a total of 22 symbols, for the 23 of the left branches, whereas in (1) the right branches summed 17 symbols, against the 28 of the left branches). But (2) creates a hopeless instability for linguistic objects. Thus, imagine substituting *drastic and utterly incomprehensible* for *drastic* in these structures. Now that material is heavier than the rest (including 26 new symbols), and hence would seek its place in the linearized structure to the right of *the effects of this very minor difference*.

In other words, depending on how large a predicate is, it may be pronounced before or after the subject. As a system of communication of the sort we have, a general procedure of that sort would be insane. Then again, Kayne’s linearization applied to natural trees would yield heavily inclined trunks, which is probably also insane for adequate photosynthesis, the chlorophilic function, pollinization, and what not. Differently put, nobody can seriously deny the role of use and others (e.g., learnability in the case of language) in certain structural decisions – and nobody does, to my knowledge. But to borrow Kauffman’s expression, here is where selection goes on to mold spontaneous order.

I should clarify that. I’m not saying that communicative reasons directly *yield* Kayne’s procedure, as opposed to the plant one. It’s hard to imagine how at the stage where evolving hominids went from not having the linearization procedure to finding it (assuming that is actually what happened) anything other than the crudest proto-language could have been in place. If the picture Chomsky paints in his (1995) book is remotely close to accurate, even word-formation a la Hale and Keyser (1993), or any such variant, is dependent on a transformational process that has to involve Kayne’s linearization procedure. This is easy to show.

A transformation involves a phrase-marker K and a target symbol T from inside K that is added to the root of K. As a consequence, a dependency or chain is formed between two pairs: the moved T and its (after movement) immediate syntactic context K, and T’s copy, call it (T), and its immediate syntactic context X:  $\{\{T, K\}, \{(T), X\}\}$  (see Chomsky (1995: 252)). The minute movement takes place, you involve at least four symbols, all appropriately arrayed into some phrase-marker. Remember, the phrase-marker per se codes no linear order, but mere hierarchical arrangements that the linearization procedure lays out in an appropriate phonetic row. Now, if you just have one symbol, you only



have one linearization possible: with two symbols, you obviously have two, in principle; with three symbols, six possible linearizations ensue; more generally, with  $n$  symbols you have  $n!$  linearizations possible (these are the possible permutations of those symbols). For example, the sentence *Tarzan loves Jane* can be linearized in six ways – without counting any possible movements. If you add those (at least a couple of A-movements, a couple of head movements, and perhaps others), the linearizations jump to over five thousand; even if you took a mere millisecond to consider each of those orderings, it would take you five seconds to parse the sentence unequivocally. As far as anybody knows, that's just unworkable as a communication system: it is as unstable as a Calder mobile on a windy day, with the complicating factor that we actually see a mobile, but we only hear words one at a time...

Plainly put, no linearization equals no overt movement – hence nothing remotely close to human language – and furthermore not even (at any rate, appropriately complex) words (formed by movement). Maybe that lets you go by with Me-Tarzan stuff, but you can hardly speak of any transitive actions, for instance, which presuppose movement in Chomsky's system. Nonetheless, Me-Tarzan stuff certainly worked much better in Saturday afternoon classics than Cheetah's 'chimpanzeese', and it's not trivial to go from Tarzan to us right here just by the alleged selective pressure that lack of communication with Jane-like figures would arguably impose. It is more likely, it seems to me, that Tarzan's child just stumbled onto something like Kayne's linearization, the way one probably stumbled onto the linearizing device that translates hierarchical musical structures to a whistling tune. Once an accident like that took place within the evolving human brain, the benefits of linearization could all be harvested, perhaps from whistling to word-formation. God only knows.

Admittedly, that was a just-so story of my own, but Chomsky's elegant system invites just this sort of speculation, for anyone who cares to look at the details. The Minimalist syntax is so subtle and far reaching that a minor change in one of its components can carry you from something like language to something like a plant. This might seem like autonomous syntax by a hair's length, but isn't everything concerning form out there, and by even smaller lengths? The difference between a "sparsely connected" network and one less or more connected appears to be one, so Kauffmann shows in 1995, between nothing at all, utter

chaos, and complex order: curiously, binarity seems to do the trick among possible relations: less leads to nothing, more to chaos – two does it. Of course, we don't need to go into the arcane issues I'm talking about here to illustrate this point about subtlety in this universe. Change the one in one trillion imbalance between protons and antiprotons and the known universe vanishes, matter annihilating antimatter (thus no familiar forms: stars, atoms, we or anything).

Why is this relevant to the general point of this exercise? I cannot put it better than Fodor (1998: 12), in his recent critical review of Pinker's new book (1998):

[W]hat matters with regard to . . . whether the mind is an adaptation is not how complex our behaviour is, but how much change you would have to make in an ape's brain to produce the cognitive structure of a human mind. And about this, exactly nothing is known. That's because nothing is known about the way the structure of our minds depends on the structure of our brains. . . Unlike our minds, our brains are, by any gross measure, very like those of apes. So it looks as though relatively small alterations of brain structure must have produced very large behavioural discontinuities in the transition from the ancestral apes to us. If that's right, then you don't have to assume that cognitive complexity is shaped by the gradual action of Darwinian selection on pre-human behavioural phenotypes. . . [M]ake an ape's brain just a little bigger (or denser, or more folded, or, who knows, greyer) and it's anybody's guess what happens to the creature's behavioural repertoire.

The subtly dynamic system that the Minimalist Program implies illustrates Fodor's point from a different angle. When all is said and done (?!) about the ultimate physical support of the syntax of natural language, you may well find something as deeply surprising as the honeybees story I reported above.

## 6. Autonomous syntax redux

Even if autonomous syntax comes from a remote corner of structuring – albeit one whose consequence is the possibility of forming words (hence a lexicon, hence anything socially useful about linguistic structuring) – we should really welcome it. This is not just a matter of turf. The only reasonable alternative is functionalist, and it reduces to some variant of Montague's skepticism noted before. Personally, I'm

willing to wait and see what Montague grammarians have to say about why local movement, expletive replacement, agreement, and all the rest. Unfortunately, the answer so far has been nothing much.

In Chomsky's (1995) view, particularly in chapter 4, the stuff that transformations manipulate is features, understood as properties of lexicon units (the equivalent of *charge* or *spin* in a sub-atomic particle). The movement of T to K that I sketched above is broken down into smaller parts, with a feature F of T being the trigger. It's as if K tries to attract F (Chomsky's actual term) and the item that contains F is forced to move as a result, much as a box full of nails moves when the nails are pulled by a magnet. From this perspective, we expect that a feature F' which is closer to K than F should interfere with K's relation to F, just as a magnet cannot "ignore" a paper clip, say, to attract a nail that is further away. This "dumbness" of the linguistic system is not even surprising if matters are, in some appropriate sense, the way I have been presenting them.

In an interesting paper, Fukui (1996) extends these technical points to an equally technical point about physics. He emphasizes an analogy between Chomsky's economy of derivations and Maupertuis's principle of Least Action. One of Chomsky's main ideas is that alternative derivations (in some precise sense that I describe immediately) compete in grammaticality. That recalls various scenarios in mechanics and optics where, of several alternative paths that an object or a beam of light may follow, only the optimal one is chosen.

It is curious to note, as Schoemaker (1991) observes regarding these matters, that optimality principles in physics raised, virtually from the time they were proposed, the same sorts of questions that Chomsky's idea has, in the recent critical literature. Perhaps nobody expresses this so well as Feynman in his lectures, which Schoemaker appropriately cites (p. 209):

The principle of least time is a completely different philosophical principle about the way nature works. Instead of saying it is a causal thing, . . . it says this: we set up the situation, and *light* decides which is the shortest time, or the extreme one, and chooses the path. But *what* does it do, *how* does it find out? Does it *smell* the nearby paths, and check them against each other? The answer is, yes.

Needless to say, Feynman's little joke at the end comes from the fact that he has a quantum-mechanical explanation.

I only wish I had such a quantum-mechanical explanation about Chomsky's optimal derivation smelling the nearby alternatives: unfortunately I don't. I wouldn't be surprised, however, if there is one, even outside the reach of present-day science.

In fact, Chomsky's derivations are behaving somewhat like beams of light in an even more obvious way. In his treatment of the impossible *\*there seems someone to be in the room*, as opposed to *there seems to be someone in the room* (see his (1995) pp. 344 and ff. and 366 and ff.), Chomsky wants the derivation leading to (3b') below to outrank the derivation leading to (3a'). But how can that be, if the sentences involve exactly the same words and exactly the same numbers of mergers and movements?

- |     |   |                |
|-----|---|----------------|
| (3) | [to [be [someone [in [the room]]]]]                             | {there. seems} |
| a.  | [someone [to [be [(someone) [in [the room]]]]]]                 | {there. seems} |
| b.  | [there [to [be [someone [in [the room]]]]]]                     | {seems}        |
| a'. | [there [seems [someone [to [be [(someone) [in [the room]]]]]]]] |                |
| b'. | [there [seems [(there) [to [be [someone [in [the room]]]]]]]]   |                |

Topmost is the chunk of structure that both derivations share, with the remaining words to be used in a lexical array (which Chomsky calls a *numeration*). In (3a) we see how *someone* moves, leaving a parenthesized copy or trace, while in (3b) *there* is inserted instead. Assuming (non-trivially) that movement is more expensive than merging, then it is clear that (3a) is outranked by (3b). But now consider (3a'), the continuation of (3a); here, *there* is merged, while in (3b'), the continuation of (3b), *there* moves leaving a trace behind. So now it seems that, after all, both derivations are equally costly: one takes an extra step early on; the other takes it later – but both take the extra step. ...

It doesn't matter. Chomsky invites us to think of derivations as unfolding in successive cascades of structural dependency, narrowing down the "derivational horizon", as it were, as further decisions are made. Intuitively, the horizon is completely open when no words are arranged into a phrase-marker, and it shrinks down as some words are attached (e.g. as in the top-most structure in (3)). Only derivations with

the same derivational horizon compete, like (3a) and (3b). By the time we're asking (3a') and (3b') to compete they are already part of two entirely different derivational histories, like those science fiction characters that get killed in a parallel universe but still make it in this one.

To see that light behaves in similar ways, consider an illustration of Feynman's that is meant to show why often the path of least action is not the shortest. You're lying on the beach and suddenly somebody starts drowning two hundred yards to your left. What do you do, run in a straight line? Not so, because swimming is harder than running: you run the shore until a critical point, and then you swim. Light acts somewhat similarly when going from air to water, maximizing the "easy" path vis-a-vis the "difficult" one (across a denser material), even if the combined path is not the shortest. But now imagine a more complicated scenario. You're still on the beach, but suddenly you see somebody trapped inside a building on fire: you could run directly to the building, or actually take a small detour to the water and then go inside the building. Here, obviously, you first get wet, and then run to save the person on fire. Light doesn't have such a "look ahead". You can construct scenarios where it would have to transverse three media, say air, oil, and water, in such a way that you could optimize the total path by doing this, that, or the other. But what light does instead is optimize *the transition* from air to oil, and as its traveling horizon narrows (that is, whatever the result of that first transition is), a new optimization takes place for the transition from oil to water. The trick is as dumb as the one played by the syntactic derivation because neither is really smelling anything – they are just a bunch of photons or words going about their business, bumping against other stuff.

I make much of this syntactic dumbness (as opposed to the interpretive smartness of semantics, say), as an argument not just for the autonomy of syntax, but in fact for its primacy as well. Many, if not all of Chomsky's (1995) core principles could be seen in this light. His Inclusiveness Condition, that "any structure formed by the computation. . . is constituted of elements already present in the lexical items selected for [the numeration]; no new objects are added in the course of computation" (p. 228), coupled with the Recoverability Condition ensuring "that no information be lost by [an] operation" (p. 44), immediately recalls Conservation Laws in physics and chemistry (except those deal with quantities, and syntax deals with qualities). His Last Resort condition, "that computational operations must be driven by some

condition on representations, as a ‘last resort’ to overcome failure to meet such a condition” (p. 28), resembles Haken’s Slaving Principle in synergetics: stable modes of the old states of a system are dominated by unstable modes (see Mainzer (1994)). The Condition on Chain Uniformity, that “the chain C is *uniform with respect to P*. . . if each ?i [a link of C] has property P” (p. 91) is best treated as a condition on the stability of an object constructed by the derivation, thus relating to the stability of wave functions in quantum mechanics: collapsing (and hence interpreting) a chain, in the sense of Martin’s (1996) developments of Chomsky’s ideas, could be then akin to collapsing a quantum wave – to my mind a fascinating prospect, suggesting that interpretation amounts to observation of a quantum state.

Is all of this metaphoric reminiscence or day dreaming – or folly? Perhaps. Then again, the alternatives (denying the facts, blaming adaptations, going metaphysical) don’t seem all that promising. What does it all mean, though? Well, I don’t know – how could anyone? I do know, however, what it does not mean. In this respect, I think it is rather interesting that, if something makes the Minimalist Program different from the Principles and Parameters model, which it springs from, that is the new reliance on economy, rather than the modules of the predecessor. Where one found Theta, Case, Binding, and similar modules, one now seeks just economy in different guises (or pushing the phenomenon out of narrow syntax). This is rather crucial.

Again, Fodor puts it well in his 1998 piece:

A module is a more or less autonomous, special-purpose, computational system. It’s built to solve a very restricted class of problems and the information it can use to solve them is proprietary. . . If the mind is massively modular, then maybe the notion of computation that Turing gave us is, after all, the only one that cognitive science needs. It would be nice to believe that. . . But, really, one can’t. For, eventually, the mind has to integrate the results of all those modular computations and I don’t see how there could be a module for doing *that*.

It is not surprising that Fodor was never too happy with the modular property of the Principles and Parameters system, since it basically postulated modules (Theta, Case, Binding modules) within a module, the Language Faculty. The modules ruled some particular local interactions, and such notions as *government* were thought to determine non-local, interactive relations among modules – a sort of “central

system” in Fodor’s terminology. This is, clearly, not the sort of architecture that Fodor initially (and plausibly) sought. Quite simply, if you allow modules within modules, you may then have modules within modules within modules (e.g. Conditions A and B and C, which were also modularly defined), and it’s then modules all the way down. Which is a form of connectionism. The new architecture is much more in consonance with Fodor’s view: there is a syntax, its own module, which interfaces with other modules – whatever those are. Period. Now, this has a consequence.

The modular view of mind lends itself nicely to the adaptationist view of linguistic evolution (putting aside the problem of our ignorance about the brain support). The more modules you have that connect in reasonable ways, the more you expect *the connection* to be adaptive. Even Fodor would accept that, I think, so long as the module itself is left untouched. Note, in fact, that his argument immediately above takes no issue with the Turing interpretation of the module – what he sees implausible is a Turing interpretation of the central system. By parity of reasoning, the Principles and Parameters model could have been interpreted (not necessarily, but somewhat plausibly) in similar evolutionary terms: you have Theta and Case modules, for instance, that evolved for whatever reason (even a crazy reason), but *the way they got connected*, through government, let’s say, is not implausibly adaptive. No Case/Theta connection, no visibility of arguments, no interpretable structures. But all of that is now gone, and with it goes another possible adaptationist argument for language in its glorious complexity. You’re left with structural economy of the sort we’ve seen, and good luck connecting that to any direct function of the usual sort.

## 7. By way of a conclusion

In his Fall 1997 class lectures, and again in unpublished work, Chomsky has pushed some of the ideas discussed above even further, going into what I like to think of as a more “dynamically derivational” way – particularly when seriously exploring the possibility of multiple applications of Spell-Out, or various consequences of accessing to the initial numeration cyclically. All this talk of dynamic systems, of course, is very much intended in the sense of Goodwin’s “dynamics of emergent processes”, mentioned above. As far as I’m concerned, the more research goes in this direction (and there is a long way to go), the closer we are to

speaking in terms that complexity theorists can relate to, thus moving the syntax project in a new direction.

There, I should say, lie two presently serious problems. One is that (although there is no “complexity theory”) many of the “complexity” pioneers come from very different assumptions from the ones linguists usually make, and in particular from the connectionist arena that is alien to Chomskyan concerns – particularly if interpreted in the modular ways that Fodor has naturally advocated. A second problem is that, up to now at least, these people are usually profoundly ignorant of linguistic facts, and even when the best among them try in good faith to discuss language, the result is often gibberish (see e.g. the deep misunderstandings of the otherwise intriguing book by Cohen and Stewart (1997), particularly around p. 247). I don’t think either of these are fundamental problems, but they should be kept in mind.

At any rate, mine has been a mildly ontological take on the Minimalist program. I say “mild” because I’ll be the last one to want to fall onto “hard” ontological commitments; my argument hasn’t been that at all. Rather, the issue is simple: stuff out there, in the natural world of physics, chemistry, or if one looks, organisms, has the core properties that Chomsky thinks language exhibits. Whatever that “optimal” form is, it is far away from a simple consequence of some (unclear) function.

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**Impressão e Acabamento**

**JETGRAPHIC**

Fone: (11) 5575-7602

**DOCUMENTAÇÃO DE ESTUDOS  
EM LINGÜÍSTICA TEÓRICA E APLICADA**

D.E.L.T.A., Vol. 16, Nº ESPECIAL, 2000

Revista publicada com o apoio oficial da ABRALIN - Associação Brasileira de Lingüística

**Tesouraria / Treasurer**

Sumiko Nishitani Ikeda - *PUC-SP*

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**Revista D.E.L.T.A.**

Programa de Pós-Graduação em Lingüística Aplicada e Estudos da Linguagem (LAEL)

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ET 026771



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