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Contents

The Development of Female Speech in Japanese	1
<i>Hideko Abe</i>	
Answerhood and LF Pied-Piping in Japanese	13
<i>Shoji Azuma</i>	
Morphemic Planes and Templates in Keley-i	26
<i>David Basilico</i>	
Wh-Phrases: Substitution to Spec of CP or Adjunction to IP?	37
<i>Edmond Biloa</i>	
Quantifier Scope and the Case Theory	49
<i>S. Chandrashekar</i>	
"Written in Their Hearts": The Element of Text	60
in Extemporaneous Pastoral Prayer <i>Jeanice Seitz Conner</i>	
On the Fixed Syllabic Structure of Chinese	70
<i>San Duanmu</i>	
A Case for Default Values?	81
<i>Nigel Duffield</i>	
The Role of Sonority in Spanish Stress Assignment	94
<i>Elaine Dunlap</i>	
The Past, Present and Future of Neurolinguistics	103
<i>Victoria Fromkin</i>	
What a Phrasal Affix Looks Like	130
<i>Chris Golston</i>	
Secondary Predication and Null Operator in English	140
<i>Michael Hegarty</i>	
Morphology and the Use of Space in ASL Verb Agreement	154
<i>Wynne Janis</i>	
Strictly Binary Metrical Structure	166
<i>C. Douglas Johnson</i>	
Relating Thematic Relations and Aspectual Interpretation	178
<i>Elizabeth Klipple</i>	
Views on Grammatical Voice from and for the '90s	189
<i>D. Terence Langendoen</i>	
Toward a Formal Characterization of Topic Construction	199
with Special Reference to Korean <i>Hyunoo Lee</i>	

Allomorphy in Tagalog Reduplication	211
<i>James Myers</i>	
A Functionalist Argument for the Autonomy of Grammar	223
<i>J. Newmeyer</i>	
Sentential Subjects and Predictive Parsing	239
<i>Jaye Padgett</i>	
Old English man/mon: The Historiography of	252
Anglo-Frisian(?) Sound Change	
<i>Herbert Penzl and Daniel Brink</i>	
Prospects for Generative Grammar in the 1990's	257
<i>Geoffrey K. Pullum</i>	
An Unexploited Rule for Morphological Naturalness	277
<i>Irmengard Rauch</i>	
Wide Content: a Cognitive Illusion	287
<i>Paul Saka</i>	
Prepositions in Persian and the Neutralization Hypothesis	300
<i>Vida Samiiian</i>	
Echo Question Formation in English	310
<i>Nicholas Sobin</i>	
HPSG Cannot Avoid Head Wrapping in Thai	318
<i>Prapa Sookgasem</i>	
Theta-Marking and Subject Extraction in Causatives	325
<i>Esthela Treviño</i>	
The Distribution of Tone in Taiwanese	336
<i>Jane S. Tsay</i>	
Arizona Regional Usage of Lexical Items:	347
Roller Shades, Submarine Sandwich, Swamp Cooler, and Arcadia Door	
<i>Linda van der Wal</i>	
The Semantics of Adverbs and the Perception Problem	355
<i>Adam Zachary Wyner</i>	
Phonological Descriptions of Diglossia: A Laboratory for	363
Deciding how to Avoid Homonyms and Nonsense Forms	
<i>Lofollah Yarmohammadi and J. Ronayne Cowan</i>	
Sentence Types: A Look at Imperative Constructions	370
<i>Shi Zhang</i>	

The Development of Female Speech in Japanese
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I. INTRODUCTION

The Japanese language is widely believed to have a true female form which is considered distinctively different from its male counterpart. Many linguists have pointed out that the difference between women's and men's speech in Japanese is so pronounced that one easily can tell from transcriptions whether the speaker is a woman or a man. When Seidensticker translated Tanizaki's *Tade kuu mushi* into English, he had to indicate who the speakers were (Takanatsu-male/Misako-female) whereas in Japanese it was obvious for the readers because of the difference in male and female speech. Miller also claims that the differences between women's and men's speech are not only so "far-reaching" but also so "closely interdependent upon content and style" that "simple summary" can not explain the differences (1969:289).

Japanese scholars admit the differences, but do not see them as something that tells the overall relationship between women and men in the Japanese culture. Most Japanese scholars insist that the differences are a positive characteristic in the language and that there is no need to discuss what kind of implications or functions the differences serve in the culture.

Differences do not imply automatically "sexism" in the language. However, differences become problems if one sex is expected to speak in a certain way, or is restricted to using only a certain category of words or expressions while the other sex does not have that restriction. It seems safe to say that most of the differences in male/female language are culturally learned, and can be explained by role expectations and beliefs within the society. In some societies such role expectations are much stronger than other societies; hence the differences become more obvious and stabilized.

This paper will first look at the historical aspects of female speech in Japanese, and see how female speech has been created or adopted. Secondly, it will discuss the current differences between male and female language and see how these differences imply and function in current Japanese society.

II. HISTORICAL INSIGHT IN FEMALE SPEECH

A. The Role of Honorific Language in Japanese

Since honorific language reveals the relative status of speakers involved in the conversation, it is necessary to discuss the roles and functions of honorific language in relation to female speech.

In Japanese there are three types of honorifics: polite, respectful and humble. The characteristics of honorific language are expressed usually by means of auxiliary as well as nouns or verbs which by themselves possess an honorific meaning. The polite form of honorific is used to show a sense of formality or politeness towards the person to whom the speaker is talking. In modern Japanese, adding *desu* to a noun or an adjective and *masu* to a verb indicates politeness. It is interesting to note that polite language does not appear in narratives since the author's attitude to the reader is neutral. The respectful form of honorific is used to show respect for a person mentioned by elevating his/her status. On the other hand, the humble form is used to show the relative status of two people mentioned by making the actions of one person humble and as a result elevating the status of the other person.

Tsujimura (1975:11) cites Kindaichi's discussion on the two origins of honorific language. One is that there were some taboos which prevented people

from using certain words and expressions. Kindaichi gives this example: Ainu women were not allowed to mention their husbands' names and this was the origin of the honorific in the Ainu language. A second origin lies in people's belief that there is spiritual power in language. Thus, speaking of God with beautiful or respectful words, they believed, would bring them happiness or fortune. This is described as *Kotodama Shisoo*. Tsujimura adds one more origin. He argues that there has always been a relative power difference between people so people use certain honorific language to indicate their relative status(12). In classical Japanese, there were two types of honorifics: absolute and relative. Kindaichi argues that absolute honorifics were used when the speaker considered him or herself as the highest being in all circumstances. Tsujimura believes that absolute honorifics were used when the speaker felt the power of God or Emperor as the absolute. Relative honorifics require the speaker to consider the relative status of the listener.

Tsujimura points out that historically honorifics have been used by different people differently. During the Heian period, honorifics were used by parents to their children, and by husbands to their wives, as well as by children to parents and wives to husbands. It is also characteristic of Heian period that people used honorifics towards women. In *Taketori Monogatari*, it was Okina, Kaguyahime's stepfather who used honorific language when he and Kaguyahime talked with each other. Tsujimura explains the reason for this as the general tendency of treating women with respect, a characteristic which existed among imperial court nobles in the Heian period.

However, when the political power moved from imperial nobles to warriors in the next period, Kamakura, the situation changed remarkably. No longer did parents use honorifics to children or men toward women, but the honorific language was used distinctively to indicate the warriors' relative status. Here the use of honorific language started having an interrelationship with the relative power between individuals. Thus, the status among warriors that was clearly marked in this period was reflected in the use of honorific language. This relative power relationship becomes more apparent in the Muromachi and the Edo period. In the Edo period, while generally the lower(social) status people had to use honorifics with their higher status counterparts to show respect, sometimes the higher status people such as samurai had to use honorifics in order to borrow money from the lower class people such as a merchants. This is the case of the power of social status influenced by the power of money.

One of the claims that researchers make in the study of female speech in Japanese is that women use more honorific expressions than men. Ide claims that women tend to use polite types of honorifics more often than men(The Encyclopedia of the Japanese Language: 558). For instance, women are likely to say "*Itsu irrasharu no*" while men would say "*Itsu ikundesu ka*" meaning "when are you going?" Women also add the prefix *o* or *go* to nouns to make the words sound more polite. Kanamaru(The Encyclopedia of the Japanese Language: 558) investigated the number of incidents that men and women use the sentence ending particles *desu* and *masu* between both young and middle-aged couples from movie scenarios. She found out that middle aged wives used the *masu* or *desu* form 55.1% of the time while young wives used it only 7.9% of the time when they talked to their husbands. This statistic is interesting because it shows the difference of the relative frequency of polite form based on age.

B. The Development of Female Speech

1) Heian Period(794-1192)

Mashita claims that it was the Heian period when for the first time the differences between female and male speech were recognized. He quotes Sei Shoonagon, the author of *Makura no sooshi*, saying that there are differences in intonation and choice of words between men and women(317).

The first female speech recognized in the language is called "*imikotoba*." This form of speech belongs to the category of absolute honorifics introduced earlier. *Imikotoba* was used first by a group of women who served the Shinto shrine in Ise. They were all from the noble class. This form of speech was used to avoid such words as those related to Buddhism or to something dirty or to misfortune. Soon the use of *Imikotoba* became recommended for ordinary women to use.

Along with *Imikotoba*, Sugimoto lists some words which were exclusively used by men and the ones used by women from *Tosa nikki* by Ki no Tsurayuki in 935 (148).

	exclusively used by men	by women
a.	karakushite	yarayara (barely)
b.	tagahini	katamini (each other)
c.	hanahada	ito (very much)

Sugimoto also found out that in the Heian period men added *na* (e.g. *kuruna* meaning "Don't come") and women *so* (*kuruso*) at the end of the verb in the imperative mood. He also found that the auxiliary *masu* was exclusively used by women in the *Manyooshuu* compiled around 759 in the Nara Period.

In the Kamakura period(1192-1333), which follows the Heian, the difference between men and women's speech was recongized in verb ending. Men used *sooroo* while women used *saburoo* (Sugimoto 150).

2) Muromachi Period(1336-1573)

A second, but different type of female speech was created in the Muromachi period by a group of women who served in temples or at the imperial court. The form of speech is called "*nyooboo kotoba*." The first *nyooboo kotoba* appeared in the literature *Amano Mozuku* in 1420 which includes 14 *nyooboo kotoba* words. *Nyooboo kotoba* first started as a kind of jargon among those women to use only inside the temple or court, but soon it became the ideal language for general women to learn. Mashita(320-321) claims that the difference between *imikotoba* and *nyooboo kotoba* is that the former had to be created to avoid certain words while the latter was created not to avoid certain words but to be mutually and exclusively understood among women. Behind this, there was the notion that men and women who had different social roles and expectations based on their sex must use different language. The *nyooboo kotoba* includes a vast number of words which are first related to food, kitchen utensils, and clothes, but soon it expanded to all sorts of things.

There are several reasons why *nyooboo kotoba* was created. Sugimoto(1974:744) argues that one reason was that lower class people could not use the same language in the presence of higher status people that they would use among themselves. To be polite they had to use different words for objects while in the presence of the higher status people whom they were serving. Here, the power relationship dictated language use. A second reason was that those women who served in noble houses or temples may have come from different parts of Japan, so they had different dialects. Thus, *nyooboo kotoba* played the role of teaching those women to speak the same language.

There are about ten ways *nyooboo kotoba* were created. Both Sugimoto(1974:765) and Mashita (1989:323) list the process of how *nyooboo kotoba* was created. Some words are based on the shape of the thing or the color, or some other characteristics of the thing. For instance, azuki beans, generally called *azuki* is called *aka* in *nyooboo kotoba* indicating the color red. Water, *mizu*, was named *hiyashi* meaning cold. Some words are made by adding *mono* to general words. Examples are *yoru no mono* (literally means "thing for night") for *yagi* (pajamas) and *tsuki no mono* ("monthly thing") for *mensu* ("menstruation").

Nyooboo kotoba first started as jargon among those women who served in imperial courts or temples and gradually expanded among those imperial court nobles, and later was imported into the language among ordinary women in the next period, Azuchi momoyama.

3) Edo Period(1603-1968)

The *nyooboo kotoba* of the Muromachi period is called *Jochuu kotoba*, meaning women's language in the Edo period. *Nyooboo kotoba*, which influenced the speech of women who served the noble class, was transformed to the general audience of women in the Edo period with a slightly different function and implication. *Jochuu kotoba* was recognized as the ideal women's language in contrast to men's language.

Jochuu kotoba is different from *nyooboo kotoba* in that the former taught women how to speak the language as women no matter whom they are talking to while the latter was learned for the purpose of not offending the higher class people. The former does not tell how to use honorific language but simply how to speak or act like ideal women whereas the latter taught how to use honorific language. In order to be accepted as a woman, females had to learn the proper way of speaking *jochuu kotoba*.

In addition to being forced to learn *jochuu kotoba*, women were also forbidden to use Chinese words. Chinese words, *kanji*, had been exclusively used by men until the Heian period. It was the language of intellectuals and high class people. Men claimed that Chinese words were too harsh and less elegant. Examples are "*onaka*" for "*hara*", "*oashi*" for "*zeni*" and "*oshimeri*" for "*ame ga furu*." The notion of Chinese words being too harsh and less elegant is obviously male plausible excuse. After all it was men having power over women who tried to exclude women from using Chinese words. Interestingly enough, in modern Japanese, the statistics show that men use more Chinese words than women. Tsuchiya (The Encyclopedia of the Japanese Language: 559) finds out that in formal situations men use Chinese words 22% of the time while women use them 15.5%, and in informal situations men use them 18.6% and women 12.0% of the time. It is still believed that Chinese words are too harsh for women to use.

Sugimoto argues that *jochuu kotoba* was politically imported in the life of women at that time along with Confucianism which taught that women are inferior to men. He claims that women were not only controlled in terms of how to serve men, but also how to speak the language. There were a number of books published at that time which dealt with educating women. Those books even listed words that women could use. One of the most well-known books is called "*Onna choohooki*" published in 1692. The book told women to be humble, silent, and obedient to their husbands like an ornament for an alcove. Sugimoto claims that this kind of enforcement lasted the next three hundred years. Interestingly

enough, the word "*shitsuke*" (kokuji: Japanese-created kanji, meaning discipline) was created at this time.

In the latter half of the Edo period, there was another form of women's language introduced. It was called "*goshō kotoba*." At that time it was recommended a common young girl leave home to work for a samurai family to learn the proper way to be a woman. The language spoken at those samurai families was called "*oyashiki kotoba*." *Goshō kotoba* and *oyashiki kotoba* are sometimes used interchangeably and they played the role of teaching girls or women how to speak properly. The book "Ukiyoburo" published in 1809 included a lot of examples.

	words to be used by women	regular words
a.	itadaku	taberu (to eat)
b.	oshamoji	shamoji (wooden spoon)

Sugimoto claims that during that time all the interjections and auxiliary words except *wa* which are considered as characteristics of women's words nowadays were introduced. Sentence endings such as *ne*, *sa*, *yo*, were also introduced to women to make them sound soft. Sugimoto argues that *oyashiki kotoba* and *goshō kotoba* were derived from *jochū kotoba* and embedded in women's language.

There is another interesting factor that affected women's language at that time. It was the language used by prostitutes. It is called "*yuuri kotoba*." *Yuuri kotoba*, however, was far from lower class language. Sugimoto explains how vivid and lively *yuuri kotoba* was, which later attracted women in general. It attracted women because it was language that sounded very feminine and it was language used to attract men as well. It was a sort of tool to make men comfortable and superior. Since women at that time were taught to serve men, they had to learn the way to live. Sugimoto claims that *yuuri kotoba* has influenced Kansai dialect more than Kanto dialect (now standard). The characteristics of *yuuri kotoba* is to add *su*, *yansu*, and *sansu* at the end of sentences or adding *o* (in front of the verb) and *asobasu* or *kudasarimasu* at the end of verbs. Here are some words of *yuuri kotoba*.

	Yuuri kotoba	regular words
a.	nomansu	nomimasu (to drink)
b.	okisansu	okimasu (to get up)
c.	ikashiyansu	ikimasu (to go)
d.	o tabe asobasu	taberu (to eat)

As we see, some of these characteristics are still used as women's language now.

It is interesting to note that certain prefixes such as *ki* and *go*, and suffixes such as *sama* and *kata*, were introduced at this time.

It seems that the Edo period played the most important role in formulating how Japanese women speak nowadays. It was consciously planned and embedded in women's language.

4) Meiji Period (1868-1913)

The most important characteristic in the Meiji period was that women were allowed to use Chinese words. Also several new first and second person pronouns and address terms which are still used appeared. Examples are:

- a) First person pronoun: *boku*, *wagahai* (for men only)
- b) Second person pronoun: *omae*, *anata*

- c) Address terms: okusama(someone's wife), saikun(someone's wife),
fujin(Mrs.), reijoo(someone's daughter)

Historically the Meiji period is characterized as modern since the samurai(warrior) gave up their power, and the Emperor again became the ruler of Japan with the modernization policy which was influenced by the industrialization in Western countries. Sugimoto describes women's language in the Meiji era. He argues that in spite of the fact that the Meiji period claimed on the surface that men and women are equal(not in terms of legal rights though), women's language was strictly controlled even worse than the Edo period(198). Women were allowed to receive more education if their parents wished; however, a big part of the education a woman received was spent on teaching her how to become a respectful and suitable woman, wife and mother.

The Meiji period is different from the Edo period in terms of women's consciousness about their status. During the Edo period, women's own will in any respect was denied. They had to do whatever they were told. However, in the Meiji period, women earned the security of being a woman. The newly established Ministry of Education in 1872 ensured that women can receive an elementary school education as a part of compulsory education. By 1885, there were ten mission, one private, and two public high schools for girls and by 1900, there were forty mission schools for girls(Condon: 115-116). Within ten years, almost all boys and girls finished elementary school. By 1901, there were one public and two private universities that allowed women to enter.

III. Functions and Impact of Female Speech in Current Japan

1. Social expectations and social roles

The notion that men and women are different beings comes to link with the notion that men and women speak differently. Since men and women are different, maleness in men's language or femaleness in women's language should be accepted. The problem is how to define maleness or femaleness in language. Here, language and society intervene implicitly. This has to do with sex roles in society. Sex roles are learned during the first year of life. As the child gets older, little boys are allowed to use more rough talk and non-standard forms of the language, whereas girls are discouraged from using this type of language and more closely imitate their mother's speech.

"Verbally as well as in their physical actions girls are expected to be more restrained and considerate than boys and in time these expectations affect the speech patterns of both sexes. Males adopt a more direct, forceful way of talking, females a more tentative, questioning approach"(Swift and Miller 108).

Shapiro claims gender greatly determines and limits how men and women think, feel, and act. This sexism is so embedded in our thinking that people are not aware of reinforcing the sex roles.

Most Japanese scholars accept that men and women do speak differently and treat that fact as one of the most positive characteristics in the language. There was an article in the newspaper written by a Japanese scholar who could not accept the fact that women, especially teachers, now use *kun* for addressing boys or men. Statistics show that 60% of men over 50 years old claimed that it is not acceptable for women to use *kun* for addressing men. However, the number decreases as the age goes down. Thirty-three percent of men around 30 years of age said 'no' for women to use *kun*, and only 9% of high school boys said no (Sugimoto 1985:207-7). The reason for this is that women lose their

femaleness by using the same address term as men do. *Kun* belongs to men's territory.

2. Actual differences in speech between men and women

A. Intonation:

Ooishi found that women use the rising intonation 84% of the time while men use it 60.7% of the time (The Encyclopedia of the Japanese Language: 558). He adds the comment that women's intonation is more rich. He does not get into the fact that women use the rising intonation because of other factors. It has been found out that women use more rising intonation because of their uncertainty about themselves as well as their tendency to avoid the direct statement. Lakoff argues that the rising intonation makes speech sound more polite and thus, it leaves a decision open, and does not impose the speaker's mind or views, or claims on anyone else (18). Nomoto found out that women use stress more than men. Women use stress in their speech every 10.3 seconds while men use only every 17.7 seconds (The Encyclopedia of the Japanese Language: 558).

B. Person Pronouns:

The use of first- and second person pronouns exhibit the most asserted sex-differentiated characteristics of Japanese language. The most recent study by Ide is as follows.

a. the first person pronoun

	men	women
level of formality		
	(high)	watakushi watakushi
		watashi atakushi (standard)
		boku watashi
		ore atashi

		wagahai atai
	(low)	washi uchi (non-standard)
level of formality		
(high)-----		----- (low)
Men		
-----watakushi-----		
----watashi-----		
-----	-----boku-----	
	-----ore-----	
Women		
-----watakushi-----		
-----	-----	
-----	-----watashi-----	
	-----	-----

These pronouns are chosen based on the sex of the speaker as well as the listener, the formality of the situation as well as the social and emotional relationship between the two. Ide claims that pronominal forms that are used by both sexes are *watakushi* and *watashi* and forms used exclusively by men are *boku* and *ore* and by women *atakushi* and *atai*. However, the interesting thing is that in formal situations both men and women use the same pronoun *watakushi* or *watashi* while in informal situations the distinction between men and women becomes clear. While men use *boku* or *ore*, women use *watashi* or *atashi*. That means women use *watashi* for both formal and informal situations whereas *watashi* by men is used only when they have to speak more formal language. This fact is interesting because it may show women's attitude toward the use of

language in general. Trudgill claims that women use more polite form of language because

"women are more status-conscious than men; their insecure and subordinate social position makes it 'more necessary for women to secure and signal their social status linguistically and in other ways.' Men can be rated socially by their occupation, by what they do, while women are rated on how they appear, hence reliance on non-occupational signals of status, such as speech"(91).

b. the second person pronoun

level of formality	men	women
(high)	anata	anata
	anta	anata
	kimi	anata
	omae	anata
(low)	kisama	anta

level of formality
(high)----- (low)

Men
|----anata-----|
|-----anta-----|
|-----kimi-----|
|-----omae-----|
|---kisama---|

Women
|-----anata-----|
|---anta---|

The choice of the second-person pronouns is very limited to women.

There are only two forms *anata* and *anta* whereas men have five forms such as *anata*, *anta*, *kimi*, *omae* and *kisama*. Here again women use *anata* and *anta* both in formal and informal situations while men make clear distinctions between the two in usage. This fact implies two things. One may be because of the subordinate and insecure social position as Trudgill claims, and the other may be because women really never had a life outside their home. In other words, their life was pretty much limited to inside their home so that they did not have to make a distinction between formal and informal language. They were never in formal and public circumstances. Men being in both private and external, the public, political world had to have two distinct forms while women being in the internal, the private world did not need two forms. Thus, their different experiences in life may have limited their use of language.

In modern Japanese, the use of both the first and the second pronouns for women are limited, however, both in the Muromachi and the Edo periods, women had much larger scale of words for pronouns.

a) the first person pronoun

	male	female
Muromachi	watakushi/ soregashi/ware/ warera/mi/wagami/mizukara	wagami/mizukara/wara/wa
Edo	male watakushi/watashi/washi/ washi/ore/ora/oira/sessha	female watakushi(high class) watashi(high,middle class) watai(general) watchi(middle,low class)

ore,ora,oira, (lower class)
wachiki(prostitutes)

According to Komatsu(113), *ore* which was used by men and lower class women in the Edo period came to be considered not polite in the Meiji period, instead a new word *boku* was created and women dropped the word *ore*. There are two interesting characteristics in the Edo period in terms of person pronouns. One is, in the Edo period, there were first person pronouns exclusively used by women, which is very different from modern Japanese since all the person pronouns used by women except *atai* are also used by men. A second characteristic is that the use of person pronouns by women depended on their social class whereas for men it was based on the situations, or to whom they were talking. In other words, women's person pronouns were more like fixed based on class while men's ones were more situation-oriented. This fact is still true now. It is not yet known why the number of person pronouns women used in those time decreased in modern Japanese.

My own research in person pronouns using a Japanese movie called *Makioka Sisters* reveals that even though pronouns are distinguished with respect to sex of speaker, social status of referent and degree of intimacy with referent, factor of sex is more important than social status in choosing the use of person pronouns. Though the Makioka sisters are higher in social status than their male counter-parts, they still use the first and second person pronouns which reflect their lower status.

C. Interjections:

Ide found out from her study on the use of language by university students that men use more interjections than women. Men used them 655 times in 1000 utterances while women used them only 395 times. Typical examples for men are *yoo* and *che*, and for women are *ara*, *maa*, and *oya*. She also found out men use more varieties of interjections than women(The Encyclopedia of the Japanese Language: 559).

D. Adjectives and adverbs:

It has been found that men use more rough adjectives and adverbs such as *ikasu*, and *dekkai* whereas women use more soft sounded ones such as *suteki*, *subarashii*, *tottemo* and *sugooku* (The Encyclopedia of the Japanese Language: 559). It is also true that women tend to repeat an adjective such as *watashi wa ureshikute ureshikute*. It is said that by repeating an adjective women try to show their emotions or women prefer exaggerating the phrase. However, since there has been no study on this, we cannot go beyond speculation.

In writing, they also found that women use more adjectives and adverbs than men. Hatano's study shows that women use 20% more adjectives and adverbs than men when they write essays. It was also found that women writers used more simile in their writings. Yasumoto found that women used simile 40% more often than men(The Encyclopedia of the Japanese Language: 560).

E. Chinese derived words:

As mentioned earlier, men use more Chinese derived words than women. Based on Tsuchiya, the statistics show that men use Chinese derived words 22.5% of the time in formal situations and 18.6% in informal situations while women use them 15.5% in the formal and 12.0% in informal situations(The Encyclopedia of the Japanese Language: 559). The reason for this difference in number may be the fact that there is still an accepted notion that Chinese derived words sound too harsh, too formal and too intellectual for women to use.

F. Inversion:

According to Peng, it is men who use inversion more than women. Men use it 17% of the time whereas women used only 11.0% of the time. Peng argues that men added the inversion after finishing a sentence to make sure the point they are trying to make is understood(The Encyclopedia of the Japanese Language: 559).

G. Requests:

It was found that when men and women ask someone for a favor, women use more apologetic expressions such as *moshi yokattara*, or *dekireba*, etc. Kawanari's study shows that among fifty university students, women used 18% more of those apologetic expressions and 30% more of explanations of why they need a favor. Moreover, when women want to refuse the favor from their friend, they use more general and vague excuses such as *chotto kyuyyoo ga dekitanode dekimasen*(I have a sudden business to take care.), or *chotto tsugoo ga waruinode* (It is not convenient for me) whereas men give more precise and direct answers(The Encyclopedia of the Japanese Language: 560).

H. Back Channel: Ide found out that among university students, women do the back channeling twice as much as men when they are in the conversation. She argues that it means men are leading the conversation.

I. Portrait of women: It has been thirteen years since Lakoff in *Language and Women's Place* pointed out two facts about women's language. One is that women speak or use language differently from men and the other is that general treatment of language for women is different from the one for men.

Endoo(1983) found out that not only the description of women in Japanese dictionaries is different from the one for men but often is distorted and discriminatory.

Endoo looked up the words *otoko* (man) and *onna* (woman) and did some comparison between the two. Among several definitions there are a few that are worth mentioning.

otoko (man): -male sex whose character is strong and independent

onna (woman):-female sex whose character is gentle and kind, and not direct nor strong

-female sex who has the biological capability of bearing a child (Endoo:3-4)

The description of men being strong and independent and of women being gentle and kind, and not direct nor strong certainly does not portray the real world. There are strong and independent women while there are kind, gentle, indirect, weak men. These are not characteristics of men or women but of ideal types. However these are the typical and most accepted portrait of men and women. This kind of description tells people's different expectations toward women and men in society. There are a lot of other words which describe women in a discriminatory way. Examples are follows:

a) *umazume* : According to the Iwanami Japanese dictionary, it means women who do not have a capability of being pregnant. It is written as "stone woman" in Chinese words. There is no counterpart for men to describe this(Kokugojiten ni miru josei sabetsu: 15-16).

b) *funinshoo* : According to Gakken's Japanese dictionary, it means that it is a condition that a woman does not get pregnant even after being married for a while. It is possible that either a husband or wife has the problem. But this word is used only when a woman has the problem. There is no counterpart for a man(Kokugojiten ni miru josei sabetsu: 16-17).

c) *onna bara* : A woman who bears only girls. (no counterpart for a man)

d) *otoko bara* : A woman who bears only boys.(no counterpart for a man)

e) *chikishoo bara* : A woman who bears more than two children at one time.(no counterpart for a man)

As we see, there are a lot of descriptions of portraying women as biological beings.

f) *teisetsu* : The fact that a woman keeps her fidelity. The fact that a woman does not sleep with other men besides her husband.(no counterpart for a man)

g) *urenokari* : A woman who is not married even after the appropriate age. It literally means "left over" in Japanese. (no counterpart for a man)

There is a list of words in relation to *shoonen* (boy) and *shoojo* (girl) in the dictionaries. Examples are:

boy *koogan no bishoonen* (gentle beautiful looking boy)

mada chichi kusai shoonen (a yet young boy)

shoonen kanbetsusho (jail for boys)

hikoo shoonen (bad boys) / *furyoo shoonen* (bad boys)

girl *nikoyakana shoojo* (shining beautiful girl)

kawaisoo na shoojo (unfortunate girl)/ *aisubeki shoojo* (beloved girl)

kokoro no yasashii shoojo (gentle-hearted girl)

Here, words related to girls are something positive while words for boys include some negative ones. It is also true that words that describe girls have something to do with how they look. It is basically the same for the words for sons and daughters. However, when it comes to the words for a man and a woman, the former includes more positive and strong words while the latter includes a lot of words which are related to marriage.

When a woman and a man are introduced in the newspaper articles, they are treated very differently. When a woman is the subject of the article, there is always a description of her husband's name, occupation, and age if she is married, but the opposite is not true.

There is an interesting survey about women done in six industrial countries. Seventy-one percent of Japanese women still believe in separate roles for men and women, and 72% of them think that their husbands or families are the top priority, and 89% of them believe that housework is the women's responsibility (Condon 1985:295).

Condon argues that Confucian teachings are still very much embedded in Japanese people's thinking. Confucian teachings taught not only that women are inferior to men, but that women are for making babies. The description of women in dictionaries supports that. In Japan eighty percent of women are content with their lives which is even higher than men(74%). Japanese feminist Hiratsuka sums it up in her words, "the most significant barriers lie within ourselves"(Condon 289). Japanese women, being taught to be self-denying rather than self-assertive for a long period of time still believe that men and women have different roles in their lives. At present, it is amazing to know that "only 61% of working women believe in equal pay for equal work, only 64% oppose any kind of sex discrimination in hiring or employment"(Condon 289). Sixty-six percent of male university students believe that women's place is in the home (Condon 1985:290).

IV. CONCLUSION

The differences in male and female speech have been accepted with positive attitude in Japan. Peng(1981:230-231) claims that the differences in the spoken words between men and women in Japanese become apparent in early age of the child and that these differences are reinforced more likely in school.

His research shows that teachers in kindergarten tend to correct girls when they speak like boys rather than boys when they speak like girls.

The fact that there are differences in the use of language between men and women and that there are restrictions for people to use certain style leads us to think that there must be some sort of social function behind this. What has been lacking in the previous research on differences between male and female language in Japanese is to give an insight as to how the differences manipulate or adjust the human relationship between the two sexes. For instance, honorifics may or can show the respect of the speaker toward the listener, but may show the speaker's intention of putting the distance between him/her and the listener at the same time.

It has been found that in the course of history, it is women whose language has been adjusted or manipulated. However, things have been changing for women in Japan. It would be interesting to find out changes in women's language when their social, economical and political status is valued equal or even higher than men.

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ANSWERHOOD AND LF PIED PIPING IN JAPANESE¹

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0 Introduction. Nishigauchi (1986) claims that it is subadjacency, rather than the Empty Category Principle (hereafter ECP) which governs *wh*-movement at LF in Japanese. The apparent violation of the Complex Noun Phrase Constraint (hereafter CNPC) is solved by pied piping a whole complex noun phrase (hereafter CNP) into the operator position at LF.

Based on the assumption that an elliptical answer with *desu / da* ('be') to a *wh*-question must match the value for the operator expression of the question, the pied piping analysis makes the following prediction: an elliptical answer matching a CNP is acceptable because the whole CNP is in the operator position via the pied piping movement. In a case where an elliptical answer matching a wh-expression (not a CNP) is acceptable, Nishigauchi (1986:67) claims that "some sort of deletion operation" is responsible.

However, Kuno and Masunaga (1986) clearly point out the lack of a detailed analysis of the deletion operation in Nishigauchi (1986):

Nishigauchi's truncation analysis . . . , unless it is coupled with a precise formulation of the condition on truncation, faces the danger of turning his island-raising hypothesis contentless. (Kuno and Masunaga 1986:26)

In this paper, I propose a discourse based condition on truncation, which is distinct from both Nishigauchi's (1986) and Kuno and Masunaga's (1986) solutions. A notion relevant to the condition on truncation is that of set salience. A short form answer (matching a *wh*-phrase) is felicitous when the set over which the *wh*-quantifier ranges is the most salient set in the context. On the other hand, a long form answer (matching a CNP) is felicitous when the common noun in the CNP refers to the most salient set in the context.² In other words, an answer must match the most salient set in the discourse. Equipped with the discourse based condition on truncation, the pied piping analysis can be saved from Kuno and Masunaga's (1986) critique, and thus subadjacency can be maintained as a locality principle at LF in Japanese.

1. Nishigauchi's (1986) truncation mechanism. Nishigauchi (1986) offers two brief suggestions regarding the truncation mechanism, which yields a short form (matching a *wh*-phrase) from a long form (matching a CNP). Observe the following exchanges.

- (1). A (Sore-wa) [[Tanaka-san-ga Nakasone-san-ni kai-ta] tegami] desu-ka?
 it -topic -N -D write-P letter be -Q
 '(Is it) a letter that Tanaka wrote to Nakasone?'
 B. * lie, Miyazawa-san-ga Takeshita-san-ni desu.
 no -N -D be
 'No Miyazawa did. to Takeshita.'
 C. lie, [[M.-ga T.-ni kai-ta] tegami] desu.
 no -N -D write-P letter be
 'No. (it's) the letter that Miyazawa wrote to Takeshita.'
 (Nishigauchi's (85) on p 70)

As a short felicitous answer to the question A. C is acceptable, but B is unacceptable. Nishigauchi (1986:73) suggests that 'a complex NP cannot be reduced to more than one argument expression which does not form a constituent.' (Thus, the truncated form B is unacceptable.) However, it is not difficult to find an example which shows that his suggestion cannot be generalized. Observe the following exchange

[In front of a Christmas tree, people are guessing who sent which present to whom.]

- (2). A. Kore-wa dare-kara dare-ni ateta okurimono da-roo-ka?
 this-T who-from who-D gave present be-will-Q
 'This is a gift from who to whom?'
 B. John-kara Marii(-e) da-roo.
 -from (-to) be-will
 'From John to Mary.'
 ?? C. John-kara Marii-e ate-ta okurimono da-roo.
 -from -D give-P present be-will
 'A gift sent from John to Mary.'

B is felicitous, whereas C is marginal, which is just the opposite of what Nishigauchi's (1986) suggestion predicts. Next, observe the following exchange.

[Two fishermen are discussing their concerns about Soviet patrol boats capturing Japanese fishing boats.]

- (3). A. Dono suiiki-kara dono suiiki nikakete sougyou shite-iru
 which area-from which area to fish do-ing
 fune-ga soren-ni daho sare-yasui no
 boat-N Soviet-by capture get-easy Q
 'Is a fishing boat that fishes from which area to which area likely to be captured by a Soviet patrol boat?'

B Nemurooki-kara Saharinoki nikakete da-roo.
 -from to be-will

'From Nemuro-oki to Saharin-oki.'

??C. Nemurooki-kara Saharinoki nikakete sougyoo shite-iru fune da-roo.
 -from to fish do-ing boat be-will

'It will be a boat which is fishing from Nemuro to the Saharin area.'

Again, Nishigauchi's (1986) suggestion wrongly predicts the opposite judgments. Thus, it is clear by now that his account cannot go further beyond his own examples.

Secondly, Nishigauchi (1986) offers another suggestion based on the following data.

(4) [[Dono kyoojyu-ga suisen-site-iru] hito]-ga
 which prof. -N recommend-be-PR person-N
 saiyoo-sare-soo desu-ka?
 appoint-be-likely be-Q

'(A) person that which professor recommends is most likely to get the position?'

A. *Suzuki-kyoojyu desu.
 '(It's) Prof. Suzuki.'

B. [[Suzuki-kyoojyu-ga suisen-si-te-iru] hito] desu.
 -N recommend-be-PR person be

'(It's) (the) person that Prof. Suzuki recommends.'

(Nishigauchi's (87), (88) on p. 74)

Nishigauchi (1986) judges A as infelicitous, but my informants (including me) judge it felicitous (at worst marginal). Thus, I do not share Nishigauchi's (1986) judgment. However, for the sake of argument, I will assume that his judgment is correct. Nishigauchi (1986) argues that candidates (for a faculty position) and professors are close to each other in reference, and possibly intersect. He claims that this set similarity somehow contributes to the preferred answering pattern B. But it is not clear to me how these two sets are close enough to intersect. Persons who are seeking a teaching position are very unlikely to be interpreted as persons who already hold a position in Japanese society. (But this may not be the case in other speech communities such as the U.S.) It may of course be argued that these two sets intersect in that both have human beings as their members. But that this type of argument cannot go further is illustrated in the following exchange.

[Two persons are discussing a recent news report about the assassinations of two mob bosses, Taoka and Nishi, by gangsters.]

- (5). A. Dotti-o koroshita chinpira-ga tsukamatta-no?
 which-A killed gangster -N captured -Q
 'A gangster who killed which mob boss was caught?'
- B. Taoka-kumichoo da-yo.
 -mob boss be-tag
 'Mob boss Taoka '
- ??C. Taoka-kumichoo-o koroshita chinpira da-yo
 -mob boss-A killed gangster be-tag
 'A gangster who killed mob boss Taoka '

There are two relevant sets in the above exchange. One is a set of mob bosses. Taoka and Nishi. The other is a set of gangsters, which includes a gangster who killed mob boss Taoka, and another gangster who killed mob boss Nishi. Both sets are very close in terms of [+ human] (and also [+brutality] [+gang] if these are allowed in our feature representation). Yet, the preferred answer is the shorter form B, not the longer form C in (5). Notice that, in Nishigauchi's (1986) example (4), the longer form C is the preferred answer. This shows that the notion of closeness of sets involved is not a sufficient determinant.

It is now clear that neither of Nishigauchi's (1986) suggestions can correctly account for the "truncation" phenomena. In what follows, I will examine another relevant notion, "D-linking" in Pesetsky (1987).

2. D-linking. Pesetsky (1987) claimed that the choice between a short form answer (matching a *wh*-phrase) and a long form one (matching a CNP) is closely related to the notion of D-linking (from 'discourse-linking'). In this section, I will examine Pesetsky's (1986) D-linking analysis, specifically his two types of binding approach, and show that it is not satisfactory.

Since Pesetsky (1987) does not give any formal definition of D-Linking, I will quote the following statement concerning D-linking from Nishigauchi (1986), who seems to follow Pesetsky (1987).

If the range of value possibly associated with a given *wh*-expression is determined in the given situation of discourse, that *wh*-expression is D-linked. (p.47)

Observe the following sentence.

- (6) Which book did you read?

The range of felicitous answers to (6) is limited by a set of books which has been previously referred to in the discourse. Thus, the *wh*-expression, 'which book' is D-linked.³

(after pied piping) is responsible for prohibiting A1 answer. The second solution is to resort to subadjacency violation by moving the non D-linked *wh*-phrase *nani* ('what') at LF. The first solution (i.e., truncation mechanism) is discussed neither explicitly nor implicitly in Pesetsky (1987), although the precise formulation for the truncation mechanism is crucial for the pied piping analysis. Given that Pesetsky (1987) failed to offer truncation mechanism, it is not unreasonable to conclude that the only available solution to Pesetsky (1987) is the second one, namely subadjacency violation caused by the movement of the non D-linked *wh*-phrase: non D-linked *wh*-phrases must move at LF. This amounts to saying that Pesetsky's (1987) line of analysis predicts the following condition for having both short and long elliptical felicitous answers: a *wh*-phrase must be D-linked. But, this prediction is turned out to be wrong in the following example, where a *wh*-phrase is not D-linked, although both short and long answers are felicitous.

- (9) Kimi-wa [[*dare-ga kai - ta*] hon]-o yomi-mashi-ta -ka?
 you-T who-N write-P book -A read -P -Q
 'You read books that who wrote?'
 A. Austen-desu
 '(It's) Austen.'
 B. Austen-ga kai-ta hon desu.
 -N write-P book be
 '(It's) the book that Austen wrote' (Nishigauchi's (57), (79) on p 66)

Here, as in (7), both A and B are felicitous answers. However, as we see, there is one crucial difference between (7) and (9), namely D-linking. In (7), the *wh*-phrase *dono conpyuutaa* ('which computer') is D-linked, whereas (9)'s *wh*-phrase, *dare* ('who') is not D-linked. (9) does not require a set of authors to have been established in the previous discourse. Pesetsky's analysis (1987) wrongly predicts that the answer A is not felicitous, because according to Pesetsky (1987), the non-D-linked *wh*-phrase *dare* ('who') must move at LF and this violates subadjacency. (All that the pied piping analysis tells us is that B is an felicitous answer. It does not explain anything about the felicity of A, given that Pesetsky (1987) does not have any account for truncation.) This flaw shows that the two types of binding approach in Pesetsky (1987) are not satisfactory. Nishigauchi's (1986) pied piping analysis can solve our problem, given that we have a precise formulation of the truncation mechanism. (But as I showed in the previous section, Nishigauchi's (1986) truncation conditions are rather unsatisfactory.)

How can we solve our truncation problem? In the following section, I will introduce my solution.

3. Felicity Condition. I will hypothesize the following felicity condition as a principle governing the truncation (i.e., the choice between a short form answer and a long form answer).

Felicity Condition:

A felicitous answer to a WH question must select an object from the most salient set that matches the common noun in the wh-phrase or some common noun that dominates the wh-phrase in the discourse.⁶

I assume that there is the most salient set in most situations. Then, the Felicity Condition (hereafter FC) makes the following predictions: A short form answer is felicitous when the set that the wh-phrase ranges over is the most salient in the discourse. A long form answer is felicitous when the set that the CNP dominating the wh-phrase refers to is the most salient set in the discourse. The relative saliency between the two sets is determined by the discourse/extralinguistic factors. With this FC in mind, let me go back to our truncation problem. (9), which I repeat here for convenience.

- (10) Kimi-wa [[dare-ga kai-ta] hon]-o yomimashi-ta -ka?
 you-T who-N write-P book-A read -P -Q
 'You read books that who wrote?'
 A. Austen-desu.
 '(It's) Austen.'
 B. Austen-ga kai-ta hon desu.
 -N write-P book be
 '(it's) the book that Austen wrote.'

Both A and B are felicitous answers. What are our possible candidates for the most salient set? They are "author" and "book." Under usual circumstances, it is likely that either one (i.e., book or author) can be the salient set. A is the felicitous answer when "author" is established as the most salient set. (*Austen* was picked out from the most salient set.) B is the felicitous answer when "book" is established as the most salient set. (*Austen ga kaita hon* was picked out from the most salient set.)⁷

Recall that in the previous section example (10) was shown to be problematic for Pesetsky's (1987) D-linking analysis in the previous section. Although the wh-phrase (*nani* 'what') is non D-linked, both short and long form answers are felicitous. Pesetsky's (1987) line of analysis predicts that A is infelicitous because the non D-linked wh-phrase has to move at LF and this movement violates subadjacency

In what follows, I will show that Pesetsky's (1987) analysis based on the notion of D-linking is not sufficient if the full range of data are examined. This leads to the further confirmation of the validity of the FC.

4. The FC vs. D-linking. According to Pesetsky (1987), if a *wh*-phrase is non-D-linked, then a long form answer (but not a short form answer) is predicted to be a felicitous answer. But the following example shows that this is not true.

- (11). *nani-o yoku taberu hito-ga gan-ni nariyasui no.*
 what-A often eat man-N cancer-D become easily Q
 'A person who often eats what gets cancer easily?'
- A. *karai mono desu.*
 spicy thing be
 'Spicy food'
- ??B. *karai mono-o taberu hito desu.*
 spicy thing-A eat man be
 'A person who eats spicy food.'

Notice that the answering pattern is just opposite to what Pesetsky (1987) predicts. The non D-linked *wh*-phrase *nani* ('what') has to move at LF and this movement violates subadjacency; thus the answer A must be infelicitous. But it is felicitous. My FC correctly predicts the answering pattern. It is likely that both speaker and hearer tend to focus on food rather than on a person, because their immediate concern is the food which causes cancer. They may want to know the name of food so that they can avoid eating the food (which leads to their healthy cancer-less life). Thus, the set referred to by 'food' is the most salient one in the discourse, and the answer A picks an object out of this set.

It may be argued that in the above exchange, the range of foods is rather limited in both speaker and hearer's mind. Thus, the *wh*-phrase *nani* ('what') is a D-linked one. But the fact that this line of argument cannot be maintained is shown in the following example.

- (12). *nani-o taberu hito-ga toshokan e kenkoo shyokuhin*
 what-A eat man-N library to healthy food
nitsuiteno shirabemono ni kimasuka ka.
 about research to come Q
 'A man who eats what will come to a library to do research on healthy food?'
- ??A. *shizenshyokuhin desu.*
 natural food be
 '(It's) natural food.'
- B. *shizenshyokuhin-o taberu hito desu.*
 natural food-A eat man be
 '(It's) a man who eats natural food.'

Notice that the answering pattern is just opposite to (11). If *nani* ('what') is D-linked, then A must be felicitous, but it is not. My FC correctly predicts the answering pattern. Under the usual circumstances, it is likely that both speaker

and hearer are interested in the identification of a man who will visit a library to do some research on healthy food, thus the set of men is the most salient set in the discourse and the answer picks an object from this salient set.

Pesetsky's (1987) analysis based on the notion of D-linking also predicts that if a wh-phrase is D-linked, a short form answer is felicitous, because there is no subjacency violation (i.e., the D-linked wh-phrase in situ does not have to move and Q-indexing takes place). But this prediction is shown to be wrong in the following example.

- [Gengogaku to keizaigaku to tetsugaku no uchide]
 ('Among linguistics, economics and philosophy')
- (13). dore-o oshieteita kyoojyu-ga kinou yakuzu to kennka-o
 which-A taught professor-N yesterday gangster with fight-A
 shite keisastu ni tsukamatta no
 did police by arrested Q
 'A professor who taught which was arrested by the police for fighting with a gangster?'
- ??A. Keizaigaku desu
 be
 '(It's) economics.'
- B. keizaigaku-o oshieteita kyoojyu desu.
 taught professor be
 '(It's) a professor who taught economics.'

The wh-phrase *dore* ('which') is clearly D-linked, yet the felicitous answer is the long form B, not the short form A. This is opposite to what the notion of D-linking predicts. My FC can predict that the long form answer is felicitous, because under usual circumstances, both speaker and hearer are more interested in the identification of a professor who was arrested by the police rather than that of an academic discipline. Then the most salient set is that of professors, not academic disciplines. The long form answer B is picked out of this set. On the other hand, if discourse makes the set of academic disciplines the most salient one, then the short form is predicted by the FC. Observe the following exchange.

- (At the registration site, two persons are looking at a list of classes.)
 [Gengogaku to keizaigaku to tetsugaku nouchide]
 ('Among linguistics, economics and philosophy')
- (14). Dore-o oshieteita kyoojyu kara kimi-wa A-o moratta no.
 which-A taught professor from you-T -A received Q
 'You received a grade A from a professor who taught which?'
- A. keizaigaku desu.
 '(It's) economics.'

??B. keizaigaku-o oshieteita kyoojyu desu.
'(It's) a professor who taught economics.'

In the situation where the questioner has to decide on a subject for registration, the immediate concern is a subject, not a professor. The questioner may want to find out a subject in which he/she can get a good grade without much study (if he/she is a lazy student). Thus, the most salient set is the set of subjects and the short answer A is picked out of this set.

If the most salient set can be either the common noun in a CNP or that in a wh-phrase in the discourse, then the FC predicts that both a short form answer and a long form answer are felicitous. Observe the following exchange.

- [Ford to Toyota to Honda no uchide]
(‘Among Ford, Toyota and Honda’)
- (15). dono kuruma-o utteita diiraa-ni doroboo-ga haitta no
which car-A sell dealer-D thief-N entered Q
‘A thief broke into a dealer that sells which car?’
- A. Toyota desu
be
‘(It’s) Toyota.’
- B. Toyota-o utteita diiraa desu.
sell dealer be
‘(It’s) a dealer that sells Toyota.’

A is the felicitous answer when the set matching the wh-phrase is the most salient set. B is the felicitous answer when the set matching the CNP is the most salient set.

Observations up to this point show that an answering pattern (i.e., short form or long form) cannot be accounted for syntactically as Pesetsky (1987) argues. Instead, I want to emphasize that the answering pattern is a discourse phenomenon, not a syntactic one. Thus, Pesetsky’s (1987) attempt to relate the notion of D-linking to syntax (i.e., movement/non-movement) is not only unsuccessful, but unwarranted. My FC, which is not syntactic but discourse based, can correctly account for the observed data.

The analysis based on the FC yields a welcome result. It virtually nullifies Pesetsky’s (1987) claim that non-D-linked wh-phrases move, but that D-linked wh-phrases do not have to move at LF. Against Pesetsky (1987), I will argue that there is no distinction between D-linked and non-D-linked wh-phrases in terms of movement at LF, and that all wh-phrases must move at LF. It is always nice if we can get rid of unnecessary complications in our theory.

5. Conclusion. I have shown that Nishigauchi's (1986) two explanations for the "truncation" mechanism should be rejected. Instead, the discourse based Felicity Condition (FC) was hypothesized in order to account for the "truncation." The choice of answering pattern (i.e., a short form answer matching a wh-phrase or a long form answer matching a CNP) is attributed to the set salience. A short form answer is felicitous when the common noun in the wh-phrase is determined as the most salient in the discourse. On the other hand, a long form answer is felicitous when a set matching the common noun in the CNP is determined as the most salient in the discourse. Further, the FC was shown to have some generalized explanatory force: the FC nullifies Pesetsky's (1986) two types of binding approach.

As a whole, by providing the FC as a feasible solution to Nishigauchi's (1986) incomplete truncation analysis, this paper intends to support Nishigauchi's (1986) pied piping analysis which allows us to maintain subadjacency as the locality principle at LF in Japanese. By doing this, the paper demonstrates that discourse considerations are quite relevant to syntax.⁸

NOTES

1. For comments, criticism and encouragement, I am grateful to Ileana Comorovski and Carlota Smith. All errors and oversights are mine.

2. I use the term "common noun" in Montagovian sense. A common noun is a constituent headed by a noun and denotes a set.

3. Since D-linked wh-phrases are those wh-NPs whose range of felicitous answers is limited by a contextually defined set, "who/what" can be used as D-linked wh-phrases, although they are not inherently D-linked, like "which." All examples of "who/what" that I used in this paper are non D-linked.

4. According to Baker (1970), the following question (i) can be understood in two ways, namely (i a) and (i b).

(i) Who remembers where we bought which book?

(i a) John and Martha remember where we bought which book.

(i b) John remembers where we bought the physics book and Martha and Ted remember where we bought *The Wizard of Oz*. (Baker's (1970) (67), (69) and (70) respectively)

Baker (1970) argues that the scope of wh-phrases (both moved wh-phrase and wh-phrase in situ) is represented by coindexing of the wh-phrases with initial Q morphemes in interrogative clauses. In (i a), its topmost Q morpheme is indexed with 'who', whereas in (i b), its topmost Q morpheme is indexed with 'who' and 'which book.' Notice that in (i b), the scope of 'which book' is not assigned via movement (but via indexing). The wh-phrase in situ (i.e., 'which book') does not move, thus no violation of the wh island constraint results.

5. I give one ? to A2.

6. In a discourse where no salient set is established, no infelicity results. This is observed in the following example.

- Speaker A: Doo iu shyru i no hito-ga Bentsu no supootsukaa-o kau no?
 how say kind 's man-N Bents 's sport car -A buy Q
 'What kind of people would buy a Bents sport car?'
- Speaker B: Igai to wakai hito da yo.
 unexpectedly quot. young man be tag
 'Unexpectedly, young people.'
- Speaker A: Chigau yo boku-ga kiiteiru no-wa shyokugyou da yo.
 different tag I-N asking 's-T occupation be tag
 'No, what I am asking for is an occupation.'

The first utterance of speaker A fails to specify the set from which the answer must pick an object. Thus, speaker B's utterance is felicitous. Although it is felicitous, it has to be repaired by speaker A's second utterance, which establish the most salient set (i.e., a set of occupation).

7. Kuno and Masunaga (1986) hypothesized that the choice between the short form answer (referring to the *wh*-phrase) and the long form answer (referring to CNP) depends on the questioner's primary interest. This appears to yield the same result as my FC does. However, the crucial difference between K&M (1986) and my FC is that K&M (1986) refer only to the questioner, whereas my FC refers to both questioner and hearer. Thus, for example, in the situation where the questioner's primary interest is in the object which the *wh*-phrase asks about, but the hearer's primary interest is in the object to which the CNP refers K&M (1986) wrongly predict that the short form is acceptable. My FC correctly predicts that it is not, because the set saliency is not established in the discourse.

8. The analysis proposed in this paper suggests the "weak interaction" between syntax and discourse claimed in Crain and Steedman (1985:325): syntax independently "proposes" alternatives (i.e., a short form answer matching a *wh*-phrase and a long form answer matching a CNP via pied piping), while discourse "disposes" among these alternatives (i.e., the FC).

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Morphemic Planes and Templates in Keley-i

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In Keley-i, a Phillipine language (Hohulin and Kenstowicz, 1979; Kenstowicz and Kisseberth, 1979), verbal morphology in non-past (future and present) tenses exhibits a complex pattern of behavior which is partly determined by the shape of the verb root and partly determined by the type of prefix attached. I propose that this behavior can be explained easily given (i) the Morphemic Tier Hypothesis (McCarthy, 1979) which requires that separate morphemes appear on separate phonological planes, (ii) that morphological operations and templates are supplied to planes, with the possibility for more than one template to each receive its own template and (iii) a recent proposal concerning plane conflation (Schlindwein, 1988) which states that planes must be conflated if there is a rule of phonological spread between them.

The organization of this paper is as follows. First, I will describe the different patterns of non-past verbal morphology. Next, a proposal for the non-past template and morphological operation will be motivated. Finally, the analyses of the different patterns of behavior in the non-past tenses will be given.

1.0 Keley-i Non-Past Morphology

The appearance of Keley-i verb roots in non-past (future and present) tenses is dependant on two criteria. First, it is dependant on the sequence of consonants and vowels in the root and the type of vowel that appears in the intial syllable. Verb roots can be divided into three groups on this basis: CVCCVC, CVCV(C) and CeCV(C). Second, the type of prefix added will affect the form of the root. Three different patterns which are dependant on the type of prefix will be analyzed here. Pattern one is observed with the prefix [ʔum], which indicates subject focus, and the prefix [ka], which indicates present tense:

	CVCCV(C)	CVCV(C)	CeCV(C)
root	duntuk 'punch'	dilag 'light'	behat 'cut rattan'
future, subj. focus	ʔum-duntuk	ʔum-dillag	ʔum-bebhat

present,
ref. focus ka-duntuk-i ka-dillag-i ke-bebhat-i

In the non-past for these forms, CVCCVC roots undergo no change, CVCVC roots will double the medial consonant while CeCVC roots copy the initial consonant after the [e].

Pattern two is seen with the prefix [ʔi]:

	CVCCV(C)	CVCV(C)	CeCV(C)
root	galgal	bitu	hepun
	'chew'	'put'	'break a stick'
future, stative	me-ʔi-ggalgal-an me-ʔi-bbittu?-an me-ʔi-hhehpun-an		

This pattern is similar to that above, with an additional change that the initial consonant in each group is doubled. Note that the doubling of the initial consonant cannot be considered to be solely the property of the prefix [ʔi], because in the past tense, this prefix is used without a doubling of the initial consonant. Doubling of the initial consonant with [ʔi] occurs only in the non-past tenses, the same tenses that double the medial consonant in CVCVC roots and copy the initial consonant in CeCVC roots:

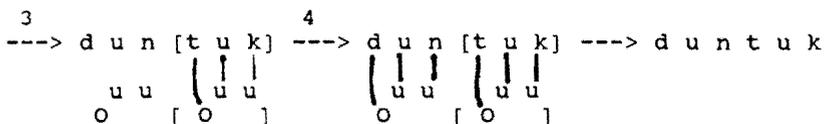
past, stative ne-ʔi-galgal-an

Finally, pattern three is observed with the prefix [me]:

	CVCCV(C)	CVCV(C)	CeCV(C)
root	duntuk	gubat	beka?
	'punch'	'fight'	'dig'
future, contrastive ident.	me-nuntuk	me-nubbat	me-mka?

This pattern is also similar to the first pattern except that the initial consonant of each root is nasalized. Also, CeCVC roots do not copy the initial consonant as expected, and the [e] does not appear.

Although these patterns appear complex, a unifying

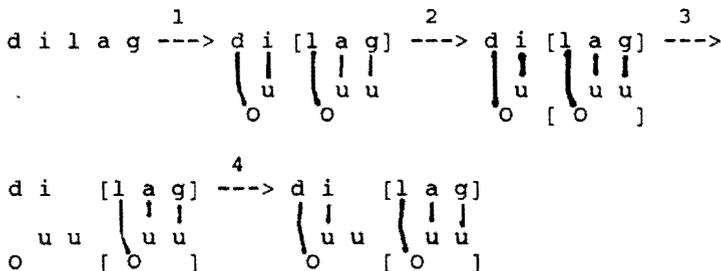


Some further explanation is needed to account for CVCV(C) roots. Mapping into the Ou_1u_2 template is considered to be right to left. With $CV_1CV_2(C)$ roots, right to left mapping would allow V_1 to map into the u_2 mora of the template. If this vowel also maps into the u_1 mora, CVVCVC forms are expected in the non-past tenses.

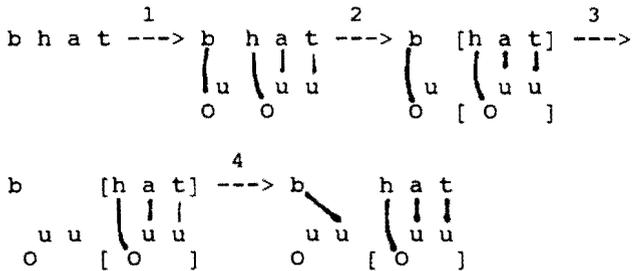
However, this problem can be avoided by recognizing that there are no long vowels anywhere in Keley-i. Thus, it appears that Keley-i does not allow vowels to appear in u_2 position. A simple filter can be posited which avoids the above mapping problem. Mapping of CVCV(C) roots would proceed as follows:

CVCV(C): dilag

1. Syllabify
2. Final Syllable Extrametrical
3. Supply Template
4. Map One to One Right to Left

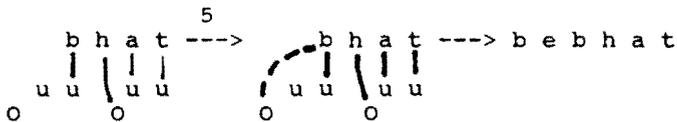


This mapping leaves an open mora. This mora can be filled by allowing the onset of the succeeding syllable to spread into this position. This would occur after this syllable is no longer considered extrametrical and once again is visible to the phonology. The correct doubling of the medial consonant is generated:



The consonant in u_2 position will then spread to create the onset of the syllable, fulfilling the requirement that all syllables must have an onset:

5. Spread to Onset Position



In summary, in this section a bimoraic syllable has been proposed as the template which will derive the form of the root non-past tenses. Mapping into this template is one to one right to left. The final syllable of each root is marked as extrametrical. In addition, CeCV(C) roots are considered to be underlyingly CCV(C) roots. It was also suggested that the morphological template is supplied to a plane. This will be discussed more fully in the following section.

3.0 Morphological Templates are Supplied to Planes

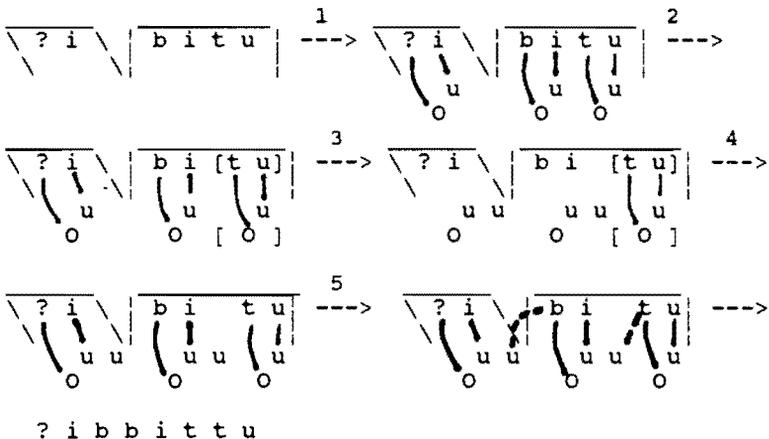
As seen above, with the prefix [?i] (pattern two), the initial consonant of the root is doubled in the non-past. This extra consonant is added in addition to the other changes that occur with CVCV(C) and CeCV(C) (CCV(C)) roots.

By allowing the morphological template to be supplied to a plane, and the morphological operation which derives the non-past to occur on a plane by plane basis, the doubling of the initial consonant can be derived quite easily. When the bimoraic syllable template is added to the prefix plane, the [i] will map into the u_1 position (it cannot map into the u_2 position

because of the constraint mentioned above). This leaves an open mora, which can be filled by consonant spreading. A sample derivation for a CVCV(C) root is given below:

CVCV(C): bitu 'put'

1. Syllabify
2. Final Syllable Extrametrical
3. Supply Template
4. Map One to One Right to Left
5. Spread Consonant to Open Mora



It is important to note that the prefix, being monosyllabic, will not be marked as extrametrical. This parallels stress assignment in languages which use extrametricality; monosyllabic words are not exempt from stress assignment.

At this point, the derivation of the form of the roots in pattern one should be discussed. With the prefix [ka], it would seem that supplying the template to the prefix plane would generate the wrong form. Because this prefix is an open syllable, doubling of the initial consonant is expected in the same way that the doubling of the initial consonant was derived for the prefix [?i]. However, if [ka] is added to the root after the morphological operation which generates the non-past tenses, the correct form is obtained. At the point when the non-past is derived, only the root plane is present

internal changes. Prefixes added after the non-past morphology will not be supplied a template, and the initial consonant of the root will not double.

Both patterns one and two have been accounted for. In the next section, pattern three will be examined.

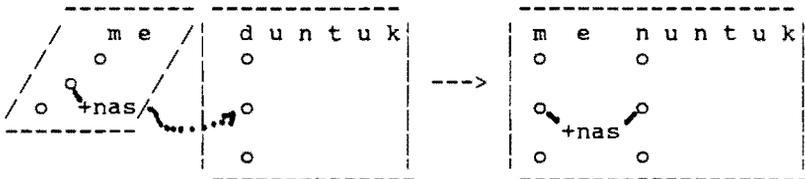
4.0 Feature Spreading, Plane Conflation and the Non-Past Template

Pattern three was seen with the prefix [me]. As mentioned above, with this prefix, the initial consonant of the root nasalizes. Also, CeCV(C) (CCV(C)) roots do not copy the initial consonant as they do in pattern one.

This behavior of CeCV(C) (CCV(C)) roots cannot be accounted for by ordering the addition of the prefix before or after the non-past morphology. If [me] is added after the non-past morphology, the roots should appear exactly as they do with [ka] above. The initial consonant of the root should copy after the [e]. If the prefix is added before the non-past morphology, then the pattern should be the same as that with [ʔi], with the doubling of the initial consonant as well as copying the initial consonant after the [e].

The clue to solving this problem is found in the nasalization of the initial root consonant. Following Archangeli (1987), I propose that this change can be captured by a rule which spreads [+nasal] from the initial consonant in the prefix to the initial consonant in the root. The [e] is considered to be unspecified for all features, which allows this to occur.

The spreading of the feature [+nasal] is across two planes. A recent proposal in Schindwein (1988) suggests that phonological features cannot be shared between segments lying on different planes. A rule which spreads features between planes causes these planes to conflate, and they become one plane. The spreading of the feature [+nasal] between the prefix plane and the root plane, then, will cause these two planes to become one:

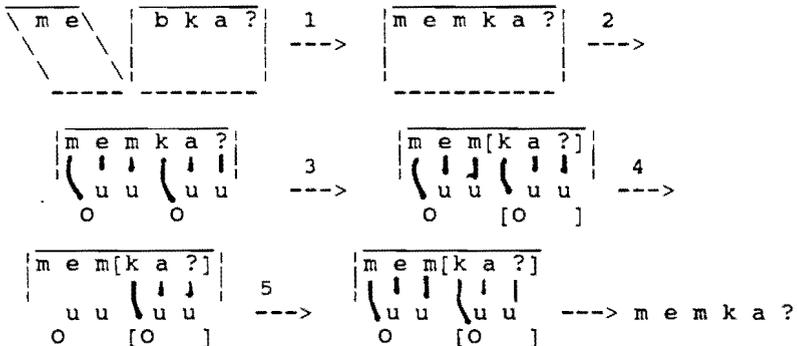


- o Root node
- o Supra-Laryngeal node
- o Place node

The morphological operation for the non-past will occur after plane conflation. Consequently, there will be only one plane and the non-past template will be supplied to only one plane. However, this plane will contain phonological material from the prefix and the root. It is because the prefix and root phonological information is on the same plane that CeCV(C) (CCV(C)) roots do not behave as expected:

CCV(C): bka? 'dig'

1. Spread Nasal; Planes Conflate
2. Syllabify
3. Final Syllable Extrametrical
4. Supply Template
5. Map One to One Right to Left



Here, again, we see the need for morphological templates to be supplied to planes.

5.0 Conclusion

The varied appearance of Keley-i verb roots in non-past tenses has been reduced to an interaction between morphemic templates and morphemic planes. A bimoraic syllable template has been used to generate all the

forms; variation occurs because this template is supplied to different planes. Keley-i shows that morphemic templates are supplied to planes, with the possibility for more than one plane in a polymorphemic word to under a morphological operation and receive a template. Furthermore, this language also supports the proposal of Schindwein (1988) that feature spreading between planes causes those planes to conflate.

I would like to thank Diana Archangeli, Mike Hammond, James Myers and Kyoko Yoshimura for helpful comments and suggestions.

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**Wh-Phrases: Substitution to Spec of CP or Adjunction
to IP?**

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0. Introduction

Chomsky (1986) rules out adjunction to IP of operators of the wh-phrase type. Chomsky's stronger condition against adjunction of wh-phrases to IP seems to be motivated by the impossibility of (1)*who_i thinks that who_j John loves x_j?

if structures such as:

(2) Bob thinks that Mary_i John loves x_i

are derived by adjunction to IP.

We will provide evidence here that in Tuki and Duala, both Bantu languages of Cameroon (West Africa), wh-items adjoin sometimes to IP instead of substituting to Spec of CP. While Lasnik & Saito (1984) and Rudin (1988) have indicated that Polish adjoins wh-words to IP, they fail to provide an explanation as to why that operation is licensed in some languages and disallowed in others. We will offer an account (based on subcategorization) which will be argued to have implications for language acquisition.

1. Tuki

Wh-movement in Tuki is optional. The questioned constituent may remain in situ or move to pre-IP position:

- (3) a. Mbara a nyam ate
Mbara SM eats what
"what does Mbara eat?"
b. Ate Mbara a nyam
what Mbara SM eat
"what does Mbara eat?"

1.1. Indirect Questions

The wh-item in an indirect question can be either what Baker (1970) calls the yes-no particle ngi "if, whether" or one of the wh-words:

- (4) a. Puta a t idzima ngi aneme waa a nw
Put a SM neg know whether husband her SM fl

aram nambari
come tomorrow

"Puta does not know whether her husband will
come tomorrow"

b. Mbara a sesam mwana waa ni a nu suwam
Mbara SM asks child his when SM fl wash
tsono
clothes

"Mbara asks his child when he will wash his
clothes"

Tuki has a lexical complementizer ee "that": it appears in pre-IP position of clauses introduced by verbs such as say. The presence of ee "that" is compulsory, unlike its English counterpart.

(5) Mbara a m(u) udza *(ee) Puta a nu nambam cwi
Mbara SM pl say that Puta SM fl cook fish
"Mbara said that Puta would cook fish"

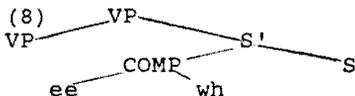
The lexical complementizer ee "that" can occur in the same clause as a preposed wh-item in a Tuki indirect question:

(6) a. Mbara a sesam mwana waa ee ni a dzumeta
Mbara SM asks child his that when SM comes
"Mbara asks his child when he will come back"
b. Mbara a sesam ee ate Puta a nambam
Mbara SM asks that what Puta SM cooks
"Mbara asks what Puta cooks"

Sentences such as (6) beg the question as to what is the exact structure of CP in the language. Notice that in (6), Tuki seems to violate the Doubly-Filled COMP Filter as stated below:

(7) Doubly Filled COMP Filter (DFC Filter)
* $[\text{COMP } X^{\text{max}} \text{ complementizer}]_{\text{comp}}$
(where X^{max} and complementizer are both filled)

If the wh-phrase is considered part of COMP, COMP in (6) would have the following structure:



The structure exhibited in (8) is surprising in that languages which generally violate the DFC Filter order the elements in COMP as follows:

(9) wh ee

Thus in Old English and Dutch, one obtains sentences of this kind:

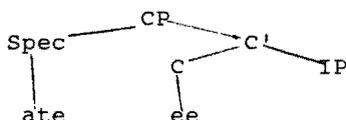
(10) John wonders [who if] will come (English gloss)

In Middle English clauses having both a WH and a that occur frequently (see Bresnan 1970):

(11)a. Til it was noon they stoden for to see who
that her com- Chaucer
b....to know yf that any planete be directe
or retrograde- Astrolab.

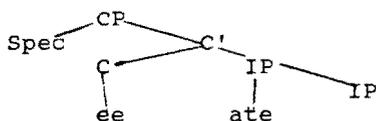
Adopting the Barriers'system (Chomsky (1986)) will not solve our problem right away. That is Barriers provides an analysis for (10), (11), but not for Tuki's case. In (6b) since ate "what" has been moved in Syntax, it is presumably in the Spec of CP while the lexical complementizer ee "that" is under C. But then if the structure is:

(12)



we should have the order ate ee "what that". That is not the case in Tuki. So, it must be the case that ate in (6b) is adjoined to IP:

(13)



Now what evidence is there that Tuki wh-items adjoin sometimes to IP instead of substituting to Spec of CP? Tuki exhibits constructions in which a relative and an interrogative wh-element have been fronted:

(14) Puta a dingam [mutu [odzu_i[andzu_j[x_i a benam
x_j]]]]
Púta SM loves man who whom SM hates
"whom does Puta like the man who hates?"

Assuming that the specifier and the head, Spec of CP and C, are occupied respectively by odzu and andzu, then the above sentence is expected to be both

interrogative and noninterrogative (relative), an expectation which is obviously counterfactual since the sentence is a question. We have to posit that odzu is in Spec CP and andzu is neither in Spec CP nor in C, but is adjoined to IP.

2. Duala

The Duala empirical material discussed here is almost exclusively borrowed from Epée (1976).

2.1. Direct Questions

As in Tuki, wh-movement in Duala is optional: wh-words may move or remain in situ. When the wh-element is preposed to clause-initial position, the invariant marker no must be present after the first verbal element of the clause¹.

- (15)a. o bodi nja moni?
 you give who money
 "who did you give the money?"
 b. nja o bodi no moni?
 who you give money
 "who did you give the money?"

The presence of the particle no is compulsory in sentences in which wh-items occur in pre-IP position; similarly the presence of no is ruled out in sentences in which preposing has not applied.

- (16)a. *o bodi no nja moni?
 you give who money
 b. *nja o bodi moni?
 who you give money

The (a) sentence in (16) is ungrammatical because of the presence of the unwanted particle no, whereas the (b) sentence is ruled out because it lacks that particle. Epée (1976) argues that no is inserted in Duala as a reflex of any rule that moves a category past the verb to pre-IP position.

2.3. Indirect Questions

The yes-no question particle nga or the other wh-phrases appear in indirect questions. Notice that nga does not trigger no insertion.

- (17) na si bi nga a menda po
 I not know if SM fut come
 "I don't know if he will come"

While preposing is optional in direct questions, it is compulsory in embedded contexts:

- (18)a. baise mo njika ponda Madiba a busi no

ask him wh- time Madiba SM go-out
 "ask him at what time Madiba went out"
 b.*baise mo Madiba a busi njika ponda
 ask him Madiba SM go-out wh- time

(18b) is ungrammatical because the selectional requirements of the verb baise "ask" are not satisfied. That verb subcategorizes for a [+wh] item. We saw above that the lexical complementizer nga occurs in embedded contexts as below:

(19)a.na si bi nga a mende tilea mba
 I not know if SM fut write me
 "I do not know if he'll write me"
 b.Kuo a baise Njo nga a tondi di data
 Kuo SM asks Njo if SM like that picture
 "Kuo asks Njo if he liked that picture"

Let us now turn to cases that are crucial for the topic of this paper. In Duala, nga can cooccur side by side in a sentence with a fronted wh-item:

(20)a.[nga [nja o bodi no moni]], langwea mba
 if who you give money tell me
 "tell me who you gave the money to"
 b.[nga [onola nje o bo no mo]], na si bi
 if for what you kill him I not know
 "why you killed him, I do not know"

The presence of the particle no indicates that there has been wh-fronting in all the constructions above. Assuming that the moved wh-element goes to Spec of CP, where is nga located? This question brings us inevitably to inquire about the structure of CP in Duala. As shown extensively above, Duala moves wh-phrases to a position adjacent to the yes-no question particle nga. Now before we determine exactly what the structure of CP looks like in this language we have to ask ourselves whether there are other elements that are moved to CP apart from those that we have already seen.

Duala has a lexical complementizer na which means "that" and which functions almost exactly like its English counterpart that or French que. However, Na cannot be omitted in Duala where it is required.

(21)a.Esombe a ta a kwala na a mende po
 Esombe SM past SM say that SM will come
 "Esombe said that she would come"
 b.muleedi a pula na bautu bao b-okwe
 teacher SM want that pupils his they-study

"the teacher wants his pupils to study"

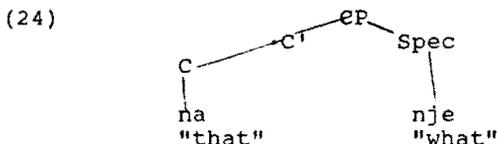
Na can occur in the same clause as a fronted wh-item:

- (22)a. na baise mo [na [njika buna [a wu no]]]
 I ask him that wh- day SM return
 "I asked him when he returned"
 b. na si bi [na [nje [ba domise no]]]
 I not know that what they decide
 "I do not know what they decided"

Moreover nga "if, whether", the lexical complementizer na "that" and a wh-word can all form an adjacent string in the same clause:

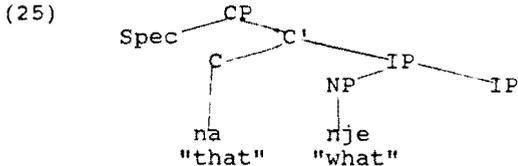
- (23)a. [nga [na [nje [ba domise no]]]], ba si
 if that what they decided they not
 langwedi Kuo
 tell Kuo
 "they did not tell Kuo what they decided"
 b. [nga [na [nje [Dikoso a kwadi no]]]], na si
 if that what Dikoso SM say I not
 bi
 know
 "what Dikoso said, I do not know"

Assume that Duala na "that" is in C of CP like English that, French que and Tuki ee. What about the alleged moved wh-element nje "what"? What is its position? It is generally assumed that syntactically moved wh-phrases land in Spec of CP. Given the constructions exhibited in (23), if we posit that nje "what" lands in Spec of CP, then we are claiming that CP in Duala has the following structure:

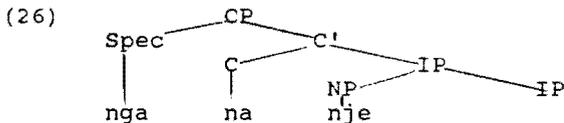


The structure in (24) is at best ad hoc. For one thing, there is no SVO language that we know of which positions its SpecCP to the right of CP (and Duala is an SVO language). Furthermore the above structure makes the prediction that Duala is a left branching language, a prediction which is untenable. It seems then to be the case that in (23) the wh-

phrase nje "what" adjoins to IP:



We are now left with the problem of the yes-no particle nga "if, whether". Recall that we said above that nga occurs clause initially in both direct and indirect questions. Phonologically, direct questions in which nga is used have the rising intonation characteristic of genuine direct questions in Duala. This may suggest that while wh-phrases in this language adjoin to IP, nga is generated under Spec of CP as follows²:



The above structure seems to be motivated by sentences such as the ones below (27).

- (27)a. Dikoso a baise[nga[na[nje [Esombe a tondi no]]]]
 Dikoso SM ask if that what Esombe SM like
 "what does Dikoso ask that Esombe likes?"
 b. Esombe a tondi moto [n-[ena [nja [a bi no]]]]
 Esombe SM like man who(relative)whom SM know
 "whom does Esombe like the man who knows?"

In (27a) nga and a complementizer co-occur with and precede a wh-item. (27b) displays the co-occurrence of relative and interrogative wh-phrases. We cannot assume that in (27b) the head and Specifier, C and SpecCP, reflect the interrogative nature of the construction; both wh-elements cannot occupy the specifier position in this construction: rather than being both an absurd interrogative and relative construction, (27b) is a question. Consequently, we have to say that n-ena is in Spec of CP whereas nje is adjoined to IP as illustrated below:

- (28) Esombe a tondi moto [Cpn-[ena[IPnja[IP a bi no]]]]
 Esombe SM like man who whom SM know
 "whom does Esombe like the man who knows?"

To claim that nga "if, whether" is in Spec of CP in (26) leads us to equate it with the English whether, which unlike the Romance (French and Italian) si/se "if" and English if, occupies a specifier position:

- (29) a. Bill does not know whether to go to Cameroon.
 b. *Darleen does not know if to go to the movies.

The contrast between (29a) and (29b) is accountable under the view that if is a C of CP and whether is a Spec of CP. It can be observed that in Tuki and Duala lexical complementizers and yes-no particles can occur side by side with extracted wh-elements. This may be one of the reasons why adjunction of wh-phrases to IP is licensed.

In the next section, we will bring up another argument in favor of the assumption that wh-phrases can adjoin to IP in Tuki. It will be shown that selectional restrictions in the latter language can be satisfied either at S-structure or LF, contra the situation which prevails in English and French where selectional requirements may be met only at S-structure. We will argue that the subcategorization facts of Tuki are accountable under the view that adjunction of wh-items to IP is a possibility. Since English and French are bereft of that possibility, their selectional restrictions may be satisfied at one level: namely in the Syntax. This state of affairs will be argued to have implications for language acquisition.

3. Selectional Restrictions

In this section, we consider at what level(s) of the grammar selectional restrictions are met in Tuki. Following Bresnan (1970, 1972) and Baker (1970), we assume that the feature +WH introduces direct and indirect questions, and -WH introduces the other complement clauses and relatives. This feature must be realized in COMP at the appropriate level. Consider, then, the following Tuki verbs:

- (30) a. obungana "to think"
 b. osesa "to ask, to wonder"
 c. widzima "to know"

- (31) a. John a bunganam [ee [Mary a ma kusa yendze]]

- John SM thinks that Mary SM p2 buy house
 "John thinks that Mary bought a house"
 b.*John a bunganam [ate [Mary a ma kusa]]
 John SM thinks what Mary SM p2 buy
 c.John a bunganam [ee [Mary a ma kusa ate]]
 John SM thinks that Mary SM p2 buy what
 "what does John think that Mary bought"

The Tuki verb obungana 'to think' does not subcategorize for questions. Its subcategorization frame is as follows:

- (32) obungana [--[-wh]]
 "to think"

The Tuki verb osesa 'to ask, to wonder' subcategorizes for questions:

- (33)a.John a sesam [ate [Mary a ma kusa]]
 John SM asks what Mary SM p2 buy
 "John asks what Mary bought"
 b.*John a sesam [ee [Mary a ma kusa yendze]]
 John SM asks that Mary SM p2 buy house
 c.John a sesam [ee [Mary a ma kusa ate]]
 John SM asks that Mary SM p2 buy what
 "John asks that Mary bought what"
 d.John a sesam [ee [ate Mary a ma kusa]]
 John SM asks that what Mary SM p2 buy
 "John asks what Mary bought"

Osesa 'to ask, to wonder', then, has the following subcategorization feature:

- (34) osesa [--[+wh]]
 "to ask, to wonder"

This feature, it appears, may be satisfied either at S-structure or LF. In (33 a.), selection is met at S-structure, and in (33 b.), selection is not met; consequently (33 b.) is ungrammatical. Sentence (33 c.) shows that even if the subcategorization feature is not satisfied at S-structure, the corresponding sentence is acceptable if the feature can be satisfied at LF. We assume that ate in (33 c) raises at LF to yield the following LF-representation:

- (33 c') John a sesam [ate [ee [Mary a ma kusa]]]
 John SM asks what that Mary SM p2 buy

In (33 d) selectional restrictions are met at LF. So, selectional restrictions for the verb osesa 'to

ask, to wonder' are satisfied either at S-structure or at LF. If they are met at S-structure first, no problem arises. If they are not satisfied at S-structure, then they must be satisfied at LF. Notice that according to our analysis ate 'what' in (33 d) is adjoined to IP at S-structure, then at LF it moves to CP to satisfy the subcategorization frame of the verb osesa "to ask". The verb widzima 'to know' seems to subcategorize for questions and ee-complements:

- (35) a. John a ti dzima [ee [Mary a ma kusa
John SM Neg know that Mary SM p2 buy
yendze]]
house
"John does not know that Mary bought a house"
b. John a ti dzima [ate [Mary a ma kusa]]
John SM Neg know what Mary SM p2 buy
"John does not know what Mary bought"
c. John a ti dzima [ngi [Mary a ma kusa
John SM neg know whether Mary SM p2 buy
yendze
house
"John does not know whether Mary bought a
house"]

The subcategorization frame of the verb widzima 'to know' is as follows:

- (36) widzima [--[±wh]]
"to know"

In (35 a), it subcategorizes for [-wh] at S-structure, while in (35 b) and (35 c), it subcategorizes for [+wh] at S-structure. To summarize, we can say that selectional restrictions are met in Tuki either at S-structure or at LF. If they are not satisfied at S-structure, they are satisfied at LF. Furthermore, once selectional restrictions are met for a given verb, no changing from one level to another is possible. Let us illustrate briefly, for the sake of clarity, how selectional restrictions can be satisfied at two levels in Tuki.

- (37) a.) Bill a sesam [ane [Betty a nu banam]]
Bill SM asks who Betty SM f1 marry
"Bill asks who Betty will marry"
b.) *Bill a sesam [ee [Betty a nu banam David]]
Bill SM asks that Betty SM f1 marry David
c.) Bill a sesam [ee [Betty a nu banam ane]]
Bill SM asks that Betty SM f1 marry who

"Bill asks that Betty will marry who"

Bear in mind that 'ask' subcategorizes for +wh elements and not for -wh-items. (37 a) is licit because the selectional requirements of the verb 'ask' are met at S-structure. (37 b), however, is ruled out since the subcategorization frame of the verb is [--[-wh]] in that sentence, contrary to reality. Given the ungrammaticality of (37 b), we would expect (37 c) to be also disqualified. As it turns out the latter construction is acceptable, suggesting that the verb 'ask' in Tuki has to wait until the level of LF for its selectional needs to be satisfied:

(38) LF representation of (37c) (irrelevant details omitted)

Bill a sesam [ane_i [ee [Betty a nu banam x_i]]]

The above facts constitute prima facie evidence that some Tuki verbs can meet their selectional requirements either at S-structure or LF. Is it unreasonable to suggest that selectional restrictions can be satisfied in Tuki at two different levels of representation because Wh-Movement in this language licenses adjunction of wh-elements to IP? Two very important facts are well known about English and French: selectional restrictions must be satisfied at S-structure and adjunction of operators of the wh-phrase type to IP is strictly disallowed. In view of the Tuki, English and French contrasts, there seems to be a correlation between the satisfaction of selectional requirements and adjunction possibilities of wh-elements. It is plausible to posit that the subcategorization phenomena of Tuki follow directly from the adjunction possibilities of wh-elements in this language. On a purely intuitive basis, since there is no evidence that children have trouble acquiring the constructions discussed in this paper, it seems to be the case that once they determine how the subcategorization frames of certain verbs are met in a given language, then they must infer that this language (does or does not) license adjunction of wh-phrases to IP.

Notes

1. No is not a resumptive pronoun since it does not agree in noun class with the noun phrase that has been extracted. Also no appears with non-referring expressions. Moreover Duala does not violate Subjacency, suggesting thereby that a resumptive analysis

QUANTIFIER SCOPE AND THE CASE THEORY

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0.0 Introduction

Aoun and Li (1989) (hence forth A &L) attempt to construct the grammar of quantificational phrases (QPs) in English and Mandarin Chinese in terms of a Minimal Binding Requirement (MBR) and the Scope Principle given in (1) and (2).

- (1) Minimal Binding Requirement
Variables must be bound by the most local potential A-bar binder.
- (2) The Scope Principle
A quantifier A has scope over a quantifier B in case A c-commands a member of the chain containing B

Consider the following QP structures in English and Chinese.

- (3) a. *Someone* loves *everyone*
b. *Everyone* saw *someone*
- (4) a. *Meigeren* dou xihuan yige nuren
'everyone all like one woman'
b. yaoshi *liangge ren* zhaodao meige xiansuo...
if two men found every clue...
'If two men found every clue...'

Qps in English given in (3) get an ambiguous interpretation. (3)a is interpreted either as (i) there is someone who likes everyone or (ii) everyone is liked by someone or other. The LF representation of (i) and (ii) would be:

- (5) a. [everyone_j [someone_i [t_i loves t_j]]]
b. [someone_i [everyone_j [t_i loves t_j]]]

In contrast QPs in Chinese as given in (4) get an unambiguous interpretation (Huang (1982), A & L (1989)). (4)a can only mean that each person loved one woman or the other but does not assert that they loved the same woman. If it happened that they loved the same woman, it would be a matter of coincidence and not the message intended by the speaker. In

order to get the second reading where 'one woman' gets the wider scope, one has to prepose it or topicalise it (Huang (1982)). A & L trace back the different behavior of QPs in English and Chinese to their constituent structure. Following Kitagawa (1986), Koopman & Sportiche (1985), Kuroda (1985), Speas (1986), and Zagana (1982), they assume that the subjects in English are base generated in the specifier of VP. Further, they claim that Subject Raising is not available in Chinese due to the degenerate nature of its Infl. Adopting the framework of Chomsky (1986a), A & L suggest that the degenerate nature of Infl prevents V-raising and subsequently Subject Raising (for further details see Chomsky (1986a) and A & L 1989)).

The purpose of this paper is to show that A & L's proposal for quantifier interpretation in Chinese and English is correct, but for the wrong reasons. In other words, I claim that the reason for the lack of Subject Raising in Chinese, claimed by A & L to be due to the lack of Infl, is erroneous. Further, I argue, following Koopman & Sportiche (1988 ms), that the difference in the constituent structure in English and Chinese can be accounted for in terms of the Case Theory.

Support for my claim comes from the facts from Kannada, a Dravidian language spoken in the southern Indian state of Karnataka. QP structures in this language exhibit unambiguous interpretation like in Chinese. But, unlike Chinese, Kannada has a rich verbal inflection. Given A & L's analyses, QPs in Kannada should get an ambiguous interpretation. But this is not the case. As I mentioned earlier, QPs in this language gets an unambiguous interpretation. How do we explain these facts? I argue that the presence or absence of Infl triggering V-raising has nothing to do with Subject Raising. In fact, I assume that V-raising is for purely morpho-phonological reasons. Following Koopman & Sportiche (1988 ms), I claim that the difference in QP interpretation in these languages in terms of constituent structure can better be explained by resorting to Case Theory.

This paper is organised as follows. In section 1, I give A & L's analysis of the QP structure in Chinese and English. In section 2, I show that their analysis fails to account for the QP structure interpretation in Kannada. In section 3, following

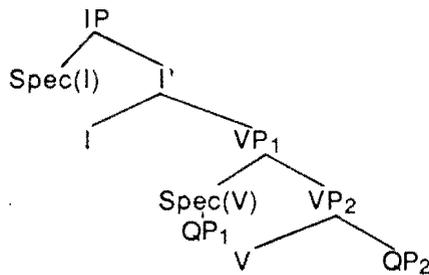
Koopman & Sportiche (1988ms), I will provide an alternative analysis for the interpretation of QP structures in these languages adopting the Case Theory. In section 4, I conclude my discussion.

1.0 Constituent structure of English and Chinese.

1.1 Constituent structure of English.

A & L, following Kitagawa (1986), Koopman & Sportiche (1985), Kuroda (1985), Speas (1986), Zagana (1982) assume the following constituent structure for English sentences in (3).

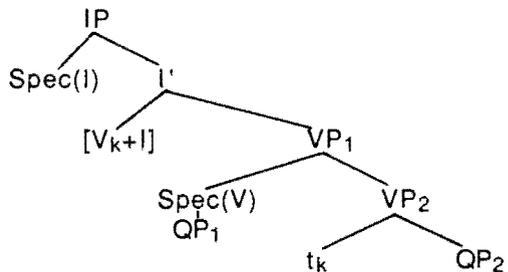
(6)



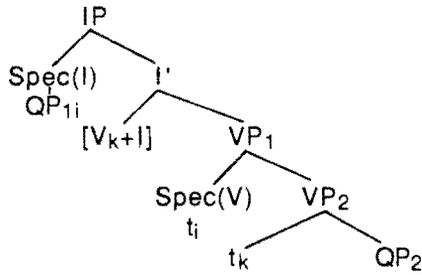
Following Chomsky (1986a), A & L assumes that the verb is raised to Infl followed by Subject Raising in Syntax as in (7).

(7)

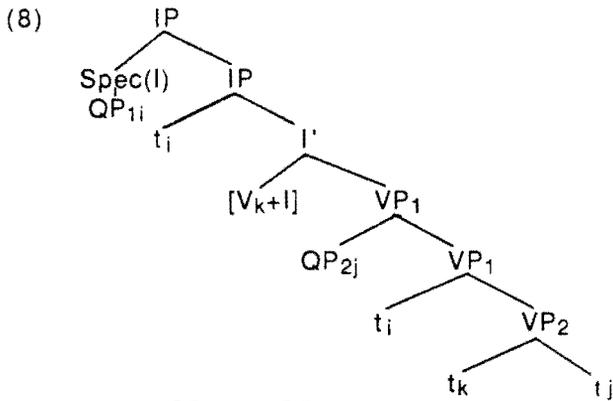
a. V-Raising.



b. Subject-Raising.



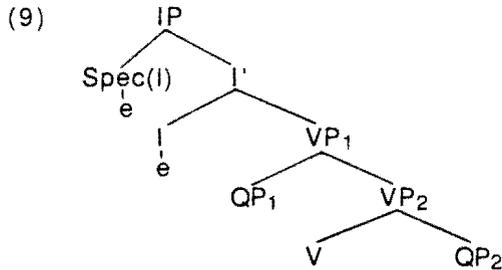
At LF, following May (1977, 1985), QP₁ is adjoined to IP and QP₂ to VP₁¹.



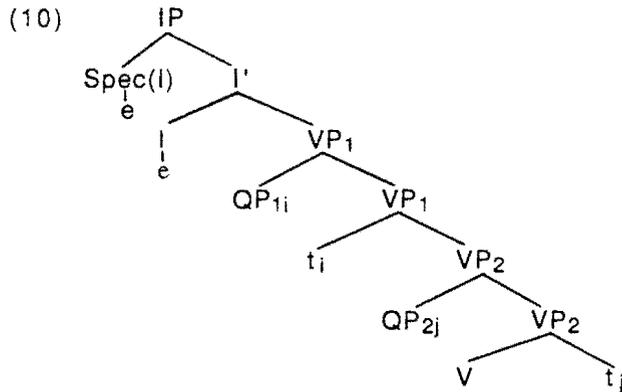
In (8), QP₁ c-commands QP₂ and QP₂ in turn c-commands the trace of QP₁. Given the Scope Principle in (2), QP₁ has scope over QP₂ and QP₂ has scope over QP₁ resulting in an ambiguous interpretation (For details, see A & L (1989)).

1.2 Constituent Structure in Chinese.

A & L assumes the following constituent structure for Chinese sentences in (4).



Given the QP structure (9), A & L argues, contrary to English that the Subject Raising does not take place in simplex sentences in Chinese due to the absence of V-raising, which they claim, can be traced back to the absence or degenerate nature of its Infl. As a consequence, they argue that VP is always a barrier for extraction and movement from Spec(ifier) of VP to Spec(ifier) of IP is not permitted. Moreover, such extraction would result in the violation of ECP. At LF, QP_1 c-commands QP_2 . Given the Scope Principle (2), QP_1 gets a wide scope interpretation over QP_2 .

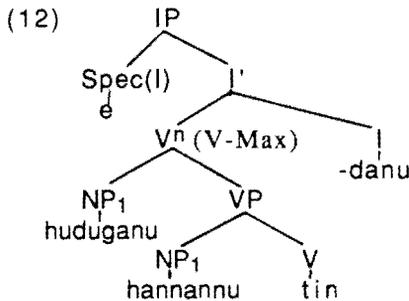


2.0 QP structure in Kannada.

Kannada, like Japanese and Korean, is a verb final language. But, unlike Japanese and Korean, it has a rich inflection. Verbs agree with the subject in person, number and gender. Consider the following sentences.

- (11) a. hudugan-u hann-annu tindanu
 boy-Nom fruit-Acc eat+pst+AGR
 'The boy ate an apple'
- b. amman-u adugey-annu madidalu
 mother-Nom food-Acc cook+pst+AGR
 'Mother cooked the food'
- c. meriy-u a pustakaw-annu odidalu
 Mary-Nom that book-Acc read+pst+AGR
 'Mary read that book'

Following Koopman and Sportiche (1988ms), I assume the following constituent structure for (11)a (to be revised)



2.1 QPs in Kannada

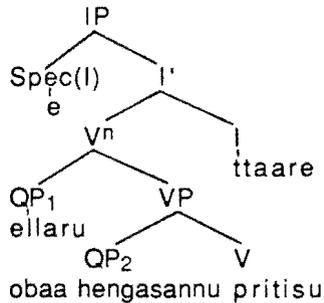
QPs in Kannada are ellaru 'everyone', kelavaru 'someone', bahala 'many' ondu 'one' etc. Sentences with QPs are given in (13).

- (13) a. ellar-u obba hengas-annu pritisuttaare
 everyone-Nom one woman-Acc loves+AGR
 'Everyone loves one women'

- b. pratiyobba manushyan-u obba
 every man-Nom one
 hengas-annu noodidaru
 woman-Acc saw+AGR
 'Every man saw one woman'

The structure of (13a) is given in (14).

(14)



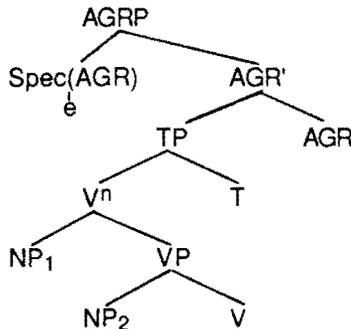
(13) a. only means that each person loved one woman or the other, but doesn't mean that they love the same woman, just like in Chinese. Given the assumptions of A & L, nothing prevents us from raising V to I in (14) since Kannada has rich inflection. Once V is raised to I, [V+I] becomes a lexical item which would then allow Vⁿ (VP₁ in A & L) not to count as a barrier for Subject Raising. This situation is similar to English. If this analysis is correct, then we should obtain an ambiguous interpretation of QPs in Kannada. As I mentioned earlier, this is not true. In the following section I will provide, following Koopman & Sportiche (1988ms), an alternative analysis based on the Case Theory that could explain the unambiguous interpretation of QP structures in Kannada.

3.0 Case Assignment and the Constituent Structure.

Koopman & Sportiche (1988ms) assume that the nominative case in English is assigned by Infl under Spec-Head agreement. Further, they also assume that in Chinese the nominative case is assigned to an NP from Infl under government. They attribute this difference in case assignment to a particular category X⁰, the head H it contains, and the

language L. Given this, Koopman & Sportiche (1988ms) argue that the tensed Infl in English only assigns case by agreement, forcing raising of Spec(ifier) of VP when it needs case. Following their analysis, I argue that Spec(ifier) of VP in Kannada gets its case structurally by government from tense and not by AGR under Spec-Head agreement in spite of having rich AGR. For the purposes of this paper I will adopt the following, highly articulated, IP structure where Infl. is not considered as one constituent with two different sets of features ([+/_ Tense, +/- Agr.]) and that instead each of these sets of features is the syntactic head of a maximal projection, AGRP and T(ense)P. (cf. Pollock (1989); Chomsky (1988ms)). The revised version of the IP structure of (12) is given in (15).

(15)



Given (15), I claim that AGR doesn't assign case under Spec-Head agreement in Kannada. Support for my claim comes from the participial relative clauses and the modal sentences in this language.

3.2 Participial relative clauses.

Consider the following sentences in (16).

- (16) a. aake- \emptyset bareda pustaka- \emptyset \checkmark ennaagide
 She-Nom. wrote book-Nom good is
 'The book she wrote is good'
 b. avanu maadida kelasa- \emptyset \checkmark ennaagide
 He-Nom. did work-Nom. good is
 'The work he did is good'

(Tirumalesh, K.V. (1979))

In (16) a & b, the relative clause aake bareda and avanu maadida contains past tense but no PNG markers. Similarly, the modal sentences in this language doesn't show PNG markers on the auxiliary verb that follows the modal as given below.

3.3 Modal sentences.

Modals in Kannada are beeku 'should', bahudau 'may', etc. Consider the following sentences.

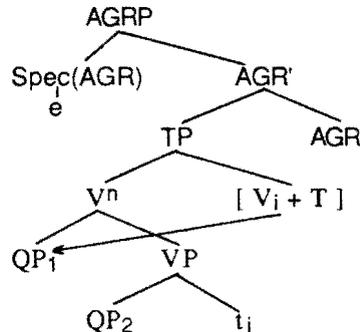
(17) a. avanu avana kelasa maaḍabeekaagittu
 He-Nom his-Gen.work-Acc do+should+be+past
 'He should have done his work'

b niivu ninne barabahudittu
 You-Nom yesterday come+may+be+past
 'You might have come yesterday'

(Tirumalesh, K.V. (1985))

In (17) a & b, the modal verbs beeku 'should' and bahudu 'may' are followed by an auxiliary verb 'ir' which is marked for tense and not for PNG. However, the subjects NP's in (16) a&b and (17) a&b are marked for nominative case. This evidence supports my claim that it is tense not the AGR that assigns nominative case in this language. Given this I claim that in Kannada, unlike English, subject is not raised at S-structure to Spec(Agr) in order to get the nominative case inspite of having a rich AGR. On the other hand, subject NP is assigned nominative case from Tense under government as shown in (18)

(18)



In (18), V is raised to Tense node at S-structure. [V + T], now a lexical element, assigns nominative case to the subject QP under government. This analysis explains why QP structure in Kannada gets an unambiguous interpretation inspite of having a rich AGR. The crucial issue here is, whether a particular language chooses to assign nominative case to its subject by AGR or Tense. If a language chooses AGR as nominative case assigner, like English, then the Subject NP is raised to Spec(AGR) where it is assigned nominative case under Spec-Head agreement. If a language chooses Tense / Aspect as nominative case assigner, then Subject-raising is blocked and the subject NP gets its case assigned by [V + T/Aspect] under government².

4.0 Conclusion.

I have shown in this paper that the difference in quantifier interpretation in English, Chinese and Kannada can be explained in terms of the Case Theory instead of the presence or absence of AGR (Infl in A & L) that allows V-raising, subsequently Subject-raising. I have argued that Subject-raising is not available in languages like Kannada and Chinese where AGR is not the nominative case assigner resulting in different constituent structures from that of English. As a consequence, QP structures in these languages get different interpretation.

Footnotes:

1. In A & L (1989), it is not clear why V₁ constitutes a maximal projection in English and not VP₂.

2. Further empercal evidence to support my claim is yet to be worked out.

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"Written in Their Hearts":
The Element of Text in Extemporaneous Pastoral Prayer

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Observations on the influence of text on text are as old as the beginnings of literary criticism; observations on the interactive influence of text and talk, however, are much newer. The two methods of discourse have been studied together in recent years, for instance at the 1981 Georgetown University Round Table on Languages and Linguistics, chaired that year by Deborah Tannen (ix-x). I borrow the phrase, "text and talk," from the name of the proceedings for that year. Studies like those have often emphasized either text or talk, for example, Basso's "The Ethnography of Writing" (425), or have traced the validity of conversational style within a literary work as does Sattel in "Men, Inexpressiveness, and Power" (120-22). This paper will instead look at some ways in which writing influences extemporaneous speech.

The languages of nonliterate societies are highly mutable. Bright (272) suggests that this language mutability exists even in those societies with a strong tradition of oral performance or oral "literature." He says, though, that as soon as a language begins to be written, the requirements of writing prolong change (273), not stopping it, but like a dam, slowing down and channeling it. Informal, intimate speech in a literate culture remains highly variable with rules which describe that variability. Writing and formal speech, on the other hand, must be learned, and their rules are therefore defined, limited, controlled, and made reasonably simple and comprehensible.

Within a literate society, especially one which emphasizes a particular written work, I suggest that text and talk are conjoined in special ways, the more formal the speaking register, the more pervasive the influence of text. A formal spoken discourse either presupposes a firm understanding of textual grammar, or the discourse is written out ahead and at least cursorily memorized before delivery. When, however, spontaneity and formality are joined in extemporaneous public speech, then the use of a mutually familiar written text within the talk becomes a highly effective means of achieving cohesiveness and mutual understanding between a speaker and an audience.

Public prayer is just this sort of discourse. Judeo-Christian prayer, especially, evidences its text base in the Bible. An analysis of actual

extemporaneous pastoral prayers with a conservative, Bible-based orientation reveals much of this kind of textual influence.

The larger version of this study examines quotes, near-quotes, paraphrases, special diction and phraseology, and grammatical forms from the King James version of the Bible in eighteen prayers spoken by seven prayer leaders. Fifteen of the prayers, spoken by four of the prayer leaders, were tape-recorded during February and March of 1987 in one church in Phoenix, Arizona. The paper demonstrates that in these speech events, text and talk are joined in such a way that they establish a crucial sense of community between the speakers and their audiences. We will look briefly at two of the prayers spoken by two of the prayer leaders, as being representative of the entire study. Before examining to the data, however, I will remark on the nature of Bible prayer itself, then touch upon the special purpose of prayer as a discourse form around the world.

Prayer is an especially polite but often intense form of what Searle calls a directive (13). A directive is sometimes as blunt as a bald command, but a prayer has embedded in it the assumption of a strong superior/subordinate relationship. Therefore, while the directive toward God the Hearer is intense, the force of the directive is alleviated with special politeness forms of the sort studied by Brown and Levinson (1978) and more recently by Lim (1988).

One of the ways force of the directive can be soothed is to remind the hearer of what he or she has said in the past. The Psalmists' prayers therefore remind God of his promises, and the New Testament biographies of Christ record his use of Old Testament quotes and paraphrases in prayer. The Jews of the Old Testament were enjoined to pattern their lives on the scriptures, to think of them when they were resting on their beds, to speak of them when they rose, and to teach them to their children (Deut. 6:6-9). Those writings therefore became an intrinsic part of their lives, nearly inseparable from conscious thought. Today, orthodox Jews and Christians alike refer to the scriptures as a source of power for eloquence and expression in prayer, and indeed, the Book of Romans says that the law is "written in their hearts" (2:15).

LeFevre (152) explains the belief of the Jewish Rabbi, Abraham Heschel, that "in prayer we confront the word, face its dignity, singularity, and potential might." Holmes (39), speaks of Christian prayer as an extension of the word, meaning, as does Heschel, their scriptures, while Gallen (xi) actually suggests that

prayer by Christians is an extension of the prayer of God. This is an interesting concept, considering the traditional, conservative perception of the Bible as the written expression of God himself, who in the Bible is called the living word (cf., John 1).

Storey (64-65) calls public prayer "cathedral" prayer. Opposed to "monastic" prayer, which is solitary and private, cathedral prayer is the prayer of a group, usually spoken by one leader, but at least theoretically participated in equally by all. Both cathedral and monastic prayer can be part of a liturgy or be extemporaneous. The practice of extemporaneous public prayer spans humankind from the choral response of African societies (cf. Shorter), through the group participation of charismatic worship, to Quaker worship with its basis in group silence, described so eloquently by Bauman in his work, Let Your Words Be Few.

The role and responsibility, then, of the prayer leader, often the pastor in Bible-based churches, is well expressed by McManus when he says, "The burden placed upon . . . the ordained ministers or clergy--to pray on behalf of the whole community . . . is a constant in church life" (137-38). The pastor must remain aware as he prays that the "the church should pray as a community, . . . the church's unity with its Head [meaning God] is such that the community of believers, praying in the Spirit, continues the prayer of Christ" (141). A most effective means to accomplish this communal sense is through the use of the kind of familiar Bible text which I will illustrate permeates my data. Following are transcriptions of the two representative prayers. Occasionally the tape recording is not clear because the tape recorder was placed in the main auditorium with the congregation. When this happens, I indicate elision by dots (...). This may indicate one word or several.

Prayer 1

SPEAKER 2 - March 1, 1987 - Evening service -- S2M1E

1 Father, we do give praise to thy name for the
2 wondrous work of grace thou hast brought to our
3 hearts. Thank you, Lord, that we have the
4 confidence that thou art with us even unto the end
5 of the age. We bless thee tonight as we come
6 together. We come in the name of Jesus Christ. We
7 ask thy blessing on the gathering and pray that thy
8 spirit may be present in power. Lord, we ask that
9 thou would bless the one who comes to break the word
10 of life. Grant, Father, that our souls may be fed
11 tonight and that those in our midst that are still
12 strangers to grace may be brought to the foot of the
13 cross this night. We pray in Jesus name. Amen.

Prayer 2

SPEAKER 3 - March 15, 1987 - Sunday School -- S3M15SS

1 Father in heaven, we thank thee for this the Lord's
2 Day, as we think of that first Lord's Day, the
3 resurrection of our Lord...and the victory of the
4 cross over sin. We thank thee for that solid rock
5 that was smitten once and does not need to be
6 smitten again, the Lord Jesus Christ. And we thank
7 thee, too, for your precious word and the privilege
8 we have of studying it and of letting it have its
9 effect upon our lives. And I pray that that would
10 indeed be the case this day. I pray for our pastor
11 as he brings us the Word this day. I pray for each
12 one of our hearts that we might be in tune, that we
13 might be...Humble us, Lord. Make us ready to
14 respond to the Word of God as the Holy Spirit
15 convicts our hearts. And I pray for the young
16 people that are coming this Tuesday from Maranatha.
17 I pray that not only the...enjoyment or
18 entertainment, but that they would be...benefit for
19 our hearts and lives, too. Father, bless this day
20 now. We pray that all these things done may be to
21 the honor and glory of our precious Lord and Savior,
22 Jesus Christ, in whose name we pray. Amen.

I touch briefly on quotes for the sake of example. The full data, however, displays quote freely.

Prayer 1/L4-5--"even unto the end of the age"--Matt. 28:20--"Even unto the end of the world." (The diction here is didactic.)

Next are two examples of paraphrase, a particularly rich category. I explicate one of the examples to show the nature of compressed meaning which can be achieved within a paraphrase.

Prayer 1/L12--"strangers to grace"--Eph. 2:12--"ye were without Christ, being aliens from the commonwealth of Israel, and strangers from the covenants of promise, having no hope, and without God in the world."

Prayer 2/L4-6--"that solid rock that was smitten once and does not need to be smitten again"--cf. Ex. 17:1-7; Num. 20:2-13; Deut. 4:21-22; 32:4; I Cor. 10:4; and Heb. 6:6. This prayer fragment is an intensely compressed paraphrase, indeed, probably a summary of the above verses.

Moses was first commanded by God to strike the rock. When he did so, water flowed out and assuaged the great thirst of the Israelites. The second time this need occurred, however, God told Moses to speak to the rock. Moses instead struck it twice, a disobedient action which earned divine judgment for him.

"Rock" is used many times in the Bible as a metaphor for God or Christ, symbolizing for the eyes of humankind qualities such as stability, firmness, and perfection. In the New Testament, the "spiritual Rock that followed them [i.e., water that flowed to the Israelites]. . . was Christ" (1 Cor. 10:4). The first striking of the rock represents to New Testament Christians the crucifixion of Christ. Since his death was "once for all" the sins of all humans (Heb. 10:10), that "rock" does not have to be "struck" again when sin is committed by Christians. All they need do is "speak to the rock," that is,

ask for forgiveness. To do otherwise is to "crucify to themselves the Son of God afresh, and put him to an open shame" (Heb.6:6).

While there are differing amounts of knowledge of the KJV among those who heard this prayer, most would still probably concur that all of the above scriptures are implied in the short passage by Speaker 3.

When I refer to the full data, the following code will refer to each prayer. S plus a number designates a particular speaker; F or M refers to February or March; the number following the month is the day of the month; the letter following the date refers to the particular Sunday meeting--M - Morning, E - Evening, and SS - Sunday School; and, following a slash, the particular line of the prayer is designated by L#. A complete reference like S2M8E/L6, then, means: Speaker 2, March 8, Evening meeting, Line 6.

The four speakers often use special diction, i.e., words which are used in an "unordinary" sense; they cannot be transferred to everyday conversation and remain in the same context. From the full data this word choice includes the following:

Appropriate (as a verb)--S4F22M/L5-6
(three times)
Burden--S2F15E/L12
Delivered--S2F15E/L12
Lost--S4F22E/L15 (twice)
Midst--(in our) S1F15M/L21; S1F22M/L16;
S2M1E/L11;(in the) S2F15/L16; (into
our) S2F15/L8
Precious--S1F15M/L2; S1M8M/L6 (twice);
S1M15M/L8,16; S2F15E/L7;
S3M15SS/L7,21
Revealed--S2F15E/L4
This (day)--S1F22M/L7; S3M1SS/L11,14;
S3M15SS/L19
(hour)--S1F15M/L1; S1F22M/L2,3-4;
S1M8M/L4;
(Lord's day)--S3M1SS/L1
(night)--S2M1E/L10
(place)--S2M15E/L10; S3M8SS/L5
(service)--S1M8M/L16
(the Lord's day)--S3M15SS/L1
(time)--S1M1M/L3; S3M8SS/L10

One of the most interesting of these word choices is the determiner "this" with a time or location designator. "This morning" and "this afternoon" are used regularly in modern American English, but the prayer leaders employ it a little differently. The determiner-plus-time or place designator often appears at or near the beginnings of the prayers and is used as a substitute for words like "today," "now," or "here." It has the effect of formalizing these simple concepts through a commonplace KJV usage, not surprising in this very formal and polite context.

The KJV translators, however, probably intended specificity when they chose "this" for the qualifier. Take, for instance, "For unto you is born this day. . ." (Luke 2:11). While the specificity would be lost, the phrase could easily be reworded "born today." The same kind of specificity might also be intended by these prayer leaders. In Prayer 2, for instance, Speaker 3 says, ". . . we thank thee for this the Lord's day, as we think of that first Lord's day, the resurrection of our Lord. . . ."

"Precious," a KJV word found an unusual number of times in these prayers, is an adjective modifying the nouns "love," "Lord," "Lord Jesus," "blood," and "word." It calls to mind the frequent use of similar adjectives (e.g., "sweet") by medieval and Renaissance Catholic devotional writers. The other special diction is all frequently found in the King James but not in current American English conversation, probably not even that used among the church group of the pastors whose prayers are being considered here.

Two special grammatical forms are prevalent in these pastoral prayers, one of which forms, the subjunctive, does not often appear its true form in modern American English. When it does, it is usually in the polite, formal context described by Brown and Levinson (201). The other form, the pronominal intimate second person singular (thee/thou/thy/thine), is now never used in everyday American English, except privately among some conservative Quaker groups. Both forms are not unusual in the KJV.

The prayer leaders probably choose the subjunctive because of the nature of prayer. The verb "pray" is used very rarely today in conversation, and when it is, it means to "ask," "request," or "entreat" in a particularly polite manner. It is now almost exclusively a religious word, employed in speaking to deity. In Latin, a directive is followed by the subjunctive, and, while English is not inflected, following Latin grammar it nevertheless provides for a subjunctive form in a dependent "that" (whether present

or buried) clause. Some examples of this form from the full data follow:

S3MlSS/Ll3-14--Bless this congregation, Lord, that
decisions . . .be made. . . .
S4F22SS/Ll1-14--I pray right now (that). . .this
be the means. . . .

One could theorize then that because the true subjunctive is used frequently in the KJV, according Harsh (46), it might also appear in the very formal context of these extemporaneous prayers. This does not prove to be so. Instead, the deeper, subconscious knowledge of current usage takes precedence. We find in place of the true subjunctive, special uses of the modal auxiliary construction. Examples of these forms from the data follow:

S1F15M/Ll1--I pray that. . .we may be open. . . .
S2M1E/L7-8--We. . .pray that thy spirit may be
present. . . .

James says that the true subjunctive in modern, spoken English is more common in North America than in Britain, but even here it is rare and diminishing. Nevertheless, subjunctive ideas remain. "Because people have the capacity to imagine the world as other than it is, they will attempt to find ways to express imagined wish and condition" (112).

In a total length of 2996 words, 53 subjunctive expressions appear in the prayers. Of those 53, only four are true subjunctives; one is a catenative "let" clause; and 48, or approximately 91%, of the subjunctive intention is expressed by the modal auxiliary construction, also the overwhelming choice for most American English expression of possibility in talk.

The second grammatical form, the archaic intimate second person singular pronoun, is probably the most immediately noticeable characteristic of these prayers, that is, the KJV feature which most obviously sets them off from other modern, extemporaneous American English speech forms. While there was a wide difference in the percentage of use of this form among the speakers, each of them included it freely. The following tables give statistics for the prayers by each speaker and for second person singular pronoun use (SPSF), both archaic (ARC) and modern (MOD), among the speakers.

A. PRAYER STATISTICS BY SPEAKER

<u>Speaker</u>	<u>Number of Prayers</u>	<u>Ttl Nbr of Words in All Prayers</u>	<u>Avrge Nbr of Words Each Prayer</u>
1	6	1398	233
2	3	451	150.33
3	3	514	171.33
4	3	633	211

B. SECOND PERSON SINGULAR PRONOUN (SPSP) BY SPEAKER

<u>Speaker</u>	<u>Total All SPSP</u>	<u>Ttl Nbr ARC</u>	<u>Ttl Nbr MOD</u>	<u>Total % ARC</u>	<u>Total % MOD</u>
1	69	38	31	55	45
2	28	27	1	96	04
3	15	7	8	47	53
4	21	6	15	29	71

C. SPSP FREQUENCY BY SPEAKER

<u>Speaker</u>	<u>One of Any SPSP Every * Words</u>	<u>One ARC Every * Words</u>
1	20.26	36.79
2	16.11	16.70
3	34.27	73.43
4	30.14	105.50

My present study of this data centers on the semantic content of the archaic SPSP forms. While originally the form was the one used toward intimates and inferiors, in the T/V sense, today its nearly exclusive use in prayer produces and maintains a highly formal, negative politeness from humans toward God. Its semantic intention is directly opposite of what it was to the seventeenth century Quakers.

The short prayers of the seven pastors presented demonstrate overwhelming textual influence. Altogether, KJV text-related features in the prayers of the Phoenix pastors and their guest constitute approximately 24% (731 words) of the total number of words (2996) in all prayers of all the speakers. While this percentage is a loose estimation, it would still seem that this must be considered a significant amount, even without correlation with other studies. I intend to do further work in this area, choosing varying topics of conversation in interview situations with members of this same group to see where, if at all, the special diction, phraseology, and grammar forms begin to be used outside the formal context of prayer,

The text-within-talk character of all these speech events may be directly and singularly related to the philosophical base of the individuals studied. It may, however, be an obvious example of a more pervasive principle, that, within a literate society, especially one which emphasizes a particular text, text becomes embedded in oral expression, emerging to supply vocabulary, from quotes to special diction and phraseology, as the formality level increases. Further study of highly text-influenced cultures, perhaps political caucuses or law firms, should strengthen this hypothesis.

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On the Fixed Syllabic Structure of Chinese¹
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0. Abstract This paper argues that Chinese languages have fixed syllabic structures. Two dialects are discussed in detail, Mandarin and Shanghai. The Mandarin syllable has a fixed length of three slots, an onset slot and two rime slots. The Shanghai syllable has a fixed length of two slots, an onset slot and a rime slot. Arguments are drawn from underspecification, feature geometry, segmental distribution, duration, tone bearing unit, reduplication, riming in poetry, and tone sandhi.

1. Mandarin In the standard Pinyin transcription (XHC 1982), the Mandarin syllable may have from one segment, V, to four, CGVC

- (1) a. e 'goose' b. tian 'sweet'

I propose below that all Mandarin full syllables have the same length of three segments, so that (1a-b) have structures (2a-b). In particular, I argue for points in (3):

- (2) a. X X X b. X X X (O = zero onset)
 | √ | | |
 O e tʃ a n

- (3) a. Every full syllable has an obligatory onset.
 b. CG- is a single onset.
 c. Every rime has two X slots.

1.1. Obligatory 'zero onset' Chao (1968:20) notes that Mandarin syllables that are not written with an onset have a 'zero onset', which has four variants

- (4) a. velar nasal /ng/
 b. velar or uvular unaspirated continuant /Y/
 c. glottal stop /ʔ/
 d. glottal unaspirated continuant /H/

The presence of the zero onset not only prevents linking between the vowel and the preceding coda, but may also assimilate the place of the coda to velar. (Li 1961:300, Chao 1968:20):

- (5) i. mian ao ----> a. mian ?ao
 'cotton coat' b. mian Yao (Y = [+dor, +cont])
 c. miang Yao
 d. miang ngao
 e. *mianao
- ii. Tiananmen ----> a. tʃan ?an men
 'Tiananmen' b. tʃan Yan men
 c. tʃang Yan men
 d. tʃang ngan men
 e. *tʃanan men

replaced in the first syllable, otherwise it should stay. Consider

- | | | | | |
|------|----|----------------------|----------------|---------------|
| (14) | a. | ie --> ne-ie | (*nie-ie) | 'grandfather' |
| | b. | niang --> nang-niang | (*niang-niang) | 'goodness' |
| | c. | xua --> xa-xua | (*nua-xua) | 'desolve' |
| | d. | guai --> nai-guai | (*nuai-guai) | 'strange' |
| | e. | hsye --> ne-hsye | (*nye-hsye) | 'snow' |
| | f. | chyan --> nan-chyan | (*nyan-chyan) | 'curl' |
- (y = [front high round])

In all cases, -G- is replaced with the onset, suggesting that it is in the onset, and not in the rime. Other Fanqie languages support the same conclusion. See Bao (to appear) for discussion.

The fourth argument is from tone bearing unit. Howie (1976) notes

- (15) the domain of tone in Mandarin is not the entire voiced part of the syllable, as is traditionally described, but rather is confined to the syllabic vowel and any segment that may follow it in the syllable. (Howie 1976, p218)

By 'syllabic vowel' Howie means the vowel after G⁻⁵, and not including G-. In his FO tracings (pp201-214, Types 2-3), it is seen that the FO contours are quite irregular on the pre-nucleus G-, but the FO contours from the nucleus vowel onwards agree with the expected contours well. All this shows that the tone bearing unit is the rime, and that G- is not in the rime.

The final argument comes from riming. Two syllables rime in Chinese if they have the same nucleus and coda; the presence or absence of the medial -G- has no effect (Qin 1975:7)⁸. For example, /Cian Cuan Cyan Can/, where the C's need not be the same, always rime. If CG- are in the onset, the explanation is simple: two syllables rime if they have the same rimes. However, if -G- is in the rime, then one has to explain why different rimes can rime.

1.3. Rime has two slots (not three or one) Three points need be shown: a). all Mandarin rimes have the same length, b). the rime has at most two segments, and c). the rime has at least two segments. We take them in turn.

First, it is well known (e.g. Woo 1969, Howie 1976) that all full Mandarin syllables have about the same duration. This is not true, however, for English, where 'heart' and 'hat' markedly differ in duration. There is then direct evidence that Mandarin rimes have the same phonological length.

Second, Mandarin vowels do not contrast in length. In other words, vowels are long in open syllables but short in closed ones. In addition, diphthongs do not occur in closed syllables. This shows that both open and closed rimes have two slots. Furthermore, when

the diminutive suffix /-r/ is added to a syllable, the original coda has to be dropped

- (16) ya --> yar yan --> yar guai --> guar
tooth edge (*yanr) staff (*guair)

All this is natural if the Mandarin rime has just two segments.

Third, the Mandarin rimes may contain diphthongs, and that codas have place contrast, e.g. /ai au; an ang/. Therefore, the rime must have at least two segments.

There is further evidence that the rime has two slots. It has been shown (Woo 1969, Lin & Yan 1988, Yang 1988) that the rime duration of a weak syllable is about 50% the rime duration of a regular syllable, while the onsets of weak and full syllables do not differ so much in length. In addition, Lin & Yan (1988) show that when a full syllable is weakened, the duration loss mostly comes from the drop of the coda or the off-glide of a diphthong. For example, when *fang* (in *ti fang* 'place') is weakened, the coda /-ng/ is dropped, and the vowel is nasalized and reduced towards schwa. In our terms, a weakened syllable loses the final rime slot. Note that coda loss need not be a universal consequence for a weakened syllable. In English, for example, it is the nucleus that seems to undergo most weakening:

- (17) a. student /dent/ --> /dnt/
b. Sunday /day/ --> /di/
c. Whitsun /sun/ --> /sn/

1.4. Summary I have argued that a regular Mandarin syllable has a fixed length of three slots, one for the onset and two for the rime, and that the medial -G- is in the onset rather than in the rime. The analysis for Mandarin applies to most other Chinese dialects. For reasons of space, we will omit them here.

2. Shanghai In the transcriptions of Zhu et al (1986), the Shanghai syllable may have from one segment, V or C, to four, CGVC

- (18) a. Minimum: e 'salty' ng 'fish' z 'word'
b. Maximum: guang 'broad' die? 'stumble' gua? 'scrape'

Shanghai has syllabic consonants /ng z/, which Mandarin does not have. Unlike for the Mandarin analysis, I propose that syllables in Shanghai have a fixed length of two slots, instead of three⁷. In particular, I propose that (18) have the structures in (19)

- (19)a. X X X X or X X X X
 | | | | | | | |
 0 e ng 0 ng z
 'salty' 'fish' 'word'
- b. X X X X X X
 | | | | | |
 g^w a[~] dʒ e[?] g^w a[?]
 'broad' 'stumble' 'scrape'

At first sight, Shanghai (18) resembles Mandarin (1). What then motivates a different analysis? There are two main reasons. First, Shanghai has no diphthongs. Second, Shanghai no longer maintains those coda contrasts that are found in Mandarin. Indeed, we will see that there is no pure coda contrast in Shanghai at all. To show that the Shanghai syllable has two slots, I argue for the following points

- (20) a. Every syllable has an obligatory onset.
 b. CG- is a single onset segment.
 c. -VN is a nasalized vowel
 d. -V? is a glottal vowel
 e. the syllabics /ng z/ are onset-rime geminates

The arguments for (20a-b) are the same as for Mandarin. We take (20c-e) in turn.

2.1. -VN is one slot In Mandarin, nasal codas /-n -ng/ are contrastive. In Shanghai, however, there is no /-n -ng/ contrast. Zhu et al use /-ng/ for the nasal coda after all nuclear vowels. In narrower transcriptions (Chao 1928, Xu et al 1981), the nasal coda is written /-n/ after /i/, /-ng/ after /o/, and /~/ after /a/, i.e. homorganic to the nuclear vowel. What we see is that the nasal coda has no place features. Indeed, in nonfinal positions the nasal coda is either deleted or is homorganic to the following onset. We are then left with two possible analyses, either the nasal coda is a de-buccalized nasal glide (Trigo 1988), or it is not a coda, but a nasal feature of the nuclear vowel.

There are two arguments for the latter analysis. First, the vowel with the nasal 'coda' is nasalised throughout. Phonetically, it is more accurate to reflect this fact by writing the nasalised vowels as such, and phonologically, there is no need to write a 'nasal glide' after a nasalised vowel. Second, Shanghai has no diphthongs, and so no glide codas. If we allow a nasal 'glide' to occupy an independent coda slot, we have to explain why other glides (and consonants) are not found there. If, however, the nasal 'glide' is just a feature on the vowel, not a separate segment, then the absence of diphthongs needs no explanation.

2.2. -V? is one slot The argument for considering -V?

a single segment is similar to that for considering -VN a single segment. First, the glottal closure, often a weak one, is made only in phrase final positions; in non-final positions, there is no glottal closure (Chao 1928:39; Xu et al p149). Second, the vowel in -V? is a glottalized vowel. Compare the following

- (21) English: gut duck cock sock ship tip
 Shanghai: gu? du? ko? so? shi? ti?
 'crowded' 'pedel' 'cry' 'shrink' 'rest' 'kick'

It should be obvious to the ear that the vowel qualities are different in the two languages. What is more, in non-final positions, when there is no glottal closure, the glottal quality of the vowel remains unaffected, so there is never confusion between glottal and non-glottal vowels (Xu et al, p149). What this tells us is that it is the glottal quality of the vowel, rather than the glottal closure after the vowel, that is distinctive. Therefore, phonetically we should represent the glottal vowels as such, and phonologically it is redundant to introduce the /ʔ/ coda in addition.

It is nevertheless possible that underlyingly there is a glottal coda, which first spreads its glottal feature to the vowel, and then is deleted in nonfinal positions. Similarly, it is possible that underlyingly there is a nasal glide coda, which first spreads its nasal feature to the vowel and then is deleted after /a/ and in nonfinal positions. This analysis agrees with the fact that in citation syllables, both /-ʔ -N/ can have closures. In other words, citation syllables seem to have two rime slots, while nonfinal syllables seem to have one. One may take the citation form to be the underlying one, and add a coda deletion rule for nonfinal syllables. Or one may take the nonfinal form to be the underlying one, and add a coda slot insertion rule for syllables in isolation. I will not try to choose between the two analyses here. What I want to show is that in non-final positions, the Shanghai syllable has just one rime slot.

2.3. /ng z/ are onset-rime geminates It remains to be shown that the syllabic consonants /ng z/ occupy two slots, i.e. they are onset-rime geminates. I will again use evidence from Fanqie languages. Consider a game language from Shanghai⁶

- (22) a. reduplicate the syllable
 b. replace the rime of the first syllable with /o/.
 (23) ma --> mo-ma 'mother'

As stated in (22), this language first copies the syllable, and then replaces the rime in the first syllable with /o/. Thus, ma is changed to mo ma, shown in (23). Now, if the syllabic consonant is in the rime only, it will be replaced by /o/. On the other hand, if it is in both the onset and the rime, it will appear in the

onset position after the rime part is replaced by /o/. Consider

- (24) a. ng --> o-ng (speaker A) 'fish'
 ngo-ng (speaker B)
 b. z --> zo-z (both speakers) 'word'
- (25) a. X X --> X X X X --> X X X X
 | | | | | | | | | |
 0 ng 0 ng 0 ng 0 o 0 ng (0 = zero onset)
- b. XX ----> XX XX ----> XX XX
 ∨ ∨ ∨ | | ∨
 z z z zo z

(24a) shows that for speaker A, /ng/ is in the rime only, but for speaker B, it is an onset-rime geminate. (24b) shows that for both speakers, /z/ is an onset-rime geminate⁹. The derivations of (24a) for speaker A and (24b) for both speakers are in (25a-b)¹⁰.

Syllabic consonants, or onset-rime geminates, are found in several other Chinese dialects (Chao 1931). McCarthy (1989) suggests that if a language has predictable C/V orders in a syllable, there must be C/V segregation. Shanghai has fixed syllabic structure, and basically predictable C/V orders, namely CV. However, if there is planar C/V segregation, to which plane should the zero onset and the syllabic consonants go to?

3. Summary Evidence from underspecification, feature geometry, segmental distribution, duration, tone bearing unit, reduplication, and riming in poetry shows that Mandarin and Shanghai have fixed syllable structures. The Mandarin syllable has one onset slot and two rime slots, while the Shanghai syllable has one onset slot and one rime slot. The analysis for Mandarin applies to the Northern, the Yue, the Min, and the Hakka dialect families. The analysis for Shanghai applies to the Wu dialect family. For lack of space, I will not give analyses for other dialects here. However, it is reasonable, I believe, to assume that all Chinese languages have fixed syllable structures.

4. Syllabic length and tone sandhi There is a close correlation between rime length and the occurrence of contour tones. In the XX rime dialects, simple contour tones (HL or LH, but not HLH or LHL) freely occur on any syllable, and African style tone spreading¹¹ is lacking. However, in the X rime dialects, contour tones are rare but tone spreading is common; a good example of this is Shanghai (Duanmu 1988). The difference between Mandarin (and Taiwanese, Fuzhou, Cantonese, etc.) on the one hand, and Shanghai (and most of the Wu family) and African languages on the other, could be that, in the former, tones are

pre-associated in the lexicon, whereas in the latter, tones become associated only after morphology; it is the post-morphological association that contributes to tone spreading (Zhang 1988). An alternative explanation is that in the former group, contour tones act as units, whereas in the latter, level tones are the basic units (Yip 1989). However, the close correlation between the XX rime, the presence of contour tones and the lack of tone spreading in the former, and the X rime, the lack of contour tones and the presence of tone spreading in the latter, strikes one as more than a coincidence.

NOTES

1. I wish to thank F. Dell, K. Hale, M. Halle, Z.M. Bao, Y.F. Li, S. Meredith, and the phonology session participants of WEOOL 1989 for discussions.

2. I thank Morris Halle for the formalization in features.

3. In other dialects, what corresponds to the Mandarin zero onset has just one realization. In Baoding (informant Yafei Li), for example, it is realized as /n-/. In Chengdu, it is realized as /ng-/. In Taiwanese (K. Hale p.c.), it is realized as /ʔ-/:

MANDARIN	BAODING	CHENGDU	TAIWANESE	
ʔan/Yan/ngan/Han	nan	ngan	ʔan	'peaceful'

The number of realizations of the zero onset does not affect the argument that an onset is obligatory in full syllables.

4. The Pinyin letters 'z c j q x' have the following approximate values: z as in rods, c as in cats, j as in 'just', q as in chop, and x as in shop.

5. Howie used G- only, and no -G-. But since both G- and -G- are traditionally considered part of the rime, the argument for G- to be in the onset applies to -G- as well.

6. There is also requirement on tones, which we will ignore here.

7. To be more accurate, I mean that Shanghai syllables at non-final positions have two slots. This will become clear later.

8. This is a game language used among children. My two informants A and B are native speakers of Shanghai. Both were born and raised in Shanghai. They came to the US about two years ago. A is in mid twenties, and B is in late twenties.

9. We exclude the possibility of rimeless syllables; the syllabics /z ng/ cannot be just in the onset.

10. Shanghai has a third syllabic consonant /m/ in m-ma 'mom'. For speaker B, /m/ is also an onset-rime geminate, shown below:

(i) ■ ma --> ■o-■ mo-ma

11. I use 'spreading' here to mean the one-to-one mapping between tones and TBUs (tone bearing units):

Ta	Tb	...	---->	Ta	Tb	...
\	/					
TBU	TBU	...		TBU	TBU	...

That is, the tones (except the first one) that used to appear on the first TBU, e.g. when in citation, are 'spread' or 'shifted' to other TBUs in the sandhi domain.

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A Case for Default Values?

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I. This paper has three related goals. First, by investigating properties of certain Irish-English dialects, to provide some motivation for a generalized mechanism of "default" case assignment, that is, nominal case assignment in the absence of any governing case-assigner. Second, to account for the rarity of this case-marking mechanism, and to preserve the explanatory value of much of Case theory, to propose constraints on the application of this mechanism, relating its availability to the status and composition of INFL. Specifically, it will be proposed that default case-assignment is only available in "intermediate" grammars, in languages shifting between Case-parameter options. This is argued to be the case for Irish English and Early Modern English. Third, the exploration of these case-marking options will lead us to doubt the fairly standard assumption that NP-movement is of necessity motivated by the requirements of Case.

It is with the mechanism of default case assignment that the present study is chiefly concerned; in particular, with the characterization and use of this mechanism in papers by Chung & McCloskey (1987), and McCloskey & Sells (1988).

1. It is no wonder, he to get the job.
2. D'you min' the day and we in the old castle?
3. She told him for to bring the mare.
4. He has his work finished.
5. Anyone wasn't at home.

The first constructions to be considered (given in 1-5) are attested in those varieties of Irish-English termed "Anglo-Irish" by Henry (1957,1977). Whilst the majority of examples are drawn from dialect studies, many examples considered here are also taken from modern literary sources. With the exception of set (4), none of the above sentences is grammatical in current Standard English. The grammaticality of these syntactic constructions in Irish English has been used by a number of writers to argue for significant structural "interference" from Modern Irish. While this influence is undeniable, the sentences in (1-5) nonetheless differ in interesting respects from their Irish equivalents.

In terms of Government-Binding theory, the ungrammaticality of sentence-type (1) Standard English is attributable to the lack of any available nominative case-assigner in the embedded infinitival clause. The standard assumption is that nominative case-assignment is always achieved structurally and therefore always dissociated from θ -role assignment. Indeed, it is this dissociation of θ -role assignment and Case-assignment which makes possible various types of NP-movement in English. The dissociation of nominative case assignment from theta-role, combined with the fact that "subject" case assignment is often anomalous with respect to directionality of Case-government, has prompted a proposal (Mohammad 1987, Koopman & Sportiche 1988) that, for a given language, structural case assignment is achieved either under government or by {Spec,Head} agreement. These, then, are the two possible values (or settings) of what is termed the **Case Parameter**. In several current models' clausal structure,

INFL is taken to be the head of S (=IP), while the subject NP occupies the {Spec,IP} (or external subject) position, at least at s-structure. Under this proposal, nominative case assignment in Modern English is achieved via this {Spec, Head} association, rather than under government.

We can go some way towards accounting for the apparent anomaly of (1) by looking at the Modern Irish equivalent of these Irish-English (IE) forms. Irish has a highly productive non-finite verb-form; the so-called verbal noun. Consider the Irish examples below:

6. **Is éadóigh** [s iad [vp ionsaí a dheanamh orainn]]
 COP-Prs unlikely [them-ACC [attack-ACC do-VN on-us]].
 They are unlikely to attack us. (McCloskey & Sells)

Is mór an suaimhneas don gheata [s iad [a bheith pósta]]
 COP-Prs great relief to-the gate [they-ACC [to be married]]
 It's a great relief to the gate that they're married.

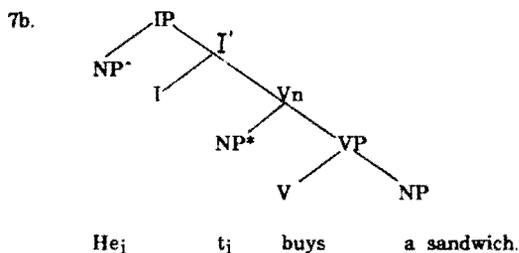
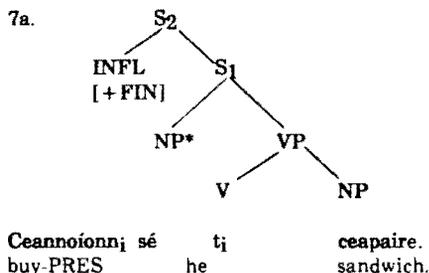
For sentences of this type, McCloskey & Sells (1988) provide an analysis under which the surface SOV order of the embedded clause is derived object-preposing (adjunction to VP). McCloskey & Sells propose that this NP-movement is motivated by Case: the object NP cannot receive Accusative Case from the VN, and so it moves to be assigned Case by the particle a. What is most significant for present purposes is the case-marking of the embedded subject iad, which receives Accusative case in the absence of any possible structural case-assigner. The existence of such structures, as well as the behavior of logical subjects in Irish small clauses, lead McCloskey & Sells to propose that Irish "**possesses a productive rule for assigning default accusative case to the subject of a non-finite clause... [and]... this rule is optional**".

Evidently, the default mechanism available in Irish forms part of the solution to (1-5) above. There is, however, a important additional difference between the Irish and Irish English infinitival subjects, which is that in the Irish English cases, the subjects are assigned **nominative** rather than **accusative** case. To explain how default-assigned NPs come to be realized with nominative case, it is helpful to consider Irish small clauses.

Chung & McCloskey (1987) offer a detailed discussion of a particular Modern Irish construction-type which they term small clauses. They have two findings which are relevant to the present discussion. First, it is proposed that the NP-subjects of these small clauses receive **default** (accusative) case. Such clauses clearly represent the Irish equivalent of the Irish English sentence-type (2). Second, they propose that small clauses are the underlying "bare predicational structures" involved in all Ir. sentences. In Irish, they suggest, S = SC, rather than IP. This contrast is illustrated in (7) on the handout, (assuming the IP-structure of Koopman & Sportiche for 7b). Chung & McCloskey suggest that this difference (as to what S is a projection of) should be taken as a "parameter" of language variation. For discussion purposes, let us call this proposed parameter the **S-parameter**.

There is, clearly, a significant implicational dependency between the values assigned to the two parameters. It is currently assumed that Verb-movement to INFL in Irish is motivated (in part, at least) by the requirements of Case: nominative case can only be assigned by a lexically realized INFL. (In this

respect, the verb-raising in Irish equivalent to the type of verb-raising of so-called "verb-second" languages to be discussed in the second section).



Now, if INFL is not the head of S, then it will be impossible for nominative case assignment to be achieved via {Spec,Head} agreement, since by definition, INFL does not head any maximal projection; INFL, therefore, must always assign Case under government. Returning to the Irish English data of (1-5), it is now possible to offer an account of some of the discrepancies between Irish English and Standard English.

We might tentatively assume that in acquiring a second language, the parameter-values of the first language are assumed to hold for the second language, just in case the initial parameter-values are "minimally compatible" with the available evidence of the second language. Now clearly, there are some parameter values for which a language provides reasonably direct evidence. What is intended here that parameter-settings will be kept unchanged as long as the second language can be made to fit these values, even if a different set of parameter settings would provide for a "less marked" grammar. Assume, for present purposes that a native-speaker of Irish has internalized the following parameter settings: (i) that INFL does not head its own projection (S = SC); (ii) that nominative Case is assigned under government by a lexicalized INFL; (iii) his grammar makes available a mechanism of default case assignment to the lexical subjects of non-finite clauses. Encountering English sentences such as those given in (9), the learner must assign them a syntactic analysis which is if possible, consistent with values (i-iii).

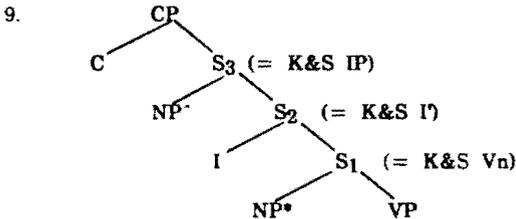
Taking (8a) and (8b) first, the learner might assume that Irish-English clausal structure corresponded exactly to that of Irish. If this were the correct analysis, then to the Irish speaker, English main clauses would differ from their Irish counterparts in only one respect: in the English sentences, the verb does

not raise to INFL. If INFL is not lexicalized, then nominative case cannot be assigned structurally to the subject NP. But, nominative case cannot be assigned by {Spec,Head} agreement, since this is not unavailable by definition.

- 8a. He bought the horse.
 b. What did he buy?
 c. He couldn't see me.
 d. Can't he see me ?
 e. *It is a shame, (for) to Michael leave.
 f. *I don't want for to John win.

Nonetheless, there is overwhelming evidence of the grammaticality of (8ab). This creates a certain tension, to be resolved in principle in one of two ways. Either, the learner could "reset" his S- and Case-parameter in his analysis, or he could retain these values and generalize (iii) to finite clauses; in other words, he could analyze **nominative** Case as the case assigned by default. After this first analysis, subsequent analyses by acquirers of Irish-English as a first language could involve re-fixing of parameters; but still, the prevalence of small clause structures like (2) would provide sufficient positive evidence for some time of the continuing availability of default nominative case assignment in non-matrix clauses.

Keeping this scenario in mind, there is a further necessary complication to the account. Although the structure [sc NP* VP] in (7a) will account correctly for the "small clause" examples in (2) and, fortuitously, for (8ab), it cannot account for other simple sentences such as (8cd) and falsely predicts the grammaticality of (8ef). The interaction of modal verbs, usually assumed to be base-generated in INFL, negation, which is either taken to be a feature of INFL or to head its own projection above the maximal VP, and the position of "to", a feature of a non-finite INFL, forces a different structural analysis than the one presented in (7a): in order to account for the large number of sentences in which the subject NP appears to the left rather than to the right of INFL, the Irish English learner is forced to posit a second NP position (labelled NP*) outside the S(=SC) and "above" INFL, a structure like (9), below. This clause-structure is very similar in important respects to that in (7b), the structure proposed by Koopman & Sportiche for Standard English. The present account differs from that of Koopman & Sportiche in one fundamental respect: it assumes the independence of the S-parameter from the Case parameter. The principal consequence of this for the clause-structure in (9) is that both NP* and INFL are adjoined to S1 (= SC = Vmax):



Before turning to the theoretical implications of this proposal, it is important to show at least that it "works", that it gives an adequate account of all the Irish English sentence-types (1-5). Sentence-type (1) sentence is grammatical in Irish English since, by contrast with Standard English, (Spec,Head) agreement is irrelevant to nominative Case assignment of the subject NP. Nominative Case can therefore be assigned by default to the NP* position; therefore, it is irrelevant whether the clause is tensed or not. This setting of the S-parameter, given the availability of default case assignment, predicts the grammaticality of overt subject NPs in embedded infinitival clause. Small clause sentence type (2) is predicted to be grammatical, given the availability of default case assignment. If we assume that it is the presence of INFL which triggers raising to NP* (for reasons **independent** of Case), then in case (2) the small-clause subject can remain in situ. Default case assignment is available to case-mark this subject NP: this default case-marking is nominative for the same reason as in (1).

In the case of (3), the availability of default case assignment and the setting of the S-parameter have the joint effect that in such contexts for is analyzed in IE not as a case-marking preposition but rather as a complementizer. Henry (1987) provides an analysis of for-to complements in Belfast English, under which for is base-generated in INFL and raised to COMP to case-mark the subject NP, unless an IP-selecting matrix verb, such as want, can exceptionally case-mark the subject of the embedded clause. For the Irish English dialects under discussion here, I suggest that even in those contexts where for raises to COMP, it does not function as a Case-assigning preposition. In (3), PRO remains ungoverned (as required), since for does not raise out of INFL. Some evidence that for does not function as a case-assigner in these contexts in IE is provided by the following sentence, attested in Henry (1957):

10. It is a point of law, for she to put them out.
It is legally debatable whether she is entitled to evict them.

In section two, it will be suggested that one of the diachronic consequences of the re-setting of the S-parameter is exactly this change in the functional status of for from that of a complementizer to that of a structural case-assigner.

- 11.
-
- have -ed make the dinner.
have the dinner made tj

The split perfect construction, exemplified in (4) is the subject of a separate paper. It is included here briefly because it is amenable to the same general treatment. I suggest that in contrast to the Standard English perfect, the Irish

English split perfect construction should be treated as a type of passive small clause, with have functioning as a main verb, selecting a passive SC complement, and exceptionally case-marking the SC subject (underlying object), as illustrated in (11), above.

Finally in this section, consider sentence-type (5) involving anyone. This sentence-type provides some independent support for the correctness of the (apriori somewhat improbable) clause structure given in (10), and, in turn, supports the assumption that the S-parameter is independent from considerations of Case. In Irish English, in contrast to Standard English, anyone is within the government domain of not, since there is no intervening maximal projection, headed by INFL, blocking such government. The S-parameter value of Irish English predicts the grammaticality of such sentences.

In this section, the primary concern has been to motivate the default case assignment, in particular, default nominative case. Admitting default case into the theory, however, would seem to create more problems than it solves, since, unless default case can be adequately constrained, it threatens to void case-theory of much of its explanatory value. To deal with this, two suggestions might be made. First, that indeed, certain types of NP-movement standardly assumed to be case-motivated should in fact be handled by another theory, whatever theory will explain topic-movement. Second, that default case assignment is not an independent mechanism but is rather crucially dependent on the changing composition of INFL. Another way of expressing this to view default case assignment as available only in "intermediate" or transitional grammars.

II. These suggestions can be usefully explored by investigating certain diachronic developments in English. In this second section, I want to consider briefly a number of hypotheses which have been entertained by several syntacticians (including van Kemenade (1987) and Roberts (1985) concerning "parametric shifts" in the history of English. Van Kemenade's thesis may be briefly summarized. Starting with Old English, she provides evidence that the underlying structure of this language was SOV, and that the surface TVX order in main clauses resulted from the interaction of verb-raising to a S-initial position and some type of topic-movement; in other words that OE was a "verb-second" language, comparable to many current Germanic languages. The difference between OE and Present Day English is claimed to result from two independent "parametric shifts": a base-change from OV to VO (related to the loss of morphological case) and the loss of "Verb-second" as a result of the reanalysis of surface SVO structures (diagrammed in 12). The first change is thought to take place around 1200, the second c.1450. My purpose here is to interpret aspects of this thesis in terms of the parameters outlined in the first section, and to draw attention to certain facts about Middle English and Early Modern English, instances of default case assignment, which up to now have been ascribed no theoretical significance.

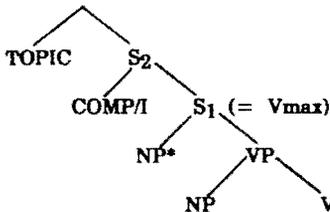
Beginning with Old English, van Kemenade provides extensive evidence for her claim that Old English was a fully-fledged "verb-second" language. Most GB analyses of the verb-second phenomenon, assume that verb-second sentences are "derived" structures, (as illustrated in 11) the result of two independent movements: verb-raising to a landing-site (α) to the left of the minimal S, and topic-movement obligatorily preposing an XP, (usually the subject NP) to a landing-site (β) itself to the left of (α):

11. V_2 structure: $[\beta \text{ XP}_i [\alpha \text{ V}_j [\text{NP} [\text{vp } t_i \ t_j]]]]$

GB analyses of verb-second differ with respect to the values given to α and β in this bracketing. In the analysis adopted here, however, it is assumed that landing site α should be considered the COMP node, where INFL is in COMP (or alternatively, the INFL node where COMP is in INFL). This assumption accounts for the complementarity of complementizers and the finite verb. Standardly, the verb-raising is usually motivated by Case considerations: in order to receive nominative case, the subject NP must be governed by a lexically realized case-assigner, where the case assigner in question is [\bar{v} -Tense].

Regarding the S-parameter value of verb-second languages, it can be assumed that verb-second languages share with Irish the property that the head of S is not INFL. This is by no means an original claim. Taraldsen (1983) proposes that an essential feature of Verb-second languages in general is that V rather than INFL is the head of S. One of the principal parametric differences between Old English and Present Day English, then, can be stated in terms of the S-parameter. Taking V to be the head of S in V_2 languages, the following underlying structure for Old English, given in (12), is posited. It is interesting to compare the structure in (12) with the clausal structure proposed for Irish English (9) and Modern Standard English (7b). If these are the correct clause-structures for the languages in question, then (9) represents a clausal structure "intermediate" between (12) and (7b). In Old English, in contrast to both Irish English and Present day Standard English, the VP is head-final and COMP and INFL share a terminal node outside the minimal S; in Old English, the topic-position adjoined to S may host any XP; in the other two languages, the XP must be the subject NP. On the other hand, Old English and Irish English both share the property that INFL does not head its own projection, rather it is adjoined to S; therefore, in both these languages, nominative Case cannot be assigned via {Spec,Head} Agreement, but rather via another mechanism, either under government or by default.

12. Old English (V_2):



Om twang þingum hæfde God þæs mannes sawle gogoded (vK. 18)
 God had endowed man's soul with two things.

Leaving aside for a moment the setting of the Head Parameter it seems that the crucial difference between Old English and Present Day English can be traced to the status and composition of the INFL node. To see what is involved here, compare again (7b), (9) and (12). If there is a real parallelism between these three clause structures, then the node labelled NP* in (7b) and

(9) is exactly equivalent to the node labelled TOPIC in (12): that is, NP-movement of this sort can be viewed as a special instance of the topic-movement in V2 languages which obligatorily moves a topicalized XP to sentence-initial position. Whatever triggers the latter obligatory raising, triggers the former movement. (To my knowledge, it has not been suggested that this movement is Case-motivated). In Modern English and other languages which case-mark their subjects by {Spec, Head} agreement, only subject NPs may move to this TOPIC/NP^{*} position, since only these elements are eligible to receive the agreement features from INFL. This restriction naturally does not apply when INFL does not head its own projection; in such cases as in (9) & (12) there is no special relationship between the content of INFL and the content of TOPIC/NP^{*}.

It has been proposed that a structure such as (9) represents an intermediate step between two parametric settings. In terms of our usual understanding of parameter-setting, this suggestion may seem problematic at best. Parameter-setting is usually viewed in terms of a set of (partially-) independent, discrete choices, by which the child determines the range of structural possibilities available for his/her particular language. In terms of the type of diachronic syntactic theory proposed by Lightfoot (1979), there can be no intermediate stages. Here, I want to suggest that there are circumstances under which the "positive evidence" which is required for the setting of parameters is contradictory and inconsistent, and that this may result in a grammar which is less fully determined than it might otherwise be. Given the considerable measure of disagreement as to what should count as triggering evidence, I do not believe that this suggestion should be rejected out of hand. On the other hand, it is naturally important that there be some data which are best accounted for by incorporating this suggestion within the theory. In the final section, just such data will be presented.

Van Kemenade (1987) claims that the structural change from OE to Modern English took place in two stages. First, around 1200, English changed from an SOV to an SVO language. Van Kemenade relates this change to the loss of oblique (inherent morphological) Case. She supposes that at this stage (c1200 - c1400) English still exhibited verb-second movements in main clauses. The loss of (inherent) case meant that NPs could only satisfy the Case filter by being structurally governed, since this was the only mechanism available in principle. The main effect of the second shift which van Kemenade proposes is the loss of verb-second. Verb-second structures cease to be interpreted as such when subject pronouns are come to be interpreted not as clitics but rather as subject NPs. The reanalysis is facilitated by the fact that in SVO structures with V2 movement the resulting s-structure is often string-vacuous, and there is therefore often no s-structure evidence of movement.

There are in fact three distinct, but related changes involved in van Kemenade's second structural change: (i) COMP and INFL come to be realized as independent nodes¹; (ii) INFL comes to head its own projection (S = IP); (iii) only the subject NP^{*} can appear in the former TOPIC (now NP^{*}) position; (iv) Raising from NP^{*} to NP^{*} becomes obligatory. (iii) follows from (ii): once nominative case is assigned by {Spec, Head} agreement (which itself becomes a possibility only when INFL heads its own projection), only the subject NP can appear in the {Spec} position. Notice that a change of the S-parameter does not automatically force a change of the Case parameter.

Van Kemenade's thesis ignores one further crucial change in the history of English, which is the loss of verb-movement to INFL and its replacement by Affix-movement. This change took place in Early Modern English. Roberts (1985) provides an analysis of this change in terms of an independent "parametric shift", relating the development of modals, the loss of verbal agreement and the dramatic rise of "do-support", to the changing composition of INFL. For Roberts (1985) this shift is to be explained by a shift in the nature of agreement, from a system of morphological to syntactic agreement.

The following scenario of changes and intermediate steps is proposed, adapting the proposals of van Kemenade and Roberts and referring Visser (1963) for supporting evidence. The changing of the Head parameter (c.1200) was motivated by the loss of oblique (inherent) case: case now had to be assigned structurally or by default. The NPs directly affected by the loss of inherent case were verbal indirect objects, "dative subjects" of impersonals and datives dependent on nouns such as wonder, pity etc. Throughout the Middle English period and up to EME, there is some evidence that for is treated as a complementizer, generated in INFL, and not yet necessarily as a prepositional case assigner. Evidence is provided first of all by the sentences in Visser (1963):

- 1384 So hevye was therof the fame that for to bere it was no game..
 1425 And forto walke bi grauelous places helpeþ hem, as seiþ Alexander..

Such sentences indicate that for functions as a complementizer, but although the orthography in (b) is suggestive, these sentences do not firmly establish that for is in INFL. This is made clearer by the sentences in (14), below. Notice that these sentences also show overt nominative subjects in embedded infinitival clauses, directly comparable with the Irish English sentence-type (1). Sentences such as those in (14) are attested from the end of the 14th to the beginning of the 17th century:

14. Default nominative subjects in embedded infinitivals:

- a. 1402 Men to seye to women wel, it is best, And nor for to despise hem ne depraue.
 b. 1443 Oon man forto receive of an oþer man ziftis of larges greet and manye and ofte, and forto neuer zelde and zeue azen to so large a fre zeuer summe ziftis ... were a greet boistose ruydnes.
 c. 1470 thou to love that loueth not the, is but grete foly.
 d. 1570 she to dy so dangerously... that was the thing that greued me so.
 e. 1611 Of him I gathered honour, - which he to seek of me again, perforce - Behoves me keep at utterance.

After this time, nominative case-marked lexical subjects in embedded infinitival clauses are no longer attested. I would suggest that it is no accident that the emergence of this form should have co-incided with the loss of verb-second and the reanalysis of the COMP/INFL node, while its loss co-incides with the rise of do-support and the development of modals. The grammaticality of the forms in (14) may be accounted for if we assume the following: while English was V2, the case-assigner in COMP/INFL that had to be lexicalized was the feature [+Tense]. The reanalysis of the S-parameter -- causing the loss of V2 --resulted in the splitting of COMP and INFL. Under this reanalysis, [+Tense] remained

in COMP and was dissociated from AGRreement, which developed as the head of S. Under this arrangement, although the verb still moved to INFL, it did not move to COMP. In tensed main clauses in Middle English, the COMP node was no longer obligatorily lexicalized, therefore [-Tense] could not assign nominative case structurally. AGR, however, had not yet developed as a Case-assigning head. Let us suppose that, as with Irish English, in main clauses nominative case was analysed as being assigned by default. By extension, it would have been available for non-finite clauses also.

It is possible that there may have been an intermediate stage between verb-second, when any XP could be raised to Topic position, and the present state, where only the NP-subject (NP*) can and must be so raised. This is a stage at which any argument NP could be raised. Whilst the evidence for such a stage is hardly over-whelming, in the context of a "developing" INFL node, it might be predicted. The sentences in (15: Visser p.968) could be analyzed as involving movement of the object NP to NP' position.

- 15.a. 1392 Hit by-comeþ for clerkus [? crist; for to seruen t_i]
 b. 1420 Hit were fulle tere for a tung [? my tourments; for to telle t_i]

It may also be that the object-preposing in northern dialects of Irish, (cf. 6 above), should be viewed as an instance of this type of Topic-movement, rather than as being Case-motivated, as McCloskey and Sells (1988) suggest. Whatever the status of the landing-site of these preposed objects, the sentences in (14) point to another related development; that involving indirect objects. When inherent case ceased to be available as a means of case-marking, NPs which had previously been inherently case-marked dative had to be case-marked structurally. Initially, there was often no available structural case-assigner, and it seems as if, for a period, indirect objects received case by default. In many contexts, however, for came to be adopted as the structural case assigner. Given the concomitant developments in INFL, this triggered a "reanalysis" of indirect objects of matrix clauses as the (structurally case-marked) subjects of embedded clauses, as diagrammed in (16):

16. Restructuring.
 [A/NP NP-DAT [s PRO [(for) to [VP]]]
 == =>
 [A/NP [PP for NP] [s PRO [(for)to [VP]]]
 == =>
 [A/NP [s for NP [to [VP]]

This reanalysis is evidenced by (17). Between c1400 and 1550 constructions in which for has been reanalyzed as a structural case-assigner, assigning case to an embedded subject, become increasingly common; by 1600, "for-less" constructions are virtually obsolete:

17. Intermediate adjectival (a) and nominal (n) complements with default oblique case (prior to restructuring) – ungrammatical after development of {Spec,Head} Agreement:

- OE Hit is earmlic and sorhlic eallum mannum to gehyrenne.(a)
 1382 it is lizter a camel for to passe throwz a needles eize than a riche man to entre the kyngdam of heuenes. (a)
 1422 remember what peyn it is a man to losse lyberte. (n)
 1439 It were weel meriere a man to gon at large, Than with irenes be nailed to a block. (a)
 1500 It is no litel þinge a man to dwelle in monasteries and congregacions..(n)
 1539 How good and joyful a thing it is brethren to dwell together in vnite. (a)

18. Restructuring Adjectival (a) and Nominal (n) complements – after development of {Spec,Head} Agreement, this restructuring became obligatory:

- 1392 The sauter seith hit is no synne for suche men...for to seggen as thei seen. (rn)
 1436 that it shall be lefful for every man to shippe..Cornes and Greynes oute of this Roialme. (ra)
 1450 It is folie for hir to sett hir hert on any man that be so changeable. (rn)
 1582 It was very needful and necessary for him to take a pilot.
 1611 It is not vainglory, for a man and his glass To confer in his own chamber. (rn)

In terms of the proposals developed thus far, the re-analysis diagrammed in (16) can be viewed as motivated by two conspiring factors: (a) the requirement that previously inherently case-marked NPs be structurally case-marked; (b) the requirement that the subject in NP' position (previously structurally case-marked by Tense-in-INFL) be case-marked in some way. At the beginning of the EME period, the further structural change discussed by Roberts (1985) took place; verb-movement to INFL ceases to be obligatory and is replaced by Affix-hopping in tensed main clauses. At this point there is the dramatic rise of do-support, and modals come to be base-generated in INFL. It is at this point, I suggest, that INFL has fully developed as (non-lexical) head with case-assigning properties. From this point on, nominative case is assigned by {Spec,Head} agreement. Since default case assignment is not available in the domain of a structural case assigner, it is no longer available as an option after this point.

1. It is possible that this split may be better understood as a splitting of INFL into "Tense" and "AGR", rather than COMP and INFL. This would accord better with Pollock's (1988) of French and English, in which he supposes that V2 languages have "lost" their AGR node. This idea is not pursued here, although it seems to be broadly consistent with the proposals being developed.

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THE ROLE OF SONORITY IN SPANISH STRESS ASSIGNMENT

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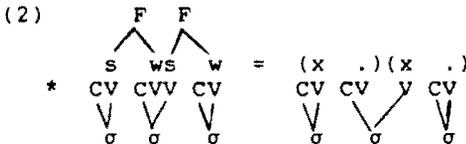
In this paper I will compare two principles constraining stress assignment, Syllabic Integrity (SI) and Sonority Prominence Consistency (SPC) as they affect Spanish. First I will explain the notion of Syllabic Integrity as it is conceived in Prince (1976), Hayes (1980) and Hayes (1987). Second I will introduce a principle I refer to as Sonority Prominence Consistency. To compare the two principles, I will first introduce an analysis of Spanish Stress based on the moraic trochee of Hayes (1987). I will show that stress assignment to Spanish rising heavy syllables presents a rare situation in which there is a mismatch between the foot and the sonority contour of the syllable. This mismatch provides evidence in favor of Sonority Prominence Consistency and against Syllabic Integrity as a fundamental principle of stress systems.

A. Syllabic Integrity

Syllabic Integrity was explicitly formulated in Prince (1976), but has been implicit in most current work on stress. Prince's formulation appears in (1):

- (1) Principle of Syllabic Integrity (Prince 1976:2): The contents of a syllable may not be divided between two metrical units.

This principle disallows foot assignment of the sort schematized in (2). On the left appears the tree notation of Prince (1976) and Hayes (1981), among others, and on the right the bracketed constituents of Hayes (1987). In this paper I will adopt the latter notation. The point to notice is that the long vowel of the second syllable is divided between two metrical feet. Prince claims that SI is responsible for the ill-formedness of (2), explaining the fact that while stress may be assigned to moras, in his words, "it is retracted from syllables." (Prince 1976:15)



Syllabic Integrity is built into the Hayes (1981)

system through a mechanism that builds feet only upon existing syllable rime projections. For that reason the left and right edges of a foot always coincide with syllable edges as well.

An important innovation in Hayes (1987), for languages like Spanish, is the moraic trochee, or left dominant foot assignment which counts moras. In this approach, Syllabic Integrity is achieved in the formulation of the moraic trochee rule, since otherwise the rule would count only moras, disregarding syllabic constituency. The rule appears in (3):

(3)

(x .)

Moraic Trochee (Hayes 1987): Form m m if possible,

(x .)

(x .)

(x)

(.)

where m m is either ∪ ∪ or - ; otherwise form ∪ .

(Notation: m = mora, ∪ = light syllable, - = heavy syllable)

The algorithm forms a binary moraic trochee over two adjacent light syllables, a unary stressed foot over a heavy syllable and a unary stressless foot over a single light syllable. For example it will parse the series of heavy and light syllables represented in (4)a. as three distinct feet, with a stress only on the heavy syllable, whereas a series of light syllables as in (4)b. will be assigned alternating stresses.

(4)

a. (.) (x) (.)
CV CVV CV

b. (x .) (x .)
CV CV CV CV

It should be clear that the second line of Hayes moraic trochee rule ensures that a syllable will never be divided between two feet. In this way it provides another instantiation of the principle of Syllabic Integrity.

2. Sonority Prominence Consistency.

I turn now to an alternative account of so-called Syllabic Integrity. Returning to the ill-formed foot assignment in (2), I would like to propose that the problem with (2) lies not in the division of the syllable between metrical feet, but rather in the fact that stress has been assigned to the second or weak

mora of a heavy syllable in the sense of McCarthy and Prince (1986). I claim that such an assignment is prohibited by the principle of Sonority Prominence Consistency whose effect is to match up stressed elements with strong moras, the latter presumably being the sonority peaks of syllables. SPC is formulated in (5).

(5)

Sonority-Prominence Consistency:

$$\begin{array}{c} * x \\ | \\ m_w \end{array}$$

In words the principle states that a stress (or x) may not be assigned to the weak mora of a syllable. In most cases, SI and SPC make similar predictions. To see why, let us review a general typology of syllables and moraic foot types as schematized in (6). All languages with heavy syllables have syllable rimes of the type shown in (6) a., that is, where the first mora is strong (or more sonorous) and the second weak (or less sonorous), given the sonority scale shown in (7).

(6) a.



E.g: VC--fusil 'gun'
VG--peyne 'comb'

b.



GV--eryazo, aowana
'untilled' 'customs'

c.



d.

(7) Sonority Scale

Vowels	most sonorous
Glides	
Nasals	↑
Liquids	↓
Obstruents	least sonorous

Syllables of the type in (6)b. in which the second element is more sonorous than the first are very rare, but they exist in Spanish, in diphthongs of rising sonority, for example in eriazo 'untilled'. What is unexpected is that the first mora of these syllables, which is a glide, contributes to the weight of the

syllable; however, Harris (1983) has demonstrated that the rising diphthong in (6)b. acts just like any other heavy syllable in attracting stress, and therefore in the present terms must be bimoraic.

Having noted that cross-linguistically, syllables tend to have a falling sonority contour, we further note that while the moraic trochee of (6)c. (drawn in tree form for the sake of comparison to the syllable types in a. and b.) is well-attested, the right dominant moraic iamb in (6)d. is not. This fact is noted by Hayes (1987) and provides the basis for his asymmetric foot typology which includes the moraic trochee but excludes the moraic iamb. The point is that since only (6) a. and c. are well attested cross-linguistically, languages tend to match strong-weak sonority contours with strong-weak moraic feet. In these languages both syllabic integrity and SPC will predict a match-up of the strong and the weak elements of syllables and feet. However, in a language with syllables of the type in (6)b. and feet of the type in (6)c., a mismatch will arise. I claim that Spanish is such a language, and provides evidence that in those situations where the two principles make different predictions, SPC gives the correct result.

What follows are two proposals. First I will outline an account of Spanish main stress based on the moraic trochee of Hayes 1987. Second, I will argue that the interaction of sonority and stress assignment in Spanish rising heavy diphthongs under this account gives evidence in favor of Sonority-Prominence Consistency over the principle of Syllabic Integrity.

3. Evidence from Spanish Stress

The stress data to be accounted for appear in (8). Verbs are excluded as I assume them to acquire stress in a different way from nouns, adjectives and adverbs. Harris (1983,1989) distinguishes three stress classes in Spanish. The majority (Type A) have stress on the penultimate syllable in Vowel-final words and on the final syllable in Consonant-final words. A significant minority (Type B) have stress on the antepenultimate syllable in Vowel-final words and on the penult in consonant-final words. A small group of Vowel-final words have final stress (Type C). I assume that stress in Type C words is assigned lexically rather than by the general stress algorithm, and I will not discuss them here. Finally, if the penult is heavy and the final light, Type A and Type B words are indistinguishable as stress on the penult is obligatory.

(8)

- a. majority = Type A words
 V-final: bar'ata kalaβ'aza
 'bargain' 'pumpkin'
 C-final: fus'il salas'ar
 'gun' (name)
- b. minority = Type B words
 V-final: s'aβana rek'amara
 'bedsheet' 'chamber'
 C-final: m'oβil alm'iβar
 'mobile' 'syrup'
- c. special = Type C words
 kaf'e panam'a
 'coffee' (name)
- d. Heavy Penult:
 kan'asta as'eyte aδw'ana
 *k'anasta *'aseyte *'aδwana
 'basket' 'oil' 'custom house'

The algorithm in (9) accounts for the forms in (8)a.

(9) Spanish Main Stress

1. Build moraic trochees (= (x .)) right to left on the derivational stem domain
2. Main stress is assigned to the rightmost x.
 (= End Rule Right of Prince 1983)

Notice that this algorithm does not contain any reference to the syllable. I claim that this is not necessary since Sonority Prominence Consistency will accomplish the same goal without the additional complexity of referring to syllables in a rule sensitive to moras. Moreover, I would like to suggest that Sonority Prominence consistency provides an explanation for the syllable-sensitivity of moraic rules in general, since it instantiates the phonetically natural process of matching up sonority peaks with stresses.

Before demonstrating the application of rule (9) in Spanish words, I will first review the relevant morphological structure of the Spanish words under discussion, assuming a version of lexical phonology with level-ordered rule application. Referring now to (10), the root and derivational suffixes constitute the derivational stem, the domain on which the stress rule applies. The final vowel which often marks gender lies outside the derivational stem and outside the stress domain. In this way it behaves like the inflectional plural marker -s. Evidence for excluding

the final vowel from the stress domain comes from the behavior of diphthongizing stem forms, but will not be discussed in this paper.

- (10) Root + Derivational suffixes] _{DS} Final Vowel
 DS = the stress domain
 Examples: kalaβas] a
 salasar]
 kanast] a

The examples in (11) illustrate how stress is assigned in the majority of words. The moraic trochee rule applied from right to left creates a binary foot whenever it encounters two moras. Since the final vowel is excluded from the stress domain, calabaza and Salazar will be parsed similarly. At this point in the derivation, the a of calabaza constitutes the second mora of a heavy syllable in the derivational stem. This follows from the maximal syllabification of the melody at this level.

- (11)
- a. kala βas] a
 m m mm
 (x .)(x.) Moraic Trochee
 x End Rule Right
 kalaβ'asa
- b. sala sar]
 m m mm
 (x .)(x.) Moraic Trochee
 x End Rule Right
 salas'ar

This proposal captures the fact that the majority of Spanish words are stressed on the penultimate mora. When we allow the possibility of final mora extrametricality for words of Type B, we are able to account for all of the forms in (8) a. and b., requiring the same algorithm (9) for words of both type A and B.

- (12) Type A words: no extrametricality
 Type B words: Mark the final mora
 extrametrical

The examples in (13) illustrate the algorithm on Type B words, where the final mora of the derivational stem is extrametrical. Since the n of s'abana and the l of m'ovil are extrametrical, the foot will be formed over the first two moras of each word, and End Rule

Right picks out the first mora to receive main stress.

(13)

- a. saβa n] a
 m m(m) Final mora extrametrical
 (x .) Moraic trochee
 x End Rule Right
- b. moβi l]
 m m(m) Final mora extrametrical
 (x .) Moraic trochee
 x End Rule Right

Next I will turn to the facts shown in (8) e. concerning heavy penults. Recall that Spanish has heavy syllables with both rising and falling sonority, as given in the examples of (6). When a word has a heavy penult (as in canasta, aceite and aduana) stress is always assigned to the sonority peak of the penult. In Spanish, this can be either the first or the second mora of the heavy syllable since rising diphthongs count as heavy syllables. This point is exemplified in (14), where the syllable structure and stress are given for both falling and rising penults. The moraic structure of the penult is shown on the right.

(14)

- a. Falling heavy penult:



- b. Rising heavy penult:



If we assign moraic trochees (disregarding syllable boundaries) to the form in (14a), we will get the correct result, namely (15):

(15)

- kan a st]a
 m m m
 (.) (x .) Moraic Trochee
 x End Rule Right

Main stress will be assigned to the sonority peak aof the heavy penult. This outcome is consistent with both Syllabic Integrity and SPC, since the sonority contour of the syllable and the moraic foot match up.

However, (14b) presents the crucial case in which Syllabic Integrity and SPC predict distinct results. In (16) the moraic trochee rule is applied to the derivational stem of eriazo, with End Rule Right picking out the main stress:

(16)

* e r y a s l o	
m m m	
(.) (x .)	Moraic Trochee
x	End Rule Right

This yields an incorrect result, since stress will be assigned to the less sonorous glide (or weak mora) rather than the low vowel a. Notice, however, that the foot structure is well-formed according to Syllabic Integrity since the foot respects syllable boundaries. Sonority Prominence Consistency, on the other hand, does not allow foot assignments like the one in (16) because the weak (or less sonorous) mora of the syllable is matched up with the stress or x. The effect of SPC will be to shift the x onto the sonority peak of the syllable yielding a structure like (17)a. or b., both of which are compatible with the data. Since secondary stress is assigned postlexically (as argued in Roca 1986), it would be difficult to distinguish between the two structures empirically. The point relevant to this discussion is that neither structure is consistent with Syllabic Integrity since in both cases the heavy syllable is divided between two feet.

(17)

a. e r y a s l o	b. e r y a s l o	
m m m	m m m	
(x .)(x)	(.) (.) (x)	Moraic Trochee
x	x	End Rule Right

As noted earlier it would be impossible for this kind of form to respect syllabic integrity in a trochaic system, since there is a mismatch in a rising heavy syllable between the foot structure (strong-weak) and the sonority contour of the syllable (weak-strong).

To conclude, I have argued that Spanish gives us a situation in which there is a mismatch between the foot and the sonority contour of the syllable. This mismatch allows us to compare empirically the different predictions made by Syllabic Integrity and SPC. I suggest that the advantages of the SPC approach are:

1. SPC correctly predicts stress will be assigned to the most sonorous mora of a rising heavy penult in Spanish, while syllabic integrity incorrectly predicts that stress will be assigned to the less sonorous prenuclear glide.

2. In all other cases the two principles make identical predictions. Therefore, SPC subsumes SI.

3. SPC explains why most languages have syllabic integrity "effects". The reason is that most languages have only falling sonority in heavy syllables and if they count moras, they are trochaic, not iambic. This means that a mismatch like the one in Spanish between foot structure and syllable structure usually will not arise.

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The Past, Present and Future of Neurolinguistics¹

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I. The Past

In a recent book, Caplan (1987) states that the goal of neurolinguistics is to characterize the relationship between a theory of "language structure and processing, and that of neural tissue and its functioning". He distinguishes this from what he calls linguistic aphasiology, the study of acquired disorders of language, i.e. aphasia, and the "pattern of (language) breakdown in terms of principles of language structure and processing." This division seems somewhat arbitrary since aphasia data provide much of the evidence for the development of a theory of neural structures underlying the representation, acquisition, and use of language.

This paper will thus view neurolinguistics in more general terms, as the study of the brain/mind/language interface and the biology of language. Much of neuro-linguistic research involves the study of aphasia and other cognitive disorders. If such data can be shown to bear on this question, they should be of interest to linguistics.

While the interest in the neural and genetic substrates of language represents a relatively new subdiscipline of linguistics, the issue is not of recent origin. Three long standing problems of science and philosophy concern the nature of the brain, the nature of human language, and the relationship between the two. This relationship has been assumed for over 2000 years.

Observations concerning language loss are found in the medical records written on papyrus in 1700 B.C.E. by Egyptian surgeons who wrote that "the breath of an outside god or death" had entered their patients who henceforth became "silent in sadness." (Breasted, 1930)

The philosophers of ancient Greece also speculated about the brain/mind relationships. But neither Plato nor Aristotle recognized the brain's crucial function in cognition or memory. Aristotelian wisdom failed miserably on this issue as shown by his suggestion that the brain is a cold sponge whose function is to cool the blood. (Clarke and O'Malley, 1968) However, others

writing in the same period and basing their views on more empirical evidence, reveal a greater understanding. For example, one of the Hippocratic treatises -- 'On the sacred disease (epilepsy)' -- states that the brain is "the messenger to the understanding" and the organ whereby "in an especial manner we acquire wisdom and knowledge". (Riese, 1959)

For over five centuries, (from ca. 400 B.C.E to 135 C.E.) these Graeco-Roman physicians produced a corpus of writings which show that they recognized the distinction (which still eludes a number of linguists today) between linguistic competence and performance, i.e. between language and speech as revealed by their observation that loss of language may occur without the loss of speech (articulation) and vice versa. They were aware that language and speech disorders result from cerebral trauma or brain disease and noted that loss of speech often occurred simultaneously with paralysis of the right side of the body. These ancient neurologists may have understood the nature of contralateral motor control, i.e. that the right side of the brain controls the left side of the body and vice versa; they state, for example, that "an incised wound in one temple produces a spasm in the opposite side of the body". But, as pointed out by Benton and Joynt (1960), while "it is seen that the essential ingredients for relating aphasia to a lesion of the left hemisphere were present in the Hippocratic writings...there is no evidence that the correlation was actually made."

Other writers and scholars of the ancient classical world also reveal a knowledge of language loss i.e. aphasia; an early reference to alexia and agraphaia -- the loss of the ability to read and/or write following brain trauma -- occurs in a paper of the Latin author, Valerius Maximus (ca. 30 C.E), who writes about an Athenian scholar who "lost his memory of letters" after being struck in the head with a stone. Pliny (23-79 C.E.), also refers to this same Athenian noting that "...with the stroke of a stone, (he) fell presently to forget his letters onely, and could read no more; otherwise his memory served him well ynough." (Benton and Joynt 1960), showing that the 'modular conception of the mind' did not arise full blown from the head of Fodor (1983).

It has been suggested that the skeptic philosopher, Sextus Empiricus (ca. 200 CE) was the first to use the term 'aphasia', although the meaning he gave it had no reference to clinical language loss. (Patrick 1899).

Aristotle's view of the mind as composed of specified primary faculties (i.e. the Common Sense, Imagination and Phantasy, Conceptual Thought and Reasoning, and Memory) held sway from the classical period through the renaissance. Herophilus of Alexandria (ca 300 BCE) located these cognitive functions in the ventricular system of the brain and this notion guided the work of all the major anatomists in this period, as exemplified in the illustrations by Johann Eichmann in the 16th century shown in Figures 1.



Figure 1: The ventricular system as drawn by Johann Eichmann (1500-1560)

In the 16th and 17th centuries, a raft of illustrations of the brain based on autopsy dissections were produced, such as this head by Vesalius published in De fabrica in 1543.



Figure 2: Drawing of the head by Vesalius, (1543)

More important than the anatomical drawings of the brain from the viewpoint of neurolinguistics, were the clinical descriptive reports of patients with language deficits and preserved non-linguistic cognitive systems that were published from the 15th to the 18th century.

In 1481, Antonio Guainerio's description of a patient who "rarely or never recalled the right name of anyone" was, according to Benton and Joynt (1960) the first reference to a case of anomia.

Descriptions of other kinds of aphasic disorders were reported in the 15th century by Baverius de Vaveriis, Paracelsus, Francisco Arceo, and the anatomist, Nicolo Massa.

In 1585, Johann Schenck von Grafenberg, reported on a patient whose speech production was severely impaired (limited to producing only a few words) but whose comprehension seemed to be intact, and concluded that this was a case of central brain damage rather than tongue paralysis: "I have observed in many cases of...major diseases of the brain that, although the

tongue was not paralyzed, the patient could not speak because, the faculty of memory being abolished, the words were not produced". (Benton and Joynt 1960)

In 1676, Johann Schmidt presented a case study of a patient suffering from dyslexia (loss of ability to read) who nevertheless preserved his ability to write, and patients who could "write to dictation but could not read back what they had written" (Arbib et al. 1982).

Carl Linnaeus in 1745 published a case study of a man suffering from jargon aphasia, who spoke "as it were a foreign language, having his own names for all words". (Benton and Joynt 1960)

An important observation regarding word substitution errors was made by Ryklof Michel von Goens in 1789 in his reference to a patient which he described as follows: "After an illness, she was suddenly afflicted with a forgetting, or, rather, an incapacity or confusion of speech...If she desired a chair, she would ask for a table... Sometimes she herself perceived that she misnamed objects; at other times, she was annoyed when a fan, which she had asked for, was brought to her, instead of the bonnet, which she thought she had requested." (Crichton 1798; Winslow 1868) The description of this and other similarly afflicted patients reveals that they substituted words that were semantically or phonologically similar to the intended ones, producing errors similar to normal word substitution errors (cf. Fromkin 1980) or to those produced by the agnosia patient of the Drs. Damasio in Iowa who called Ronald Reagan 'John Wayne'. (Damasio, personal communication).

In 1770, Johann Gesner published a summary of earlier aphasia studies, adding many of his own observations. Included were descriptions of jargon aphasia in which the patient not only produced neologisms but in writing spelled words using orthography which reflected the phonology of the spoken jargon. He also discussed bilingual asymmetry in which, for example, an abbot, following brain damage, retained his ability to read Latin but not German.

Another case was of a 'deep dyslexic' patient who, in reading aloud, substituted semantically similar words, not unlike the patient of Newcombe and Marshall (1981, 1984) who read 'cake' for bun, 'poison' for arsenic and 'pixie' for gnome.

Gesner did not attribute these language difficulties to either general intellectual deficits or loss of memory "in general" but instead to a specific impairment to language memory, stating: "just as some verbal powers can become weakened without injury to others, memory also can be specifically impaired to a greater or lesser degree with respect to only certain classes of ideas."

However, Gesner failed to conclude that the neuro-anatomy of the brain underlay cognitive functions, since he also wrote: "The vessels of the brain are surely not arranged in accordance with categories of ideas and therefore it is incomprehensible that these categories could correspond to areas of destruction."

Franz Joseph Gall (1791,1810) argued against the view that the brain was an unstructured organ and in favor of discrete anatomical areas (or cortical organs) which were directly responsible for specific cognitive functions (or faculties), including language. He further suggested that the size of the relevant organ determined the kind of resultant behavior. Having noticed in his youth that the most articulate of his fellow students had protruding eyes which he believe was due to overdevelopment of the underlying brain, he concluded that language was localized in the frontal lobes. Gall believed that his hypothesis was confirmed by the word finding difficulties which followed damage to the left frontal lobe of a man wounded in a duel.

These notions led Spurzheim, a follower of Gall, to establish the pseudo-science called phrenology, the practice of determining personality traits, intellectual capacities and other matters by examination of skull configurations, which led to elaborate maps and skull models that depicted traits and characteristics of individuals, and also to the development of ingenious instruments for measuring the bumps and hollows of the skull. Societies were formed devoted to the study of phrenology and the unfortunate and unsound social and educational type-casting of persons on the basis of skull archetypes.

One should not, however, reject Gall's insights concerning distinct cortical/cognitive modules because of Spurzheim or because one is not "a practicing witch" as the writer of a recent book on phrenology describes herself.

Franz Joseph Gall's support for his 'modularity' view resulted in his being expelled from Vienna in 1802 and excommunicated in 1817. In the long run, however, Gall finally "convinced the scientific community that 'the brain is the organ of the mind' " (Young, 1970)

It was not until 1861 that language was specifically related to the left side of the brain. In a seminal paper, Paul Broca (1861) presented autopsy evidence showing that a localized (anterior) left hemisphere lesion resulted in a loss of ability to speak, whereas focal lesions in similar parts of the right brain did not. He managed to convince his Parisian audience (and most of neurology) that "On parle avec l'hémisphère gauche". 2

In 1874, Wernicke (1874), however, pointed out that damage in the posterior portion of the left temporal lobe results in a different form of language breakdown than that occurring after damage to the frontal cortex (now called Broca's area). These different kinds of acquired language loss -- aphasias -- continue to be corroborated.

The speech output of Broca's aphasia patients is characterized by word-finding pauses, loss of grammatical morphemes, and quite often, disturbed word order. Auditory comprehension for colloquial conversation gives the impression of being generally good, although controlled testing reveals considerable impairments. The term **agrammatism** is almost synonymous with Broca's aphasia, although some patients with lesions in Broca's area are not agrammatic and some agrammatics would not classify neurologically as Broca's.

Wernicke's aphasia patients, on the other hand, produce fluent speech with good intonation and pronunciation, but with many word substitutions (both semantically similar and dissimilar), neologisms as well as phonological errors. They also show comprehension difficulties.

One Wernicke's aphasia patient, for example, replied to a question about his health with: "I felt worse because I can no longer keep in mind from the mind of the minds to keep me from mind and up to the ear which can be to find among ourselves." (Kriendler et al. 1971) Another patient, when asked about his poor vision said: "My wires don't hire right." And an aphasic physician, asked if he was a doctor, replied: "Me? Yes,

sir, I'm, a male demaploze on my own. I still know my tubaboys what for I have that's gone hell and some of them go." One aphasic described a fork as "a need for a schedule" and a spoon as "How many schemes on your throat."

These fluent but uninterpretable utterances are very different from the Broca aphasic's answer when asked what brought him to the hospital: "Yes -- ah -- Monday -- ah -- Dad and P.H. (the patient's name) and Dad...hospital. Two...ah doctors..., and ah...thirty minutes...and yes...ah...hospital. And, er Wednesday...nine o'clock and eh Thursday, ten o'clock ... doctors. Two doctors...and ah...teeth. Yeah...fine. (Badecker and Caramazza 1985)

Broca's region and Wernicke's region are now regularly designated in modern textbooks such as in this one drawn by the neurologist, Hanna Damasio.

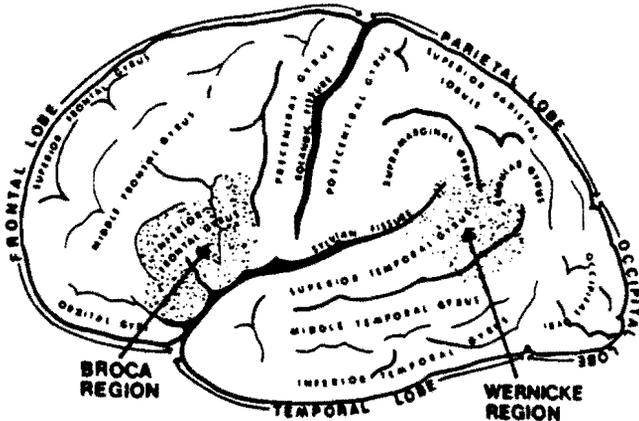


Figure 3: Lateral (external) view of the human left hemisphere, showing Broca and Wernicke regions. two key areas of the cortex related to language. (Hanna Damasio)

Although one finds general confirmation of the localized sites which result in distinct aphasias, one should not expect a one to one -- aphasia type to brain site -- correlation as was pointed out in a now classic paper of Bogen's entitled, "Where is Wernicke's Area" in which he demonstrates wide variety of lesion sites in autopsy examinations of the brains of patients classified as having Wernicke's aphasia. (Bogen and Bogen 1976)

Generally, however, the fact that different lesion sites produce differential language breakdowns reinforced the search for localized areas of the brain and led to the construction of diagrams and models with boxes representing anatomical and functional centers and arrows connections between them.

Wernicke insisted that these models be consistent with theories in both neuroscience and psychology (Arbib and Caplan 1979). But given the state of the art and science in the late nineteenth century this constraint did not necessarily lead to new insights. The models did account for both lesion sites and aphasia syndromes in a descriptively adequate way, but did not go beyond such description to reach a level of explanatory adequacy. This is exemplified by Lichtheim's 1885 model, which did little more than list the linguistic impairments that clustered to form certain aphasic syndromes. For example, motor aphasia was characterized by Lichtheim as impaired speech production with intact speech comprehension whereas he described transcortical sensory aphasia as a loss of comprehension with retention of spontaneous and repetitive speech. A major problem in Lichtheim's approach was that he treated speech production and speech comprehension as separate and unanalyzable processes and made no attempt to relate different aphasic symptoms to the separate components of language.

Among the critics of the model makers was Pick (1913) who showed great linguistic sophistication for his time by observing the distinction between lexical and grammatical morphemes. He pointed out that Broca's aphasics had difficulties in retrieving or using inflectional affixes and grammatical formatives while apparently having few problems with nouns, verbs, and adjectives. This proved to him that no model which treated speech and comprehension as a single process could be adequate.

The great Soviet aphasiologist, Luria (1947), in his Traumatic Aphasia suggested that it was important to propose "concrete suggestions as to the character of the disorders which are introduced into various functional systems by damage to specific focal areas." Luria attempted to account for the fact that the kind of speech or language deficit that resulted from brain injury was dependent on the lesion site, and he designated specific psycho - physiological functions to distinct areas of the brain and applied these functions to the analysis of language. Luria's system is an interactive modular system which relates psychological processes to the physiological mechanisms which govern motor and sensory functions. Luria recognized that certain areas of the brain were related to speech but, because he accepted the Pavlovian view of language as a "second symbol system", he did not posit neurophysiological or neuropsychological mechanisms that were specific only to speech or language.

Jakobson (1940, 1955, 1964) was the first linguist to conduct aphasia research, following up on the insights of de Courtenay in 1885 and Saussure in 1879 who had expressed the belief that a study of language pathology could contribute to linguistics. Jakobson used aphasia data to support his notions of phonological markedness and then later his views of syntactic theory.

Except for Jakobson, few linguists followed up the early interest in linguistics by neurologists who drew on linguistic concepts in their investigations of aphasia. As mentioned above, Luria (1947), reveals both the influence of and his interest in linguistic concepts to explain different forms of language breakdown and the relationship between brain and language. A similar interest was shown by Goldstein (1948) (with different interpretations of the data). Even much earlier, the years which followed Broca's and Wernicke's discoveries stimulated neurologists throughout the world such as Broadbent (1879) and Bastian (1887) in Britain, Pick (1913) and Salomon (1914) in Germany, and Moutier (1908) in France to apply linguistic analyses to aphasia data.

It is not surprising that Jakobson's pioneering work in this area lay dormant for many years. The study of the brain and, in particular, the mind was outside the scope of linguistic research in the dominant linguistic post-Bloomfieldian, pre-Chomsky paradigm in America. The mind did not exist in the early

behaviorist philosophy of language, since "the universe of discourse" of linguistics was constrained to "a set of utterances" and "noises" produced by speakers (Hockett, 1942), a position specifically put forth in Bloomfield's (1926) criticism of non-linguists who he says "constantly forget that a speaker is making noise, and credit him, instead, with the possession of impalpable 'ideas'. It remains for linguists to show, in detail, that the speaker has no 'ideas' and that the noise is sufficient."

II. The Present

Once Chomsky put the mind back into the brain, it was possible for linguists to ask questions about the brain/mind/language interface. The changes which took place in cognitive psychology and in the relatively new area of psycholinguistics as a result of Chomsky's influence were as dramatic as those in linguistics. Newmeyer (1980) shows this in his citation of the statement of George Miller "I now believe that mind is something more than a four letter, Anglo-Saxon word -- human minds exist and it is our job as psychologists to study them". (Miller 1962)

Furthermore, the basic distinction between linguistic competence and performance put forth by Chomsky made it possible to investigate both, with the recognition that "Performance provides data for the study of linguistic competence. Competence...is one of many factors that interact to determine performance." (Chomsky 1972)

Patients who can perform a linguistic task in one modality but not in another, appear to have an intact competence which is neutral as to production and comprehension. This observation led Weigl and Bierwisch (1970) to suggest that "aphasia syndromes in general are to be understood as disturbances of complexes of components or subcomponents of the system of performance, while the underlying competence remains intact." They did, however, suggest an exception to this -- agrammatism when it effects both speech and comprehension. They conclude that "competence and performance must be psychologically different aspects of the general phenomenon of speech behaviour...the distinction ... is not merely a heuristic or methodological assumption but reflects a fact that can be established neuropsychologically."

The new research objectives which influenced the field of linguistics brought changes in neuropsychology as well as psychology, and led to the development of the new 'hyphenated' area -- neurolinguistics -- the subject of this paper. The dramatic explosion of new journals and books in the field attest to this fact. A journal called Brain and Language whose editor received his Ph.D. in linguistics would have been as impossible pre-Chomsky as would journals like Mind and Language, Cognition, Cognitive Neuropsychology, Language and Cognitive Processes with editorial boards that include many linguists and whose contents are filled with articles by linguists.

The entry into the area of brain/mind/cognition studies was a logical development of the goal to understand the nature and form of human linguistic knowledge, how it is acquired, "how ... this system of knowledge arise(s) in the mind/brain?" and "How ... this knowledge (is) put to use in speech?" (Chomsky 1988)

The research on the theory of grammar which has been conducted these last thirty years provides ample evidence in support of the view that the human animal is able to acquire language because it is genetically endowed with "a distinct 'language faculty' with specific structure and properties."

The search for the biological basis of this language faculty underlies much neurolinguistic research, spurred on by Lenneberg's seminal work on this question. (Lenneberg 1967)

Blumstein (1973), one of Jacobson's students, followed his lead in her dissertation which, upon publication, further stimulated linguistic investigations of aphasia. She, and those that followed her, added a new dimension to aphasia research since they were interested in what aphasia data could contribute to our understanding of language, rather than with clinical concerns.

Aphasia research by linguists has been motivated by the fact that focal damage to specific brain areas results in the disruption of distinct cognitive functions, as well as motor and perceptual abilities. The selectivity, as discussed above, appears to be specific as to the parts of language which are effected, supporting a modular conception of the grammar itself, in which the components are interactive but independent of

each other. Neurolinguistic research continues to show that these components as well as the hierarchy of linguistic units posited by linguists appear are just those parts which can be differentially destroyed or damaged.

Linguists ask different questions than those that have been asked by the neurologists and neuropsychologists in their aphasia research. It may be of interest to linguists to find that specific lesion sites produce different aphasic symptoms, particularly when these differential deficits reflect different grammatical components or even units and rules, since such findings provide the biological framework for the language faculty. But knowing whether the anterior portion of the left temporal lobe is more directly responsible for syntactic processing than the posterior portion does not provide the syntactic theory by which the nature of the syntactic deficit is understood. Neurology can neither analyze nor explain agrammatic utterances of the patients who omit main verbs, such as the following:

- (1) The young...the girl...the little girl is ... the flower. (in trying to describe a picture of a girl giving flowers to her teacher)

or uses nominalizations instead of verbs:

- (2) The girl is flower the woman.
- (3) The man kodaks...and the girl...kodaks the girl (describing a picture of a man photographing a girl.)

(Badecker and Caramazza, 1985)

Nor can neurologists explain the utterances of patients referred to as agrammatics who have difficulty in producing fluent speech, and most often omit inflectional affixes and free grammatical formatives (e.g. determiners, pronouns, prepositions).

Without the input of linguists, aphasiologists are unable to explain why certain agrammatic patients have difficulty with inflectional affixes but not with derivational affixes (Badecker and Caramazza 1985; Kean 1977). A plausible account is provided by Grodzinsky (1986), on the basis of his study of a number of Hebrew-speaking aphasics for a formerly unexplained difference between agrammatic patients who omit gram-

matical formatives and paragrammatic patients who do not omit them but substitute incorrect grammatical morphemes and inflectional affixes. He points out that vowels in Hebrew words are predictable, according to inflectional and derivational morphological rules. For example, the vowel in the word for a single male child is 'e' yeled, and is 'a' for a female child yalda; the plural for these two singular nouns is viladim and yeladot, respectfully. Since the roots of Hebrew words consist only of consonants, e.g. /y-l-d/ in the examples given, agrammatic aphasic Hebrew speakers would be unable to talk at all if they omitted the inflectional and derivational morphemes which are realized vocally. What these Hebrew speakers did instead, was to substitute incorrect vowels in words such as those exemplified, and omit free standing grammatical morphemes.

Hyams (1988) also discusses agrammatic aphasics who differ cross-linguistically in whether they tend to omit inflections or not. She points out that her analysis, based on Government-Binding syntactic theory which distinguishes core from peripheral inflectional systems, makes exactly the right predictions regarding the agrammatic output that occurs. When inflection is a core property of the grammar as in Italian, Russian, or Hebrew, agrammatic aphasics do not omit inflectional morphemes; when inflection is a peripheral property of the grammar as in English, they do.

Caplan, (1985, 1988), a linguist-cum-neurologist, suggests that these patients not only have difficulty with grammatical morphemes but "have an impairment in the construction of normal syntactic structures". (Caplan 1985; Caplan 1987) Contrary to the earlier notion that agrammatism is solely a production deficit, linguistic investigations show a relationship between production and comprehension syntactic deficits (Heilman and Scholes 1976; Schwartz et al. 1980) For example, Caramazza and Zurif (1976) demonstrated that such patients were unable to understand sentences whose meanings depend on syntactic structure.

Thus, the results of studies of agrammatism demonstrate the independence of the syntactic component in processing as well as in linguistic competence since agrammatics illustrate that syntactic component or access to it can be damaged while the rest of the grammar remains intact.

A problem, however, has recently arisen in relation to the characterization of agrammatism as a syntactic dysfunction with the discovery by Linebarger, Schwartz, and Saffran (1983), that although many agrammatic patients have difficulty in understanding a sentence when the meaning depends on syntactic structure, they retain the ability to make correct 'grammaticality judgments'. This seemingly contradictory behavior has been termed the "syntax-there-but-not-there" paradox (Cornell et al. 1989) since the ability to produce grammatical judgments would seemingly depend on access to an intact syntactic component, whereas the production and comprehension problems suggest that either the component itself or access to it is damaged.

Further follow up studies (Lukatela, Crain and Shankweiler 1988) have shown that agrammatic patients make significantly more errors in their grammaticality judgments when the well-formedness or lack of same depends on inflectional features. In addition, the agrammatics seem to accept more ungrammatical sentences as grammatical than to reject grammatical sentences.

A linguistic processing model proposed by Cornell and extended by Mauner (Cornell, Fromkin, and Mauner 1989, 1990) seems to offer a solution to this apparent paradox. A basic assumption of the model is that grammaticality cannot be directly linked to intelligibility since we are able to understand ill-formed sentences. Yet, if grammaticality does not depend on the same process as comprehension, i.e. parsing, then grammaticality judgment becomes inexplicable. In addition, in speech comprehension, we must be able to assign some kind of representation to ungrammatical input, in order to understand it at all. However, to make a grammaticality judgment it can't be the case that this representation is a fully adequate syntactic structure. To account for this, the model includes a parser which can assign fragmentary representations, and a grammaticality judgment facility which can evaluate the completeness of such representations.

The solution offered by the model was suggested by recent linguistic proposals concerning "closed class" or "functor" categories in the work by Emonds (1985), Anderson (1982), Anderson (1987), Speas and Fukui (1986), and Lebeaux (1988). They present linguistic evidence for a separate "inflectional" level of representation in the grammar, which is similar in its general outline to the Positional Level of representation in the processing model of Garrett (1975).

The proposed model incorporating these theories involves two separate processors -- a syntactic processor which handles the traditionally syntactic functions such as determining constituent structure, and an inflectional component that contains a set of "spell-out" rules which provide the phonological representations of the inflectional features (of the grammatical morphemes) which occur with lexical stems. In comprehension, these rules analyze the phonological input structures into lexical stems and their inflectional features.

Note that a distinction between grammatical morphemes (in our terms represented by inflection features) and lexical formatives, is strongly supported by various kinds of performance evidence including speech errors (Garrett 1984, Garrett 1980, Garrett 1976), jargon aphasia (Buckingham 1981; Lecours 1974), acquired dyslexia (Marshall and Newcombe 1973; Patterson 1982), phoneme monitoring tasks (Mehler, Segui and Carey 1978, Newman and Dell 1978), and lexical decision tasks (Egido, Carey, and Garrett 1981).

The hypothesis embodied in the model is that the agrammatic deficit is caused by a disturbance of the interface between the inflectional and syntactic processors. Because of the disruption, the relevant inflectional features are not always available. In languages like Hebrew where the stem without such features is not an allowable word, an incorrect form is substituted. In languages like English, in which the bare stem is a well-formed word, the stem alone is usually selected.

In the model, the processor attempts to construct or assign the most complete representation possible which does not contradict the input. Since inflectional features are missing, all possible well-formed strings will be constructed which accounts for the Lukatela et al. (1988) findings that ungrammatical sentences were accepted more than were grammatical sentences rejected, when the source of the ungrammaticality depends on incorrect inflectional marking (which are not available to the agrammatic). It also explains why these subjects accept grammatical sentences correctly.

The details of the problem and the linguistic solution are given in Cornell et al. (1989, 1990) and are referred to here simply to illustrate how linguists can contribute to the understanding of linguistic

deficits following brain damage. Furthermore, since the model is a model of the normal processor, language disorders are seen as the result of damage to the normal grammar and in this way contribute to linguistic theory in general.

III. The Future

But what of the future? As we have seen, the earliest attempts to scientifically approach the brain/mind/language relationship arose with the study of individuals with localized brain lesions and resultant aphasic and cognitive disorders. Until recently, the linguistic analyses of these patients were for the most part quite naive. For many years, attempts to explicate this relationship depended almost solely on the investigation of the abnormal brain and the disordered cognitive behavior of brain damaged patients since methodology for studying the normal brain was lacking. The research, however, led to important discoveries and provided insights despite their limitations.

In addition, traditionally the major approach to studying the biology of language and cognition was through neuroanatomical investigations made at autopsy, as the only available means to do so. The problems of such an approach are obvious particularly if one is concerned with the 'living brain' and the representation and dynamic use of a system of knowledge like language.

As in the physical and 'other' biological sciences, the development of new technologies and new methodologies is now making it possible for us to ask questions of the 'living' brain and mind never possible previously. The use of X-ray computerized tomography (CT) of the brain since 1973 permits the neurologist to look at multiple and parallel brain 'slices' in the living subject at the time a deficit appears or any time afterward.

Magnetic resonance imaging (MRI) became available almost one decade later; this new technique provides images of the living human brain of a quality that rivals direct physical inspection. Figures 4 and 5 (on the pages that follow) show the templates traced from the MR images of the brain of a Broca's agrammatic aphasia and of a fluent Wernicke's aphasic, respectively. (Damasio and Damasio, 1989) One can see the different lesion sites leading to selective language loss.

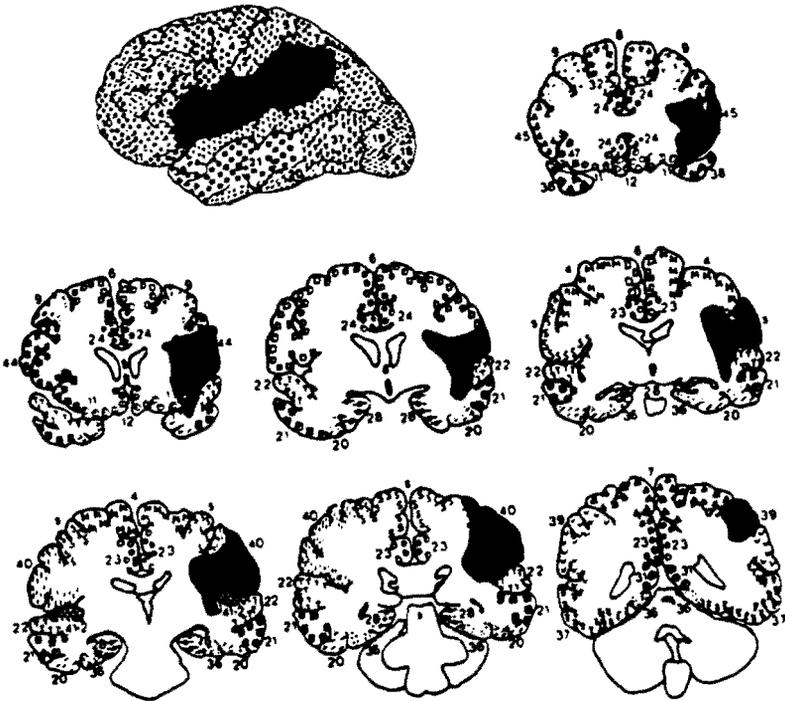


Figure 4: Templates taken from weighted MR images of a 67-year old, woman with Broca's aphasia, i.e. nonfluent, effortful speech, severe word-finding difficulty, and paraphasic (both semantic and phonemic) word substitutions. Comprehension of grammatically complex sentences was severely defective. The black areas show the site of the lesion. Each diagram represents a brain 'slice'. (Damasio and Damasio 1989; p 53)

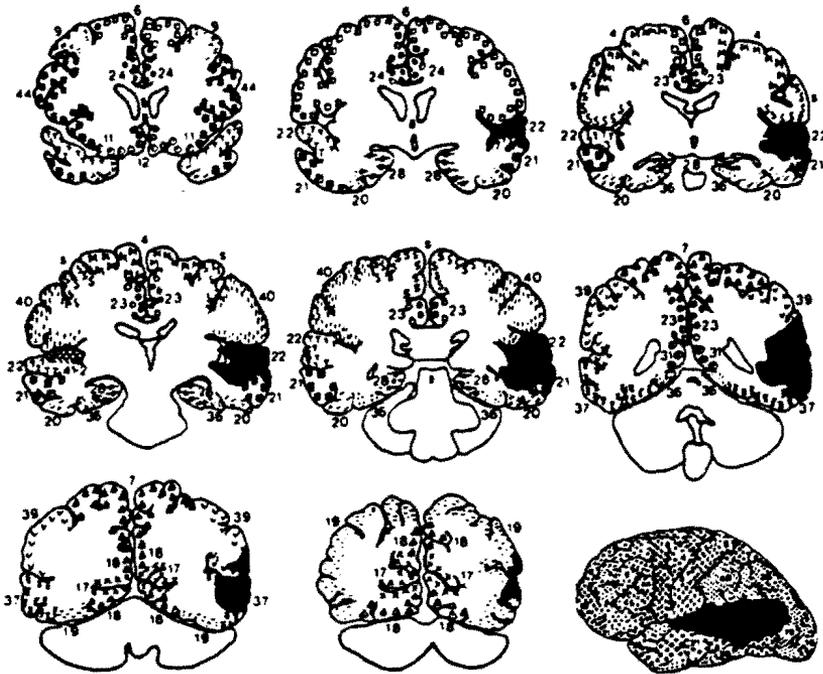


Figure 5: Templates taken from MRIs of a 63 year old woman with Wernicke's aphasia. Her speech was marred by neologism, but was well-articulated. Comprehension of words and sentences was severely defective. (Damasio and Damasio, 1989, p. 107)

Positron emission tomography (PET) is also a non-invasive technique for studying physiological processes in the brain, and surface electrical recordings provide a way to trace the time course of brain events subserving language and other cognitive behavior.

As we approach the 21st century, it has become apparent that an understanding of the brain/mind/language historical problem can not be realized through strict disciplinary approaches. This is cogently pointed out by Marshall (1980) : "Biologists...have accumulated a vast body of knowledge concerning the gross anatomy of those parts of the central and peripheral nervous system which seem to be implicated in the acquisition and exercise of linguistic abilities. Some knowledge is even available about the slightly less gross physiology of the relevant brain areas ... (And) psycho-linguists ... have amassed alarming amounts of data of the progression from the birth cry to the multiply-embedded relative clause. The problem is...that we have so far failed to construct a theory that (can) mediate between noun phrases and neurons."

One obvious reason for this failure is that we continue to cultivate our own gardens without the necessary cross-fertilization that can explicate the complex mapping between brain mechanisms, cognitive systems, and psychological processes, a connection that must exist.

The boundaries between the physical, biological and cognitive sciences are becoming less distinct. Nowhere is this more apparent than in the emerging study of neurolinguistics. Driven by well-developed linguistic theories, the new biological technologies can be utilized to gain a deeper understanding of the biological basis of human language. The future for neurolinguistics is certainly as challenging and exciting as the past and present have been. It may even be possible to finally construct a theory that will mediate between noun phrases and neurons.

- 1 Sections of this paper are taken from Fromkin (1987, 'in press')
- 2 Broca, unfortunately, also held extremely racist and sexist views, as shown by his statement: "In general, the brain is larger in mature adults than in the elderly, in men than in women, in eminent men than in men of mediocre talent, in superior races than in inferior races (1861. p 304) ... Other things equal, there is a remarkable relationship between the development of intelligence and the volume of the brain" (p 188) Stephen Jay Gould (1981) demolishes this unscientific position held not only by Broca but many other 'head measurers' of that time.

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What a Phrasal Affix Looks Like¹
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There is now a sizable amount of work that treats special clitics as phrasal affixes, including work by Zwicky (1977), Klavans (1980, 1985) and Kaisse (1985). Anderson, for instance, proposes "that a grammar contain a set of phrase formation rules closely parallel to word formation rules" (1988:177). Still lacking in the literature, however, is a specific proposal for treating special clitics (SCs) as phrasal affixes in a way strictly parallel to lexical affixation. In this paper I take a stab at remedying that situation by proposing a formal representation for phrasal affixes based on a proposal for lexical affixation developed in Inkelas (1989).

But why postulate phrasal affixes at all? What exactly *is* a phrasal affix? Simply put, a phrasal affix is an affix with phrasal rather than lexical distribution: a lexical affix occurs at the edge of a word, while a phrasal affix occurs at the edge of a phrase. To see the difference clearly, consider the different distribution of possessive *-s* and plural *-s* in English. The plural morpheme may be attached only to nouns (1a,b,c), not to an entire noun phrase (1d):

- | | | |
|-----|--------------|--------------------------|
| (1) | a. five dogs | c. five dogs and cats |
| | b. five cats | d. * five [dog and cat]s |

Similarly, a modified noun (2a) is made plural in English by adding *-s* to the head noun (2b), not by adding *-s* to the full noun phrase (2c):

- | | | |
|-----|-----------------------|--------------------------|
| (2) | a. the girl nextdoor | c. *[the girl nextdoor]s |
| | b. the girls nextdoor | |

The distribution of possessive *'s*, on the other hand, is noticeably different. In (3a, b) the possessive marker seems to have the same distribution as the plural marker in (1); but with a conjoined noun phrase, the possessive marker can be factored out (3c). Indeed, this seems to be preferable to marking each noun phrase singly (3d).

- | | | |
|-----|---------------------|----------------------------------|
| (3) | a. the bride's baby | c. [the bride and groom]'s baby |
| | b. the groom's baby | d. ?the bride's and groom's baby |

(3c) shows that possessive *'s* is a phrasal rather than a lexical affix. The possessive counterpart to (2), seen in (4), leads to the same conclusion:

- | | | |
|-----|-----------------------------|-------------------------------|
| (4) | a. the girl nextdoor | c. *[the girl's nextdoor] cat |
| | b. [the girl nextdoor]s cat | |

(4b) and (4c) show that the possessive marker *must* be affixed to a phrase, while (2b) and (2c) show that the plural marker *cannot* be affixed to a phrase.

Zwicky (1977) makes a distinction between *simple* and *special clitics*. Only the latter may be characterized as phrasal affixes. Simple clitics involve no change in relative order; the reduced form of *be* in (5a) occurs in the same position as the unreduced form of *be* in (5b):

- | | | |
|-----|---------------------|----------------------|
| (5) | a. What's his name? | b. What is his name? |
|-----|---------------------|----------------------|

Special Clitics (SCs), on the other hand, involve a change in the syntactic position of the clitic. (6a,b) show that the object NP must follow the verb in French; (6c,d) show that a clitic pronoun must precede the verb:

- (6) a. Je vois le garçon. 'I see the boy.' c. Je le vois. 'I see him.'
 b. *Je le garçon vois. d. *Je vois le.

Another example of SCs are the so-called prepositions of early classical Latin². (7a) has the relative order we would expect inside a prepositional phrase: [P NP]. (7b), with the same meaning, has the 'preposition' inside the noun phrase:

- (7) a. cum magna laude 'with great praise'
 b. magna cum laude 'with great praise'

Note that although the 'preposition' in (7a,b) may be characterized as a SC because of its peculiar positioning, it is not a *phonological* clitic. Thus, not all SCs are phonological clitics (cf. Klavans 1985).

SCs, then, may be characterized as phrasal affixes. English possessive 's, French pronominal *le* and Latin *cum* may all be characterized as affixes (consisting of a consonant, a syllable or an unstressed word, respectively) with phrasal distribution. Of course, just because these items *can* be treated as affixes does not mean that they *should* be. What evidence is there that supports such an analysis?

Kayne (1975) cites a number of properties of French pronominal clitics to motivate his claim that they are dominated at S-structure by the node V, as in (8):

- (8) [V le [Vvois]]

Kayne does not use the term 'phrasal affix' and does not analyze pronominal clitics as such; nevertheless, the properties of pronominal clitics that he cites to support (8) are all properties of affixes. They may thus be used as evidence that SCs are (phrasal) affixes. The properties Kayne cites are listed below (9a-f):

(9) Properties of SCs

- a. Nothing can come between C and its host (except other Cs)
 *Elle va les beaucoup apprécier. 'She's going to appreciate them a lot.
 Elle va les=apprécier. 'She's going to appreciate them.'
- b. Cs cannot be contrastively stressed
 *Jean la préfère. 'Jean prefers her.'
- c. Cs cannot be conjoined
 *Jean la et le voit. 'Jean sees him and her.'
- d. Cs occur in a fixed order
 Jean me le donnera. 'Jean will give it to me'
 *Jean le me donnera.
- e. Cs may not occur alone
 *Le/la/les. 'Him / Her / Them.'
- f. Cs may not be modified
 *Ils tous partiront bientôt. 'All of them will leave soon.'
 Eux tous partiront bientôt.

(9a-f) are not properties of syntactic categories like N, N' or NP, each of which can be conjoined, may occur alone, etc. (9a-f) are *all* properties of affixes, however, as (10a-f) make clear:

(10) Properties of affixes

- | | | |
|----|---|------------------------|
| a. | Nothing can come between A and its host (except other As) | |
| | two dogs with fleas | *[two dog with fleas]s |
| b. | affixes cannot be contrastively stressed ³ | |
| | ugliest | *ugliest |
| c. | affixes cannot be conjoined | |
| | jumps and jumped | *jumps and ed |
| d. | affixes occur in a fixed order | |
| | man | manliness |
| | manly | *mannessly |
| e. | affixes may not occur alone | |
| | *'s 'belongs to' | *s 'many' |
| f. | affixes may not be modified | |
| | cats | *cat[both] |
| | both cats | *cat[sboth] |

If SCs are to be treated as phrasal affixes, it should be possible to give subcategorization requirements for phrasal affixes just as we do for lexical affixes. Adapting Inkelas' (1989) formalism for lexical affixes, we may do just this. Inkelas argues that lexical affixes have two distinct sets of subcategorization requirements: one involves the morphological sister required for affixation (N, V, etc.) and the morphological result of affixation (A, N, etc.); the other involves the prosodic sister required for affixation (usually phonological word, ω) and the prosodic result of affixation (usually ω again). Consider Inkelas' treatment of the English adjectival suffix *-al* in (11):

- (11) *-al* Morphological Frame [[A]_]A
 Phonological Frame [[ω]_] ω

The inner set of brackets in the M(orphological) Frame states that *-al* is suffixed to adjectives; the outer brackets state that the result of affixation is a new adjective: [cursory]A > [cursorial]A. The inner brackets in the P(honological) Frame state that *-al* is attached to a phonological word; the outer brackets state that it forms a new phonological word with its host, evinced by the re-assignment of stress in the affixed form: [cúrsory] > [cúrsórial]. The M and P frames of an affix need not line up, according to Inkelas. The nominalizer *-ness* provides an instance of a mismatch between these frames:

- (12) *-ness* Morphological Frame [[A]_]N
 Phonological Frame [[ω]] ω _

The M frame says that *-ness* makes a noun out of an adjective by attaching to it as a suffix; the P frame says that *-ness* is not included phonologically in the resulting

word--that is, the resulting word is accented as if *-ness* were not there: [primary] > [primari]ness.

SCs can be analyzed in the same way. English possessive 's, for example, may be represented as follows⁴:

- (13) = 's Morphological Frame [[NP]_]NP
 Phonological Frame [[ω]_]ω

(13) states that 's is a function from noun phrases into noun phrases and from phonological words into phonological words. 's often involves a type of mismatch between frames--whenever the NP contains more than one phonological word, the P frame will be a proper subset of the M frame. French pronominal clitics such as *le* have the following type of frame:

- (14) *le*= Morphological Frame [_[V]]V
 Phonological Frame [_[ω]]ω

Clearly, (14) never involves a mismatch in frames since French verbs consist of only one phonological word.

The formalization proposed here is consistent with a curious type of mismatch between subcategorization frames: one in which the M frame requires a sister on a different side than the P frame requires. Two such possibilities are shown in (15):

- (15) a. MFrame [[NP]_]NP b. MFrame [_[NP]]NP
 P Frame [_[ω]]ω P Frame [[ω]_]ω

(15a) describes a morphological suffix that is phonological prefix; (15b) describes a morphological prefix that is a phonological suffix. I will argue below that such mismatched phrasal affixes are well attested in natural languages (especially of the form (15b)).

Anderson (1988) notes that SCs occur in six positions with regard to phrases: initial, peninitial, pre-head, post-head, penultimate and final. As (16) shows, these positions are derivable from the formalism given for SCs above:

- (16) Position as a product of morphological and phonological frames

		Morphological Sister			
		[_[X]]	[_[XP]]	[[X]_]	[[XP]_]
Phono- logical Sister	[_[ω]]	PreHead	Initial	3	Penult
	[[ω]_]	5	Peninitial	PostHead	Final

Phrase-initial and pre-head SCs are subcategorized for right M and P sisters; phrase-final and post-head SCs are subcategorized for left M and P sisters;

penultimate SCs are subcategorized for right M sisters but left P sisters; penultimate SCs for left M sisters but right P sisters.

Cell 5 describes a 'mismatched' SC that is morphologically prefixed to a head but phonologically suffixed to a phonological word. What would a language look like that had this type of affix? In the simple case, where the morphological host consisted of a single phonological word, the affix would show up as a suffix: [[word]=affix]. For compound hosts which consisted of more than one phonological word, however, the affix would be infixated after the first phonological word: [[word]=affix=[word]]⁵. Lithuanian seems to provide such a case. Joseph and Nevis (1989) point out that the reflexive marker *-s(i)-* typically occurs at the end of non-prefixed verbs (*maryti-s* 'to see oneself') but in second-position in prefixed forms (*pa-si-maryti* 'to see oneself PERF')⁶. If *pa-maryti* were phonologically [[pa]_ω [matyti]_ω] and if *-s(i)-* were an affix of the type in cell 5, the second-position of *-s(i)-* in compounds would be explained. I have not yet been able to find a case of the mismatch of cell 3--hopefully future research will turn up such a language.

A different type of mismatch which the formalism allows should be ruled out in principle. Inkelas' formalism (and my extension of it so far) allows for inner and outer M and P brackets to be labeled; while labeling of outer M brackets may be required for category-changing affixes, labeled outer P brackets leads to too powerful a formalism. Consider (17):

- (17) Morphological Frame [[NP]__]NP
Phonological Frame [[φ]__]_ω

(17) describes an impossible situation in which an affix demotes its phonological sister from φ to ω. Numerous other monstrosities are imaginable; the formalism as stated is clearly too powerful insofar as it does not rule out such impossibilities. A more restrictive approach is to assign an inherent prosodic category to the affix itself and let stray adjunction determine the category of the result of affixation.

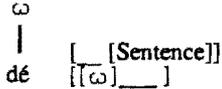
Take possessive 's as an example. Rather than stating that it forms a ω with its phonological sister, we may simply state that it consists underlyingly only of a consonant (C):

- (18) English possessive 's (final formulation)
- | | |
|---|------------|
| C | [[NP]__]NP |
| | |
| s | [[ω]__] |

Stray-adjunction will adjoin this C to a syllable (σ), forming a 'new word' automatically. Similarly, French pronominal clitics may be specified underlyingly as σ, as in (19):

- (19) French pronominal *le*= (final formulation):
- | | |
|----|-----------------------|
| σ | [__ [V]] _v |
| | |
| le | [__[ω]] |

- (27) Ancient Greek
- dé*
- 'and, but':



The prosodic specification of *dé* as a phonological word means that it will be adjoined to a clitic group, the next highest prosodic constituent on the prosodic hierarchy (Hayes 1989, Nespor & Vogel 1986). The claim made here, then, is that Latin =*que* and Hittite =*as* differ from Greek *dé* only in their underlying prosodic status: all three are phrasal affixes and all three have identical morphological and phonological subcategorization requirements.

The analysis of Phrasal Infixes given here crucially presupposes something I have not made explicit so far, namely, that the phonological sister of an affix must be a subset of the phonological material that comprises the morphological sister. I will call this the Sister Convention and assume that it holds for all languages:

- (28) Sister Convention: The Phonological Sister
- \Leftarrow
- Morphological Sister

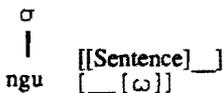
Note that the Sister Convention does *not* claim that the phonological *host* must be an element of the morphological sister; rather, it is a requirement on the phonological sister of an affix, not on the phonological mother--i.e., not on the dominating prosodic category created by adjunction. In terms of the representation given above, the Sister Convention requires that the inner set of brackets of the P frame occur inside the inner set of brackets of the M frame. But the Sister Convention makes no claims about the outer set of brackets of either frame.

To clarify the issue somewhat, consider the interesting case of "clitics with dual citizenship" discussed by Klavans (1985). Citing data from the Australian language Nganhcara, Klavans points out that there are languages in which a clitic is structurally a member of one constituent but phonologically a member of another. Consider the enclitic =*ngu* in (29) (=Klavans' (23)):

- (29) *nhila pama-ng nhingu pukpe-wu ku?a=ngu wa:*
he_{nom} man_{erg} him_{dat} child_{dat} dog=dat_{3sg} give
 'The man gave the dog to the child.'

Given Klavans' description of the positioning of =*ngu*, (29) can be characterized here as requiring a left-sister sentence and a right-sister phonological word⁸ as in (30):

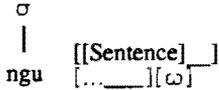
- (30) Nganhcara =
- ngu*
- (preliminary formulation)



Klavans points out, however, that =*ngu* does *not* attach phonologically to the last word in the sentence; instead, it attaches to whatever word immediately precedes it. That is, although it requires a right-sister consisting of a phonological word, it

forms a phonological constituent with the word to its left. (30) must thus be changed to (31), where the outer brackets of the P frame are moved to the left:

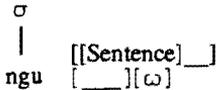
- (31) Nganhcara =*ngu* (final formulation)



The P frame in (31) states that =*ngu* requires a right ω sister but does not form a phonological constituent with it--instead, it is (phonologically) adjoined to the preceding word.

Languages like Nganhcara, then, help motivate both the inner and outer phonological brackets of the representation presented here. The three dots in the P frame in (31) are not gratuitous: they ensure that =*ngu* forms a constituent with preceding material rather than being stranded as a constituent of its own, as in the hypothetical (32):

- (32) Hypothetical stranded infix

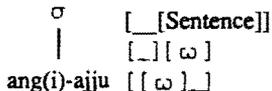


Surprisingly, there seems to be evidence that such stranded affixes exist. Simpson and Withgott (1986) present data from two central Australian languages, Warumungu and Warlpiri, that show that some clusters of clitics need not have hosts. Consider (33a), from Warumungu, and (33b), from Warlpiri (1986:150, 159):

- (33) a. Ang(i)-ajju nyanjjan b. Ngaka-rna-ngku nya-ni
 you-I see-present later-I-you see-nonpast
 'You see me' 'I'll see you later.'

S&W call *ang(i)-ajju* (33a) and *rna-ngku* (33b) 'pronominal clitic clusters' and note that such clusters appear in first or second position within a sentence⁹. In initial position in Warumungu (33a) "the pronominal cluster need not even be a clitic; it can form an independent word" (1986:159); in second position, however, it is cliticized to the preceding word as in (33b). (33a) might be characterized as a case of clitic stranding, insofar as the clitic cluster has no phonological host. (34) gives possible subcategorization frames for (33a):

- (34) Warumungu *ang(i)-ajju*:



The first P frame says that *ang(i)-ajju* requires a word to its right but does not form a phonological constituent with it or with any other word; the second P frame says it requires a ω -size sister to its left and forms a phonological constituent with it. The two P frames jointly indicate that the clitic cluster can appear either initially (freestanding) or pen-initially (as a suffix on the first word). (A later rule of the phonology is required to upgrade the stranded syllable *ang(i)-ajju* from σ to ω ; I will not attempt to formalize such a rule here.)

I have tried to show that SCs have many of the same properties as affixes and that they may be represented much like lexical affixes are. I suggested that SCs have both morphological and phonological subcategorization requirements that interact with each other and with the underlying prosodic status of the clitic to produce a surprisingly diverse set of phenomena. What a phrasal affix looks like determines how it behaves: phrasal affixes look and behave like lexical affixes, only bigger.

Footnotes

1. I am grateful to the participants of the 1989 WECOL for their thoughtful criticism and comment, especially to Eloise Jelinek; this paper is thus better than the talk it represents. Also responsible for improvements were George Bedell, John-Dongwook Choi, Harold Crook, Bruce Hayes, Sue Banner Inouye, Roger Janeway, Hiroyuki Nagahara and Emily Sityar. Special thanks to Cheryl Chan for extensive comments on an earlier version of this paper.
2. For a more detailed discussion of the same phenomenon in Homeric Greek, see Golston 1988, in which I claim that structures of the forms given in (7ab) are *both* cases of phrasal affixation.
3. George Bedell (pc) points out that this is possible in meta-linguistic contexts: 'I said uglier not ugliest.'
4. This is a preliminary formulation; both (13) and (14) will be revised below.
5. I am indebted to Bruce Hayes for pointing out that cells 3 and 5 might occur in languages with the right kind of compounding. For compounds that consists of more than one phonological word, see Inkelas 1989.
6. Examples are from Joseph and Nevis's 1989 LSA talk; my discussion here is sketchy because as this goes to press I do not have a copy of their final paper. Central to my analysis is the assumption that the verbal prefix *pa-* consists (at least at the appropriate level of representation) of a phonological word; at present I have no independent evidence for this assumption.
7. *ie*, *are* in complementary distribution with full NPs, cannot be separated from their hosts, cannot be conjoined, etc.
8. Klavans shows that this enclitic can also occur sentence-finally. To capture this within the present framework requires two Phonological frames, $[[\omega]]$ and $[_ [\omega]]$; this corresponds directly to Klavans' 'unspecified value' for her P2 clitic placement parameter (1985:104 ff.)
9. "(U)nlike affixes, their position is determined syntactically. They appear obligatorily either in sentence-initial position or after the first constituent in the sentence, regardless of its category" (1986:150). Presumably, S&W mean that clusters can occur after the first *syntactic* constituent (e.g., after first XP) rather than phonological constituent (e.g., after first phonological phrase). If so, they are not analyzable here as phrasal affixes. In any case, they stand as a case of clitics that needn't have a host.

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Secondary Predication and Null Operators in English
Michael Hegarty
M.I.T.

Infinitival adjuncts in English function as secondary predicates modifying a nominal element external to the adjunct clause. A Rationale Clause is an infinitival adjunct which modifies the TNS of the clause to which it is adjoined; it is therefore predicated of the event expressed by its host clause. Purpose clauses, infinitival relatives, infinitival complements of easy-tough, and degree adjectivals will all be analyzed here as infinitival adjuncts, and all modify an external NP. These infinitival adjuncts (unlike the RatC) may contain an NP gap coreferential with the external NP that they modify. As analyzed within the Extended Standard Theory, and subsequent work within the Government-Binding Framework, these infinitivals are introduced by a null operator which binds the gap. In this paper, it is argued that the null operator is an ordinary empty category (PRO) whose presence follows naturally once we posit mechanisms of secondary predication.

1. The Null Operator Constructions

1.1 Purposives

These include rationale clauses (RatCs), object purpose clauses (OPCs), and subject purpose clauses (SPCs). The latter two only will be called purpose clauses (PCs).

RatC

- (1) a. The shipwrecked crew lit a fire (in order) to attract the attention of searchers
- b. Lisa stayed up all night (in order) to finish her paper
- c. John bought a piano for Mary in order for her daughter to play sonatas on it
- d. John bought a piano for Mary in order to play sonatas on it
- e. For Mary, John bought a piano in order (for her daughter) to play sonatas on it
- f. John bought a piano for Mary to play sonatas on it
- g. John bought a piano to play sonatas on it

The RatC is not a null operator construction, but there is much to be learned by comparing RatCs with PCs, including an explanation of why the RatC isn't a null operator construction. The RatC denotes a rationale on the part of the agent to carry out the action described in the matrix clause. The RatC can optionally be introduced by in order. A non-subject gap cannot occur. A subject gap alternates with a lexical NP introduced by complementizer for. As illustrated above, such a sequence of for + lexical NP must be distinguished from a benefactive PP which can occur independently, adjoined to the matrix clause in a position immediately preceding the RatC, or preposed to the front of the matrix clause. In (1c), for Mary is a benefactive PP, and the RatC has a lexical subject introduced by complementizer for. In (1d), for Mary is a benefactive PP, and the RatC has no overt complementizer and has a null subject controlled by John. (1e) illustrates the preposability of the benefactive PP. (1f) is ambiguous: it can have a benefactive PP, for

Mary, adjoined to the matrix clause, with the RatC lacking an overt complementizer and having a null subject controlled by John; alternatively, for Mary can be analyzed as complementizer + lexical NP subject of the RatC. In (1g), the RatC lacks an overt complementizer and has a null subject controlled by John.

Following Jones (1985), I'll assume that the empty subject of a null subject RatC is PRO, controlled by the agent of the action described in the matrix clause, if such an agent is lexically present in the matrix. This claim is illustrated in (2).

- (2) a. The boat was sunk to collect the insurance
 b. The sign was hung to attract customers
 c. The boat was sunk by John to collect the insurance
 d. The sign was hung by the proprietor to aggrandize himself
 e. * John bought a piano for Mary in order to entertain herself with it

In (2ab), with no agent lexically present, the reference of the null subject of the RatC is quite free. In (2cd), the lexically present agent controls the null subject of the RatC. In (2e), the reflexive must be bound by the null subject of the RatC in order to satisfy requirements of binding theory; hence a matrix NP is a possible controller of the null subject of the RatC in (2e) if and only if the reflexive can be construed with it (assuming agreement in person, number, and gender). Thus (2e) demonstrates that matrix NPs other than the thematic agent can't control the null subject of the RatC when an agent is lexically present in the matrix clause.

OPC

- (3) a.i. John bought a piano_i yesterday to play sonatas on e_i
 ii. John bought it_i to play sonatas on e_i
 iii. John bought a piano_i to play sonatas on e_i
 b.i. Harry bought telescopes_i today to ship e_j to Switzerland
 ii. Harry bought them_i to ship e_j to Switzerland
 iii. Harry bought telescopes_i to ship e_j to Switzerland
 c.i. Mary bought a car_i last week for driving to work in e_j
 ii. Mary bought it_i for driving to work in e_j
 iii. Mary bought a car_i for driving to work in e_j
 d. John bought Mary a car_i (yesterday) to drive herself to work
 e. [This rack]_i is to hang coats on e_i
 f.i. John bought the piano_i today for Mary to play sonatas on e_j
 ii. John bought it_i for Mary to play sonatas on e_j
 iii. John bought the piano_i for Mary to play sonatas on e_j
 g.i. Harry bought telescopes_i already for us to ship e_j to Switzerland
 ii. Harry bought them_i for us to ship e_j to Switzerland
 iii. Harry bought telescopes_i for us to ship e_j to Switzerland

- h. A piano_i was given to John to play sonatas on e_i

An OPC denotes a purpose for the referent of a matrix NP which is thematically a theme. An OPC typically, but not invariably, contains an "object" (i.e., non-subject) gap. The object gap, when it occurs, is construed with the matrix theme. Sentence (3d) contains an OPC without an object gap. To see that the adjunct in (3d) is an OPC rather than a RatC, note that the subject of the adjunct clause must be controlled by Mary in order that the reflexive herself will be bound in its governing category (= the adjunct IP). A corresponding RatC with PRO subject, on the other hand, takes control of PRO by the agent of the matrix clause, as already discussed.

A subject gap in an OPC alternates with a lexical NP introduced by complementizer for. When the gap occurs, it consists of a PRO whose control is quite free--generally any NP whose referent might benefit from the action described in the matrix. Thus in John bought a broom to sweep the floor with, the broom can be intended for use by anyone who cares to sweep the floor. This can be seen equally well in the OPCs in (3), except that in (3d) the PRO subject of the adjunct can only be controlled by Mary due to binding theory requirements on the reflexive, as already discussed. As with the RatC, a benefactive PP of the form [for NP] can precede the OPC. Thus the sentences in (3fg) are ambiguous between structures with a (benefactive) PP preceding an OPC lacking an overt complementizer and having a null subject, and structures with no benefactive PP, but just an OPC with for complementizer and lexical subject.

OPCs must be carefully distinguished from infinitival relatives. The infinitival relative forms a constituent with the nominal head of the relativized NP, whereas the OPC does not form a constituent with the NP of which it is predicated. Faraci (1974) gives arguments for these structural differences. I'll assume that the infinitival relative is adjoined to its NP head (or its N' head) and that the OPC is not adjoined to the NP of which it is predicated. The (iii) sentences in (3) above are ambiguous as containing an infinitival relative and an OPC. To get the OPC reading, they should be assimilated in interpretation to the (i) and (ii) sentences.

SPC

- (4) a. John designed telescopes_i e_i to sit on Kitt Peak
 b. We brought John_i along e_i to talk to the children
 c. They hired John_i e_i for teaching syntax

The SPC denotes a purpose for the matrix theme, but in the SPC the matrix theme is construed with the subject of the adjunct; the subject of the SPC must therefore be null. The SPC is regarded in the literature as having a subject controlled by the matrix theme and no gap. However I will argue later that it is just a purpose clause with a subject gap instead of an object gap.

Arguments by Faraci (1974) and Jones (1985), repeated in Browning (1987), establish a VP-internal attachment site for PCs and a VP-external attachment site for RatCs. I'll assume that PCs are adjoined to the matrix V' and RatCs are adjoined to the matrix I'; further reasons for the X'-level adjunction sites will emerge later.

1.2 Infinitival Complements of Adjectives and Degree Phrases

Easy-tough

- (5) a. [This violin]_i is easy (for us) to play sonatas on e_i
 b. [This problem]_i is hard (for John) to solve e_i

Degree adjectival constructions

- (6) a. [The clothes]_i are too wet (for us) to hang
 b. John_i is too stubborn (for us) to talk to e_i

In the easy-tough constructions, I'll assume that the infinitival is an adjunct to the AP headed by easy, hard, tough, etc. Although syntactically an adjunct, it is a complement of the head of AP, being selected by it by autonomous θ -marking.

- (7) a. [ADJP easy [CP for [IP us to play sonatas on e]]]
 b. [ADJP easy [CP to play sonatas on e]]

Likewise, in the degree adjectival constructions, the infinitival is adjoined to an AP, and selected by the degree phrase too within AP by autonomous θ -marking. I'll assume that the matrix subject in these constructions is generated in spec of the matrix VP (accepting the VP-internal subjects hypothesis; otherwise in spec of IP); the subject has not moved out of the position of the gap. Support for these assumptions will be given later.

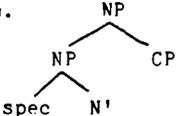
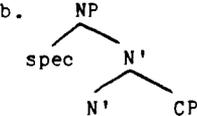
1.3 Infinitival Relatives

The gaps in infinitival relatives (IRs) also can take an empty operator analysis.

Infinitival Relatives (IR)

- (8) a. [a person]_i e_i to fix the sink
 b. [a professor]_i to talk to e_i

We have the choice of analyzing an infinitival relative as in (9a) or as in (9b).

- (9) a. 
 b. 

The choice is between adjunction of the infinitival CP to NP or to N'. In (9a), the infinitival CP modifies the lower NP, which is fully referential. In (9b), the infinitival CP modifies the N', thereby restricting the referent of N'. To get an argument in favor of (9a), consider the infinitivals in (11) and contrast them with the tensed relatives in (10).

- (10) a. Motorists who received a ticket lined up at the window
 b. Engineers who performed well received a raise.
- (11) a. Waiters to serve the soup arrived
 b. Engineers to fix the space shuttle climbed aboard the bus

In the tensed relatives in (10), motorists and engineers are not independently referential, but implicitly quantificational, as all motorists, the motorists, or some motorists, and all engineers, the engineers, or some engineers. But in the infinitival relatives in (11), waiters and engineers are referential; they refer to all and only the waiters or engineers on hand at the time. This gives a reason to adopt (9a) over (9b) for infinitival relatives.

1.4 Comparatives

The gaps in clausal complements of comparatives can also be analyzed as operator bound.

- (12) a. John is as tall as Bill is e
 b. John is taller than Max is e
 c. John is as smart as Bill said that Mary is e
 d. John is as tall as Bill said he has to be e in order to play basketball

I'll assume that these constructions are analyzed into a comparative degree adjectival, as tall, taller, as smart, taking a CP complement introduced by the complementizer as or than. The gap, marked by e in (12), is evidently an AP gap. How the gap is construed depends on the semantics that is given for the construction.

1.5 Extant Theories

For adjuncts that contain gaps construed with a matrix NP, null operator analyses have been standardly invoked, at least as far back as Chomsky (1977).¹ In extant analyses, at least the ones I'm familiar with, the null operator originates in the position of the gap and then moves to a canonical null operator position introducing the adjunct. One of these analyses, which I will call the $\bar{\sigma}$ -theory, has at least two versions, as follows.

Version 1. An empty operator or null wh is generated in the position of a surface gap (a $\bar{\theta}$ -position, subject to case assignment) and moves to an A'-position, binding its trace.

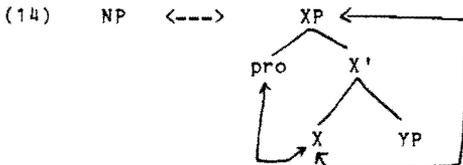
Version 2. A wh word is generated in the position of a surface gap and moves to an A'-position, binding its trace, and is deleted at PF.

An empirical problem with this theory is that it leads one to expect that null operators should alternate with overt wh words, which is not the case, as seen in (13)

- (13) a. * John bought a piano which to play sonatas on t
 b. * They hired John who t to teach syntax
 c. * This violin is easy which to play sonatas on t
 d. * This violin is easy on which to play sonatas t
 e. * the man who to fix the sink

As a theoretical point, we might expect a theory of these constructions to explain why null operators should occur in infinitival adjuncts, and not elsewhere. Conversely, we might expect our theory of these constructions to explain why PCs, easy-tough complements, and degree adjectival complements must be infinitival. In the $\bar{\sigma}$ -theory, the null operator is an autonomous entity, and purpose clauses, easy-tough complements and degree adjectival complements are simply CPs with $\bar{\sigma}$ in spec of CP. Postulating this environment for $\bar{\sigma}$ goes no distance towards explaining why these adjuncts must be infinitives.

An alternative to the $\bar{\sigma}$ -theory is the argument chain analysis of Browning (1987), in which the null operator is pro. In this account, the null operator pro is generated in the position of the gap, moves to the specifier of a maximal projection adjunct XP and thereupon enters into an agreement chain, as diagrammed in (14).



The chain in (14) is (pro, X, XP, NP), where pro and X agree by spec-head agreement, X and XP agree by feature percolation, and NP and XP agree as subject and predicate. But nothing in the role that pro plays in this theory suggests that an overt wh word could not play this role just as well. The only way to block the sentences with overt wh words in (13) is to stipulate that only pro can play the role that it plays in Browning's account. There is thus no principled way to block the sentences in (13), so this analysis suffers the same empirical difficulty as the $\bar{\sigma}$ -theory. It suffers the same theoretical drawback as well. The environment for operator pro is spec of a CP adjunct; but this goes no distance towards explaining why PCs, easy-tough complements, and degree adjectival complements must be infinitival.

2. A Theory of Secondary Predication

2.1 Outline

Predication by a head proceeds by local processes of θ -role discharge. Assuming the VP-internal subjects hypothesis, we can require all θ -roles of a head to be discharged within a maximal projection of the head, a strong locality condition. Secondary predication by maximal projections, such as PP, is non-local and therefore requires separate mechanisms. On the basis of semantic argument structure, I'll argue that prepositions take an external syntactic argument, PRO. The PP is predicated of the controller of PRO.

Consider sentences like those in (15).

- (15) a. John saw Mary in the kitchen
 b. John waved to Mary on the balcony
 c. Mary helped John in a difficult situation
 d. Mary talked to Susan with a parakeet standing on her shoulder

In (15a), the PP in the kitchen can be predicated of John, of Mary, or of the event of John seeing Mary. These different readings are expressed in the logical forms in (16), invoking an event position, as in Higginbotham's paper, "On Semantics" (Linguistic Inquiry 16, 1985).

- (16) a. Ee[see(J,M,e) & in(J, the kitchen)]
 b. Ee[see(J,M,e) & in(M, the kitchen)]
 c. Ee[see(J,M,e) & in(e, the kitchen)]

Under the semantic analysis in (16), in is a two place predicate which takes an internal argument, the kitchen, and an external argument, construed as John, Mary, or the matrix event. Assuming that the semantic structures in (16) reflect the lexical structure of the preposition in, the lexical entry for in should have two θ -positions; by the projection principle, these should both be projected, one as the internal argument of in, and one as the external argument of in. If we wish to hold on to the strong locality constraint by which all arguments of a predicate head are projected within the maximal projection of the head, then the external argument of the preposition should be projected within PP. So we have a structure like that in (17), with θ the external argument of in.

- (17) [pp θ [p_r in [the kitchen]]]

Syntactic considerations (preposability, passive) show that the position of θ is distinct from the argument positions of the matrix verb. So θ has to be an empty category. Conditions on movement chains show that θ is not an NP-trace, and θ is not a variable since it is not case marked. It remains that θ could be PRO or pro, depending on assumptions about the directionality of government. If government by the preposition is unidirectionally rightward, then θ in (17) could be PRO. If the preposition governs bidirectionally, then spec of PP is governed, and PRO cannot appear there, at least not at S-structure and beyond. The constraints are this: We want verbs to govern unidirectionally rightward, because we will have cases later in which a PRO subject of a sentence is generated and remains in spec of VP. But it looks like nouns govern bidirectionally at S-structure; an argument for this is given in Chomsky (1986a), p.193. These observations leave us with the following degrees of freedom in our choice of assumptions about the directionality of government.

- (DG1) Lexical heads with feature [+V] (V and A) govern unidirectionally rightward, while lexical heads with feature [-V] (N and P) govern bidirectionally.

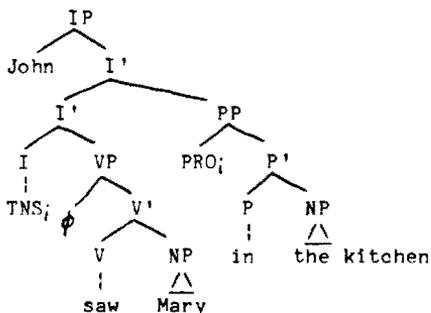
(DG2) Lexical heads with feature [-N] (V and P) govern unidirectionally rightward, while lexical heads with feature [+N] (N and A) govern bidirectionally.

Either assumption makes nouns govern bidirectionally and makes verbs govern unidirectionally rightward, as required. In order to avoid introducing pro into English unnecessarily, I'll assume (AG2), according to which prepositions govern unidirectionally rightward, allowing θ to be PRO in (17).

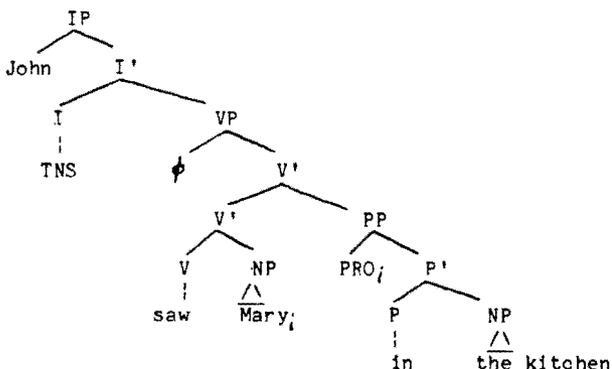
So a preposition projects an external argument in spec of PP, an ungoverned position, allowing for base generation of PRO in that position. The P' is then predicated of PRO in spec of PP; hence, via control (essentially binding), PP is predicated of the controller of PRO. When PRO in spec of an adjunct PP is controlled by a matrix NP, then the PP is interpreted as a secondary predicate which takes the NP as an argument. When PRO in spec of an adjunct PP is controlled by the matrix TNS, then the PP is an adverbial modifier of the event described by the matrix clause, since the event position of the matrix verb is bound by TNS. On this view, secondary argument-predicate structure is not accomplished directly by θ -role assignment to the argument, but indirectly, by control of PRO.

The effects of control of PRO in spec of an adjunct PP are illustrated in the structures (18) for the sentence in (15a), yielding the interpretations in (16).

(18) a.



b.



When PP is adjoined to I', as in (18a), PRO can be controlled by John, yielding the interpretation (16a) on which John saw Mary and John was in the kitchen. Or, when PP is adjoined to I', PRO can be controlled by TNS, and the PP is then an adverbial modifier of the event, yielding the interpretation (16c) on which the event of John seeing Mary occurred in the kitchen. When PP is adjoined to V', as in (18b), PRO is controlled by Mary, so that the PP is an adjectival modifier of Mary, yielding the interpretation (16b), in which John saw Mary, where Mary has the additional property of being in the kitchen.

2.2 Consequences for Binding Theory

The assumptions sketched above have some consequences for the application of binding theory to NPs within adjuncts.

- (19) a. John_i met Mary with pictures of himself_i
 b. John_i met Mary with pictures of him_i
 c. John met Mary_i with pictures of herself_i
 d. John met Mary_i with pictures of her_i

Assuming a PP adjunct with PRO in spec of PP, the governing category of the reflexives and overt pronouns in (19) will be PP. With adjunction of PP to V' in (19a), PRO will be bound by Mary, and the reflexive will be free in PP, violating Binding Theory (BT) Condition A; the corresponding reading, in which Mary possesses the pictures of John, seems, in fact, to be unavailable. With adjunction to I' in (19a), PRO is bound by John and the sentence is grammatical with the reading in which John possesses the pictures of himself. Adopting the same assumptions in (19b) yields opposite predictions of grammaticality, due to BT Condition B, with the pronoun bound within an I' adjunct PP, and free within a V' adjunct PP. These predictions are correct: the I' adjunct reading, in which John possesses the pictures, is unavailable, and the V' adjunct reading, in which Mary possesses the pictures, is fine. (19cd) are symmetric, with reversed polarity (so to speak): the PP with the reflexive in (19c) is fine as a V' adjunct and out as an I' adjunct, in accord with BT Condition A, and the PP with the pronominal in (19d) is out as a V' adjunct and fine as an I' adjunct, in accord with BT Condition B.

3. Logical Forms and Syntax of the Null Operator Constructions

Next we give some fairly explicit logical forms of the various null operator constructions. A logical form in this context is not an LF representation, but merely an expression of the truth conditions of a sentence in some fairly explicit notation. These logical forms are being drawn up with an eye to what might be required in the syntax to assure that the sentences are in fact interpreted as having the truth conditions set forth here. For the semantic machinery and the ideas behind it, I am indebted to Higginbotham (1985) and (1986).

3.1 Purpose Clauses

The infinitival adjuncts in English all seem to be interpreted with an implicit or explicit for introducing the adjunct clause. Examining these adjuncts in turn, we'll find that their contributions to the meanings of the sentences within which they are adjoined is to state that something in the matrix clause, either the event described, or an object mentioned therein, is intended to be for some irrealis (not yet realized) event, in the sense that, if an object, it is intended to be used in the irrealis event, and if an event, it is intended to assure that the irrealis event occurs. Assume that for is a 2-place predicate taking an object or event as its first argument, and an irrealis event as its second. Putting hats over event variables to indicate that they range over irrealis events, a logical form for the RatC in (1g) can be written as follows.

(20) $Ee[\text{buy}(J, \text{the piano}, e) \ \& \ \text{for}(e, \hat{e}')]$

That is, there is an event of John buying a piano, and that event is for (to assure the eventuality of) the irrealis event \hat{e}' of playing sonatas on the piano. For the PCs in (3a) and (4a), we have the logical forms in (21) and (22).

(21) $Ee[\text{buy}(J, \text{the piano}, e) \ \& \ \text{for}(\text{the piano}, \hat{e}')]$

(22) $Ee[\text{design}(J, \text{the telescope}, e) \ \& \ \text{for}(\text{the telescope}, \hat{e}')]$

In (21), it is the piano that is for (using in) \hat{e}' , and in (22) it is the telescope that is intended for the irrealis event of sitting on Kitt Peak. Applying the theory of secondary predication given before, we can assure that these are the truth conditions of (1g), (3a), and (4a) by adopting the following syntax for these constructions. Assume that the infinitival adjunct is CP with for in C; lexical subjects of the adjunct IP surface in spec of IP where they can be case-marked by for. When an adjunct IP lacks an overt subject, we can assume that the external argument of the verb in the adjunct is PRO, but then it should not occur in spec of IP since this position is governed by for; I'll assume that PRO in this case is generated, and remains, in spec of VP. In any event, for takes PRO in spec of CP as its external argument; this PRO is bound by the matrix TNS in the RatC, since the RatC is an I' adjunct, and by the matrix direct object in the OPC or SPC, since these are V' adjuncts. This will yield the first argument of for in (20)-(22). The second argument of for is the denotation of the adjunct IP. Following Stowell (1982), I'll assume that the infinitival has a tense operator, but one

that is unmarked for [\pm past]. The tense operator in the infinitival discharges the event position introduced by the main verb of the adjunct, but since it is unmarked for [\pm past], the tense operator in the infinitival fails to fix the time of occurrence of this event; as a result, the infinitival adjunct clause denotes an irrealis event rather than a truth value. It is important that the adjunct event position, while not fixed in time, is nevertheless bound by an operator so that the adjunct IP is a closed expression and can then be taken as an argument of for. The structure is then the following.

(23) John TNS [ν P buy a piano_i [ν CP PRO_i [ν C' for [ν IP to play sonatas on]]]]

This yields an explanation for why there is no operator-bound gap in the RatC: the PRO external argument of for is bound by the matrix TNS in a RatC, and so is not available for binding by a matrix NP; hence PRO in the RatC cannot bind an empty NP gap.

3.2 Easy-Tough constructions

In an easy-tough construction like that in (5a) I'll assume that easy is predicated of the violin, taking it as an argument, and that it takes a second argument consisting of a comparison class with respect to which the "easiness" of the violin is relativized. Thus the logical form in (24).

(24) Ex[violin(x) & easy(x, [y: for(y, [to play sonatas on y])])]

That is, the violin is easy with respect to being a thing to play sonatas on. Relativization to a comparison class is accomplished by autonomous θ -marking; hence, I'll assume that easy selects the infinitival adjunct by autonomous θ -marking, as shown in (25).

(25) This violin is [ν Aeasy [ν CP PRO_i [ν C'for [ν IP to play sonatas on e_i]]]]

Note that the PRO in spec of CP, the external argument of for, turns out to be arbitrary in the logical form (24); semantically, PRO in (25) effects a λ -abstract, forming a property with respect to which the violin is said to be easy. A typical effect of autonomous θ -marking is to relativize predication to a comparison class, as discussed in Higginbotham (1985). In (25), autonomous θ -marking of CP evidently makes PRO in spec of CP unavailable for binding by the matrix TNS or the matrix subject. The PRO external argument of for then becomes a variable ranging over members of the comparison class, specifically, ranging over all those objects which are items to play sonatas on. The role of for postulated above for PCs is thus corroborated, modulo the peculiarities of the easy-tough construction.

3.3 Degree Adjectivals

For the degree adjectival construction in (6a), the discussion so far motivates a logical form such as the following, where *d* is a variable over degrees of wetness.

- (26) a. Ed[wet(the clothes,d) & d > sup{d': for(the clothes,
 [to hang wet at d'])}]
 b. Ed[wet(the clothes,d) & d > sup{d': Qx[for(x,[to hang x
 wet at d'])}]

If CP is an unselected adjunct, then PRO in spec of CP is bound by the clothes, so the gap is construed with the clothes and the resulting reading is that in (26a): the degree to which the clothes are wet is greater than the degrees at which those very clothes can be hung. If CP is selected (autonomously θ -marked) by the degree adjectival too wet, then PRO induces λ -abstraction ranging over Q-things that could be hung when they have a wetness of degree d'. The reading is then that in (26b): the clothes are wet to a degree which exceeds those degrees at which things (in general) can be hung. In either event, the syntactic structure is the one in (27).

- (27) The clothes are [_{CP} PRO_i [_{C'} for [_{IP} to hang t_i]]]]

3.4 Infinitival Relatives

The infinitival relative (8a) has the logical form in (28a) and the syntactic structure in (28b).

- (28) a. λx [person(x) & for(x, \hat{e}')]
 b. [_{CP} PRO_i [_{C'} for [_{IP} e_i to fix the sink]]]]

3.5 Comparatives

Consider the comparatives in (12ab). In line with the assumptions being adopted here, the prepositional complementizer as should project an external argument in spec of CP and this argument should be generated as PRO. The gap in the comparatives, bound by the external argument of as, is construed with a degree of tallness which must be introduced in the matrix clause. For this reason, I'll assume that the adjectival expressions as tall and taller introduce reference to degrees of tallness and bind the PRO external argument of the complementizer as. The syntax is shown in (29).

- (29) a. John is [_{AP}[_{A'}as tall]_i [_{CP} PRO_i [_{C'} as [_{IP} Bill is e_i]]]]
 b. John is [_{AP}[_{A'}taller]_i [_{CP} PRO_i [_{C'} than [_{IP} Bill is e_i]]]]

4. The Internal Syntax of the Null Operator Constructions

4.1 The Syntax of the Gap

Looking at the syntactic structures just posited, note that with a PRO external argument of for (or as), the gap is construed with the binder of PRO. This suggests that the null operator in these constructions is just PRO, the external argument of the prepositional complementizer. In this case, the null operator appears in an argument position with an

independent θ -role from that of the gap, so it cannot have moved from the position of the gap. Therefore the gap is an empty NP, base generated independently of the PRO in spec of CP. The D-structure under consideration is then the following.

(30) [CP PRO_i [C' for [IP ... e_i ...]]]

Note that the relationship between PRO_i and e_i exhibits island effects.

- (31) a. John bought the piano [to persuade Mary to play sonatas on e]
 b. John bought the piano [to convince Mary that she could play sonatas on e]
 c. John bought the telescopes [to convince Mary to ship e to Switzerland]
 d. * John bought the piano [to convince Mary of the claim that she could play]
 e. * John bought the piano [to wonder who can play]
 f. * John fetched the telescopes [to show that Mary wondered who shipped e to Switzerland]
- (32) a. This violin is easy to persuade Mary to play sonatas on e
 b. * This violin is easy to believe the claim that Mary plays sonatas on e
 c. * This violin is easy to wonder who can play e
- (33) a. The clothes are too wet to persuade Mary to hang e
 b. * The clothes are too wet to believe the claim that Mary hung e
 c. * The clothes are too wet to wonder who hung e
- (34) a. [the woman] to convince Mary to talk to e
 b. * [the woman] to believe the claim that John talked to e
 c. * [the woman] to wonder who talked to e

If e_i were pro in (30), then it could be bound long distance by the empty operator PRO_i, so there would be no need for it to move, and we would have no explanation for the island effects. So e_i is not pro. Since e_i is A-bound, it cannot be a variable, else it would violate Binding Theory Condition C. There are some other options here, most of them not very well motivated and not very workable. I'll assume that e_i is PRO, which is generated in the position of the gap and moves out to avoid government at S-structure, eventually adjoining to VP of the infinitival clause; since V governs unidirectionally rightward, this PRO can appear at S-structure adjoined to the VP of the infinitival adjunct. I'll call this PRO the gap-PRO to distinguish it from the PRO external argument of *for*. The movement of the gap-PRO captures the island effects noted above.² There is independent reason to believe that, quite generally, PRO can be generated in a governed position and move in the mapping from D-structure to S-structure to avoid government, namely, in the analysis of (35a) with the structure in (35b).

- (35) a. John tried to be noticed

b. John tried [_{CP} [_{IP} PRO_i to be noticed t_i]]

4.2 Evaluation of the theory proposed here

Note that spec of CP in (30) is the external argument position of the prepositional complementizer for; it follows that wh-words cannot move into this position. Furthermore, this position is un governed and therefore receives no case; it follows that the argument generated there cannot be a lexical NP, so overt wh-words cannot be generated in place of the null operator. Thus the ungrammatical sentences in (13) cannot be generated, so the theory proposed here doesn't share the empirical difficulty of the \bar{C} and agreement chain (pro) theories discussed before. It doesn't share in their theoretical difficulty either. Since the second semantic argument of for is an irrealis event, and since only infinitival clauses denote irrealis events, it follows that for must take an infinitival IP as its internal syntactic argument. This explains why these adjuncts must be infinitival.

Notes

1. Except perhaps for the subject gap in the SPC.
2. In an SPC, we have the option of saying that the gap-PRO remains in its base position in spec of VP.

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Morphology and the Use of Space in ASL Verb Agreement^{*}
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The languages of the world typically make both morphological and syntactic distinctions between referential nominals, and locatives. It is therefore of interest for students of language typology that ASL has generally been claimed to have a single morphological paradigm in which pronominal and anaphoric referential forms, and locative forms are identical (Hofmeister 1978, McIntire 1980, Baker and Cokely 1980, Petitto 1983). For example, Petitto (1983: 36-37) describes the paradigm this way:

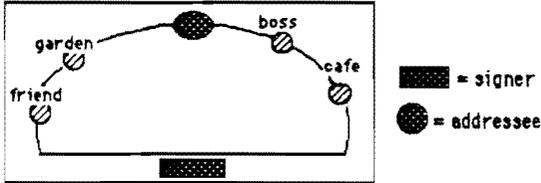
In considering the way [...] deictic terms in ASL are signed, a central difference between signed and spoken languages is revealed. In English, for example, each class of relational deictic terms takes distinctly different forms. Relational deictic terms of place, for example, can be expressed by the forms here and there, demonstrative deictic pronouns can be expressed by the terms this and that, deictic terms of movement by the forms come and go, and so forth. In ASL, however, all deictic expressions of this nature are signaled with the pointing form: the same form which is used for first, second, and third personal pronouns and used in anaphoric referencing.

When Petitto talks about a *pointing form*¹, most likely she is talking about two parts of the form. One is the handshape which is used – an extended index finger. The other is the use of space both for independent pronouns and for verb agreement.

For pronouns and verb agreement, the space in front of the signer's body is used to set up abstract loci which represent people, places and objects. These abstract loci, being simply points in space, are thus identical in form for both locatives and referentials. When space is subdivided into abstract loci in this way it is called the *structured space* (Loew 1984). An example of the structured space in which a variety of referents and locations have been set up is given in (1). Referents and places have been set up with respect to the signer and the addressee. The particular points used, and what they are associated with, are defined within a discourse.

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(1) an example of the structured space



While most of the claims that referentials and locatives are the same appear to be based on the phonetic similarities of the two semantically distinct groups, Shepard-Kegl (1985) explicitly argues that the similarities are both morphological and structural. Only a few researchers, for example Padden (1983) and Liddell (1989), have claimed that referential and locative forms differ; and their observations are limited to the phenomenon of verb agreement. Padden argues that there are three classes of verbs: plain, inflecting, and spatial. The plain verbs, as opposed to both the inflecting and spatial verbs, have no agreement morphology. An example of a plain verb is LIKE.² With inflecting verbs, the agreement morphology shows person/number agreement with the subject and/or object. One example of an inflecting verb is the sign HATE. This sign has movement directed from the subject to the object, and the palms are oriented toward the object. In the third class, spatial verbs, the agreement morphology indicates agreement with locatives.³

By focusing on how these forms are used, rather than on the forms themselves, Padden demonstrates that inflecting and spatial verbs use space in different ways. In particular, she shows that, for spatial verbs, the spatial relationship of the loci is important. Variation in their positions yields a difference in meaning; whereas for inflecting verbs, this is not the case. Variation in the positions of the loci counts as phonetic variation rather than as a change in meaning. This can be seen in example (2), taken from Padden, in which the meanings of a spatial verb and the reciprocal form of an inflecting verb are compared for three sets of agreement loci.

(2) (adapted from Padden 1983:51&61, figures 19-21 & 47-49)

inflecting verb:
reciprocal form
INFORM



(19) 'inform each other'



(20) alternate form



(21) alternate form

spatial verb:
PUT



(47)



(48)



(49)

'put the rocks next to each other'

'put the rocks behind each other'

'put the rocks in the other's place'

Following Padden's lead in looking at usage rather than at forms, I will argue that Padden's distinction in the use of space for referentials as opposed to locatives is actually a systematic distinction which pervades the language rather than being limited to verb agreement. My argument is based on four types of phenomena: the establishment of loci in space, simple pronouns, role play, and number incorporation in pronouns. Furthermore, I argue that recognition of independent paradigms for referentials and locatives based on their differential uses of space makes it possible to analyze a number of variations in verb agreement morphology as the expression of morphological differences rather than as lexical facts about particular verbs as Padden is forced to treat them.

My proposal is superficially similar to an analysis by Poizner, Klima, and Bellugi (1987) who distinguish between 'spatial mapping' and 'spatial syntax'. Spatial mapping is used for descriptions of spatial arrangements such as the layout of a room, and uses space to show spatial relationships. They call the use of space in spatial mapping 'topological' use of space. Spatial syntax is the use of space for pronominal reference and verb agreement. According to those researchers, it is only for spatial mappings that the spatial relationships of loci are important. My analysis differs from theirs in two crucial ways. First, their analysis does not address the systematic differences between referentials and locatives. In fact, I argue that the distinction they draw is actually a distinction between referents and locatives rather than between mappings and syntax since, as already shown by Padden, topological uses of space do occur in spatial syntax. Secondly, I argue that there is only one system of space but that the signer can express different perspectives on the space and the loci in it. These different perspectives relate to whether or not locative information is viewed as relevant to the utterance, and in fact are expressed by the choice of referential or locative morphology⁴.

Before going into the ASL data in depth, let me illustrate with an example from English what I mean by systematic differences in use being representative of distinct morphological paradigms. Example (3) shows the personal and locative pronouns of English. In addition to differences in form across the two groups, each has subdivisions within the group exemplified by different forms. Moreover, each group is characterized by semantic features which give it internal organization. For example, as with all deictics, both are organized according to some measure of proximity to the speaker; but personal pronouns and locatives in English differ in a small way from each other with respect to this feature. Personal pronouns make a 3-way distinction in English, while locatives make only a two-way distinction.

The two paradigms are further differentiated by the fact that personal pronouns in English are further subdivided on the basis of number (some dialects have a more complete paradigm than others, as indicated by the forms in parentheses), and to some extent on the basis of nominative versus accusative case.

(3) The personal and locative pronouns of English

personal pronouns

1st sing.	<i>I / me</i>	1st plural	<i>we / us</i>
2nd sing.	<i>you</i>	2nd plural	<i>you (y'all, you guys)</i>
3rd sing.	<i>he, she, it</i>	3rd plural	<i>they / them</i>

locative pronouns

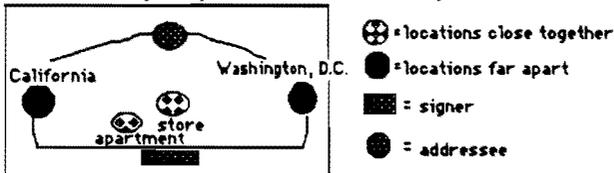
near the speaker	<i>here</i>
away from the speaker	<i>there (yonder)</i>

In order to make the discussion of the ASL data understandable, let's look in more

depth at how the structured space works. As noted above both locatives and referentials share the formational similarity of using the space in front of a signer's body and subdividing it to express a variety of referents or places. A referent or place is mentioned in the discourse and assigned to a locus in the structured space. From this point on, it is important to note that when I use the words *locus* or *loci*, I am referring to abstract points in the structured space, regardless of whether the points are associated with locative or referential forms. Since my use of the terms *referential* and *locative* are somewhat nonstandard, my first example of how they differ will also serve as an explication of my terminology.

There are a number of mechanisms which can be used to accomplish the establishment of a referent or location at a locus; the ones relevant for this discussion are pointing, glossed as INDEX, and the use of a classifier with a verb of location⁵. The pointing mechanism can be used to establish either locatives or referentials. If it is used for referents, their particular positions with respect to one another are irrelevant. However, if it is used for locatives, then the pointing not only associates the locatives with loci in the structured space, but it also expresses spatial relationships among them. In essence, the loci used for locatives are less abstract than referential loci. The importance of spatial relationships in locative uses is illustrated in (4). In (4) we see that when places are viewed as being relatively far apart, the loci are also established fairly far apart as is the case with the loci for California and Washington, D.C. indicated by the black circles. The checkered squares illustrate loci for places which the signer views as relatively close together. Thus, in this case, the loci are set up close to each other.

(4) locations relatively far apart, and locations relatively near



The use of a classifier verb of location to establish loci by necessity communicates some referential information due to the fact that ASL classifiers express features of objects or people. However, this method of establishing loci also necessarily expresses locative information. In particular, the classifier forms express the relative locations of referents, relative distances between referents, and the physical orientation of referents with respect to one another. The verb, glossed as TWO-SEATED-PERSONS-BE-LOCATED, indicates that the referents are seated relatively close together, and that they are facing each other (5).



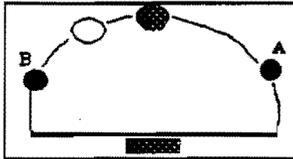
Because classifier verbs of location indicate that referents are in some particular locative relationship, I include them in my use of the term *locative*. Thus, when I use the term *locative*, I mean to include any form that expresses locative information. On the other

hand, my use of the term referential is more restrictive, and includes only forms that express purely referential information.

From the discussion of how loci are established in space, we see that, just as in verb agreement, for referentials the particular positions of the loci are unimportant; whereas for the locatives the particular positions are crucial, since the physical relationships of the loci express spatial relationships among the locatives with which they are associated.

The second example of the morphophonemic difference comes from the way that the simple pointing form of pronouns is used. Using the structured space shown in example (6)(a), compare two sentences, one for which the loci are locative (6)(b), and one for which they are referential (6)(c).

(6)(a) structured space



- (b) A = Rochester sentence 1: ME LIVE BUFFALO
 B = Buffalo 'I live in Buffalo'.
 (c) A = John sentence 2: INDEX-3B ANGRY
 B = Mary 'She (Mary) is angry'.

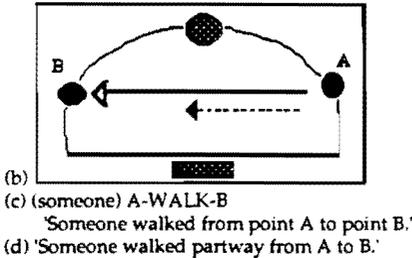
The first sentence, where the loci are associated with locatives, would normally be signed by pointing at B. Now suppose the signer points instead at the locus represented by the white circle; the meaning of the sentence changes. Instead of meaning 'I live in Buffalo' it means 'I live in between Buffalo and Rochester'.

Using those same loci, look at the sentence in (6)(c) where the loci are associated with referents. Now if the signer points at the area with the white circle rather than at B, the referent at B will still be successfully picked out. In other words, for the referential use, as long as the pointing is made closer to B than to other referents, eg. A, the meaning of the sentence does not change. A fairly wide range of phonetic variation is permitted without a change in meaning. This differs from locative uses where the space between established loci is meaningful. In the case of locatives a point at a non-established locus in space, in effect establishes a new, but unnamed locus. Phonetic variation must be minimal to avoid a change in meaning.

Another example of verb agreement will show that the difference in behavior of the agreement morphology is not restricted to the reciprocal morpheme used in Padden's example. Example (7) uses the same structured space as given in (6). Consider the sentence in (7)(a) where the loci are associated with referents. Using the exact loci set up, the verb KICK would be made with movement from A to B. This movement is represented by the solid line given in the diagram in (7)(b). If the movement goes only partway towards referent B, as indicated by the dotted line, there is still no change in meaning.

(7)(a) MAN KICK CAT

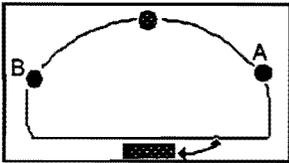
'The man kicked the cat.'



If the loci are now associated with locatives, as in (7)(c), the movement would again be from A to B as shown by the solid line in (7)(b). We can contrast that case with the one indicated by the dotted line in (7)(b) where the movement goes only partway. The sentence represented by the dotted line when the loci are locative is acceptable, but it does not mean the same thing as the sentence in (c). Rather it has the meaning of the sentence given in (d). So, once again, we see that when the loci are referential, there are no spatial relationships expressed, and phonetic variation is permissible. But when the loci are locative, spatial relationships are expressed by virtue of the presence or absence of space between them. Therefore the space itself is meaningful, and very little phonetic variation is possible.

The third type of phenomenon which exemplifies a morphophonemic distinction between referential and locative uses of space is role play (Loew 1984). Role play, similar to empathy markers in other languages, is morphologically expressed by a movement of the signer's body towards an established locus for a referent (8).

(8) Role Play: movement of the signer's body toward the locus for A.

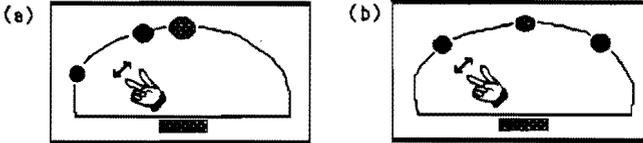


By virtue of the meaning of this construction, it is restricted to referential uses.⁶ It nevertheless is consistent with other referential uses of space in that it allows a relatively large amount of phonetic variation. In particular, although referents are set up at various distances from the signer, in role play the signer's feet stay in basically the same spot. Therefore the movement, although in the direction of the established locus, is crucially not all the way to that locus.

The final bit of evidence for a morphophonemic difference comes from number incorporation in pronouns. As with role play, the use of this construction is limited to referents. In number incorporation, plural referents can be referred to together through the use of a simple personal pronoun, INDEX, combined with the morpheme indicating the number (Chinchor 1981; Cagle 1987). So instead of saying 'they' (INDEXpl) to talk about two people, it's possible to say 'the two of them' which in ASL is a single word TWO-OF-THEM as shown in (9). The sign is made with a '2' handshape and a slight

back and forth movement localized in a central spot as in (9)(a), or staying close to just one of the referents as in (9)(b). (9)(b), where the sign is made nowhere near the referent on the right but instead stays at the locus for the referent on the left, shows that the phonetic variation permissible with referents can at times be quite large.

(9) TWO-OF-THEM (made with a '2' and a slight back and forth movement)



● = addressee ● = referents of TWO-OF-THEM

To summarize, the use of space in five different phenomena (pronouns, verb agreement, role play, number incorporation in pronouns, and establishing loci in space) shows that there is a systematic distinction between referentials and locatives. For referentials, the use of space is completely abstract.⁷ The space itself, and the relative distances among loci are irrelevant, thereby allowing a great deal of phonetic variation in the production of referential forms. For locatives, on the other hand, space is meaningful. Because the use of space indicates spatial relationships among the loci, not only the loci themselves, but also the spaces between them have meaning. As a result, only slight variations in phonetic form are permissible.

Following up on work by Schick (1987) I will label the use of space in referentials *model space*, and the use of space for locatives *real space*.⁸ Real space is like space in real life where things stand in spatial relations to one another. Model space is the use of physical differences in space to represent non-spatial differences in language.

It is important to point out that by using the term *real space*, I do not mean to imply that the use of space for locatives is analogue, or even at the same scale as in real life.⁹ Certainly the loci established for locatives represent a scaled-down version of the real world. And even in this scaled-down version, there are a limited number of relationships that can be shown (Supalla 1978). In my example of verb agreement used above, when the movement goes only partway, it is only the lack of hitting the endpoint rather than the relative distance from the endpoint that is significant. The sentence means 'partway' rather than 'half-way' or 'two-fifths of the way'.¹⁰

Returning to Padden's verb classes, given below in (10), we can see that they are split exactly along the lines that a real/model space distinction would predict. One set of verbs, the spatial verbs, uses real space; another set, the inflecting verbs, uses model space, and the third set uses no space at all.

(10) Padden's three verb classes

Plain	has no agreement morphology
Inflecting	agrees with subject and/or object
Spatial	agrees with locatives

In what follows, I will suggest that there aren't three different verb classes. Instead, I believe that there is a single class of verbs, but several different verb agreement forms which in part express differences in the signer's perspective on the use of space. Many verbs can be combined with two or three of these agreement markers allowing a signer

to use space in different ways with the same verb.

For these arguments to make sense, we need to look more closely at the forms of agreement morphology. There are four phonetically distinct forms of agreement morphology – zero-marking, locational, orientational, and directional – listed in example (11). It is also possible to have combinations of these forms on the same verb (Fischer and Gough (1978), and others), but we will restrict ourselves to cases of one agreement form per verb.¹¹

(11) phonetically possible forms of verb agreement

<u>type of morphology</u>	<u>description of its form</u>
zero-marked	(no form)
locational agreement	made at a locus
orientational agreement	palm faces locus for object, back of hand faces subject ¹²
directional agreement	movement between loci

We have already seen an example of directional morphology in the movement of the verb KICK shown in example (7). In orientational agreement, the orientation of the palms rather than movement of the hands shows agreement. The verb PITY has this kind of agreement. Locational agreement is made with the hands at the locus of the referent or locative agreed with as explained earlier (example 5) for the verb SIT. An example of a plain verb would be the verb LIKE mentioned before. The table in (12) shows which agreement forms occur with each class of verbs.

(12) table of agreement forms organized by verb class

<u>Plain</u>	<u>Inflecting</u>	<u>Spatial</u>
zero-marked		
	orientational	
	locational	locational
	directional	directional

Notice that within a class there are several different forms, and that across classes the forms are sometimes the same. As of now there is no explanation of either of these facts; they are both treated as accidents. In addition there are some verbs in different classes which are essentially the same in form and meaning except for their agreement morphology. Some of these verbs are listed in example (13).

(13) verb sets differing primarily by differences in agreement morphology

<u>verb gloss</u>	<u>class</u>	<u>form of agreement marker</u>
DRIVE/DRIVE-TO	plain/spatial	∅/directional
SIT/SIT-AT	plain/spatial	∅/locational
PAINT/PAINT-x	plain/spatial	∅/locational
STAB/STAB/STAB-IN-x	plain/inflecting/inflecting	∅/directional/locational
SHOOT/SHOOT-IN-x	inflecting/inflecting	directional/locational

The existence of pairs and triads of verbs differing only in how their agreement morphology uses space suggests that a generalization is being missed with the verb class analysis. By saying that there is only one class of verbs, and several verb agreement forms which include morphological information about whether real space, model space, or no space is used, we can explain these sets of verbs simply as single verbs which can

combine with different agreement morphemes. The agreement forms represent the signer's perspective on the use of space as determined in part by discourse considerations. My analysis also eliminates the oddness of having a single class of verbs which has two or three different agreement types, since, as I will show below, there is some evidence that the different morphological types express finer distinctions than discussed so far about the way space can be used. These finer distinctions represent the fact that there are verbs which primarily use space in one way, as a model, but which share other features with the spatial verbs. The fact that some inflecting verbs share features with spatial verbs also explains why verbs in different agreement classes sometimes share the same morphological type.

Looking back at the possible forms for agreement, we see that the two types shared by inflecting and spatial verbs are locational agreement and directional agreement. What is significant about this is that the four sets of verb + agreement forms involved share the feature that some members of each set can express some information about both referentials and locatives.¹³ (Recall that through the use of a classifier many spatial verbs express referential information even though their agreement morphology uses space locatively.) Verbs with the one form that does not occur with real space, orientational agreement, never express locative information of any sort. Furthermore the two agreement types that are shared each express different kinds of information.

Locational agreement indicates the location of a referent in both classes of verbs. Inflecting verbs with locational agreement have as their objects things that are also locations, such as body parts, or walls, floors, or ceilings. Thus verbs that fit into this category are verbs like PAINT, VACUUM, SHOOT-IN-HEAD (which does not have a preposition in ASL), and PINCH-CHEEK.¹⁴ These differ from the spatial verbs with locational agreement for which the location is predicated of the referent. Directional agreement indicates movement between either referents or locations.¹⁵ In spatial verbs the referent that moves is often indicated on the verb by a classifier. In inflecting verbs the object whose locus moves is not specified in the form of the verb. Rather, the verb expresses the transfer of an unspecified object from one referent to another as in the verb GIVE. An object must be expressed as an independent nominal. One feature which differentiates the verbs using model space from the verbs using real space for these two agreement types is that the ones using model space (the inflecting verbs) express locative information about an object, whereas the ones using real space express locative information about a subject. A table summarizing the distribution of the features discussed here is shown in (14).

(14) Distribution of features associated with each agreement form¹⁶

zero-marked	no use of space hence nothing agreed with
orientational	uses model space, hence agrees with referentials; expresses no locative information ¹⁷
locational	uses model space, hence agrees with referents; may express locative information about its object uses real space, hence agrees with locative; may express a predication about a subject referent
directional	uses model space, hence agrees with referents; may express locative movement of an object referent. uses real space, hence agrees with locatives; may express movement of the subject through space.

I began this paper by saying that ASL may be typologically unusual with respect to its pronoun system. The evidence based on Padden's work and what has been shown here suggests that this is not so. ASL is pretty typical in terms of making a morphological distinction between referentials and locatives. In particular, I argue that referentials use space as a model, while locatives use space as though it were real. The recognition of this distinction allows us to analyze verb agreement morphology as multimorphemic expressing not only agreement with a particular argument of the verb, but also information about the perspective on the use of space. This in turn allows us to eliminate the three verb classes proposed by Padden in favor of an analysis which attributes differences in what a verb agrees with to the agreement morphology rather than to lexical facts about the verbs themselves.

NOTES

1. Petitto's claim about the formational identity of all of these forms is slightly misleading. There is another form which can be used to mean 'here' which does not share the pointing handshape. Additionally, some of the other forms may be made with other handshapes as well.
2. I follow standard practice in representing ASL signs by English glosses printed in uppercase letters.
3. I follow the practice in the ASL literature of calling agreement with locatives 'verb agreement' even though this is not standard linguistic use of the term.
4. An additional argument against the Poizner, Klima, and Bellugi analysis comes from Liddell (1989) who argues that their analysis cannot account for the fact that a single locus can be used both in a purely referential situation and as part of a classifier verb using space topologically, nor for the fact that locus shifting (Padden 1983) occurs.
5. Liddell (1989) points out that the locus itself does not 'stand for' the referent or location since agreement can occur vertically above the locus. This distinction is not important here since the facts I want to explain do not vary as a result of differences along an axis for height.
6. Referents may be in particular spatial relationships, however these relationships are not expressed with or through the device of role play.
7. Strictly speaking this is not true. When a referent is present in the signing area, the locus assigned to that referent is in the direction of where the referent is in the real world. Hence the loci are not always abstract in the discourse. Several people have suggested to me that this is in effect a third use of space, 'actual space'. However, nonabstract referential loci share the features of other referential loci as seen in the case of role play where the role assumed is that of one of the discourse participants. The shift is still only a partial one. Therefore, I would argue that any tightening of the permissible phonetic variation is due to pragmatic factors rather than grammatical factors. Hence the actual use of space is culturally preferred, but it is not grammaticized in the language. Furthermore, I believe it is possible to claim that first person loci are in fact abstract. If the locus for first person is not the person herself but the spot she is established at in the discourse, when she shifts in role play, her locus remains the same. Therefore a supposed first person reference is not one, because it is not at the locus for the signer in the world of the discourse. This also explains why reference to the signer's self during role play is directed at the signer's original locus.

8. Schick distinguishes a real-scale world and a model-scale world expressed by classifiers. Classifiers showing how an object is handled express real-scale, whereas classifiers which represent a feature of the object, such as its shape, express a model-scale. There appears to be some correlation between world-scale and type of space. A rough attempt to explain this connection is to say that real-scale world occurs only with real use of space, whereas model-scale world can associate with real space, model space, or neither.
9. De Matteo (1977) claimed that the use of space in ASL was analogue but many researchers have since shown that this is not the case (eg. Supalla (1978)).
10. This is also pointed out in Liddell (1989).
11. The fact that a single verb can simultaneously occur with more than one form of agreement seems to support my claim that the differences in form reflect differences in meaning; however, much more research into these forms is needed before any claims to this effect can be made.
12. There is a subset of verbs called *backwards verbs* (Padden 1983, Brentari 1988) which reverse the markings of subject and object. So for a verb with orientation agreement the palm would face the subject and the back of the hand would face the object. At this point I have nothing to add to the previous analyses of why these verbs have backwards agreement.
13. For both the inflecting and spatial verbs with either locational or directional morphology, not all of the verbs in the group fit the following descriptions. However, it is the case that nothing with other morphology will have the features described for any of these groups.
14. The verbs with non-body part objects also have forms which are plain verbs. The plain forms are also the citation forms for those verbs. The forms which have body parts as objects use the signer's body parts for the loci of the real body part both in citation form and in discourse. Thus the signer's body parts are actually acting as classifiers for those body parts independent of referent, and these forms should therefore be considered to have locational agreement rather than being classified as plain verbs, as has traditionally been the case.
15. The morphological movement that occurs with these forms is called 'locus shifting' (Padden 1983) and has the effect that a referent's locus is moved from one place to another.
16. Additionally, it is worth noting that among the locational and directional agreeing verbs, those which share features with the spatial verbs are just those verbs which mark agreement with one fewer argument than they are subcategorized for. For example, GIVE is subcategorized for three arguments and agrees with two. PAINT is subcategorized for either one or two arguments. When it is subcategorized for one argument it agrees with none, when it is subcategorized for two arguments and expresses locative information, it agrees with only one argument. This suggests that the agreement morphology also expresses or is connected in some way to transitivity facts, but it is not yet clear what the correct generalization might be.
17. Spatial perception verbs such as LOOK-AT do have orientation agreement; however, these verbs differ from both other spatial verbs and inflecting verbs in additional ways. This suggests that there may be additional categories than are outlined in this paper.

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Strictly Binary Metrical Structure
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Binary feet are an indispensable component of metrical formalism but cannot apply fully to a domain of odd length, there being necessarily a left-over position. I will argue that such a residual position should be left unparsed in rules assigning primary stress, contrary to the prevailing view that posits unary or degenerate feet. A constituent-structured grid-like formalism of the kind proposed by Halle and Vergnaud (1987) will provide the basis of discussion.

Let us begin with a typical case of accentuation sensitive to a parity count and thus requiring binary parsing. The Cairene norm for pronouncing Classical Arabic places stress on a heavy penult, otherwise on the rightmost nonrinal syllable that is odd-numbered counting from the beginning of the word or the nearest preceding long vowel, as illustrated in (1).

(1) Cairene Classical (Mitchell 1960, 1962)
raqarátii, qattáira, 'uxbírakum, kátabaa
(my neck, he massacred, that I inform you, those
two wrote)

I assume that the stress assignment algorithm begins by constructing a syllabic profile that explicitly labels peaks and margins on a separate level, referred to as line A as in (2).

(2) Syllable profile (F = peak, M = margin)
ra.qa.ba.tii, qat.ta.la, 'ux.bi.ra.kum, ka.ta.baa
A F F F FM, FM P P, FM F F FM, F F FM

Line A is the first level of what will become a full metrical structure. Let us suppose for now that Cairene builds this structure according to the parameter setting in (3), which is modelled closely on Halle and Vergnaud (1987:60-63).

(3) Cairene parameter settings

A	extrametricals	R# (i.e. final rimes: F, FM)
E	positions	P, M
C	1 heavy rimes	FM
	2 constituents	binary
	headedness	left
	direction	LR
	3 unary rule	yes
D	stress	rightmost

Applied to the structures in (2) these rules give the more developed structures in (4). The rules of extrametricality remove the certain configurations from the end of line A, in this case final rimes. Line B marks certain certain syllabic nodes as metrical positions, in the Cairene case marking both peaks and margins so as to yield a bipositional representation of heavy syllables. Line C is constructed in several steps. Rule 1 marks the peaks of heavy syllables as strong, and rule 2 constructs left-headed binary constituents from left to right on line B with heads marked strong on line C. Rule 3 strengthens positions not yet included in any binary constituent, thereby creating unary constituents. Line D associates primary accent with the leftmost or rightmost strong position, depending on the language. For Cairene it is the rightmost strong position.

(4) Cairene metrical structures

	ra.qa.da.tii,	qat.ta.la.	'ux.bi.ra.kum,	ka.ta.baa
A	F P F,	PM F,	PM F P,	P F
B	(* **),	(**)(*),	(**)(* **),	(* **)
C	2 S,	1 S,	1 2,	1
D	*	*	*	*

I will contest this analysis on two interrelated points: the nature of the extrametrical units and the status of the unary rule. As it stands the analysis in (3) conforms to the standard theory summarized in (5), due essentially to Hayes (1981) and continued in the practice of Halle and Vergnaud. In this theory extrametrical constituents are defined principally by structural level, though perhaps restricted by segmental feature specifications, and unary constituents are obligatory where binary parsing fails.

(5) Standard theory

- (a) Possible extrametrical units (Hayes 1981:82):
consonants (C), segments (S), rimes (R), etc.
- (b) Unary rules: obligatorily constructed over
residue of binary parsing.

Let's now look at the data in (6). It was elicited from Q, a native of Jerusalem. The illustrative forms are from the Classical language, but the stress pattern equally characterizes Q's colloquial speech.

(6) Classical:

šájaratun. Šállamat. Šallámanu. kátabat
(a tree, she taught, he taught him, she wrote)

Q's system presents a challenge to the standard theory. It is easily described by the same parameter settings that we posited for Cairene, provided only that unary constituents are not constructed. Typical metrical structures for Q are shown in (7), where the strong positions that would be introduced by the unary rule are conspicuous by their absence.

(7) Q like Cairene but lacks a unary rule:

	š	a.	j	a.	r	a.	t	u	n.	š	a.	l	l	a.	m	a.	t.	š	a.	l	l	a.	m	a.	h	u.	k	a.	t	a.	b	a	t
A	F	F	F		FM	F				FM	F	F			F	F									F	F							
E	(*	*)	*		(**	*				(**	(*	*)			(*	*)										(*	*)						
C	2				1					1	2				2																		
D	*				*					*					*											*							

On the basis of this evidence we might conclude that the presence or absence of the unary rule is a language-specific matter. If this is the only change in the formalism the theory in (8) results. Under this theory Cairene and Q differ only the status of the unary rule. Cairene has the rule but Q does not.

(8) Revised standard theory:

(a) Possible extrametrical units:

consonants (C), segments (S), rimes (R), etc.

(b) The unary rule is a parametric option

Q's system is consistent with a more radical innovation: we might expunge the unary rule altogether from the formalism. The problem is that this move alone would prevent us from describing Cairene stress. If final rimes are extrametrical in Cairene, as in our original analysis, then the unary rule is essential. The reason is that the penults of such words as raqabátii and qattála cannot become the heads of left-headed constituents and can get their stress only if a unary constituent is constructed upon them. This situation is shown in (9), where X marks positions that would be stressed if the unary constituents were not present. We cannot designate final segments extrametrical either, for that would have the same effect as an extrametrical final rime in the case of qattaia.

(9) Cairene dependence on the unary rule

	(a)	(b)
	ra.qa.ba.ti.i, qat.ta.ia	
A	F F F,	PM F,
B	(* *)(*),	(**)(*),
C	2 3,	1 3,
D	X *,	X *,

Thus if we abandon unary rules we must also do something about extrametricality. A proposal along these lines is given in (10). As usual only word-edge constituents can be extrametrical, but they must now be identified in terms of node labels present on line A of the metrical structure. For example, in the Arabic dialects we have been considering, peaks and/or margins may be extrametrical, indeed, both types of nodes may be extrametrical in the same accentual system, but they can become so only through a fixed universal conjunctive ordering that first removes final margins and then final peaks.

(10) Strict theory.

(a) possible extrametrical rules in universal order of application: M#, F#.

e.g.	CVV#	CVC#	CV#	
	PM	PM	F	no rules applied
	F-	F-	F	only M# applied
	PM	PM	-	only P# applied
	--	--	-	M# and F# applied

(b) bounded constituents strictly binary
(i.e. there is no unary rule)

Under this scheme the Cairene and Q systems can be described identically except for the rule that makes final peaks extrametrical. The parameter settings are shown in (11), with X marking the only point of difference between the two dialects.

(11) Strict analysis	Cairene	Q	
A extrametricals	M#	M#, P#	X
B positions	F, M	F, M	
C 1 heavy rimes	PM	PM	
2 constituents	binary	binary	
headedness	left	left	
direction	LR	LR	
D stress	rtmost	rtmost	

Under this analysis the Q system will have the same metrical structures as before, although extrametrical rimes are now achieved by designating both margins and peaks extrametrical. Cairene is analyzed just like Q except that it makes only margins extrametrical, with resulting metrical structures as shown in (12).

(12) Cairene metrical structures (strict theory)

	ra.qa.ca.tii.	qat.ta.ia.	'ux.bi.ra.kum.	ka.ta.baa
A	F F F F.	PM F F.	PM F F F.	F F F
B	(* **)(* **).	(**)(* **).	(**)(* **)*.	(* **)*
C	2	2.	1 2.	1
D	*	*	*	*

Two rival theories are now before us: the revised standard theory and the strict theory. We shall review some evidence that might bear on the choice between them. First consider the Egyptian Arabic dialects exemplified in (13) and (14). Both dialects would put stress on a heavy penult (not illustrated), otherwise Burullus stresses the penult and North Bani Sweef the antepenult.

(13) Burullus (Bennstedt 1978)

baqara. madraas. yištaḡalu
(cow. school. they work)

(14) NES = North Bani Sweef (Bennstedt 1979)

bá'ara. ca'aritu. naDDa'it. naDDáfitu
(cow. his cow. she cleaned. she cleaned it)

Strict analyses of these dialects are given in (15) along with the analyses just postulated for Cairene and Q. Metrical structures generated for Burullus and North Bani Sweef are given as well, in (16) and (17). Look now at the logical form of the table in (15). Xs indicate parameters which show variation in the table. The position parameter is parenthesized because it probably does not act as a genuine distinguishing factor among left-headed right-to-left systems. The reason is that if such a system is bipositional, a heavy + light syllable sequence at the right end of the metrical domain would in principle call for a binary constituent with head upon the margin of the heavy syllable. But margins cannot support strong positions so the head of the rightmost binary constituent would have to fall on the peak of the heavy syllable after all, just as in the monopositional analysis. There remain

two genuine variables. First there is extrametricality. It appears that all the dialects mark margins as extrametrical and differ only in whether they treat peaks as extrametrical. The other variable concerns the direction in which the metrical constituents are constructed on line C.

(15) Strict binary analysis

	Burul. NES	Cair.	Q
A extrametricals	M#	M#,F#	M#
E positions	F	F	F,M
C 1 heavy rimes	PM	PM	PM
2 constituents	binary	binary	binary
headedness	left	left	left
direction	RL	RL	LR
D stress	rtmost	rtmost	rtmost

(16) Burulius structures: strict theory

	ba.qa.ra.	yiš.ta.ga.lu.	maD.ra.sa
A	F F F,	PM P P F,	PM P F
E	* (* **),	(* **)(* **),	* (* **)
C	2,	1	2,
D	*	*	*

(17) N. Bani Sweef structures: strict theory

	ba.'a.ra.	ba.'a.ri.tu.	naD.Da.fi.tu.	naD.Da.fi.t
A	P F,	P F P,	PM P P,	PM F
E	(* **),	* (* **),	* (* **),	(* **)
C	2,	2,	1	2,
D	*	*	*	*

The two operative variables in table (15), peak extrametricality and direction of constituent building, are represented in all possible combinations. As it happens the other parameters remain fairly constant throughout a broad region of sedentary Arabic communities in the Eastern Mediterranean and West Arabian areas. The strict theory makes it easy, then, to characterize the attested stress systems of the region: they are, to a first approximation, just those allowed by the variables and constants in table (15). A closer examination of the region reveals some other systems too, but they differ only in the positionality parameter (in left to right systems) or perhaps in having unbounded constituents.

Let us see now how the revised standard theory would handle this dialect area. A table of analyses is given in (18) for the four stress systems under consideration. The North Bani Sweef system is not

analyzed much differently under this scheme, but Buruli receives rather different metrical structures, as shown in (19). All the dialects are analyzed with final rimes extrametrical. On the other hand we now need at least three variables to distinguish the four dialects, in addition to direction, there is variation in headedness and in the presence or absence of the unary rule. Certain blanks occur in the table where the choice of parameter value makes no difference to the outcome.

(18) Revised standard theory

	Buruli	NBS	Cair.	Q	
A extrametricals	R#	R#	R#	R#	
B positions	F	P	F.M	F.M	(X)
C 1 heavy rimes	PM	PM	PM	PM	
2 constituents	binary	binary	binary	binary	
headedness	right	left	left	left	X
direction	---	RL	LR	LR	X
3 unary rule	yes	---	yes	no	X
D stress	rtmost	rtmost	rtmost	rtmost	

(19) Buruli metrical structures (revised standard)

	ba.qa.ra	ya.ta.ga.lu	ma.ra.sa
A	P P, PM F F, PM F		
B	(* *), (*) (* *), (*) (*)		
C	2, 1	2, 1	3
D	*	*	*

Notice that table (18) does not fully exploit all the possible values of its three differentiating variables. Certain combinations of values are missing. Two of the missing combinations are given in (20). They describe stress systems that would surface as in (21) if applied to Classical Arabic words. These missing systems happen not to have a unary rule, but that is an option available in the revised standard theory. Systems like these may be possible elsewhere in the world, but they have not yet turned up in a survey of the Arabic dialect region under consideration. In contrast, the strict theory offers a principled reason why such systems do not occur there: they simply cannot be generated by the regional system embodied in table (15).

(20) Implied under revised standard theory

	(a)	(b)
A	extrametricals	R# R#
B	positions	F F
C	1 heavy rimes	PM PM
	2 constituents	binary binary
	headedness	right right
	direction	RL LR
	3 unary rule?	no no
D	stress	rtmost rtmost

(21) Stress patterns generated by (20)

- (a) raqábatii, ǧállamat, ǧállamánu, katába, rámat
 (b) raqábatii, ǧállamat, ǧállamánu, katába, rámat

A possible difficulty with the strict theory is that it provides no strong position for a domain consisting of a single light syllable. Hence a word with that kind of domain will receive no stress without some further qualification. The problem is illustrated with word like ramat "she threw", which has initial stress in all the dialects considered so far. Dialects with margins-only extrametricals derive this stress without difficulty, as shown in (22a), but dialects with both margins and peaks extrametricals do not provide a strong position on which the primary stress can rest, as shown in (22b). To account for such cases as (22b) I will assume with Halle and Vergnaud (1987:71) that primary stress is associated with an edgemost asterisk of line E if there is no asterisk on line C.

(22) Light-monosyllabic domains

	(a)	(b)
	ra.mat	ra.mat
A	F F	F
B	(* *)	*
C	-	-
D	*	*!

It is now possible to consider the stress pattern of Negev Bedouin Arabic, illustrated in (23).

(23) Negev Bedouin (Blanc 1970: predominant pattern)
 mišát (she walked), zalamah (man), Haarabatih (she fought him), zalamatak (your man), maHkaman (court)

We propose to analyze this system as in (24), with metrical structures subject to the universal

well-formedness condition in (25) and consequently generated as in (26). So analyzed, this stress system is closely related to the secondary stress systems considered previously and tabulated in (15). It is like the Q system in particular except that it has initial peaks extrametrical rather than final ones, and it represents heavy syllables monopositionally. Like the Q system and all the other Arabic dialects represented in table (15), it has rinal margins extrametrical (simultaneously with initial peaks extrametrical!) and has left-headed metrical constituents constructed from left to right.

(24) Strictly binary analysis of Negev Bedouin

A	extrametricals	M#, #P
B	positions	F
C	1 heavy rimes	PM
	2 constituents	binary
	headedness	left
	direction	LR
D	stress	rtmost

(25) Metrical well-formedness condition: $(FM_C)_1$

(26) Negev Bedouin metrical structures

	mi.ṣat. za.ṣa.mah. Maṣ.ra.ba.tin.						
A	P.	F	F.	PM	F	F	F.
B	*	(*	*)	(*	*)	(*	*)
C	-	2.	1	2.			
D	*	*		*			*

	za.ṣa.ma.tak. maḥ.ka.mah					
A	F	P	F.	PM	F	P
B	(*	*)	*	(*	*)	*
C	2.		1			
D	*		*			

In this analysis we have exploited an implication of the strict theory. It allows final margins and/or final peaks to be extrametrical but must also recognize the possibility of initial extrametricals. If the possibilities are symmetrical then initial peaks ought also to have the opportunity of being extrametrical, and this possibility is realized in Negev Bedouin Arabic. Margins, however, are excluded from initial position by the well-formedness condition in (25). For this reason initial margins never come up and it is idle ever to designate them as extrametrical. By the same token it is impossible to extrametricalize the peak of an

initial heavy syllable, because then the margin of that syllable would come into initial position on line A, thereby violating the well-formedness constraint. That is why maHkamar and Haaraatih remain untouched by the initial extrametrical peak rule in Negev Bedouin Arabic, as seen in (26).

The Arabic dialects surveyed in this paper do not offer any cases in which final peaks constitute the sole type of extrametrical unit. The strict theory predicts that such cases should exist, however, and indeed some can be found. One such system is in Goroa. It stresses a leftmost long-vowelled syllable, otherwise a closed ultima, otherwise the penult, as illustrated in (27).

(27) Goroa (Hayes 1981:118-9)
 duugunoo, heninau, axemis, oromila, amrami
 (thumb, because, hear, because, ivory arm ring)

Two analyses of the Goroa system are given in (28) below. (28a) is couched in terms of the (revised) standard theory and (28b) is required by the strict theory. The two analyses differ only in what they designate as extrametrical. The standard analysis has final segments extrametrical, and the strict analysis has final peaks extrametrical. Both analyses posit unbounded constituents. Notice that the strict theory does not bar such constituents; it says only that if metrical constituents are bounded at all then they are strictly binary.

(28) Goroa metrical parameters

	(a)	(b)
A extrametricals	S#	F#
B positions	P	P
C 1 heavy rimes	FV	FV
2 constituents	unbounded	unbounded
headedness	right	right
direction	---	
D stress	leftmost	leftmost

These analyses generate the structures in (29).

(29) Goroa metrical structures

(a)	duugunoo,	heninau,	axemis,	oromila,	amrami
A	FV F F,	P F F,	F P F,	F P F,	PC P
B	(*)(* *),	(* * *),	(* * *),	(* * *),	(* *)
C	1 1,	1,	2,	2,	2
D	*,	*,	*,	*,	*

(b)	duugunoo,	heninau,	axemis,	oromila,	amrami
A	FV F FV,	F F FV,	F F FC,	P P F,	FC F
B	(*)(**),	(***) ,	(***) ,	(***) ,	(***)
C	1	1,	1,	2,	2
D	*	*	*	*	*

Notes

1. I am deeply indebted to Hamdi Qafishen (Q) for acting as consultant on the stress characteristics of his dialect. I would like to take this opportunity also to thank Michael Hammon and Richard Janda for their helpful comments on the oral version of this paper. However, I alone am responsible for the errors of fact, theory, and analysis that may exist in it.

2. One convention for pronouncing Classical Arabic places stress on the rightmost nonfinal heavy syllable, otherwise the first syllable (Abu-Fadi 1961:235-6). This system requires unbounded constituents. The Ba'i:di dialect of Egypt may have the system too, but the data in Khalafallah (1969) are consistent with a Q-like system with monopositional representation of heavy syllables. Schreiber's (1971) Meccan Arabic data are consistent with a Cairene-like stress system but again with monopositional heavy syllables.

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Relating Thematic Relations and Aspectual Interpretation*

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The study of thematic relations is the study of the semantic relationship between a predicate and its various arguments, and of how this semantic relationship is expressed in syntactic form. This paper will argue that we should view this relationship between a predicate and its arguments in terms of the semantic contribution of the arguments to the internal, aspectual structure of the event which is expressed by the predicate. We will look specifically at the aspectual properties of locatives in regard to their role in thematic relations, and see that locatives are divided into two types, based on whether or not they contribute information about aspectual interpretation. We will consider syntactic evidence from English and Mandarin Chinese as support for this distinction.

1 A note on aspectual structure

The terms *aspect* and *aspectual structure* require some clarification. Following Comrie (1976), we take *aspect* to involve "the different ways of viewing the internal temporal constituency of a situation" (Comrie, p. 3). *Aspect* thus differs from *tense*, which relates the time of an event or situation¹ to another, external time point, such as the moment of utterance. An event has internal structure – it has, at least, a beginning, middle and ending in both time and space, and perhaps a more highly articulated structure. We may think of this structure as the spatio-temporal "shape", "geometry" or "topology" of the event². Information about the internal structure of the event is provided not only by *aspect* morphemes, but also by the verb and its complements. In what follows, we will focus on the aspectual information added by locative complements.

In considering the relation of *aspect* to thematic relations and argument structure, we will be generalizing a proposal by Tenny (1987) and (1988), who follows studies on aspectual structure of verbs and verb phrases by, most notably, Dowty (1979) and Vendler (1967). Tenny argues that the direct object of the verb is just that element which provides a parameter by which the event denoted by a verb is temporally measured out and delimited. That is, in a sentence like "John ate an apple", the direct object, *apple*, delimits or marks the ending point of, the event, because the event ends at the point when the apple is totally consumed. Tenny proposes the Aspectual Interface Hypothesis:

Aspectual Interface Hypothesis: "The mapping between cognitive (thematic) structure and syntactic argument structure is governed by aspectual properties. The aspectual properties associated with internal (direct), external and oblique (internal indirect) arguments constrain the kinds of event participants that can occupy these

positions. Only the aspectual part of cognitive or thematic structure is visible to the syntax.”(Tenny (1988), p. 3)

This states that various types of semantic arguments are constrained as to the way they may be expressed in syntax, according to their aspectual properties; thus, *an apple* is allowed in direct object position in virtue of the fact that its referent is the delimiting element of the event of eating. We will support this hypothesis, and propose that an element is a semantic argument of a predicate if and only if it participates in the event indicated by the predicate and contributes to its aspectual interpretation. By “semantic argument” of a predicate we will mean simply an element that is a part of the predicate’s thematic structure, i.e. the lexical argument structure projected from the meaning of the predicate. Thus, it will follow from our proposal that thematic structure is composed only of elements which provide information about the event’s aspectual structure. We will assume, with Tenny (1987) and many others, that semantic argument structure is directly tied to syntactic argument structure³; and we will see, in the evidence from locatives in section 3, that elements that contribute to aspectual interpretation have different syntactic behavior from those that do not. First, however, we will consider the way locatives contribute semantically to aspectual interpretation.

2 Locatives and aspectual interpretation

Locatives are especially interesting in the study of thematic relations and argument structure because they are “on the border” of both the argument/modifier distinction in semantics and the argument/adjunct distinction in syntax. They may be thematically related to the verb and therefore part of its syntactic argument structure, or they may act as adverbial modifiers of the verb, and have the syntactic properties of adjuncts. We will call the argument-like locatives “Participant” locatives, for they participate in the internal structure of the event, and the others “Frame” locatives, for they serve only to “frame” the event in space – they provide only an external context for the event⁴. For example, the locative in (1a) is a Frame locative; *the living room* is merely the place where the event of knitting occurs. This locative does not supply any information about the internal geometry of the event; the place where Mary does her knitting is not involved in the action of knitting itself.

1. a. *Mary is knitting a sweater in the living room.* (Frame)
- b. *John walked to the store.* (Participant)

The locative in (1b), *to the store*, is different – the store certainly is not the location of the entire event, but rather, it is the location where the event ends. Thus, this locative does in fact give information about part of the event – its ending – and so delimits the event. Note that the Frame/Participant distinction

is in fact an aspectual distinction: Participant locatives contribute to aspectual interpretation, and Frame locatives do not.

There are various types of Participant locatives, all of which add to the aspectual information about an event, but do so in different ways. We will distinguish Goal, Source, Path, Direction and what we will call "Posture" phrases, such as the locative in *sit on a chair*. This is not meant to be a classification scheme for locatives, but only a clarification of the ways a locative can be said to participate in an event⁵.

A Goal phrase provides a spatial endpoint for the event, and by extension, also provides a temporal endpoint:

2. Goal: **Bill walked to the store.**
 Mary put the book on the shelf.
 The troops marched onto the ship.

A Source phrase indicates the spatial and temporal beginning of the event:

3. Source: **John walked from the store.**
 The cat jumped off of the table.
 The train left from the station.

Path and Direction phrases give us information about the topology of the middle portion of the event:

4. Path: **Susan walked along the river.**
 The bird flew across the road.
 The boys ran through the park.

- 5: Direction: **Bill strode towards the house.**
 John aimed the gun at the target.

"Posture" phrases, like those in (6), also add information about the internal geometry of the situation, here the state of sitting, for they have a very close spatial connection to this state:

6. Posture: **Jane sat on the chair.**
 We lived in that house for many years.

We will see below that Posture phrases pattern syntactically with the Participant locatives as well. Note that the relative "size" between the location and other participants in the event plays a role in determining whether a locative can be interpreted as a Posture phrase⁶— that is, the locative in (7) does not indicate a participant in the sitting at all, for the porch is not itself the object sat on, but rather the general area around the place where Jane is sitting:

7. **Jane sat on the porch.**

Thus, this locative is a Frame locative.

Note also that different verbs have different selectional restrictions on Participant locatives – so in example (8), we see the verbs *walk* and *stand* do not select the same sorts of phrases, although both do assign thematic roles to the locatives they take.

8. a. John walked from the school / to the store /along the river / towards the beach.

b. Mary stood on the table / at the blackboard / *from the table / * to the table / *towards the blackboard.

Moreover, whether a particular locative is interpretable as a Participant or a Frame locative depends on the selectional properties of the verb, and not just on the locative itself. For example, the phrase *along the river* can be understood as a Participant locative in (9a), but only as a Frame in (9b):

9. a. John walked along the river.

b. John ate along the river.

If a locative is a Participant locative, it is selected by the verb; if it is a Frame locative, of course, it is not selected at all.

We have seen examples above of the ways that Participant locatives provide aspectual information about the event by indicating something about its beginning, end or other internal geometry; and have seen that Frame locatives supply only information about general location, which does not involve aspectual interpretation. This aspectual distinction correlates with the argument/modifier distinction in semantics – Participant locatives are semantic (thematic) arguments, while Frame locatives are adverbial modifiers. We may maintain the semantic criterion for argument-hood proposed in the previous section, restated here as follows: An element is understood as a semantic argument, that is, it is part of a predicate's thematic structure, if and only if it provides some spatial or temporal parameter through which the aspectual structure of the event indicated by the predicate can be characterized. We will see in the next section that the syntactic evidence supports the Frame/Participant distinction.

3 Syntactic evidence for the distinction

We turn now to the syntactic evidence for the distinction between locative types. We expect that the locatives which participate in thematic structure and aspect will be theta-marked by the verb, whereas Frame locatives will not be theta-marked. If we assume theta-marked elements to be sisters of the verb⁷, then we will expect Participant locatives to be VP-internal, and Frame locatives to be adjoined to the VP, or possibly some higher syntactic level.

3.1 Evidence in English

The relative ordering of locatives in English shows that Participant locatives are structurally closer to the verb than Frame locatives:

10. a. Susan jumped onto the table in the living room.
 b.*Susan jumped in the living room onto the table.

Phenomena that are generally considered to be tests for VP constituency indicate that Participant locatives are VP internal, and Frame locatives are adjoined to the VP. The "do so" test, discussed by Lakoff and Ross (1966), is one such phenomenon. Adverbials, such as those for time, purpose and manner, may remain outside the "do so" phrase, and so outside the VP, whereas arguments may not:

11. a. John made a cake on Monday and Bill did so on Thursday.
 b. John made a cake on Monday and Bill did so, too.
 c. John made a cake and Bill did so too.
 d.*John made a cake and Bill did so a pie.

Turning to locatives, we find that Frame locatives act like the VP-adjoined adverbials, while Participant Locatives in general come out as VP-internal:

12. a. John chopped onions in the pantry and Bill did so in the kitchen.
 b. *John went to school and Bill did so to work.
 c.*John aimed the gun at the target and Bill did so at the tree.
 d.*John slept on the bed and Bill did so on the couch.
 e. John slept in a bed in New York and Bill did so in Cleveland.

In the pseudocleft construction, illustrated in (13), the wh-word *what* corresponds to a VP. The evidence provided by this test is very similar to that above; again, Frame locatives may remain outside of the VP, while Participant locatives can not.

13. a. What Mary did in the living room was knit (a sweater).
 b.*What Tom did to the store was walk.
 c.*What John did towards the rock was swim.

VP-preposing is also, straightforwardly, a diagnostic for VP constituency. The most natural VP-preposing in English is where the outermost VP moves, as in (14); however, it is also possible, although more awkward, to prepose only the innermost VP, and leave Frame adverbials *in situ*, as in (15):

14. John said he would do it today and do it today he did.
 15. John said he would do it today and do it he did today.

With the latter type of VP-preposing, we find again that it is possible to separate the Frame locatives from the VP, but not the Participant locatives:

16. a. John said he would knit in the living room and knit he did
in the living room.
b.*John said he would put the book on the table and put the book
he did on the table.
c.*John said he would push the cart to the corner and push the cart
he did to the corner.

Despite the awkwardness of the construction, there is a clear contrast between Participant and Frame locatives here, and we see that only Frame locatives can be left behind when the VP is preposed.

The pseudopassive construction can be used as a test for whether or not a locative is an argument if we assume that a Prepositional Phrase can appear in a pseudopassive only if it is theta-marked by the verb, as argued by Baker (1986).

17. a. This bed has been slept in.
b.?*New York has been slept in.
c. This house has been lived in.
d.?*This room has been knitted in.

The strength of the judgements in these cases tends to vary a bit from speaker to speaker, and also depends on the context; however, every native speaker I have checked with finds a definite degradation of acceptability with the Frame locatives.

The last test in English is the "Happened" test (inspired by Davidson (1966)). It shows that when a clause is nominalized to become the subject of a verb like *happen*, *occur*, *took place* and the like (that is, verbs which are predicates of events, whose subjects are elements that refer to events), only locatives which are interpretable as a Frame locative relative to this nominal can remain outside of the nominal and become complements of the matrix verb:

18. John's throwing the ball happened/occurred in the park
* into the park
* from the park

If we assume that the nominal must refer to an entire event, including all of its subeventual structure, then Participant locatives may not be added as modifiers of the event predicate, because they must be part of the subeventual structure of the nominal.

In summary, the tests above show a clear contrast between the syntactic behavior of Participant locatives and that of Frame locatives, although there are still a number of differences among the Participant locatives that remain to be explored⁸.

3.2 Evidence in Chinese

We turn now to Mandarin Chinese. Chinese has a syntactic criterion which straightforwardly differentiates arguments and adverbials – adverbials can in general only appear before the verb, whereas arguments can occur postverbally. For example, adverbials like *yesterday* can not appear postverbally; on the other hand, this is the normal position of direct objects⁹:

19. a. Ta zuotian lai le.
 he yesterday come CM
 He came yesterday

b. *Ta lai le zuotian.

20. Ta kan le yi ben shu.
 he read CM one MW book
 He read a book

Example (21) illustrates that a Frame locative cannot follow the verb, and thus is like other adverbials.

21. a. Ta zai tushuguan xuexi.
 he at library study
 He studies in the library.

b. *Ta xuexi zai tushuguan.
 he study at library

(22)-(25) demonstrate that Participant locatives, like other arguments, can be postverbal. In (22) and (23), in fact, the locative *must* be postverbal to be understood as a participant – placing the same locative before the verb yields a very different interpretation, in which the locative has a Frame reading:

22. a. Ta zai zhuozi shang tiao.
 he at table on jump
 He is jumping on the table. (i.e. He is on the table jumping)

b. Ta tiao zai zhuozi shang.
 he jump at table on
 He jumped onto the table.

23. a. Ta meitian dao caochang pao.
 he every day to field run
 Every day he goes to the field and runs.

b. Ta pao dao caochang le.
 he run to field CM
 He ran to the field.

However, note that there is not always a semantic difference corresponding to the different position – for in (24), the two sentences have approximately the same meaning:

24. a. Ta zai chuang shang shui.
 he at bed on sleep
 He sleeps in a bed. (contrastive)
- b. Ta shui zai chuang shang.
 he sleep at bed on
 He is sleeping in a bed.

This is true for the three sentences in (25) as well, although here there are some dialectal differences as to where one can put the locative when the direct object is postverbal. (25b) is acceptable for some speakers and marginal for others: the speakers who find (25b) acceptable tend to find (25c) unacceptable, and vice versa:

25. a. Wo ba zazhi fang zai shujia shang.
 I BA magazine put at bookcase on
 I put the magazine on the bookcase.
- b. OK/? Wo zai shujia shang fang le zazhi.
 I at bookcase on put CM magazine
 I put the magazine on the bookcase.
- c. */OK Wo fang le shu zai shujia shang.
 I put ASP book AT bookcase on
 I put the magazine on the bookcase.

The generalization to be drawn here is that Frame locatives are never postverbal, and not that arguments are never preverbal; for we find examples like (24a), where a Participant locative is preverbal, and (25a), where a direct object is preverbal¹⁰.

4 A note on optionality

The correlation between the syntactic and semantic properties of locatives that we have just seen evidence for allows us to maintain that the mapping from thematic structure to syntactic argument structure is quite direct. One possible objection to this is that most locative phrases, whether Participant or Frame, are syntactically optional. If we assume that all possible thematic positions must be mapped into D-structure, optionality of locatives yields a violation of the Projection Principle of Chomsky (1986). For the present, we will posit

that the optionality lies in the mapping from the lexical thematic structure to D-structure, and that this mapping is simply optional for locatives in general. There seem to be very few exceptions to this; it is difficult to find any English verbs besides *put*, *place* and *reside* which require a locative argument. We expect that this optional expression of locatives is universal across languages. Many questions about optionality are explored in Klipple (in prep.).

5 Conclusion

In summary, the syntactic evidence from Chinese and English shows that we should make a semantic distinction between locatives which give information about the internal aspectual structure of the event, and those which provide only an external context. This suggests, as we have argued, that an element is a semantic argument, and part of thematic structure, if and only if it influences the aspectual interpretation of the event, and that we can maintain a close and principled correlation between syntactic argumenthood and semantic argumenthood.

Notes

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1 In the remainder of the paper, we will tend to use the term "event" loosely to cover events, situations and states of affairs.

2 The last two terms are from Pustejovsky (1988).

3 The major problem for maintaining a tight correlation between semantic and syntactic argument structures, with regard to locatives, is that most arguments are optional. We will return to this point in section 4.

4 This distinction between locatives has been discussed in the literature as the "internal/external" distinction by Baker (1986), following Chomsky (1965).

5 The types of locatives discussed here are mainly those posited in Jackendoff (1983) (although he does not treat the "Posture" phrases in this way). Although they seem a fairly reasonable set, I make no claim as to the correctness or completeness of this classification.

6 Baker (1986) discusses this notion of relative size and its relevance to the syntax of locatives in English and some Bantu languages.

7 This assumption is problematic for the treatment of subjects, which might be theta-marked but outside of the VP; however, this issue is peripheral to the present discussion.

8 In particular, Sources seem to be more adjunct-like than other Participants, and need further subdistinctions. Also, Posture phrases may exhibit a systematic structural ambiguity between Frame and Participant readings, which needs further elucidation. Exploration of these issues is beyond the scope of the present paper; they are focussed on in Klipple (in prep.).

9 The following abbreviations will be used in this section: CM – completive aspect; MW – measure word. The data here is taken either from Li and Thompson (1981) or from my consultants. A detailed discussion of this distinction between types locatives in Chinese can be found in Tai (1975).

10 The criterion for differentiating arguments from adverbials which we have seen here only distinguishes well between Frame locatives on the one hand and Goal and Posture phrases on the other – Sources and Paths in general must in fact be preverbal. Some evidence exists in Chinese which differentiates these phrases from Frame locatives as well, although the facts are less clear; this evidence is discussed in more detail in Klipple (to appear).

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Views on Grammatical Voice from and for the '90s

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1. **Henry Sweet on Voice.** I am delighted to have been given an opportunity to address a conference on the theme of "Looking Toward the 90s", as it provides me an excuse (presuming I need one) to discuss the work of Henry Sweet, whose monumental A New English Grammar (NEG) appeared in that decade, Part I in 1891 and Part II in 1898. The references following the quotations below are to section numbers. Sweet's work and also that of Otto Jespersen on grammatical voice provide a useful background to current discussions of the problem of that topic.

Sweet defined 'voice' as follows:²

- (1) By voice we mean different grammatical ways of expressing the relation between a transitive verb and its subject and object. The two chief voices are the **active** (he saw) and the **passive** (he was seen). (311)

Sweet had nothing further to say about the active voice. Concerning the passive voice, he went on to say:

- (2) The passive voice ... is a grammatical device for (a) bringing the object of a transitive verb into prominence by making it the subject of the sentence, and (b) getting rid of the necessity of naming the subject of a transitive verb. (313)

He also noted that some languages have other voices beside the active and passive:

- (3) Some languages, such as Greek, have a **reflexive** or **middle voice** ... in which the action of the verb is referred back to the subject in various ways. (316)

Sweet contrasted passive voice, which he considered a formal grammatical device for making the object of a transitive verb its subject, with the direct manifestation of the object of a transitive verb as its subject. In the latter case, he classified the verb as 'passival':

- (4) Transitive verbs are sometimes used without an object-word for a different reason, namely that their grammatical subject is logically their direct object, as in the book sells well, meat will not keep in hot weather, the subject not being expressed because of its indefiniteness. We call sells and keep in such constructions **passival** verbs.

This inversion of the relations between subject and object is also expressed by a definite grammatical form called the **passive voice** (311). (249)

That is to say, the "inversion of the relations between subject and object" may either be a formal grammatical property of a sentence type, or a property of a verb.

The contrast between a formal property of a sentence type and a property of a verb may also be found across languages. He noted that while English lacks a middle (or reflexive) voice construction, it does have reflexive verbs, which are intransitive, and for which the action of the verb is referred back to the subject, as in the middle voice:

- (5) In such a sentence as he contradicts himself, we have a transitive verb followed by a reflexive pronoun in the object-relation. So also in to wash oneself, to keep oneself in the background. But in to wash in cold water, to keep in the background, to keep quiet, the reflexivity is not expressed by any pronoun, but is implied in the verb itself, which is thus changed from a transitive into an intransitive **reflexive** verb.

Some languages have special inflections or other formal marks to show when a verb is used in a reflexive sense, such as the Greek 'middle voice' (316). (254)

A similar class of verbs in English are the 'reciprocal verbs':

- (6) In such sentences as they fought each other, they fought one another, we quarreled with each other, we have the combination of a verb with a reciprocal pronoun. If these pronouns are dropped, and the idea of reciprocity is implied in the verb itself, it becomes a **reciprocal** verb, a transitive verb becoming intransitive at the same time. Fight and quarrel are reciprocal verbs in such sentences as those two dogs always fight when they meet; we quarreled, and made it up again. (256)

In an interesting passage, Sweet noted that a reflexive construction in some languages is sometimes used with with a passive interpretation like that of English passival verbs.

- (7) In some languages the combination of a transitive verb with a reflexive pronoun is used passivally. Thus in French, se vend, literally 'sells itself,' is used to mean 'is sold,' being thus equivalent to sells in the book sells well. (255)

Finally, Sweet pointed out a few other cases in English in which an active form of a verb is sometimes used with a passive interpretation.

- (8) The simple infinitive and supine are primarily active, but there is also a **passival supine**, as in this house is to let. (322)
- (9) The definite active forms are occasionally used in a passive sense: that house has been building a long time there is an answer waiting = '... being waited for'. This is the result of the Modern English gerunds having originally been abstract nouns (1257), which, of course, are neutral as regards the distinctions of voice. (2312)

In summary, Sweet reserved the term 'voice' for classifying sentence types containing transitive verbs, and in particular contrasted passive voice, which is a device for making the object of a transitive verb its subject and for enabling the subject not to be expressed (though it may be expressed), with a variety of 'passival' verb types, such as passival verbs and passival supines. In the latter case, the logical object simply is the subject, and the logical subject is not expressed.

2. Otto Jespersen on Active and Passive. Jespersen did not use the term 'voice' in his seven-volume work A Modern English Grammar (MEG), which appeared over the forty-year span from 1909 to 1949. Rather, he used the terms 'active' and 'passive' either alone or as a modifier of 'verb' or of 'form'. The references following the quotations below are to volumes, sections and subsections.

Jespersen's definition of 'active' and 'passive' differs somewhat from Sweet's account in (1):

- (10) In a great many cases the same idea may be expressed in two different ways, called the **active** and the **passive**. By this means two principals may change places, so that what is the object in the active is made the subject in the passive: what is the subject in the active, is in Modern English passive sentences generally added by means of by (the "converted subject"): for instance: Cats eat rats (active) = rats are eaten by cats (passive). It will be seen that the passive verb in English always has an auxiliary verb (is, sometimes gets, etc.) (II, 1.64)

By focusing on the semantic equivalence of the active and passive construction when the logical subject of the passive is expressed, Jespersen does not (at least in this passage) take cognizance of the point made by Sweet, namely that the logical subject of the passive does not have to be expressed.

Jespersen disagreed with Sweet's account of sentences of the type the book sells well, arguing as follows:

- (11) How are we to account for [the] phenomenon [of the active-passive use of some verbs]? Sweet NEG § 249 calls such verbs passival and says that "their grammatical subject is logically their direct object... the subject not being expressed because of its indefiniteness". but this evidently is neither a good description nor an explanation of the phenomenon: how is it that here subject and object seem confused, while it is utterly impossible to say, e.g. his words believe meaning "they believe his words". no matter how indefinite the subject is? Nor is there any occasion to create a new term passival verbs: our concern is not with a special class of verbs ... but with a special use of a great many verbs under special conditions.

- The peculiarity of this use consists in the passive meaning to be attributed here to the active verb, which is thus notionally passive though formally active. (II. 16.8_a)
- (12) When we say "his novels sell very well", we think to some extent of the books as active themselves, as the cause of the extensive sale, while we are not thinking so much of the activity of the bookseller. The sentence therefore is descriptive of something that is felt as characteristic of the subject, and therefore the verb generally requires some descriptive adjective or adverb Very often the pseudo-activity of the subject is shown by the use of the verb will, especially in the negative form: the figures will not add. (II. 16.8_a)

In (11), Jespersen contended that sentences of the type the book sells well constitute a construction rather than the projection of a lexical type. He characterized the construction as "notionally passive" because the logical object of the verb is the subject, and "formally active" because the verb lacks passive morphology. His claim in (12) that the subject is thought of as "to some extent ... active" in relation to the verb suggests that the construction could be analyzed as notionally middle rather than as notionally passive. However, he construed this activity as figurative rather than as literal: otherwise, he would not have described it as a "pseudo-activity". We return to the analysis of this sentence type in section 4.

3. Commentary on Sweet and Jespersen on Active and Passive. Both Sweet and Jespersen applied the terms 'active' and 'passive', and related terms, in two ways, one having to do with grammatical form and the other with grammatical function. In NEG, Sweet used the expressions 'active', 'passive' and 'middle' together with 'voice' when he had in mind grammatical form, and used other locutions such as 'passive sense' and 'passival verb', when he had in mind a grammatical function to which the form did not correspond. Jespersen, on the other hand, did not use the term 'voice' in MEG.

but his use of 'passive verb' and 'passive form' for example, corresponds to Sweet's use of 'passive voice'. Jespersen's phrase 'passive meaning' corresponds to Sweet's 'passive sense'.

However, how are we to construe the phrases 'passive sense' and 'passive meaning' if the "same idea" (cf. 10) is expressed by the active and passive forms of a sentence? I suggest that these phrases are intended by Sweet and Jespersen to be understood as applying in situations in which the subject of a verb is to be analyzed as its logical object and there are no "special inflections or other formal marks" that show that this is the case.

Clearly, both Sweet and Jespersen take for granted that one can identify the logical relations that hold between a verb and its subject. Otherwise, how would one know when an expression has passive meaning in the absence of formal indication of its status? Compare the examples (a) that chef slices beautifully and (b) that salami slices beautifully. The most natural interpretation of (a) is that it has active meaning with an understood object, and of (b) that it has passive meaning with an understood subject. The opposite interpretations are, however, possible: i.e., both sentences are ambiguous. We arrive at these interpretations from our understanding of the relations that chefs and salami may bear to the act of slicing, and of our understanding of the contribution of the adverb beautifully. If the subject is understood as carrying out the action of slicing some unspecified object in a beautiful manner, then the interpretation is 'active'. If, on the other hand, the subject is understood as undergoing the action of slicing, with the action being understood as happening in a beautiful manner, the interpretation is 'passive'.

4. The Mapping of Thematic Relations onto Grammatical Relations.

Sweet and Jespersen's ideas concerning active and passive meaning can be understood in contemporary terms as having to do with the mapping of thematic onto grammatical relations. Thematic relations can be grouped into types, of which I distinguish three: **affector**, **affectee** and **neutral**. A bearer of thematic relations may bear any one of these alone, or a combination of one affector and one affectee relation, or a combination of two neutral relations. We have then the five cases in (13).

- (13) a. affector only
 b. affectee only
 c. both affector and affectee
 d. neutral only
 e. doubly neutral

In this paper, I consider just the first three cases.² If the bearers of these thematic relation types are mapped onto the subject relation, we would be tempted to call the first mapping 'active', the second 'passive' and the third 'middle'.³ However, in considering the mapping of thematic relations onto grammatical

relations, it is important to consider how each of the arguments of a given predicational element is mapped. In the case of the mapping of bearers of the types in (13a) and (13c), the terms 'active' and 'middle' are satisfactory enough; however, in the case of the mapping of bearers of the type in (13b), the term 'passive' is too specific. We follow Sweet in applying the term 'passive' to the mapping of an affectee onto the subject relation and the mapping of an affector onto an optional oblique relation. Following a suggestion of Eloise Jelinek, I call the class of mappings of a bearer of an affectee-only relation onto the subject relation 'nonactive', and distinguish the four subtypes of nonactive mappings in (14) depending on whether or not there is an affector argument, and if there is whether it may be mapped, and if it may be mapped, whether it is optionally or obligatorily mapped.⁴

(14) A nonactive mapping is:

- a. **inverse**, if there is an affector argument which is obligatorily mapped onto a direct nonsubject relation, as in the Navajo bi-construction:
- b. **passive**, if there is an affector argument which is optionally mapped onto an oblique relation, as in the English passive construction:
- c. **impersonal**, if there is an understood affector argument which is obligatorily not expressed, as in the English constructions exemplified by the salami slices easily:⁵
- d. **unaccusative**, if there is no affector argument, as in the English intransitive constructions exemplified by the chef died, the book sold.⁶

With this more detailed breakdown of nonactive mappings into subtypes, we see that Sweet's passival verbs are to be analyzed as those that participate in the impersonal mapping.⁷ We answer Jespersen's objections to Sweet's analysis by maintaining that the mapping potentials of predicates must ultimately be lexically specified, though many of those potentials doubtless are consequences of other lexical properties.⁸ Jespersen's observation that the sentence *his words believe is ungrammatical in English in fact does not even bear on Sweet's claim that there is a class of passival verbs in English. What Jespersen's example shows is that English believe does not permit the unaccusative mapping, and the explanation for this fact is presumably that believe requires an affector argument. What is not so easily explained is why believe belongs to the class of verbs that also does not permit the impersonal mapping: that is, why *his words believe easily and *it believes easily that the earth is flat are also ungrammatical.

5. Principles Relating Mapping and Voice in English. In English, the use of passive voice is restricted to clauses with passive

mapping and conversely.* Putting this in the form of a principle of grammar, we have:

(15) Passive voice is used if and only if the mapping is passive.

If every clause type has voice, then we conclude that in English, active voice is used whenever the mapping is defined for English and is not passive.¹⁰ That is:

(16) Active voice is used if and only if the mapping is defined for the language and is not passive.

Principle (15) straightforwardly accounts for the ungrammaticality of sentences such as *100 kilos were weighed by the chef, on the interpretation in which the chef's weight was determined, since the voice is passive but the mapping is neutral. However, the account of the ungrammaticality of unemphatic passive sentences with reflexive or reciprocal by-phrases, such as *they were washed by themselves / one another¹¹ rests on a subtlety. Since the oblique object in such examples corresponds to a bearer of combined affector and affectee relations, the mapping is not passive, the passive mapping being defined as providing for an optional oblique affector-only argument. A similar account can be given for the ungrammaticality of the sentence *their hands were washed by them (with their and them coreferential).

Principle (15) also entails that what is often referred to as the experiencer thematic relation is sometimes to be considered an affector relation and sometimes an affectee. For example, both (a) the chef enjoyed the wine and (b) the wine pleased the chef, have passive voice counterparts, namely (c) the wine was enjoyed (by the chef) and (d) the chef was pleased (by the wine). By principle (15), the mapping in (c) and (d) must be passive. Hence in (c) and ceteris paribus in (a), the chef must correspond to the bearer of an affector role; but in (d) and ceteris paribus in (b), the chef must correspond to the bearer of an affectee role. Thus, the grammatical subjects of active-voice psych-predicates such as please correspond to affectors, just as do the grammatical subjects of active-voice nonpsych-predicates such as enjoy.

6. Other Principles Relating Mapping and Voice. In languages which distinguish among active, middle and passive voices, principle (15) appears to remain intact. The fact that in many such languages middle voice is used when the mapping is impersonal (cf. 7) is not a counterexample, since the passive mapping is distinct from (albeit closely related to) the impersonal mapping. What would be a counterexample to (15) is the use of passive voice when the mapping is impersonal. Such a situation would be hard to verify, since the only difference between the passive mapping and the impersonal mapping in that case would be the optional occur-

rence vs. the obligatory nonoccurrence of a term corresponding to the affector.

As English, as Sweet analyzes it, shows, middle mapping is possible without middle voice being available. However, the devices that are available for expressing middle mapping in languages which lack middle voice are restricted, being limited to the expression of reflexive and reciprocal relations. Middle mapping that is expressed by middle voice permits relations that are neither strictly reflexive nor strictly reciprocal, as in the French example les femmes se libèrent, which could be used to describe a situation in which two of the women in question are liberating each other and the third is liberating herself. As noted in Langendoen (1978), there is a dialect in English in which the sentence the women are liberating themselves can be understood exactly as the corresponding French example, in which case, we should conclude that this dialect of English, at least, has middle voice, and the reflexive pronoun is part of the construction.

The principles that govern the mapping domains for active and middle voices are not yet entirely clear to me.¹² The following principle, however, may well be correct.

(17) If the mapping is middle, then the voice is middle.

A principle corresponding to (17) for active mapping and voice does not appear to hold in general, since there are languages in which middle voice is idiosyncratically used in situations where the mapping appears to be active. However, active mapping does appear to be used invariably with both the unaccusative and the neutral mapping. Hence:

(18) If the mapping is unaccusative or neutral, then the voice is active.

Finally, languages do not appear to permit a true inverse mapping unless they have a particular construction for it. If this construction is called 'inverse voice', then we have:

(19) The mapping is inverse if and only if the voice is inverse.

Note that the form of (19) is just like that of (15). If (15) and (19) are correct, then the existence of both passive mapping and inverse mapping in a language depends on the existence of a particular voice to express it. Without the appropriate voice, the passive and inverse mappings are not found.

Notes

1. For a useful survey of the use of the term 'voice' in traditional and modern grammatical analysis, see Lyons (1968).

2. Neutral thematic relations are borne by arguments of stative relations, as in the chef is tall and the chef weighs a hundred kilos. The combination of two neutral thematic relations is borne by the subject of certain symmetric relations, as in those chefs are similar.

3. The term 'unergative' is sometimes used for a subclass of middle mappings exemplified by the boy leaped.

4. Note that I distinguish between nonactive and neutral as separate types of mappings. Though the mappings of neutral thematic relations are not the topic of this paper, a few remarks concerning them appear at the end of the paper.

5. The name for this mapping should perhaps be elaborated to indicate that a bearer of an affectee relation must appear as subject, to distinguish it from impersonal mappings with empty or expletive subjects and obligatorily understood affector and affectee arguments, or just an obligatorily understood affectee argument. The former mapping appears in many languages, such as German, and the latter mapping occurs in Romani (Dana McDaniel, personal communication). We do not consider these other impersonal type mappings further here.

I reject the term that is most commonly employed for the mapping in contemporary discussions, namely 'middle', because I use that term exclusively in its traditional sense. I do not know who is responsible for introducing the term 'middle' for what I here call 'impersonal', but it apparently arose early in the history of generative grammar.

6. The term 'unaccusative' for this mapping is to be preferred to 'ergative' used by Keyser and Roeper (1984) and Fagan (1988): the latter is actually an excellent synonym for 'active'.

7. The unaccusative and impersonal mappings are sometimes hard to distinguish, and some of the examples that Sweet and Jespersen used to illustrate the impersonal (passival) mapping are properly analyzed as involving the unaccusative mapping, for example the book sells well. The impersonal mapping (pace Fagan 1988; I construe her example this dress buttons as unaccusative rather than impersonal in mapping) in English requires the use of a certain kind of adverbial modifier, which 'introduces', so to speak, the understood affector argument, but not all adverbial modifiers do this successfully, in particular well does not. One that does work successfully is easily.

8. For example, the fact that sell permits the unaccusative mapping (cf. n. 7), as in that painting sold recently, whereas buy does not (cf. *that painting bought recently), can be explained by the fact that a selling event can take place without a seller (as

when one leaves money for a newspaper at a newsstand) whereas a buying event cannot take place without a buyer. The explanation for why certain predicating elements can occur with an impersonal mapping and others cannot is not so easily explained, as is pointed out in the text in connection with the discussion of believe. Interesting discussion of this and related problems can be found in Hale and Keyser (1987) and Fagan (1988).

9. However, passive voice can be used when the subject appears not to bear any thematic relation at all to its predicating element, as in sentences such as the chef is said (by the waiter) to bake excellent pies. The problem here is to show that the mapping is in fact passive. One possibility is to analyze the subject in such cases as an affectee of a complex predicate, in this case say to bake. Another is to alter the definition of the passive mapping, so that the subject may be athematic with respect to its predicate.

10. For example, inverse mapping is not defined for English and hence is not expressed by any voice.

11. These sentences are grammatical if the reflexive or reciprocal expression receives contrastive stress. This can be accounted for by analyzing the subject as corresponding to a bearer of an affectee role only, as in the corresponding cleft sentences: it was themselves / one another they were washed by.

12. See Kemmer (1988) for a thorough discussion of middle voice.

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Toward a Formal Characterization of Topic Construction with Special Reference to Korean
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0. Introduction

This paper proposes a formal account for the so-called topic constructions with special reference to Korean which has a morphological marking for the topic of sentence. I define the notion of 'topic' as context-setting, in the spirit of Reinhart (1981) and Stalnaker (1974, 1978), and identify the Korean topic marker *-nûn* with a linguistic device encoding the context-setting function of sentence topic. Stating explicitly the pragmatic and/or semantic nature of the topic NPs, we can get theoretically more rigorous explanations for what contribution they are making toward the overall meaning of the sentences in which they occur, and for how it is carried out. Although this paper deals with only a limited class of the Korean Topic Constructions (KTCs), I hope, its results will contribute to the development of the formal theory of topic constructions in natural languages.

Section 1 briefly outlines some peculiarities of the KTCs, focusing on the basic differences between the sentences with a topic marked NP and the corresponding ordinary sentences. Section 2 points out some problems that previous proposals about what the 'topic' is may face in handling the KTCs. Section 3 introduces a formal model of 'conversational records' consisting of ordered pairs the first conjunct of which is an entity and the second is a proposition associated with that entity, and then define the topic of sentence as setting a context in which the referent of a topic-marked NP is the most salient among the entities of a conversational record at the time of utterance. Handling the KTCs within the framework of two-stage semantics, I further argue that defining topic as context-setting derives an expressive presupposition. Section 4 discusses some theoretical consequences of the proposed definition of topic, and shows that some of the constraints on KTCs follow from this definition.

1. A Sketch of Korean Topic Constructions

One of the most prominent features of Korean (and Japanese) that distinguish them from English and other languages is that the former languages have a morphological marking for the sentence topic, as in (1), but the latter languages do not.^{1,2}

- (1) a. John-ûn michyðtta b. John-i hakkyo-e katta
 -top (was) crazy -sub school-to went
 'Speaking of J, he was crazy' 'J went to school' or 'It is J who went to school'

Sentence (1a) has the topic *John*, to which the particle *-nûn* is attached. This particle, then, is thought of as a topic marker and bifurcates the sentence into the topic and comment. Sentence (1b), however, contains no topic NPs indicated by the overt topic marker and is

neutral to the topic-comment distinction, when it is normally uttered.³

The first observation by previous works on KTCs concerns the identification of the referent of the *-nûn* marked NP in the discourse domain. When sentences like (1b) are used as neutral description, the existence of the subject NP referent is asserted, and thus introduced directly into the domain, as soon as they are uttered. So in sentences like (1b), used as a neutral description the existence of the subject NP referent need not be presupposed. On the other hand, in sentences like (1a), the referent of the *-nûn* marked NP should be presupposed in the domain of discourse. Let us call the restriction the Context Compatibility Condition (CCC) that KTCs are only interpretable when the referent of the *-nûn* marked NP is previously registered in the domain, at the time of utterance.

The following paradigm presents the examples of CCC effects:

- (2) a. *Mary-/na-/kû saram-/John-wûi apðci -nûn ðce John-ûl mannatta*
 Mary-/I-/that (or the) man-/John's father-top yesterday -obj met
 'Mary/I/that (or the) man/John's father met John yesterday'
 b. *kæ-nûn yðngrihata 'Dogs are clever'*
 dog-top clever
- (3) a. *?motûn saram-ûn pyðngtûnghata* b. *?*motûn saram-ûn John-ûl miwðhanta*
 every man (is) equal -obj hates
 'Everyman is equal' 'Everyman hates John'
- (4) a. *tæpupun-wûi saram-tûl-ûn ttðnatta* b. **manûn saram-tûl-ûn ttðnatta*
 most-gen people-pl-top left many people-pl-top left
 'Most people left' 'Many people left'
- c. **ðtûðn namca-nûn Mary-rûl saranghanta* d. **han namca-nûn Mary-rûl saranghanta*
 some man-top -obj loves one man-top -obj loves
 'Some man loves Mary' 'One man loves Mary'
- (5) **nuku-nûn Mary-rûl saranghan-ni?*
 who-top -obj loves-Q
 'Who loves Mary?'

As sentences like (2a) show, the topic marker is free to be attached to definite, referential NPs. If non-referential expressions are topic-marked, as in (2b), the resultant sentences are only interpreted as generic statements. From this fact, it has been argued that indefinite, referential NPs cannot qualify for the sentence topic. While topic marking of universally quantified NPs results in marginally acceptable sentences, as in (3a), or in ungrammatical ones, as in (3b), topic marking of existentially quantified NPs always results in ungrammatical sentences, as in (4b-d). Sentence (4a) exhibits quite an exceptional behavior of the quantifier *tæpupun*, which is presumably corresponding to the English *most*. Unlike other quantifiers, it does not block its head noun from being topic-marked. It is on a par with the definite article with respect to the topic marking. Sentence (5) shows that the topic marking is never compatible with wh-questioning a *-nûn* marked NP.

Some of the above restrictions on topic-marking may be derived from the CCC. Since participants of conversation are not able to identify, and thus to presuppose, the entities, or groups of entities, expressed by indefinite, referential NPs, or wh-phrases in the context of

discourse available, one might attribute the ungrammaticality of (4b-d) and (5) to a violation of the CCC. However, this condition has two undesirable problems: it would wrongly predict that sentence (4a), involving categorical judgment, is ungrammatical. Thus, the grammatical status of (4a) casts doubt on the empirical justification of the CCC. Secondly, even if it is empirically correct, the CCC does not explain the KTCs, in any sense of 'explain', but gives a partial generalization about them.

The fact that grammaticality judgments vary among sentences containing a universally quantified NPs as a sentence topic calls for its own explanation. Although universally quantified expressions are usually unavailable for the sentence topic, they are marginally topic-marked in a limited set of environments. Sentences with such topic-marked NPs become more acceptable, as the VP predicate is an individual-level predicate, as in (3a), or involves some kind of modality, as in (6):

- (6) ?motûn haksæng-ûn kongpu-rûl hæyaman-hanta
 student-top study-obj do-must
 'Every student must study'
 (7) ?*kæ-nûn John-ûl mulðtta 'Dogs bit John'
 dog-top -obj bit

The deviance of sentence (7) shows that generic topic NPs, like universally quantified topic NPs, cannot be used with a stage-level predicate. In order to rule out such sentences as (3b) and (7), let us posit the following constraint:

- (8) The Predicate Condition (PC)
 Universally quantified NPs or generic NPs cannot be topic-marked, when they are used with stage-level predicates.

Yang (1973) gives further evidence for the necessity of this kind of cooccurrence restriction. Let us consider the following:

- (9) yðca-ka/*-nûn ta kwðntu-rûl hane
 women-suj/-top surprisingly boxing-obj do
 'To my surprise, women box!'

As translation shows, the adverb *ta*, which expresses the speakers' surprise or unexpectation, cannot cooccur with the topic marked NP. In our terms, *ta* turns the predicate, *kwðntu-rûl hane* 'do boxing' into the more stage-level predicate, *ta kwðntu-rûl hane*, roughly translated as 'do boxing unexpectedly', and this makes the generic term, *yðca* not to be topic-marked.

The third issue about the KTCs is that the content of the utterance that the speaker intended to convey, i.e., the proposition expressed by it, is only relevant to the *-nûn* marked NP. To be more concrete, suppose a situation where some foods such as *hamburger*, *salami*, and *pizza* are registered in a given context of utterance. When someone

utters sentence (10) below,

(10) pizza-nûn John-i cohahanta 'As for pizza, John likes it'

we interpret the content of (10), $\| \text{John likes pizza} \|_{\mathbf{M}}$, to be only relevant to the entity named pizza. That is, this proposition is never associated with any other entities, whether or not they are registered in the discourse. I will call this the Restricting Condition (RC).

2. Previous Proposals about the Topic of a Sentence

2.1 Old/New Information

One of the widely used notions of topic is that topic conveys old information. This view comes from the tradition of Prague School which analyzes a sentence as composed of the theme/rheme or the topic/comment. On this view, the topic expression of (1a) conveys a property of the referent denoted by John, namely, one that that referent is already known to the participants of the conversation, and the comment, the new information about the topic. Identifying topic with old information is crucially based on the CCC, which demands the referent of the topic of a sentence be previously introduced into the domain of discourse.

Reinhart (1981) criticized the old/new dichotomy analysis for the following two reasons: First, the old information is neither a sufficient nor a necessary condition for an expression to serve as the topic expression. Secondly, topichood cannot be defined on referents. She gave the following example (her (37)):

(11) A: Who did Felix praise? B: Felix praised HIMSELF

Here, the referent named Felix serves as the topic of B's utterance, as derived by the previous question, and at the same time, as the focus, or the comment of that utterance, as derived by the syntactic coreference rule. This leads to a plain contradiction.

In response to Reinhart's second argument against the old/new information analysis of topic, von Stechow (1981) argued that the identification of topic information with the old information is still needed. Following Stalnaker (1978), he assumed that the topic information is entailed by the common ground of the context. Then, topic information is equated with the existential closure of a λ -expression that is obtained by replacing the focus-constituent of the sentence with a 'corresponding' variable.⁴ Roughly speaking, in a situation where conversations like (11) occur, the topic information of (11B) is identified with that proposition p , $\| \exists z(\lambda x[\text{Felix praised } x](z)) \|_{\mathbf{M}}$, and the new information is the material implication between that proposition p and the proposition q , $\| \text{Felix praised Felix} \|_{\mathbf{M}}$, i.e., $\| \exists z(\lambda x[\text{Felix praised } x](z)) \|_{\mathbf{M}} \rightarrow \| \text{Felix praised Felix} \|_{\mathbf{M}}$.

He further suggested that Reinhart's first objection may not be a serious problem for the old information approach to topic, if it introduces some kind of 'rules of

accommodation', as proposed by formal pragmatic presuppositionists. Let's take the following sentence as an example:

- (12) John-un Seoul-es \emptyset Mary-rul mannatta 'John met Mary in Seoul'
 -top -in -obj met

Imagine a situation where the speaker assumes that the hearer knows about John, but, in fact, the hearer does not know anything about John, even the fact that John exists, at the time of utterance. On the part of the hearer, the topic information of (12) is nothing at all. He may request the speaker to tell him something about John, by questioning like the following:

- (13) John-i nuku-ni? 'Who is John'
 -sub (is) who-Q

or tacitly change the common ground and act as if the information were already in that common ground. In this sense, the old information can be a necessary condition for the topichood.

However, this extended version of the old information analysis is of no use to characterize the KTCs. Following von Stechow (1981), the assertion of (12) would be something like this:

- (14) $\|\exists Q(\lambda P[P \text{ is true of John}](Q))\|^M \rightarrow \|\text{John met Mary in Seoul}\|^M$

Note, however, that the antecedent of this implication is a logical truth. Obviously, (14) is not what we want to get. Since the antecedent of (14) is always true in every model, it appears that we don't need any kind of accommodation, which is independently motivated.

2.2 Aboutness

Reinhart (1981) proposed that topic should be defined in terms of pragmatic aboutness. So topichood is viewed as a relation between an argument and a proposition relative to a context. Then, the topic of a sentence is what the sentence is about. It is obviously appealing to our intuition that a sentence, or its proposition is talking about something, and this notion of topic incorporates the idea of the RC rather than that of the CCC, both of which are characteristics of the topic constructions. However, this does not mean that Reinhart's analysis of topic is incompatible with the CCC effects. What I say here is that this notion is primarily based on the Restricting Condition, or Kuroda's (1972) categorical judgment.

In order to compensate for the lack of the CCC effect in her definition of topic, she tried to reduce the CCC effects to two types of discourse devices, namely, referential links and semantic links, which are designed to link adjacent sentences. These devices, combined with the notion of context set, defined by Stalnaker's (1979), regulate the way

that discourse proceeds. I do not evaluate how well this strategy works here. I believe, as pointed out by Kuroda (1979), that the discourse or pragmatic notion approach to topic, whatever it is, is not sufficient to characterize the function of topic markers such as -nûn and its Japanese equivalent -wa.

As pointed out by Lee, H.-S. (1986), what is being talked about is not necessarily marked with the Korean topic marker, -nûn, and the topic marked NP is not necessarily what is being talked about either. I will refer the reader to Lee, H.-S. (1986) for relevant examples.

3. Topic as Context-Setting

In what follows, I will try to explain some semantic and/or pragmatic features of KTCs within the more semantically oriented framework that incorporates the concept of two stage semantics, proposed by Kaplan and others.

3.1 Structured Context Sets

Lee, H.-S. (1986, 1987) observed that the Korean topic marker -nûn serves to set up presuppositionally backgrounded information, i.e., the speaker's probabilistic assumption about the addressee's knowledge, as an unchallengeable point of departure for subsequent discourse. This presuppositionally backgrounded information is similar to what is called, in formal pragmatics, a conversational record, i.e., a set of common background assumptions built up among conversational participants. These two notions have one property in common: As much as presuppositionally backgrounded information need not be entailed by previously established information, a conversational record need not be entailed by an already existing conversational record. So if the speaker assumes something to be taken for granted by conversational participants, it may be accommodated into the stock of shared knowledge. The only difference between these notions is that a conversational record is defined as a set of pairs the first conjunct of which is an entity and the second is a proposition associated with that entity, while presuppositionally backgrounded information is not precisely defined.

As for Lee, H.-S.'s observation, one question naturally arises: what does it mean 'to set up presuppositionally backgrounded information as an unchallengeable point of departure for subsequent discourse'? It seems to mean that the topic marker, -nûn serves to reorganize the conversational record in such a way that propositions are multi-dimensionally sorted under headings, among which the one denoted by the topic expression is the most salient.⁵ Let us call this reorganized conversational record Topic-Oriented Record (TOR). I assume that the context set or record is always structured, even when the topic of a sentence is not established yet. In such a case, propositions are dimensionally catalogued under headings, but there is no salient heading in the conversational record. So we do not get a TOR.

As soon as a topic sentence is uttered in a given context, a TOR induced by that utterance is checked by the addressee relative to the given context. Unless he or she intends to violate Grice's Cooperative Principle, he accepts this TOR, if he has any information about the referent of the topic NP. If the addressee does not raise objection to the speaker's utterance, the proposition derived by that utterance is catalogued into the heading labeled by the topic NP, and conjoined with the already established propositions under that heading of the TOR in the context under consideration. In this way, the TOR is enriched. When the addressee does not have enough informations about the referent of topic NP for the TOR with this referent as the most salient heading to be set up in the context of utterance, he or she may request the speaker to give some informations about that referent. After the speaker adds some propositions that his or her addressee is willing to accept, into the heading which is formed, upon uttering the topic sentence, but which contains few, or presumably no propositions in it, he will repeat his original remark, and continue as before. Or the hearer may accept the TOR intended by the speaker as if the 'defective' referent of the topic NP is the most salient in the conversational record for some reason, and let the speaker go on without objection.⁶

When such accommodation occurs, conversational participants change the context of utterance by creating the TOR in which the heading associated with (almost) no propositions is the most salient. Let us call the contexts containing the TOR shared by all the participants of conversation the Topic-Compatible Contexts (TCCs), and define topic as setting up such contexts. Then, it follows that the Korean topic marker is a linguistic device for setting the TCCs. As will be clear, defining topic as context-setting plays a crucial role in assigning truth-conditions to the KTCs.

3.2 Two-Stage Semantics

It has been argued that the following two sentences:

- | | |
|----------------------------------|-------------------------|
| (15) a. John-ûn hakkyo-e katð | b. John-i hakkyo-e katð |
| -top school-to went | -subj school-to went |
| 'As for John, he went to school' | 'John went to school' |

have the same truth-conditions, but they differ with respect to where they are used. So previous work has been devoted to working on what their non-truth-conditional meanings are, and on how and why they differ. Contrary to this traditional view, I will argue that their truth-conditions are not exactly the same.

The point is that (15a), in contrast to (15b), may fail to be assigned truth values. Imagine a situation in which the speaker assumes that his or her addressee has enough information about the referent of John, but the addressee actually knows little about that referent. Suppose further that the speaker utters the sentences in (15), while entering the room where the intended hearer is. When the addressee hears (15b), he may request the speaker to give enough information to identify him, or reply in either of the following

ways, as long as he is faithful to the Cooperative Principle of conversation:

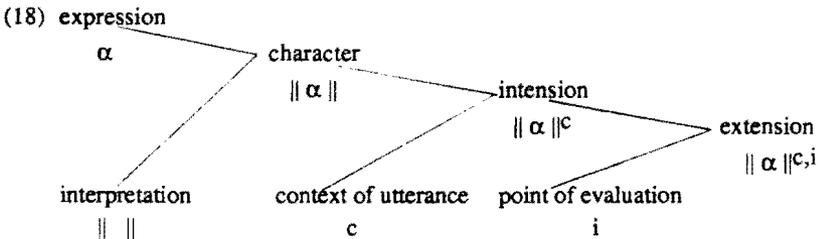
- (16) a. *kūræ?* 'Really?'
 b. *an katsũlkŏl* '(He) probably might not go (to school)'

Answers like (16a) indicate that the hearer passively agrees with the speaker with respect to the truth value of the speaker's utterance. On the other hand, responses like (16b) express doubt about, or disagreement with the truth of the speaker's utterance. In any way, the hearer appreciates the truth value of (15b). It was pointed out by formal pragmatic work that the relations like 'agreement' and 'disagreement' do not hold on the level of character nor on the level of extension, but on the level of intensions.⁷ If this is correct, it is reasonable to think that sentences like (15b) have, at least, their propositions.

On the other hand, when the addressee hears (15a), he cannot understand what it means, unless he is ready to accept the speaker's utterance in terms of accommodation. He could not appreciate the truth value of (15a), and immediately question as follows:

- (17) *nŏ cikũm musũn mal hako it-ni?* 'What are you talking about?'
 you now what word doing is-Q

Such a conversational situation occurs because of the hearer's failure of recognizing the context of utterance. Then, let us adopt the following model of semantic interpretation in order to explain how the KTCs receive their truth-conditional interpretations:



Suppose that you said sentence (15a) to me, and that I am a fluent Korean native speaker, and heard well enough what you were uttering, i.e., *John-ũn hakkyo-e katŏ*. Then, I can arrive at the character $\| \text{John-ũn hakkyo-e katŏ} \|$. But if I do not know in what context you were uttering, then the set of contexts C that for all I know we might be in will include c and c' such that $\| \text{John-ũn hakkyo-e katŏ} \|^c = \text{some proposition } p$, but $\| \text{John-ũn hakkyo-e katŏ} \|^{c'}$ is undefined. Therefore, I do not have a determinate proposition. Unless the KTCs are uttered within the TCCs containing a TOR that are inherited from previous context or formed by conversational participants' accommodation, they do not get their intensions. This process sufficiently shows that the KTCs may fail to have intentions, and thus have different truth-conditions from those assigned to their corresponding sentences

without topic marked NPs.

3.3 Topic and Presupposition

We have so far seen that Korean topic marked NPs set a context, and that sentences containing a topic marked NP can be interpreted as intension, only when they are uttered within the TCCs. In this section, we will address the question of what triggers restricting the possible utterance contexts to the set of TCCs.

I argued before that the character of a sentence with the topic marked NP does not give back any intension when it takes a non-TCC as its argument. A little more formally:

- (19) Let α be an expression of the type $\{0,1\}$ that contains the topic marked expression. Then, for any utterance context c , $\|\alpha\|(c) =$ intensions of the type $\{0,1\}$ if $c = \text{TCC}$
undefined if $c \neq \text{TCC}$

The fact that the character of the sentence with a topic marked NP is partial implies that Korean topic marking is subject to a certain condition on appropriate usage. According to this condition, people do not use topic constructions without making the topic marked NP set a TCC. It says that the context that is to admit the topic sentence contains a TOR that is shared by participants of conversation. It, then, can be viewed as a special instantiation of 'the expressive presupposition', in the sense defined by Soames (1989). For without this presupposition, the topic sentence cannot arrive at its intension.

4. Some Consequences and Further Issues

Thus far, we have defined the notion of topic as setting the context, called TCC, which contains TOR, and argued that the topic-marker $\text{_n\ddot{u}n}$ signals a speaker's intention to presuppose TCC to his/her addressee. In what follows, I will discuss some consequences that the proposed definition of sentence topic.

First, the expressive presupposition triggered by defining topic as context-setting gives evidence in favor of the admittance conditions view of presuppositions. While a speaker utters a sentence with a topic-marked NP, he conveys the content of the utterance as well as the presupposition, one that the entity denoted by the topic expression is the most salient in the structured conversational record in the utterance context. So, when the interlocutors take those propositions derived by the sentences with topic marked NPs, they are said to have the same TCCs with respect to those sentences. If it is taken for granted by conversational participants at the time of utterance that there is a TCC associated with the sentence containing a topic marked NP, entailed by previous context or formed by accommodation, it shows that the admittance conditions view is empirically superior to the conventional implicature view. Suppose a speaker uttered a sentence with topic marked NP about whose referent his hearer does not know at all. According to the conventional implicature view, it is obligatory for him to acknowledge that such utterance always has a

TCC associated with it. This is obviously not the case. It would mean that it is appropriate only when previous context ensures the salience of its topic expression, which has proved too strong. On the other, according to the admittance conditions view, the hearer does not have to acknowledge the presence of the TCC associated with the utterance. He may resist accepting the presupposition of that sentence, or be willing to accommodate it. In the former case, the conversation will be stuck, unless someone gives the speaker a chance to back up and fill in the missing presupposition. Therefore, the admittance conditions view makes a correct prediction with respect to the presupposition conveyed by sentences with topic marked NPs.

Secondly, defining topic as setting a context with TOR captures the essential aspects of the CCC and the RC which are not mutually exclusive but partially overlapping: they naturally fall out from the proposed definition of topic. Our definition already subsumed the CCC, since it says that among contexts associated with the sentence with a topic marked NP, the context in which the salience of the topic expression is induced by previous context is the most natural. As far as the RC is concerned, it is out of the question in our framework to say that the proposition to be matched with the topic heading is simultaneously associated with another headings in the context made up of the TOR in which that head is the most salient.

What remains is to explain what kind of objects can be conversational record's headings, and what relations hold for these headings and sentence topics. Let me answer the second question first. As seen above, definition of topic involves the relative degree of salience among conversational record's headings. This implies that every heading in conversational record could be a potential topic of the sentence. In a word, being a heading in conversational record is a necessary condition for being a sentence topic. Then, if we find very independently motivated answers to the first question, we can automatically solve a more linguistically significant question why only certain kind of NPs can be topic-marked. However, since it seems impossible to depict our mental computations in a non-derivative fashion, we ought to seek those answers in a derivative way, on the basis of linguistic data, which may reflect our mental structures and operations. If we succeed in characterizing the topic-marked NPs in terms of precisely defined notions, we can know indirectly what objects constitute conversational record's headings and thus have access to some of the cognitive and functional principles that regulate our speech acts. Unfortunately however, limitation of space stops me from pursuing this issue further here.

In sum, I characterized the pragmatic and semantic nature of the Korean topic marking, and clarified some contributions that the *-nūn* marked topics are making to the meaning of the sentences containing them. We saw that the KTCs are subject to the three distinguished constraint, namely, the CCC, the PC, and the RC. Most importantly, we identified the topic marker, *-nūn* with the linguistic apparatus encoding the context-setting function of sentence topic, and showed how the KTCs differ from their corresponding

sentences with no topic marked NPs, using the concepts developed by two-stage semantics: the former cannot have any truth conditional interpretations at all out of context, while the latter can always have truth conditional interpretations. The most important factor that differentiates the former from the latter was traced to the expressive presupposition, i.e., the natural consequence of the definition of topic we just made. This in turn, makes the admittance conditions view be more empirically correct than the conventional implicature view, at least, with respect to the Korean topic sentences. We deduced both the CCC and the RC from the definition of topic as context-setting.

Unfortunately, however, our analysis cannot give any account for the PC, which I think to be one of the most thorny problems regarding the KTCs. Furthermore, we do not handle several types of sentences that contain a complex NP as their topic, which might make the proposed analysis more complicated. In addition to these, there are too many unexplained aspects about the KTCs, including their contrastive usages, and their acquisition process, etc. We will leave these unsolved problems to future work.

Notes

* I would like to thank Hyo Sang Lee, Irene Heim and Babara Partee for their criticisms and suggestions on this material. I am also grateful to Ed Keenan and Seungho Nam.

¹ Although the term 'topic' has been used in the literature inconsistently, and often in a vague fashion, I simply assume that the topic of a sentence is what the sentence is talking about before that notion is precisely defined in Section 3. This seems intuitively clear, and sufficient for the purpose of this section.

² Sentence (1b) is ambiguous, as indicated by the two translations in the text. It can be used either as a neutral description or as an exhaustive listing reading. In the former case, (1b) describes the mere event of John's going to school, but in the latter case, it connotes that John is the only one under consideration who went to school. Sentence (1b), as used as a neutral description, is subject to the one constraint, according to which the VP predicate must be a stage-level predicate. For more detailed discussion, see Ogihara (1984, 1987) and Shirai (1987).

³ It seems to me that the topic-comment structure is omnipresent in natural languages, and that languages may differ as to the way that they accommodate it. For example, in languages like English, this structure is realized by means of word-order and/or prosodic features. Note that it is not quite right to say that sentence (1b) has no topic-comment structure, when some prosodic features are involved in it. However, I suspect that there are some basic differences between the morphologically marked topic-comment structure and syntactically or prosodically marked topic-comment structure. I will not go into details here. For more discussions about the latter case, see Reinhart (1981) and von Stechow

(1981), among others.

⁴ As he admits, the topic of a sentence, as defined in von Stechow (1981), is similar to what Chomsky (1969) and Jackendoff (1972) call the presupposition of the sentence.

⁵ A similar idea about the structured context set is found in Reinhart (1981). For formalization of 'salientness', see Sgall, P., E. Hajicova and J. Panevová (1986).

⁶ I suspect that this kind of accommodation rarely occurs in everyday conversations, but that it is not impossible.

⁷ For more details, see Irene Heim's class lecture notes of the Winter, 1989.

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Allomorphy in Tagalog Reduplication

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0. Introduction.

There are three forms of reduplication in Tagalog, which Carrier-Duncan (1984) labels RA, R1 and R2. In RA, the first consonant and vowel are copied, and the reduplicated vowel is always long. In R1, the first consonant and vowel are copied, but the reduplicated vowel is always short. See (1).

(1) (Carrier-Duncan 1984)

(a) RA reduplication.

li:nis	li:+li:nis	'clean'
gupit	gu:+gupit	'cut'
hintay	hi:+hintay	'wait'

(b) R1 reduplication.

kandilah	ka+kandilah	'candle'
?a:ral	?a+?a:ral	'study'
pa+sulat	pa+pa+sulat	'have someone write'

Both of these forms have straightforward analyses in a moraic templatic model of reduplication; as McCarthy and Prince (1986) show, the R1 template can be thought of as a core syllable ($\$_c$) and the RA template as a bimoraic syllable ($\$_{mm}$).

R2 reduplication is more complex. When a disyllabic word undergoes R2, both syllables are copied without modification. When a trisyllabic word undergoes R2, however, the first two syllables are copied with the following changes: (i) the final consonant of the second syllable (if any) does not appear, and (ii) the vowel of the second syllable becomes long. This is seen in (2).

(2) (Carrier-Duncan 1984)

(a) Disyllabic words

li:nis	li:nis+li:nis	'clean'
walis	walis+walis	'sweep'
pantay	pantay+pantay	'level'

(b) Words with more than two syllables

pa+labas	pala:+palabas	'cause to go out'
tahi:mik	tahi:+tahi:mik	'quiet'
baluktot	balu:+baluktot	'bent'

It appears, then, that a single template will not serve to produce these different effects. In light of this, Marantz (1982) proposes that there are two allomorphs of R2, one triggered by disyllabic words and one by longer words. McCarthy and Prince (1988:15) make this proposal more explicit, stating that

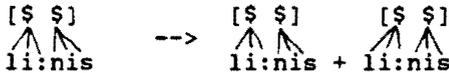
"...minimal bases reduplicate totally, while supraminimal bases have disyllabic reduplication with final \varnothing . The \varnothing is realized as vowel length in Tagalog by an independently motivated rule of deletion with compensatory lengthening...."

As given here, the difference between the two allomorphs is very large: in one case the minimal base (ie, the disyllabic base, since all content words in Tagalog are at least disyllabic) is copied in its entirety, both melody and prosody, while in the other only the melody is copied and then matched to the template [$\$ \$_{mm}$] (which I'll write as [$\$ \$?$] from now

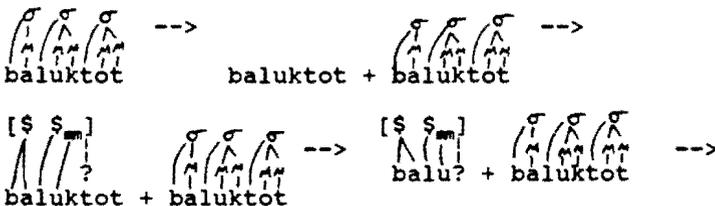
?)

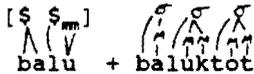
on). (The \varnothing -deletion and compensatory lengthening rules will be discussed shortly.) McCarthy and Prince, then, view R2 reduplication as represented in (3).

(3) (a) Minimal base



(b) Nonminimal base





The difference between the two proposed R2 allomorphs is reduced considerably, however, if we assume with Steriade (1988) that reduplication always involves copying the entire base (Full Copy). We can then say that R2 reduplication of minimal bases involves matching the copy to the template [\$ \$], while for nonminimal bases it involves matching the copy to [\$ \$?].¹

The goal of this paper is to argue that this difference between these proposed allomorphs can actually be eliminated entirely. As I will demonstrate, closed syllables in Tagalog are generally bimoraic; word-finally, however, they are monomoraic, the final consonant lacking any prosodic representation. Since, as we will soon see, final syllables in Tagalog are always closed, the second syllable of a minimal base always contains an unlicensed melody segment. The second syllable of a nonminimal base never contains an unlicensed melody segment. Thus minimal and nonminimal bases can be differentiated solely by the structure of their second syllable.

The crucial observation to make here is that the two proposed allomorphs [\$ \$] and [\$ \$?] corresponding to these bases are also differentiated solely by the structure of their second syllable. Generally such parallels are considered evidence against allomorphs and for an analysis involving phonologically conditioned surface variations of a single underlying morpheme. In the final section of this paper I will develop an analysis of exactly this second sort.

1. Syllable structure in Tagalog.

As a first step in our examination of the structure of the syllable in Tagalog, we should take a look at the ?-deletion rule with compensatory lengthening required for the analysis of R2 reduplication given in McCarthy and Prince (1988). Representative examples are given in (4).

(4) (Schachter and Otones, 1972).

ba:ba?	"chin"	ba:ba:+ba	"chin?"
hindi?	"no"	hindi:+ba	"no?"
lu:to?	"cooked"	lu:tu:+ba	"cooked?"

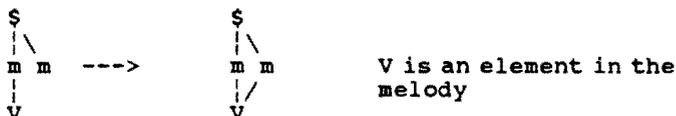
The ?-deletion rule can be given simply as in (5).

(5) ?-Deletion.

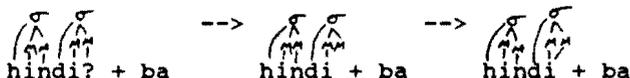
? --> ∅ / ___ C

With Hayes (1989), I will assume that compensatory lengthening results from the spreading of a segment into an adjacent mora that has been previously vacated. In the case of Tagalog, this means that the coda ? must be attached to a mora, and that a rule of Mora-Filling (from left) applies to fill this mora when the ? deletes. This rule and an example are given in (6).

(6) (a) Mora-Filling (from left).

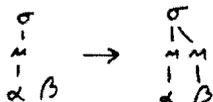


(b) Derivation of hindi:ba from hindi? + ba



I will also assume with Hayes (1989) that universally coda consonants are never moraic underlyingly, but only receive a mora via rule. The rule that does this is Weight by Position, given in (7).

(7) Weight by Position (Hayes 1989).



I therefore assume that Tagalog has the Weight by Position rule.

We are now ready to turn to the complex relationships between stress, vowel length and syllable structure in native Tagalog words, which are summarized in (8).²

(8) (Ramos 1971)

guto^ˈm "hungry" gu:to^ˈm "hunger" punta^ˈh "go
to"

		CV	CVV	CVC
word-final	stress	--	--	gu ^ˈ to ^ˈ m
	no stress	—	—	gu:to ^ˈ m
not word-final	stress	--	gu:to ^ˈ m	--
	no stress	gu ^ˈ to ^ˈ m	*	punta ^ˈ h

* unstressed long vowels only appear with RA and R2 reduplication and compensatory lengthening

The patterns to notice are the following: (i) word-final syllables are always closed; (ii) closed syllables are not stressed (with one exception); (iii) word-final syllables form the one exception to (ii).

The table in (8) suggests another generalization: (iv) vowel length is predictable, appearing only in stressed syllables (and in other cases, indicated by the * in (8), to be discussed later). That (iv) is indeed correct (ie, that stress affects vowel length and not the other way around) is seen in the data in (9), which can only be understood as an example of stress shift due to suffixation, as a shift in vowel length from one syllable to another is impossible to describe formally.

(9) (Ramos 1971).

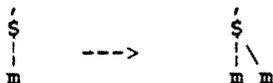
ba:sah ("read") + in --> basa:hi^ˈn ("to read")

la:pit ("come near") + an --> lapi:ta^ˈn ("approach")

Underlyingly, then, open syllables in Tagalog are monomoraic.

The lengthening of the vowel in stressed open syllables must be explained through the combined action of a Mora-Insertion rule, through which a stressed syllable gains a mora, and the Mora-Filling rule, already given above in (6). Mora-Insertion is given in (10) below.

(10) Mora-Insertion.



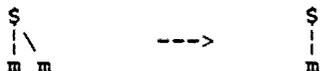
I now return to the patterns (i)-(iii) seen in (8) above. The lack of any word-final open syllables, noted in (i), may be explained by a constraint such as the one given in (11).

(11) Constraint on final segment*.

* $V]_{wd}$

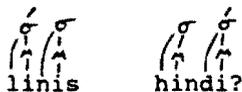
The unusual fact noted in (ii), that closed syllables in Tagalog avoid stress, can be understood if we assume that the rule of Mora-Insertion is obligatory whenever a syllable is stressed. Thus stress is blocked from falling on a closed syllable, since this would result in the insertion of an illegal third mora. If this is accepted, then an explanation for (iii) immediately suggests itself: the word-final syllable, which in Tagalog is always closed, may be stressed because it is monomoraic. This can be arranged through a rule of Mora-Deletion, given in (12), which leaves the word-final consonant without prosodic structure.

(12) Mora-Deletion.

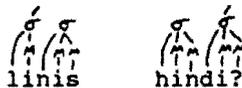


This rule would apply after any rule which adds a mora to the final syllable, such as Weight by Position or Mora-Insertion. Sample derivations are given in (13).

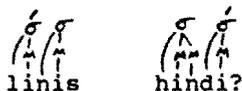
(13) (form after
syllabification
and stress-assignment)



Weight by Position



Mora-Deletion



Mora-Insertion		
Mora-Deletion ⁵	N/A	

It may seem odd that I suggest that the word-final syllable becomes monomoraic through mora deletion rather than mora extraprosodicity. Actually there is a very good reason for arguing against the latter. If the final mora were extraprosodic, we would expect that upon being stressed it would gain an extra mora without problem, thus resulting in a superheavy CVVC syllable. That the vowel remains short even when the final syllable is stressed indicates that extraprosodicity is not the mechanism operating here.

The upshot of the argument in this section is this: due to the action of a word-final Mora-Deletion rule, all word-final syllables in Tagalog are monomoraic. Specifically, the final segment is not prosodically licensed. This means that in minimal bases, which are disyllabic, the second syllable ends in an unlicensed melody segment, while in nonminimal bases, which have more than two syllables, the second syllable does not have an unlicensed melody segment.

2. R2 reduplication.

In this section I will give an analysis of R2 reduplication involving only one underlying morpheme. The alternative surface forms will be seen to result directly from the structure of the base.

I propose that the R2 template is [$\$ \ \$?$] for all bases. After the base is copied in full, both melody and prosody, this template maps over the copied prosodic structure. Other rules affecting prosody, in particular Weight by Position, then apply. This is analogous to the reassignment of stress and resyllabification that are assumed to occur universally after morphemes are concatenated. Finally, all extratemplatic elements in the copy are deleted.

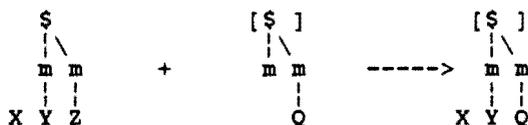
What does it mean for inserted prosodic structure (however it may be inserted) to "map over" the prosodic structure of another morpheme? Because I employ both the prosodic templates of McCarthy and Prince (1988) and the notion of Full Copy from Steriade (1988), I must face this question directly. Thus in (14) I give two formal mapping principles.

(14) Principles for mapping prosodic structure P_1 over prosodic structure P_2

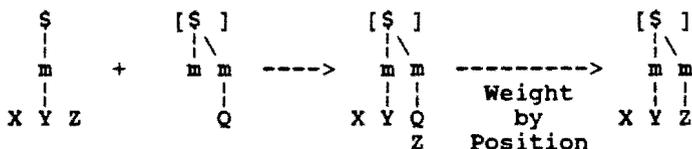
- A. Identical prosodic nodes N_1 and N_2 are conflated into a single node N that dominates the daughters of N_1 and N_2
- B. If N now dominates two melody segments, the daughter of N_1 is deleted

It seems reasonable to assume first of all that identical nodes in the two prosodic hierarchies are matched, resulting in conflation into one node which dominates the daughters of the original nodes. Secondly, if the daughters of a conflated node are not identical, in particular if a mora has been conflated with another so that the resulting single mora dominates two different melodic segments, the daughters of the node that is being mapped over are deleted; in other words, any segment linked to the "new" mora writes over any segments linked to the "old" mora. This is similar in some ways to the procedure of "melody overwriting" that McCarthy and Prince (1988) posit for the Arabic broken plurals. In (15) I provide two schematic examples.

(15) (a) Mapping over a base whose melody segments are all prosodically licensed



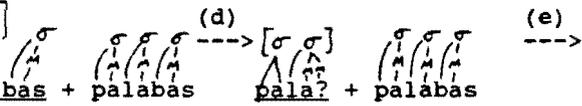
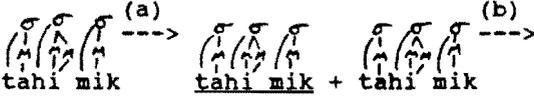
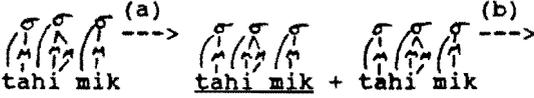
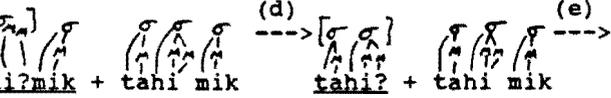
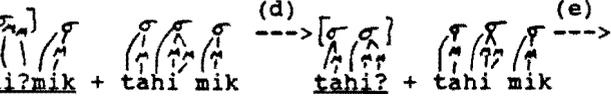
(b) Mapping over a base with an unlicensed melody segment



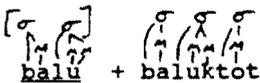
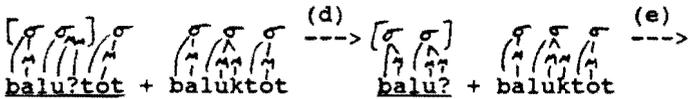
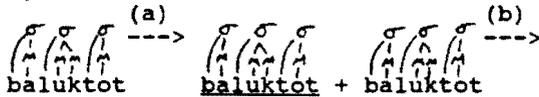
Thus deriving a reduplicated R2 form from a base involves the steps given in (16).

- (16) (a) Copy the entire base, prosody as well as melody
 (b) The template [σ σ?] maps over the copied prosody
 (c) If applicable, Weight by Position creates a new mora which maps over the old one created in step (b)
 (d) All extratemplatic copied material is deleted
 (e) ?-Deletion applies (with compensatory lengthening)

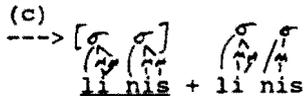
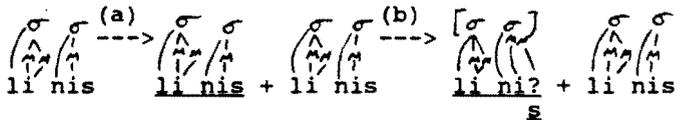
In order to test this model, we have to check four cases, namely cases where the second syllable of the base is: (i) open and monomoraic (as in palabas), (ii) open and bimoraic (as in tahi:mik), (iii) closed and bimoraic (as in baluktot) or (iv) closed and monomoraic (as in li:nis). In (17), these four words are worked through the steps given above, correctly deriving pala:-palabas, tahi:-tahi:mik, li:nis-li:nis and balu:-baluktot.

- (17) (i)
- (a)  $\xrightarrow{\text{(a)}}$  $\xrightarrow{\text{(b)}}$  $\xrightarrow{\text{(d)}}$  $\xrightarrow{\text{(e)}}$
- pala? + palabas pala? + palabas
- pala + palabas
- (ii)
- (a)  $\xrightarrow{\text{(a)}}$  $\xrightarrow{\text{(b)}}$  $\xrightarrow{\text{(d)}}$  $\xrightarrow{\text{(e)}}$
- tahi? + tahi mik tahi? + tahi mik
- tahi + tahi mik

(iii)



(iv)



3. Conclusion.

Such apparent allomorphy in reduplicative morphemes is not limited to Tagalog. McCarthy and Prince (1988) cite Dyirbal, Cebuano and Makassarese as other possible examples. It may well be that allomorphy is necessary to explain reduplication in these languages. I have tried to show, however, that a close examination of the syllable structure in Tagalog indicates that such an analysis for this language is not desirable and that in fact, an alternative analysis that does not rely on R2 allomorphs is possible.

NOTES

¹I'd like to thank Diana Archangeli, David Basilico, Mike Hammond, Rich Janda, and Jane Tsay for useful ideas and discussions. I am, of course, solely responsible for errors.

²In both templates the \$ node will be satisfied if filled with a syllable of any weight. This makes the prediction that the structure of the first syllable of nonminimal bases is transferred intact in R2, just as it is with minimal bases. That is, just as vowel length is preserved in the first syllable of li:nis -->

li:nis-li:nis and the final consonant is preserved in the first syllable of pantay --> pantay-pantay, the same should be true for hypothetical trisyllabic words like li:nisok --> li:ni:-li:nisok and pantasok --> panta:-pantasok. I have not been able to find any data with which I could either confirm or refute this prediction, however.

²Foreign borrowings are exceptional in many ways. For example, native stems are always stressed on one of the final two syllables, while foreign stems may be stressed on the antepenult: Amérika (Ramos, 1971). Moreover, in some borrowings stress appears on a nonfinal closed syllable: bénta (Spanish venta "sale"), gínko (Sp. cinco "five") (Ramos 1971). In others, stress is shifted from its place in the source word so as to conform to the usual Tagalog pattern: libró (Spanish libro "book"), martés (Spanish martes "Tuesday") (Schachter and Otones 1972). For still other exceptional characteristics involving intonation and vowel length, see Schachter and Otones (1972). The generalizations given in this paper are therefore meant to apply to native morphemes only.

³Words represented orthographically as vowel-final, such as "aso" (smoke), are actually pronounced with final h (thus, [ʔasoh]) (Schachter and Otones 1972). (Note that orthographically vowel-initial words always begin with a consonant as well.) Whether this h is inserted or underlying is irrelevant in the present discussion.

⁴Making this constraint any more formal would require rethinking the model of syllable structure I assume, adopted in toto from Hayes (1989) and McCarthy and Prince (1986, 1988). A vowel can't be identified as such solely on the melody tier, since words in Tagalog may end in a [-cons] segment: hintay (see (1) above). Unfortunately, however, in the model I'm assuming it can't be identified at the prosodic level, either, since, as we'll see, rhyme vowels and consonants in Tagalog must be assigned the same prosodic unit (a mora).

⁵Of course, since hindi? + ba becomes hindi:ba, as seen in (4) above, the ? must become moraic again via Weight by Position after the cliticization of ba.

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A FUNCTIONALIST ARGUMENT FOR THE AUTONOMY OF GRAMMAR
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 University of Washington

Two approaches to grammatical description coexist uneasily in the field of linguistics today. For want of better terms, they could be called the 'formalist' and the 'functionalist'. The former approach, whose foremost exponent is Noam Chomsky, is exemplified by the cluster of theories that come under the heading 'generative grammar'. The key concept of the formalist approach is the 'autonomy of linguistic form', the idea that central aspects of language can and should be characterized as a formal system whose primitive elements and governing principles are not derivable from or reducible to concepts outside that system.

The concept of autonomy of form has both a broad and a narrow interpretation. Narrowly, it is sometimes referred to as 'the autonomy of syntax'. In this view, syntactic patterning is not explicable on the basis of the meanings or discourse functions of the elements involved, nor is there held to be a one-to-one correlation between syntactic constructs and semantic constructs and/or discourse function. In other words, the syntax-semantics-discourse interface is seen as a complex one.

Autonomy in its broad sense refers to properties of grammar as a whole. In this view, grammar (i.e. syntax, phonology, morphology, and certain aspects of semantics) forms a well-defined system, which, while interacting with systems based in discourse, cognition, sociology, and so on, is not derivable from any of them.¹

These two senses of autonomy are logically independent. Thus one might plausibly accept the broad interpretation of autonomy, but reject the narrow, which I believe to be the position of Wierzbicka (1980). Conversely, one could logically espouse narrow autonomy, but reject broad autonomy, though I know of no one who has taken such a position.

Finally, most, but by no means all, formalists accept the idea that the central principles governing linguistic form are innate, and that these innate principles, known as 'universal grammar' (UG), help shape the acquisition of particular grammars.

The functionalist wing of the field, while quite diverse in many respects, shares the rejection of autonomy in both its forms. In its place, it advances the belief that grammatical patterning is grounded in what is seen as the most important 'function' of language, namely communication. Thus Tomlin (1989) dismisses autonomy in its narrow and broad forms respectively in the following two quotes, which seem

quite representative of mainstream functionalist thought:

Syntax is not autonomous from semantics or pragmatics...The rejection of autonomy derives from the observation that the use of particular grammatical forms is strongly linked, even deterministically linked, to the presence of particular semantic or pragmatic functions during the discourse. (1989:7)

[Functionalist approaches assume] that any linguistic system develops, both ontogenetically and phylogenetically, to achieve and facilitate communication. (1989:4)

In other words, in the functionalist view, grammatical patterning falls out as an automatic consequence of the optimal flow of discourse. There are no autonomous rules or principles of any depth; indeed, there is no aspect of form, except perhaps the most trivial, that cannot be derived from the exigencies of communication (perhaps in conjunction with demands placed by other faculties such as memory or physiology).²

It goes without saying, then, that functionalists reject the idea of innate, strictly linguistic, faculty.

The following example will serve to highlight the differences between the formalist and functionalist approaches. Emonds (1970, 1976) observed that many syntactic processes seem to apply only in main clauses (in his terminology 'root' clauses). The following examples illustrate this point with respect to the rules of 'topicalization' and 'directional adverb preposing' respectively:

- (1) a. These steps, I used to sweep with a broom.
b. *Are you aware that these steps, I used to sweep with a broom?
- (2) a. In came John.
b. *I noticed that in came John.

Emonds offered a purely structural (i.e. autonomous syntactic) explanation for this fact, based on the formal structures involved and the formal operations performed by the rules in question.

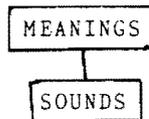
However, Hooper & Thompson (1973) proffered an alternative functionalist explanation for the same facts. First, they challenged Emonds' structural characterization of the domain of these rules, arguing that the correct generalization is that they apply in clauses whose discourse function is to make an

assertion. They then argued that this generalization follows as a consequence of the inappropriateness of emphasizing background or information-seeking material.

A major component of the functionalist program, then, is to replace the formal structure-based principles of autonomous grammar with those based in discourse-function.

There exists a strong metatheoretical argument in support of the functionalist position. It is a truism that the obligation of any theory of language is to relate sounds and meanings, as illustrated schematically in (3):³

(3)



Yet neither meanings nor sounds are language-specific. The ability to conceptualize meanings is surely independent of language, as is evidenced both by the concept-forming abilities of lower animals (see Premack 1983 for a discussion of primate abilities along these lines) and by the now widely-accepted fact that not all human thought is 'sub-linguistic'. Likewise, speech sounds can exist outside of language per se and, indeed, virtually every organ of the vocal tract serves some nonlinguistic function. Thus, it would seem that the burden is on the formalist to demonstrate that the mapping between meanings and sounds is mediated by strictly linguistic principles.

The purpose of this paper is to argue that the formalist and the functionalist approaches are not as incompatible as they may first appear to be. I will attempt to make the case that there is no incompatibility between formal and functional explanation in linguistics. In fact, I will argue that one can advocate both autonomy (in its narrow as well as in its broad sense) and innateness and still take a functionalist perspective on language.

Indeed, I will go even farther and argue that the stronger a functionalist one is, the more one should be led to support the idea of an autonomous grammatical system whose central principles are innate.

Let us begin by overviewing briefly the arguments for autonomy. The most compelling evidence in its favor derives from the many-many relation between form and function, whether semantic function or broader communicative function. We can illustrate this point with respect to a central construction of English, the inverted auxiliary. In this construction, the first auxiliary verb precedes the subject, as in does John

work, has Mary been studying, etc. Use of the inverted auxiliary signals many diverse semantic functions. For example, it can be used to signal a question, both of the 'yes-no' and the 'wh-' variety:

- (4) a. Have you been working late?
b. What have you been eating?

However, it is disallowed in embedded questions (cf. (5)-(6)) and in main clause questions if the subject itself is a wh-phrase (cf. (7)):

- (5) a. I wondered whether you had been working late.
b. *I wondered whether had you been working late.
(6) a. I wondered what you had been eating.
b. *I wondered what had you been eating.
(7) a. What has been bothering you?
b. *Has what been bothering you?

Furthermore, the construction occurs after preposed negative adverbs (8),⁴ but not after preposed positive adverbs (9); with bare subjunctives, but not with those introduced by if (10); and obligatorily after preposed so-clauses (11):

- (8) a. Under no circumstances will I take a day off.
b. *Under no circumstances I will take a day off.
(9) a. *Given any possibility will I take a day off.
b. Given any possibility I will take a day off.
(10) a. Had I known the dangers, I would have kept my distance.
b. *If had I known the dangers, I would have kept my distance.
(11) a. So tall is Mary, she can see into second story windows.
b. *So tall Mary is, she can see into second story windows.

The environments in which the inverted auxiliary construction occurs defy a uniform semantic characterization. Yet the formal differences between this construction and the 'normal' auxiliary-after-subject construction are trivially easy to state. Thus the facts surrounding this construction support autonomy in its narrow sense.

It seems clear that we learn that English has the option of fronting an auxiliary and we learn the contexts in which it is correct to do so. Presumably a functionalist would have to take the somewhat peculiar position that each time a new semantic function is learned for the construction, the construction itself must be learned de novo. For to say otherwise would be

to make a fundamental concession to the principle of the autonomy of linguistic form.

Furthermore, the syntactic principles involved are just special cases of more general ones even further removed from semantic function. For example, the fact that only one auxiliary can be fronted (as (12) illustrates) and the nonoccurrence of the construction in embedded clauses both follow from very general formal principles:⁵

(12) *Have been you working late?

The inverted-auxiliary construction supports autonomy in its broad sense as well, in that its use has a diversity of discourse effects. For example, the construction can convey a question (13a), a request (13b), an offer (13c), an exclamation of desire (13d), and a statement (13e):

- (13) a. Can you take Interstate 90 all the way to South Dakota?
 b. Could you pass the salt?
 c. Can I help you?
 d. Could I use a drink!
 e. Is linguistics easy!

Since all five types of speech acts represented in (13a-b) can also be carried out by means of other formal devices, we may conclude that the principles involved in characterizing constructions formally must be distinguished from those involved in determining the use of particular constructions in discourse.

Many spheres of language besides grammatical patterning per se illustrate autonomy. Take, for example, the conditions governing the use of roughly synonymous expressions in discourse, a subject about which, as the first Tomlin quote above illustrates, functionalists have strong opinions. To be specific, let us ask when it is normal to use the passive construction in discourse, to say (14b) instead of (14a):

- (14) a. Somebody broke the school window.
 b. The school window was broken.

A standard functionalist answer is that one would use a passive like (14b) to downplay the agent of the action if it is unknown or unimportant and at the same time to express the topichood (the 'givenness') of the nonagent. This in itself is normally related to maximizing the efficiency of the exchange of information in discourse.

However, Weiner and Labov (1983) carried out an extensive study of the use of the passive and found

something very different. While the functionalist answer is not wholly wrong, they found that the use of a passive is just as much a function of the use of a passive earlier in the discourse as of the factors mentioned in the preceding paragraph. In other words, the desire of speakers to maintain structural parallelism is an important motivating force in actual speech.

Autonomous syntactic principles are at work in language comprehension as well. Frazier (1988), for example, reports on several experiments that show that hearers access all readings of a structurally ambiguous sentence, even those that are implausible or absurd.

Finally, in first language acquisition, children typically place formal morphological or syntactic principles over semantic or discourse-based ones. This can be illustrated by reference to the errors that they make. Suppose that we take the functionalist viewpoint that structure is derivative, that is, that children first learn concepts and discourse strategies and then map these (in some fairly simple way) onto structures. If so, a prediction follows: children should at first use semantically atypical members of a syntactic category erroneously, treating them as if they were members of a category that more directly reflects their meaning or use.

For example, verbs typically denote actions (run, hit, throw), while adjectives typically denote states (red, fat, happy). But there is a minority of verbs that denote states (know, love) and a minority of adjectives that denote actions (naughty, silly). If the formal properties of language are derivative, we would predict initial errors to reflect the usual formal reflex of the semantic function of members of the class in question. That is, we would predict children's errors like the following:

- (15) a. *She naughtied.
 b. *He is sillying to them.
 c. *He is know it.
 d. *Was he love her?

However, as Maratsos and Chalkley (1980) point out, such errors virtually never occur. Instead, children make errors that suggest that they initially learn the formal category of the item in question. Thus, children seem to learn immediately that know, for example, is a verb and they therefore commonly produce errors such as *I knowed it.

In short, there is ample support for autonomy, both in its broad and narrow senses. We acquire grammatical structures and we learn how and when to put them to use.

This leads us to the question of innateness. As stressed above, autonomy does not logically entail an innate UG. Some autonomous linguists take a militantly anti-psychological stance (Katz 1981), while others remain agnostic on the question (Gazdar, Klein, Pullum & Sag 1985). There is even a heroic attempt to show how autonomous principles governing linguistic form might be learned inductively (O'Grady 1987).⁸

However, given the existence of syntactic principles as abstract as those that have been proposed and defended, it seems hard to avoid the conclusion that they are at least in part innately specified. The best arguments for innateness are those that appeal to the 'poverty of the stimulus' received by the child language learner. Given the paucity of direct evidence for these principles that is available to the child, the argument goes, it must be that the child is 'pre-wired' with them and thus literally has them available during the process of language acquisition. By way of illustration, let us take the following examples from Hoekstra and Kooij (1988):

- (16) a. Where did John say that we had to get off the bus?
 b. Where did John ask whether we had to get off the bus?

(16a) is ambiguous; where can refer either to the place of John's saying or to the place of getting off the bus. However, (16b) is unambiguous; where can refer only to the place of John's asking.

The UG principle of Subjacency explains these intuitions in a straightforward way. Yet given the abstractness of the principle combined with the relative rarity with which sentences like (16a) and (16b) are encountered by the child in contexts in which their possible meanings are transparently obvious, it seems hard to avoid the conclusion that once one has learned that the structures of sentences like (16) are possible in English, one knows automatically that where in (16b) cannot be associated with the subordinate clause. In other words, Subjacency is innate.

So far, I have defended the formalist approach to linguistics and mentioned the functionalist approach only to criticize it. The reader must therefore be wondering if and when the promised reconciliation between the two will surface. I will now take the first step towards this reconciliation, a step almost never taken by formal linguists. This involves posing the following two questions: 'Why is grammar autonomous?' and 'Why are its central principles innate?'

Now there may very well not be an interesting

answer to these questions. Evolutionary biologists are coming more and more to the conclusion that there exist an impressive number of inherited traits that have not been selected for; that is, just because a trait is innate, one cannot necessarily conclude that it contributed to the survival of the species. It appears to be commonly the case that the development of one trait by selection gratuitously carries along a host of others, 'hitch-hiking', if you will.

Chomsky (to appear) has suggested that much of UG has this property. In his view, the innate properties of language are simply emergent physical properties of a brain that has reached a certain level of complexity under the specific conditions of human evolution. And Piattelli-Palmarini (1989) devotes an entire article to arguing just this point.

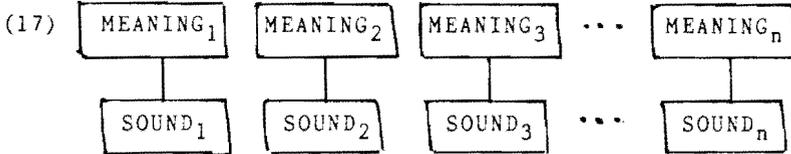
Indeed, Chomsky (to appear) goes even farther, arguing that some properties of UG are actually dysfunctional to the species. For example, he considers the 'Last Resort' principle, which insures that derivations be as economical as possible with no superfluous steps, to be dysfunctional because it causes computational difficulties. The parser would seem to have to scan globally all possible derivations before it comes across the right one. He concludes that while language might be 'beautiful', it is at the same time 'unusable', and must resort to a number of 'computational tricks' to allow structure to be recovered at all.

In one article, Chomsky (1976) casts aspersions on anyone even raising the question of the evolutionary origins of language, suggesting that it is no more or less interesting than those of any other organ, say, the heart. He implies that the age-old quest for an answer to this question must reflect religious motives, rather than scientific ones.

Chomsky and Piattelli-Palmarini may well be right, of course. It is entirely possible that the innate principles of UG arose by accident, as it were. But how much more interesting it would be if they did not do so, but instead developed to meet some need of the species. The remainder of this paper will be devoted to arguing that the latter alternative is the correct one; that innate autonomous grammatical principles were selected for because they had survival value. In short, one can deduce the functional need for formal linguistics.

Let us return to what we all agree is the task of any linguistic theory, namely, to relate sounds and meanings, as in (3). Since humans can conceptualize many thousands of distinct meanings and, with training, can produce and recognize thousands of distinct sounds, one's first thought might be that this relation would be

expressed by a simple pairing of individual sounds with individual meanings, as in (17):



At the domain of lexical meaning, no such one-to-one pairing exists, of course, and the reason for it is known to every student of introductory linguistics: It is vastly more efficient to pair sequences of a small number of distinctive sounds with meanings than to attempt a direct mapping between individual meanings and individual sounds.

But what about propositional meaning, where the question is rarely, if ever, raised? Since humans can formulate an indefinite number of propositions and produce an indefinite number of sound sequences, why shouldn't there be a one-to-one mapping between them?

The obvious answer, it seems to me, is that sound and meaning are too different from each other. Meanings, whatever their ultimate nature, are first and foremost mental realities, with no obvious physical instantiation. Sounds, physical realities par excellence, are produced by a coordinated set of articulations in the vocal tract, under control of a very different area of the brain from that responsible for meaning.

Furthermore, in the conceptual structures that represent meanings, temporality and linearity play no role. Such structures do, however, contain diverse types of hierarchies and structured relationships: predicate argument dependencies, and relations of inclusion, implication, cross-classification, and identity. Moreover, conceptual structures are discrete; in the representation of a sentence like the girl threw the ball, for example, girl, threw, and ball do not grade continuously into one another.

Phonetic representations, on the other hand, have almost none of these properties. A phonetic representation is temporal and quantitative. While partly hierarchical in nature, there is no relationship between the hierarchy of a phonetic representation and that of a conceptual structure. Indeed, the articulatory gestures, formant frequencies, tone patterns, and so on relevant to a phonetic representation have nothing in common with the properties of a conceptual structure. And this mismatch is alleviated only slightly if we talk about phonological instead of phonetic representations.

In other words, as language gradually took shape in

the early stages of human evolution, nature was faced with the dilemma of matching two seemingly unmatchable components. How could this be achieved? It is a truism that nature breaks complex operations into a series of linked smaller ones. The obvious solution then would be for there to arise an intermediate level between sound and meaning, a 'switchboard', if you will, coordinating the two.

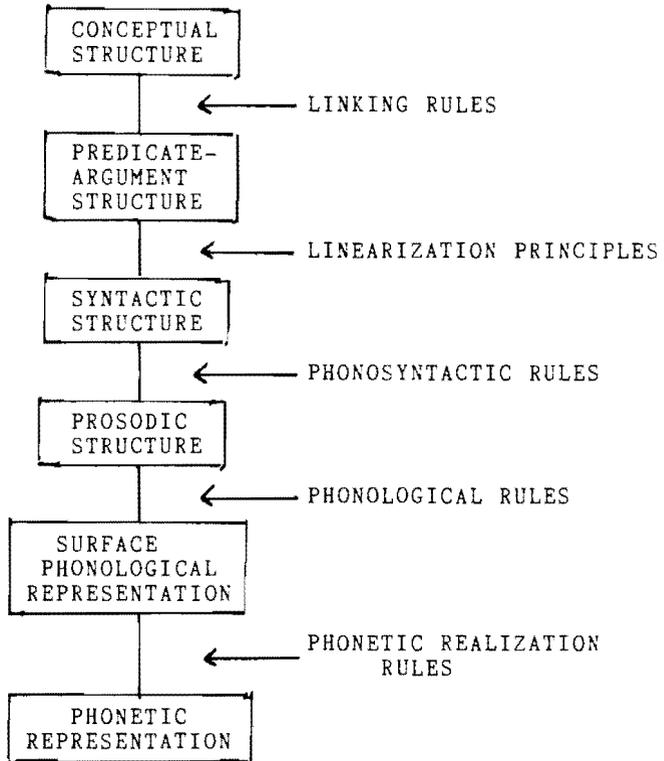
What properties might we deduce about this intermediate level? First, it would have to contain a small number of basic units. Nothing is to be gained if we have a third level with thousands of basic entities. And second, this level would need to share some properties with conceptual structures and some properties with phonetic representations, but be constructed out of units common to neither. It would defeat the purpose of this level if it were skewed too much either to the sound end or to the meaning end of the spectrum.

What we have just done, of course, is to deduce the functional need for autonomous syntax! This level contains a small number of basic units (no more than a couple dozen syntactic categories are postulated for any given language), which are related to each other by the simple notions of 'dominate' and 'precede'. In this way, a syntactic representation contrasts markedly with the complexity of a semantic or phonetic one. Furthermore, a syntactic representation shares some properties with the former (hierarchy, dependency) and some with the latter (linear sequencing), yet is governed by a calculus neither semantic nor phonetic.

Again, from the functional need to develop a workable system of communication (i.e. to pair sounds and meanings efficiently) autonomous syntax arose in the course of language evolution.

Let us look more closely at the mapping between conceptual structures and phonetic representations:

(18)



Each level is linked by a set of rules to the level above or below it, which carry a derivation a step closer to sound from meaning, or vice-versa, and each level is governed by its own autonomous principles of organization.

The linking rules take fully specified semantic structures as input and yield predicate-argument structures as output, in which the specific content of the thematic information of the former level is lost. Linearization principles (the Projection Principle, the Principle of Case Adjacency, directionality of theta-role assignment, and so on) transform predicate-argument structures into syntactic structures terminating in phonologically specified lexical items.⁸ The phonsyntactic rules are sensitive only to a subset of syntactic constituent structure, namely that provided by principles of X-bar theory, in building the phonological and intonational phrases that define the level of prosodic structure. All syntactic information is lost by the time of the application of the phonological rules, and in the phonetic realization rules, quantitative

information enters the derivation for the first time.

Thus, this autonomous systems view satisfies the functional need for small manageable operations linking the inherently disparate components of language.

It is worth raising the question why functionalists have so consistently missed the point that humans need autonomous syntax. Perhaps this has resulted from the fact that functionalists tend to be as narrowly specialized as formal grammarians. That is, one group of functionalists focuses on grammatical constructions and attempts to derive syntactic patterning from semantics and discourse. Another group focuses on phonology and attempts to provide phonological patterning with phonetic motivation. Yet few, if any, attempt to provide a functional explanation for the entire mapping between sound and meaning. Any who set their minds to this task, I believe, would come to the conclusion that autonomous syntax has a functional motivation.

Let us now look briefly at some of the organizing principles that help shape the level of syntactic structure and see if they too can be provided with functional motivation. In fact, they can. One set, including the principle of Subjacency and Principle A of the Binding Theory performs a locality function -- they keep related elements from being too far apart from each other. Another set, including the Empty Category Principle and Full Interpretation, performs an identification function -- they help keep track of hard-to-keep-track-of elements such as null elements and pronominals.

In other words, the principles of UG have the function of making syntax more accessible, of making it easier to keep track of what is what and what is where. But these are just the principles for which poverty of the stimulus arguments are the strongest, i.e. those most plausibly innate. In other words, if one is truly serious about taking a functionalist perspective on language, then one is led to become a nativist.

The idea that functionalism leads irrevocably to innateness should not be surprising. If language is as important to the species as every functionalist insists that it is -- if the ability to communicate is the paramount human attribute -- then anything that facilitates this ability would be expected to become biologized.

In sum, autonomous syntax and the major principles of UG are innate precisely because they are functionally so vital. The biologist S. E. Luria seemed to anticipate just this conclusion when he wrote:

Perfecting of these cerebral structures must have depended on their becoming progressively more useful in terms of reproductive success. For language, this must have meant becoming a better instrument in formulation and communication of meaning through a usable grammar and syntax. (1973:141)

To conclude, the formalist and the functionalist approaches to language are not as diametrically opposed as many believe them to be. Indeed, the functionalist perspective, thought through to its logical conclusion, leads to the realization that the fundamental tenet of formal linguistics -- that central to language there exists an innately-shaped autonomous grammar -- is correct.

FOOTNOTES

¹Formalists have typically exempted the semantic structure of lexical entries from the autonomy hypothesis. Chomsky (1965:159), for example, remarks that 'semantic systems [and] systems of knowledge and belief...seem to interpenetrate in obscure ways' and devotes a 1977 essay in large part to a discussion of the difficulty in constructing an autonomy hypothesis strong enough to encompass the lexicon.

²Virtually all functionalists agree that all languages contain a residue of properties that do not lend themselves readily to functional explanation. What is crucial to their position is that no autonomous principles of depth or interest govern the behavior of this residue.

³Perhaps it would be more appropriate to use the term 'expressions' rather than 'sounds', so as not to exclude signed languages.

⁴For a finer characterization of the occurrence of this construction after negative adverbs, see M. Liberman (1974) and Lakoff & Brugman (1987).

⁵The former property follows from the Structure Preserving Constraint (Emonds 1976) and the Head Movement Constraint (Travis 1984). Newmeyer (1987) argues that the latter property follows from the 'Barriers' framework of Chomsky (1986).

⁶Grady's arguments against autonomous syntactic categories depend, on my opinion, on an overly vague extension of the notion 'dependency'. While his alternatives to UG principles such as Subjacency are quite interesting, he gives no reason for why the child

should expect any restrictions to occur, and thereby effectively bypasses the question of innateness, rather than overturning it.

⁷The idea of syntax as a switchboard linking the mismatched components involved in language is mooted in Mattingly (1972), A. Liberman (1974), and Jackendoff (1989).

⁸I leave open the question the number of sub-levels of syntactic structure (i.e. D-Structure, S-Structure, Logical Form), since it is irrelevant to this discussion.

⁹One might wonder if positing a functional explanation for the origins of the major principles of UG undermines the autonomy of syntax. If the central aspects of grammar are (or were originally) functionally determined, then how can one say that it is 'autonomous'? However, autonomy is not undermined for several reasons. First, while syntactic principles may have had functional motivation, they are nevertheless not formulable in functional terms. Second, these principles interact with each other to define a structural system which as a whole performs a function. Third, since functions can be in conflict with each other, there is no way that any particular formal principle can be read directly off the function that it serves. And fourth, while these principles may have arisen to perform a function, form and function diverge over time, so there develops an increasingly remote relationship between a syntactic principle and the function that it arose to serve. For more discussion of these and related issues, see Newmeyer (in preparation).

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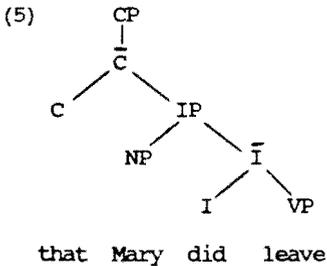
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I have been pursuing a notion that the problem exemplified in (1) is not due to the grammar directly but rather has to do with the parsing of subjects. In this paper I present experimental data from sentences with prepositional phrase subjects like the one in (4). I use these data to argue for a parser that is predictive (I explain in the next section what I mean by this), and I then suggest that the analysis of the data can be extended to give at least a partial account for the sentential subject phenomena.

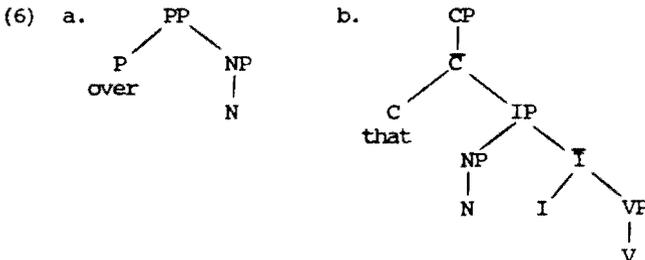
Like Koster I will assume that subjects are noun phrases, but unlike him I therefore allow that there are noun phrases without overt heads.² In these terms both prepositional phrase subjects and sentential subjects are noun phrases. This assumption makes for a clearer account of the following facts.³

From here on I will assume a sentence structure where INFL and COMP have maximal projections:

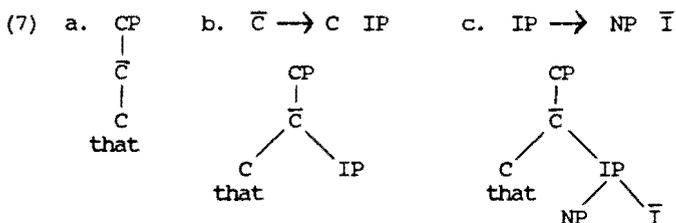


Predictive Parsing

By 'predictive' parsing I mean a form of what Kimball (1975) called 'over-the-top' parsing, where structure can be assigned top-down to input that has not yet been received.⁴ Suppose that upon receipt of the word 'over' the parser builds all of the structure in (6a), anticipating the complement noun phrase, or that upon receipt of a complementizer the parser builds all of the structure in (6b):



When would a parser do this? Since wrong predictions might be costly, requiring reanalysis of prebuilt structure, I will assume a conservative parser that predicts and prebuilds structure by expanding only obligatory nodes as they appear in rules that are accessed for parsing. A rule is accessed for parsing whenever the corresponding node is assigned to the parse tree, and the latter occurs by projection from the input. So suppose the parser receives the word 'that' and assumes it is a complementizer, as in (7). I assume, like many, that the parser builds the structure upwards that is given in (7a), projecting up to CP from the word of category C. But I also claim that the parser can then immediately access the \bar{C} rule, since \bar{C} has been assigned to the parse tree. The \bar{C} rule has the obligatory expansion shown in (7b)— every sentence must have an IP. Now the IP rule can be accessed and expanded, producing (7c), and so on.



Prediction is therefore the result of two parsing steps: Projection up to XP from a word of category X, and Expansion of obligatory nodes that appear in rules accessed after Projection.

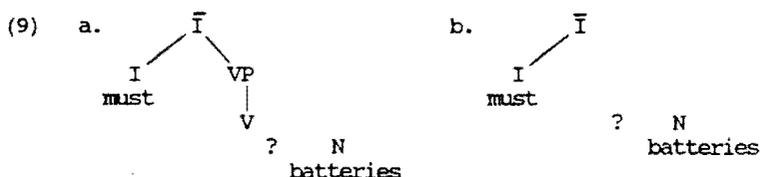
Notice that NP is obligatory in the IP rule given in (7c). The assumption made above that PP and sentential subjects are NP's actually follows from a broader assumption that all subjects are NP's, as instantiated in the IP rule. I will also assume that heads of phrases are generated (though sometimes remaining empty) and that (6b) (previous page) therefore represents the most extensive prediction possible given the input 'that'.⁵

It seems difficult in principle to show that prediction in this sense exists. Suppose we embed a lexical decision task within a context like the one in (8), presented word-by-word for reading. Subjects must decide as quickly as possible whether the highlighted word is indeed a word or not. Some subjects are presented with 'BATTERIES' and some with 'FORMULATE':

(8) If your bicycle is stolen, you must BATTERIES/FORMULATE

Subjects show longer reaction times in the case of 'BATTERIES'— see Wright and Garrett (1984). We might try to use this result to argue that, given the context, the parser prebuilds the structure shown in (9a). We would then say that

the parser's expectation of finding verb phrase material (the result of having prebuilt VP) influences lexical decision time. But we can imagine an explanation without a predictive parser. If the parser builds only the structure shown in (9b), we might still expect a slower reaction time for the word 'BATTERIES', since there is no way to incorporate this word into the tree grammatically, whether or not the parser anticipates a VP. Lexical decision in this account is influenced not by the parser's expectations but by its attempt to find some means to assign words to the parse tree.



In fact, appeals to predictive parsing are often based on data that can be interpreted equally well without prediction, since the facts are often as ambiguous as in the example here. But it would be interesting to know whether the parser can be predictive, since this would have implications about parsing efficiency— see Frazier and Fodor (1978) for discussion of that point.

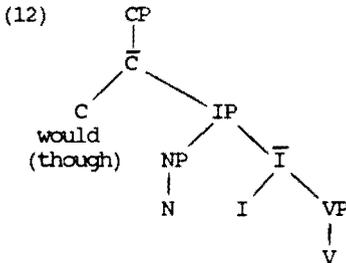
With this in mind consider the sentences in (10), with prepositional phrase subjects. I will from here on call these 'PP subjects', but bear in mind that they are NP's without overt heads in my account. In (10a) the PP-subject is initial. In (10b) it is inverted, and in (10c) it is embedded. I will be referring to 'initial', 'inverted' and 'embedded' subjects from here on.

- (10) a. Next to the shower would be a fine place to leave the shampoo
 b. Would next to the shower be a fine place to leave the shampoo?
 c. Though next to the shower seems like a fine place to leave the shampoo, Dad won't hear of it

Here are more examples of PP-subjects:

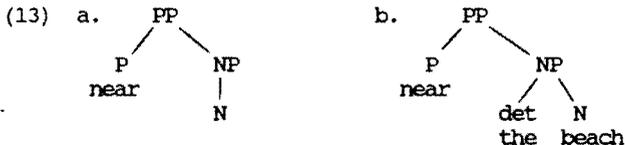
- (11) a. Near the beach has now become a commercial nightmare
 b. Under the bed could turn out to be a good place to hide
 c. Between the walls would seem like a perfect place to hide a body

What's interesting is that, given the account of predictive parsing here outlined, the inverted and embedded subjects are alike in that they will occur in sentences where the parser has predicted a subject noun phrase. If we assume that subject-AUX inversion raises material in INFL to the complementizer position, then (12) shows what the parser will predict and prebuild given a complementizer or initial AUX:⁶



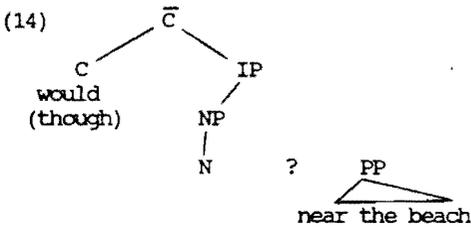
This prebuilt structure follows from Projection to CP and then Expansion of obligatory nodes.

In contrast, given an initial prepositional phrase, the parser builds only what is shown in (13). The structure in (13a) follows from Projection to PP and Expansion-- NP is an obligatory complement of P:



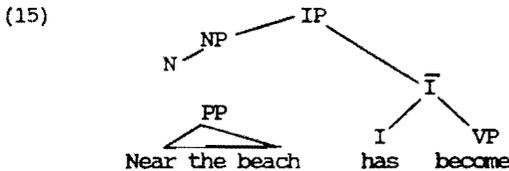
In my terms the parser cannot predict anything more at this point; there is no more Projection or Expansion of obligatory nodes available.

What would prediction of a subject NP mean in the case of inverted and embedded PP-subjects? Here I am only concerned with the amount of prebuilt structure shown in (14). Since PP subjects are grammatical, and since I am assuming they are NP's, we might expect no trouble incorporating the PP into this structure:



However, I would like to claim otherwise. Some parsing strategies favor certain structures over others (cf. the Minimal Attachment strategy in Frazier and Fodor (1978)). PP subjects are certainly non-canonical subjects (Chametzky (1985) discusses the restrictions on their use). It seems reasonable to suppose that some parsing strategy might make no allowance for them. In particular, suppose that prediction of a node XP by the parser entails a special confirmation strategy: The parser searches incoming lexical material for a head X or some other confirming item to incorporate under XP. Anything else encountered is held to the side until some confirming material is incorporated. In the case of a predicted NP the parser searches incoming material for a noun or perhaps a determiner, for instance. In (14) the parser will not encounter such material; meanwhile it must assemble the PP material and hold it to the side while conducting the search. Clearly there is a point at which the search would have to be abandoned, perhaps after a few words. The parser will simply incorporate the PP under NP. However, I am suggesting that in the special case of headless subjects the confirmation strategy could lead to observable parsing complexity.

When a PP subject occurs initially, on the other hand, as in (15), my hypothesis is that this strategy is never invoked.



Here we continue the parse in (13). The parser cannot build IP, and therefore the NP subject node, until the word 'has' is reached— giving IP by Projection and NP by Expansion of IP. Though NP follows from Expansion, this is not prediction in the sense above, where structure is assigned top-down to input that has not been received. There can be no 'confirming' strategy: The parser cannot search incoming material for a head noun, etc., since in effect there is no incoming material relevant to the NP. There is only what is on hold, namely PP. Given this

configuration, the parser incorporates the PP as subject without a delaying search.

Inverted and embedded PP subjects should cause more parsing difficulty than initial ones for two reasons, then: First, real prediction leads to a fruitless confirmation search; second, the incoming PP material must be assembled and held to the side during this search. With initial PP subjects, there is no search, and the PP has already been assigned structure at the relevant point.

This account, though tentative, does make predictions, like the following. Overt noun phrases (like 'the dog') should pose no problem at all in inverted or embedded position, since the parser would readily find a head noun or determiner to incorporate, ending the confirmation search. Therefore we expect a difference in parsing difficulty between inverted and embedded PP subjects on the one hand, and inverted and embedded NP subjects on the other (From here on I use 'NP subjects' for subjects with a head N). What's important, though, is that we do not predict a difference between initial NP and PP subjects, since initial PP subjects should avoid the difficulty of a foiled confirmation search. Therefore we expect a comparatively reduced difference in parsing difficulty between initial PP subjects and initial NP subjects.

The Experiments

I ran two experiments, both self-paced phrase-by-phrase reading tasks: subjects read sentences one phrase at a time as the phrases appeared on screen; the presentation was by micro-computer. The subjects controlled the appearance of each phrase by pressing a key. Each subject was instructed to read a phrase as quickly as possible while comprehending it, and then bring up the next phrase. Reading times were recorded for each phrase. Follow-up questions checked comprehension of each sentence, and reading times for a sentence were not taken into account if the question was not answered correctly.⁷

In Experiment I, 36 subjects were tested, and there were four conditions. Two conditions involved PP subjects, examples of which appear in (16), and the other two were identical except that a determiner and head noun were overt, as in (17). For both PP and NP subjects there were initial and inverted conditions, as shown. No subject saw more than one of the four versions of any sentence type.

(16) PP-subjects:

Initial /Near the beach has/now become/a commercial nightmare./
 Inverted /Has near the beach/now become/a commercial nightmare?/

(17) NP-subjects:

Initial /The area near the beach has/now become/a commercial
nightmare./
Inverted /Has the area near the beach,now become/a commercial
nightmare?/

The phrases appeared to the subjects as they are segmented above. In each condition the prepositional phrase is contained in the first segment. The second segment contains the main verb. Below I give the mean reading times in milliseconds for the first and second segments, for all four conditions. As it turns out, the important effects occurred within the first segment, the segment containing the PP, and so these reading times are highlighted.

Experiment I Results

Segment

1st 2nd (main verb)

PP-subjects

Initial	1693	859
Inverted	2333	797

NP-subjects

Initial	1937	752
Inverted	2094	747

Look first at the NP subject reading times in the first segment. These are the overt NP's. The inverted cases were read an average of 157 milliseconds more slowly than the initial cases (2094 vs. 1937 msec). In contrast, for the PP subjects, the inverted cases were read 640 milliseconds more slowly (2333 vs. 1693 msec). An analysis of variance revealed that the interaction between position (initial or embedded) and apparent category type, that is, whether the subject looks like an NP or a plain PP, was highly significant across subjects [$F(1,35)=23.96$, $P<.001$] and across items [$F(1,23)=12.17$, $P=.002$]. As for the second segment, we find only a main effect: these verb segments were read more slowly after PP subjects [$F(1,35)=5.29$, $P=.03$ and $F(1,23)=5.62$, $P=.03$]. There was no interaction with position of the subject.

Experiment II was just like the first, except that there were 48 subjects tested, and this time I tested initial versus embedded position, as shown in examples (18) and (19). There were two types of embedded condition, one embedding under

'though' and one embedding under 'that':

(18) PP-subjects:

/Between the walls/seems like/a perfect place/to hide a body./
 /Though between the walls/seems like/a perfect place/to hide
 a body/we know there isn't one there./
 /That between the walls/seems like/a perfect place/to hide a
 body/explains why a murderer might try it./

(19) NP-subjects:

/The space between the walls/seems like/a perfect place/to hide
 a body./
 /Though the space between the walls/seems like/a perfect place/
 to hide a body/we know there isn't one there./
 /That the space between the walls/seems like/a perfect place/to
 hide a body/explains why a murderer might try it./

Again the first segment is where the PP occurs, and again it is only within this segment that I found the relevant effect.

Experiment II Results

Segment

	1st	2nd (main verb)
PP-subjects		
Initial	959	888
Though	1458	796
That	1694	795
NP-subjects		
Initial	1448	732
Though	1778	730
That	1931	728

Once more there was an interaction between apparent category type, that is, whether the subject is on the surface an NP or a plain PP, and position within the sentence, that is, initial versus embedded: The difference due to position was greater for PP subjects. The interaction was significant across subjects in the case of embedding under 'though' [$F(1,47)=3.89$, $P=.05$ and $F(1,23)=1.65$, $P=.21$]. It was significant across both subjects and items in the case of embedding under 'that' [$F(1,47)=6.47$, $P=.01$ and $F(1,23)=5.61$, $P=.03$]. Although we should be cautious about across-experiment comparisons, I note that overall the

interactions found in Experiment II were weaker than that found in Experiment I; that is, PP subjects are harder to parse when inverted than when embedded. I have no explanation for this difference.

Again no interaction between position and apparent category was found in the verb segment reading times.

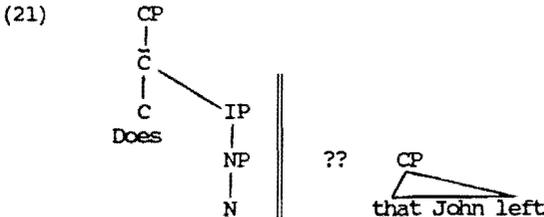
I interpret these results as support for my account based on predictive parsing. Notice that it is not sufficient to suggest that phrases without overt heads cause some parsing difficulty, though this is probably true (witness the main effect reported in Experiment I). Nor is it sufficient to suggest that inverted and embedded subjects are harder to parse than initial ones. We must here account for the interaction of the two factors: there is clearly some parsing complexity associated with PP subjects when inverted or embedded that disappears when they are initial, taking overt-NP subjects as the point of comparison. The appeal of my account based on predictive parsing is that it explains this interaction. I therefore take these data as evidence that the parser is predictive in the way I have outlined.

Sentential Subjects

The facts of interest about sentential subjects are repeated in (20):

- (20) a. That John left bothers me
 b. ?*Does that John left bother you?
 c. ??Although that John left bothers you, you should try to appear calm

What's interesting here is that, given the claims above, sentential subjects should behave just as PP subjects do. I depict in (21) the expected scenario of an inverted sentential subject. The parser predicts and prebuilds a subject NP and therefore searches incoming words for confirming NP material. But there is no such material; time given to the search while assembling the CP material and holding it leads to parsing difficulty:



Therefore it is encouraging that the sentential subject data parallel the PP subject parsing data as much as they do. As with PP subjects, we find that sentential subjects are worse when inverted or embedded. What's more, most people find inverted sentential subjects worse than embedded ones, and this again parallels the effects we found for PP subjects, although I have not explained this difference. Here too I am relying on a difference noted across experiments for the PP subjects (albeit a large one).

Whether my account based on predictive parsing is right or not, I believe that the parallel between PP subjects and sentential subjects strongly suggests that the problem with inverted and embedded sentential subjects has its basis in the domain of parsing rather than in the grammar as most have claimed. Notice that inverted and embedded PP subjects are normally considered grammatical; there can be no question of an account for them based on grammatical constraints. We might welcome the conclusion that inverted and embedded sentential subjects are a problem for the parser as well, since a principled account from the grammar in fact remains to be found.

There is a clear difference, though, between PP subjects and sentential subjects that I have not accounted for. Most, though not all, people judge embedded and especially inverted sentential subjects unacceptable, while the same is not true of the PP subjects. Why is this the case? I cannot argue for any one answer to this question here, though I offer one possibility: parsing difficulty of a high enough degree will lead to judgements of unacceptability. Sentential subjects cause more difficulty for the parser than do PP subjects, because, besides all that has been said here, they typically require the processing of more non-terminal nodes for a given span of words (see Frazier (1985), who argues that the number of non-terminal nodes being processed over a given number of words can be taken as a measure of complexity). Thus, unlike PP's, they surpass that level of difficulty beyond which speakers find a construction unacceptable.⁸

To conclude, I have argued that the human language parser is truly predictive, at least in some instances; and I have argued, independently, I think, that the facts discussed here about sentential subjects are best explained as a parsing phenomenon.

Notes

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1. Stowell (1981) also offers a principle-based account of the facts in (1)-- one that requires that sentential subjects appear in topic position at S-Str. His account, though, predicts that PP subjects as in (4) should not exist and therefore requires a stipulation on their behalf. In this paper I try rather to relate the facts of prepositional phrase subjects and sentential subjects.

2. But I will assume for consistency in what follows that a non-overt head is present: $[-NP [N] PP]$.

3. Though I believe a similar account can be constructed making the opposite assumption, i.e. that subjects can be non-NP's. One argument that sentential subjects are NP's comes from Pesetsky (1982), who shows they need CASE. The same can be shown for PP subjects:

- a. I believe [in the barn] to be the best place to sing
- b. *It was believed [in the barn] to be the best place to sing

No full argument for this view can be made here.

4. Note that Kimball used the term 'predictive' to mean something much more limited than 'over-the-top', namely not strictly bottom-up. His 'predictive' parser allowed the parser to posit a node before it was completed, positing S, for example, given only an initial NP.

5. I ignore possible mid-level categories under NP, like \bar{N} .

6. In a theory without maximal expansions for INFL and COMP, we would need to assume additionally that the parser can project up to \bar{S} given a complementizer 'that', and up to S given an initial AUX.

7. Further questions about experiment methodology can be directed to the author at the address given.

8. Some might object that lasting unacceptability must be ungrammaticality. A sentence like 'The horse raced past the barn fell', for example, is perceived as acceptable once the initial difficulty is overcome. It is not clear that this behavior should be a diagnostic for a parsing problem. Elsewhere we have seen subadjacency effects derived from a parsing model (Berwick and Weinberg (1984)). However, lasting unacceptability might indicate that the parsing difficulty has been grammaticized, and we might then look for a theory of when such grammaticization comes about. This route seems preferable to a stipulative account for sentential subjects from the grammar directly.

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Old English *man/mon*:
The historiography of an Anglo-Frisian(?) Sound Change

Herbert Penzl and Daniel Brink

(1) LINGUISTIC HISTORIOGRAPHY. There is no scholarly handbook dealing with Old English or the history of the English language that does not mention the sound-change we will discuss here. Its outline is usually given in the handbooks as: raising (and rounding) of West Germanic */a/ before nasals in OE dialects, and loss of this raising (and rounding) by Middle English times (except for a West Midland area where it was preserved until modern times). This summary, of course, can only be accepted as a mere outline of the historical development. Any full description and explanation of this sound change would have to cover the following aspects, as would be true for all historical events: time (date, chronology), location, and causal factors. For any sound change, the phonetic and phonemic description of initial and final – and possibly intermediate – stages, including nondistinctive variation, has to be attempted. Other sound-changes connected to the described one, the impact of the change on the total system, and the type of change have to be analyzed as well (cf. Penzl 1972: 81-91; Penzl 1975: 19-22).

Linguistic historiography means, in the first place, the description of the facts as recoverable from the data. The available scholarly methods of analysis of these data do not, in our opinion, include one leading to what one could call a pragmatic reconstruction of the speakers themselves, or of their language acts. Historiographically, it is a fallacy to think this possible; therefore, to write texts in an unattested protolanguage, whether it be Indo-European, Proto-Germanic, or "Pre-OE" or Anglo-Frisian, may be a stimulating intellectual or learned exercise, but has no place in historiography. Never should we omit for any reconstructed form, pattern, or phoneme the asterisk which means "estimated." Our linguistic reconstructions cannot be "reifying" or pragmatic.

If we describe *rules* or *laws* (*Lautgesetze*) for historical sound-changes, we should never forget to indicate that our use of these descriptive terms is somewhat metaphorical. A second fallacy in historiography, therefore, is an over-reliance on the accuracy of a description of historical events as rule- or "law"-determined facts. We consider as two additional fallacies, particularly in historical phonology, the assumption of uniform divergence (or convergence or invariance) among any body of speakers, and usually the assumption of graphic/phonetic (or even phonemic) uniqueness or bi-uniqueness. Can we e.g. really assume that all the speakers within any given area simultaneously shifted their [a] to [æ] in Pre-OE, or that in mss. <a> always meant [a] or [a] and <o> always meant [ɔ] or [o] (see sections (4) and (5), below)?

(2) TIME AND PLACE OF THE *man/mon* SOUND CHANGE. Statements in some handbooks indicate the frequency of occurrence of the *man* and *mon* spellings in the manuscripts of OE dialectal texts. In the Epinal glossary (early 8th century?) the *man*-type is more frequent than the *mon*-type; in the Corpus glossary (750?) it is the other way around; the Erfurt and Leiden glossaries (9th century) show both types with equal frequency. Anglian – that is, Mercian (e.g. the Vespasian Psalter, the Rushworth gloss [=R¹])¹ and Northumbrian (e.g. the Durham Book, the Rushworth ms [=R²])² – sources show almost exclusively the *mon*-type. This is also true for 9th century West Saxon; a Korean scholar, Suk-San Kim, a student of Michigan's Sherman Kuhn, has taken the trouble to count the occurrences of the spellings of the word *man* in the Hatton ms. of King Ælfred's *Cura Pastoralis*: 287 times *mon* (27 *monn*), but only 18 *man* (4 *mann*) (Kim 1977; see also section (5), below). In the 10th century, however, particularly in West Saxon and Kentish texts, *man* outnumbered *mon*, and Ælfric's writings in West Saxon scriptoria show almost exclusively *man*, which continued into ME and ModE times. Thereafter, the graphic variation observable for more than 200 years ceased – with the one exception of the area of the West Midland.³

When did the phonemic split suggested by the graphic variation begin? The oldest OE text, the glossaries of the 8th century, show it already, so the handbooks usually assume it for Pre-Old English, and some have assigned it to an alleged "Anglo-Frisian" period, based on the correspondences with <o> before nasals in 13th century "Old" Frisian: *mon, lond*.⁴ 13th century Old Frisian also shows, like Old English, fronting of */a/ in other positions, as in *dei* (cf. OE *dæg*, pl. *dagas*).

Does the time difference between the OE (8th century) and OFris. (13th century) data make it impossible to use these isoglosses historiographically? Types of speaker contact cannot, of course, be reconstructed exclusively on the basis of linguistic evidence. The concept of Anglo-Frisian has for this reason been, in general, largely abandoned, together with "Urdeutsch," the other alleged subdivision of West Germanic (see Wrede 1924). But common Anglo-Frisian, or "Ingvaenic" features exist beside the development of *a.⁵ In fact, Wrede's rejected theory aside,⁶ there are a number of rather significant features shared between Old English and dialects of the European North Sea coast, morphological as well as phonological ones. Some of the shared features relevant to the sound change under discussion include the above-mentioned fronting of *a*, the raising of lax mid vowels before *m* (OE *niman*, OFris. *nima* 'take'; OE *cuman*, OFris. *cuma* 'come'), and the rounding of *a* in cases of compensatory lengthening resulting from the loss of nasal before all voiceless fricatives (OE, OFris. *gos* 'goose' [Germ. *Gans*]; OE *oðer*, OFris. *other* 'other' [Germ. *ander*]).⁷ On the issue of whether Ingvaenic correspondences are early or late, see Brink (1983).

(3) CAUSALITY. The factors causing an historical event have often led to what seems to amount to acceptance of the fallacy of viewing historical events as rule- or law-determined. Causality can only be relative, not absolute in matters linguistic, since no laws of nature are involved.

The apparent Pre-OE change to a raised (and rounded) vowel [o] before nasals and nasal clusters (**man* to **mōn*) seems to indicate a raising influence of nasals typical of "Ingvaenic," contrasting with lowering effect of nasals among the sound changes frequently observed elsewhere (Ferguson, *et al.* 1975). A general shift of /a/ to [+front] in monosyllabic words, as in *dæg, sæt, wæs* cannot be phonetically motivated, but [a] in the plural, as in *dagas, fatu*, gen. *fata* 'vats' (vs. *saet* 'vat'), shows assimilating anticipation of a lower value [+low] before back vowels. We can point to the fact that such changes are typical of the Germanic languages: except for the isolated Gothic spoken in the 4th century in a small *Sprachinsel* in the Balkans, all umlaut changes lead to a type of vowel harmony based on the vowels of less accented syllables and affecting stem vowels. Thus, phonetic factors such as ease of articulation can hardly explain the original backing and raising of the *[a] before nasals.

Can we isolate phonemic factors, characterizing the structure of the entire vocalic system – perhaps a tendency toward greater symmetry assuring greater ease of communication – to account for our "sound change"? Not for the inception of the change, but for the final merger, an argument in favor of a teleological interpretation might be feasible, but in order not to seem to accept the fallacy of rule-determined historical events, no causality should be claimed. An intermediate short lax vowel between [a] and [o] before nasals never had a long or tense vowel [o:] as a counterpart (Krupatkin 1975: 55): long West Germanic **ā* before a nasal or only nasalized after its loss before voiceless fricatives (*brōhte* 'brought', *bōhte* 'thought', *zōs* 'goose', *iōð* 'tooth'), merged with long *ō* in pre-OE times⁸: *bōc, zōd, fōr* (Sievers-Brunner 1951: §§ 69, 80), *mōna* 'moon', *nōmon* 'took' [Germ. *nahmen*], *sōna* 'soon', *spōn* [Germ. *Span*], *zēdōn* 'done'.⁹

The short vowel /a/ in *dagas, fatu, nacod* 'naked', *assa* 'ass' merged with the /a/ of *man* in later OE: /a:/ in *mæz*, pl. *māzas* 'relative' shows a distribution like that of *dæg*, pl. *dagas*. However, OE /a:/ reflects also West Gmc. **ai*: OE *stān* [Germ. *Stein*], *rād* 'rode', *zāst* 'spirit' [Germ. *Geist*], *bān* 'bone', *hāl* 'whole', *sāwol* 'soul' and is thus amply represented (Campbell 1959: § 134).

(4) THE INITIAL STAGE OF THE SOUND CHANGE. The handbooks agree on the whole that West Gc. */a/, a vowel considered *[+low] * [+back] * [-round] before our earliest texts, suffered what Luick (1921: § 115) called an *Aufhellung* ('brightening') to *[æ] (*dæz*) – a change to

*[-back]¹⁰ – and a *Verdumpfung* ('darkening') before nasals to *[ɔ], a change to * [+round] (*lond, mon*). Some handbooks explain the *a* in *dazas, fatu* as due to the "restoration of [a]" with its West Gc. value after the original *Aufhellung*.¹¹ No evidence for this sequence of the two sound changes is ever offered. The assumption that practically all speakers of OE dialects first changed to **dazas, *fætu*, and then changed back to *dazas, fatu*, is the fallacy of uniform divergence and convergence mentioned above at the end of section (1). (Uniform divergence (of **a* to *æ*) followed by uniform but positionally limited convergence (back to *a*) may not be, strictly speaking, falsifiable, because to practice pragmatic reconstruction of the behavior of speakers of the past is based on a fallacy as well.)

(5) INTERMEDIATE STAGES? In any case, the graphic variation between the *man/mon* types during over two centuries in the texts of the OE dialects requires an explanation. It seems that most of our handbooks have escaped the aforementioned tempting fallacy of graphic/phonetic uniqueness, i.e., of assuming that the grapheme <a> of the Latin alphabet always has the unique value [a] and that the grapheme <o> always has the unique value [ɔ].¹² Overall statistics of occurrences in the texts (see section (2), above) show an increasing use of *mon* from the 8th to the 9th century, and then in the 10th century a gradual return to the *man* type in most OE dialects.

On the whole, the type of graphic variation encountered shows no differentiation in distribution among different parts of the vocabulary or before simple nasals vs. nasal clusters.¹³ E. Sievers' attempt to explain the distribution within the framework of his *Schallanalyse* ('Sound analysis'),¹⁴ on the basis of differences of intonation, is based on the fallacy of assuming the feasibility of modern scholarly reproduction of ancient language acts which we above in section (1) called the fallacy of pragmatic reconstruction. No proof has ever been submitted for historical times in the Indo-European languages that suprasegmental features have anywhere ever been systematically reflected by segmental symbols.

Are we dealing in the OE case of <æ>, <a>, <o> from former */a/ with three positional allophones of one phoneme or with three distinctive phonemes with hardly any quotable minimal pairs? The answers in the handbooks have varied. B. Strang, for example, voted for one phoneme (1970: 286). The choice of three separate symbols in the Latin and the innovating differentiation in the Runic alphabet (𐌶 for [æ], 𐌷 for [a], 𐌸 for [ɔ], Campbell 1959: § 67) support a threefold distinction. <æ> and <a> show no purely graphic variation and the development of /æ/ to /e/ (e.g. *set*) in the Kentish and the Mercian dialect of the Vespasian Psalter point to a clear phonemic distinction. The graphic <a/o> variation in *man/mon* cannot, within the same dialect, reflect two types of coexisting pronunciation, one with the /a/ of *dazas* or of Anglian *all, art* and one with the /o/ of *horn, zod, axa, word*.

The older handbooks are inclined to interpret the variation as a result of the scribe's dilemma: two alphabetic signs for three short vowels /a/, /o/, and the intermediate /ɔ/. The vowel of *mon* did not, except in a small area, eventually coalesce with that of *zod, word*. The 9th century West Saxon *mon* spellings indicate a merger with /o/, the 10th century West Saxon *man* spellings a merger with /a/. But this interpretation may be the aforementioned fallacy of graphic/phonetic uniqueness. The data from the West Saxon Hatton ms. (Kim 1977) reveal the paradox that the scribes use 287 (+ 27) times the word *mon* with *o* and only 18 (+ 4) times *man*: they followed in the high frequency word the established graphic (Anglian) norm, perhaps also because of its common occurrence in names – e.g. *Caedmon* – but revealed in 18 occasional spellings their own pronunciation.

(6) CONCLUDING REMARKS. We have used a comparatively simple OE sound change to discuss the problems of exact historiography and the aspects to be covered by an adequate description and explanation of linguistic historical events without resorting to hocus-pocus. The available methods leave some questions unanswered and perhaps unanswerable. We consider pragmatic or reifying reconstruction as well as the description of history as essentially rule-determined as based on fallacies. Contacts of the past (Anglo-Frisian) as an explanation are external language history and are as such hardly recoverable from internal language data alone. With the proven dialectal variation an assumption of uniform divergence and convergence would be accepting

another fallacy. It is a paradox that the very frequency of pertinent graphic data in this case seems to have increased the difficulty of interpretation. Of course, it has always been recognized but often conveniently ignored that exact phonetic identification of sound-values of the past is an almost utopian task, and if primarily based on graphic data can only represent another fallacy. Ambiguity of graphic data can, unfortunately, cause uncertainty even for phonemic grouping.

NOTES

1. Sievers-Brunner (1951: 5, Anm. 4). Words with *a before nasal and <a/o> in OE include e.g. *lamb* 'lamb', *hand* 'hand', *land* 'land', *gangan* 'go', *camb* 'comb', *hana* 'rooster', *nama* 'name'.
2. Sievers-Brunner (1951: 4).
3. Old West Frisian shows the same resistance to forms in <o> that West Saxon does. See Ramat (1976: 72), Nielsen (1985: § III, ix, 23).
4. For a review of the literature, leading to the conclusion that velar raising and rounding is *not* a common development in English and Frisian, see Nielsen (1985: § III, i, 56; § III, ix, 23).
5. Luick (1921: § 114) characterizes this change as "eine anglofriesische Eigentümlichkeit: . . . seine Anfänge mindestens müssen in die Zeit fallen, als die beiden Stämme noch unmittelbare Nachbarn auf dem Kontinente waren." See also, Campbell (1959: § 131): *a > æ*.
6. Wrede (1924) did more harm than good for the development of a general appreciation of the concept of Ingvaeonic by suggesting that this dialect was the main Germanic language on the prehistoric European continent; the general rejection of his theory led for a time to the dismissal of Ingvaeonic in general.
7. Other Ingvaeonic features include the general loss of nasal before voiceless fricatives, palatalization (affrication) of velars before front vowels, 'r-less' pronouns, a uniform personal ending on the verb for all three persons, a proclivity for -s plurals for nouns, etc. See Nielsen (1985: *passim*), Markey (1976: *passim*), Hutterer (1975: § IV, 3, 3), Brunner (1960: 75-81).
8. But Luick (1921: § 111) states: "Sie [= die Länge] war im 7. Jh. noch von dem sonstigen δ [aus urgermanischem δ]. . . verschieden, also noch ein δ -Laut."
9. An interesting attempt to explain this rounding as part of a phonological "conspiracy" to simplify the number of possible short vowels before nasals is given in Lass (1975: 70-73).
10. Sievers-Brunner (1951: § 49) describes details of the *æ/a* alternations.
11. Luick (1921: § 161): Urengl. *æ* to *a*. See also footnote 10 above. For the *æ/a* variation, see Penzl (1958), Brunner (1960: 76), Luick (1921: § 163).
12. But Luick (1921: § 110): "Nachher aber nahm die Verdampfung wieder ab, etwas rascher im Westsächsischen, langsamer in den englischen Dialekten. . . . Immerhin ist bald das eine, bald das andere Zeichen vorwiegend und zeigt, welchem der beiden Grenzwerte der Laut näher stand." Is this partly the fallacy of graphic/phonemic uniqueness? See footnote 13.
13. Toon (1983) devotes an entire chapter (chapter 3: "Politics and Language Change") to a discussion of our sound change, arguing that its pattern of diffusion and disappearance was only the result of the rise and fall of Mercian dialect influence in other parts of England. The change itself is taken completely at face value. It must be added that the spellings never indicate possible lengthenings of the vowels before nasal clusters like *nd*, *ng*, *mb*, etc. (Luick 1921: § 268). Unstressed position in sentences seems to have favored the *mən*-type: *on* (prep.), *bonne* (adv.), *bone* (acc.), etc. (Luick 1921: § 112.1).
14. Sievers-Brunner (1951: § 79, Anm. 1): ". . . im Fallton zu *o*, Steigton zu *a*. Mundartlich scheint dann ein Ausgleich stattgefunden zu haben."

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PROSPECTS FOR GENERATIVE GRAMMAR IN THE 1990s*

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1. Introduction

About eight years ago, early in the 1980s, I began to reflect on the then current directions that were being taken by those parts of the field of linguistics that I felt I knew. My assignment then was to write the first in what was to be a seven-year series of opinion columns under the TOPIC... COMMENT banner in a new journal, *Natural Language and Linguistic Theory*.

My predictive successes were not negligible, but they were somewhat outweighed by my glaring failures, the developments I didn't see coming at all. I foresaw a consolidation of radical lexicalist approaches, and I guess the relatively harmonious relations between GPSG, LFG, and other unification-based versions of syntactic theory attests to the fact that consolidation took place. But I did not foresee the amazing battle of the initials that Generalized Phrase Structure Grammar (GPSG) gave rise to: HPSG (Head-driven Phrase Structure Grammar), IPSPG (not currently in use, but it could be, for the Information-based Phrase Structure Grammar heralded in Pollard and Sag 1987), JPSG (Japanese Phrase Structure Grammar, developed by Takao Gunji in Japan in the middle 1980s), KPSG (Korean Phrase Structure Grammar, developed by a team in Korea some time later), LPSG (Linear Phrase Structure Grammar, also developed in Korea, by about 1988), and heaven knows how much further we may have moved toward the ultimate ZPSG theory by now.

I predicted a rapid spread of the government-binding (GB) theory, which was about as difficult as predicting continued movement of the earth around the sun. But I did not foresee the utterly extraordinary proliferation of phrase types and the reconstruction of features as abstract morphemes with X-bar projections that now adorn GB-style tree structures (more on this later).

I predicted increasing consensus about relatively surfacy syntax; the appearance at long last of a large number of relational grammar (RG) works; the failure of RG to become a mainstream paradigm for syntax; the cannibalization of RG work by GB researchers; the return of highly abstract phonology and the demise of 'natural phonology'; and the continued rise of interest in learnability issues. But there were many other significant developments that I did not see coming down the pike at all — for example, the return of so many classical generative semantic ideas in GB guise, the revival (after thirty years of neglect) of categorial grammar, the fact that RG would still be in reasonably good health by 1989, and in phonology, the rapid pace of tier inflation leading to phonological geometries that make fractals look positively unimaginative.

I have very little chance of doing much better as we stand on the threshold of the last decade of the twentieth century. But having been invited to address a conference on the theme of linguistics on the verge of the 1990s, I feel it is incumbent upon me to try. Like any other scientist attempting to make predictions, I will try to work by relying on standard historical and scientific principles — the principle of induction, which says that the future will be much like the past, and more specifically, the principle of uniformitarianism from

geology, which says that the present and future have causes of the same sort, i. e. that the processes that worked in the past to produce the record that we see in the present are working now to shape the future. Naturally, I shall do some browsing of past events and present trends before presenting any conjectures about the future.

2. Generative grammar in the decade 1979–89

Let me begin by pointing out an unwelcome truth that many will wish to resist: the actual achievements of generative grammar so far are remarkably slender. If one looks, for example, for known, uncontroversial, well-formulated, precise, universal principles, one finds there are virtually none.

My favorite example of a solid universal, one on which I felt I could give a convincing lecture to a room full of unsympathetic psychologists or biologists and make them see the point of generative grammatical study, used to be the Coordinate Structure Constraint of Ross (1967); but some recent work of George Lakoff's has changed matters decisively. Lakoff (1987) examined examples of VP coordination with multiple conjuncts, and he found an astonishing thing: any number of examples could be constructed in which some of the VPs had extraction sites and others did not. Here are a few examples, either taken from Lakoff's paper or modelled on examples of Lakoff's.

- (1) a. What did he go to the store, buy [e], load [e] in his car, go home, and unload [e]?
- b. How many courses can you take [e] for credit, still remain sane, and get all A's in [e]?
- c. Sam is not the sort of guy you can just sit there, listen to [e], stay calm, and not argue with [e].
- d. That's the stuff that the guys in the Caucasus drink [e] and live to be a hundred.
- e. This is the kind of brandy that you can sip [e] after dinner, watch TV for a while, sip some more of [e], work a bit, finish off [e], go to bed, and still feel fine in the morning.
- f. That's the type of firecracker that I set off [e] and scared the neighbours.
- g. It's a problem that I stared at [e], sat around for a while, fiddled with [e] some more, started working seriously on [e], got bored, and finally gave up on [e].

The implications of these examples for a universal principle like the Coordinate Structure Constraint are extremely serious. Whether the constraint is seen as blocking *wh*-type movement across the boundaries of a coordinate structure (Ross 1967), or coordination of dissimilar phrase types (Williams 1978, Gazdar 1981) or failure to instantiate the SLASH feature in accord with the Head Feature Convention (Gazdar, Klein, Pullum and Sag 1985; Sag, Gazdar, Wasow, and Weisler 1985), Lakoff's data motivate a fundamental re-thinking. His own analysis does not provide a serious new approach to the topic, for over and above its vagueness, it has nothing to say about all the facts concerning

coordination of other categories that the Coordinate Structure Constraint does account for, or about the related phenomena correctly predicted by the various syntactic accounts — e. g. the ungrammaticality of the following phrases:

- (2) a. *the man who you saw [_{NP} [_{NP} e] and [_{NP} a picture of [_{NP} e]]
 b. *the man who you saw [_{NP} [_{NP} the mayor of New York] and [_{NP} a picture of [_{NP} e]]

The Coordinate Structure Constraint held a special interest for me because it made predictions fairly directly about strings of words that would not have a grammatical structure. If it had been true, as Lakoff's data give us little hope that it can be, we would know exactly what to expect to be grammatical and ungrammatical by virtue of it. Other putative universal constraints in syntax have clearly been devised according to a very different philosophy. In particular, neither Subjacency nor the Empty Category Principle have this kind of closeness to the facts of grammatical constructions. Both are settling down to be organizing principles rather than hypotheses.

The ECP says that if nonterminals without dominated terminals are postulated, lexical categories governing them (roughly, immediately c-commanding them) must also be postulated. This does lead us to look for any constituent types or other specific clusters of syntactic properties. Rather, it has led to a lot of linguists postulating invisible governors (e. g. null prepositions), shifting rules to the PF component (if they seem to violate the ECP), reworking constituent structure assumptions (to make sure there is a governor), and so on.

Similarly, Subjacency says that if long-distance movement is postulated, intermediate stopping places for the phrase that moves must also be postulated (how many being a point on which languages can differ). This has led to new stopping places being postulated (COMP nodes in PPs, for example; see van Riemsdijk 1978), proposals for varying the list of categories that can define stopping places (Rizzi 1978), and so on. This may lead us to new insights into syntax or it may not; but my point is that Subjacency does not present the same kind of opportunity for falsification that the Coordinate Structure Constraint does, and it is not intended to. It represents a very abstract limitation on the way in which theories are to be constructed; it is very securely insulated from confrontation with surface facts about grammatical constructions; and sociologically it is in the position of always being assumed to be correct: no one who mentions it imagines they might discover that it is wrong.

It is obvious that both principles (and many others in current syntax) are being used as guides to organizing the construction of theory, not as hypotheses about what syntactic structures will be observed in languages. This is not a bad thing, for every theoretician needs such organizing principles; but it is not something that adds up to any discoveries about syntax that could be called significant.

There have been no major descriptive syntheses produced in the last fifteen years of work in syntax. Generative syntacticians have become quite content with the idea that it is not their job to describe languages; their job is to search for over-arching principles of great depth and generality. The problem with this is that many of them seem to have completely forgotten what these principles are supposed to be about. The principles of universal grammar are principles of

design and function for grammars, that is, descriptions of languages. If there are no efforts at describing languages — efforts at saying for some fair-sized range of sentence types which sentences are in and which are out — there can be no evaluating of principles of universal grammar.

This is a point so obvious that it sounds almost inane to reiterate it; yet it is largely forgotten in the presentations today's syntacticians make of their work. Challenges oriented toward the question of whether an adequate description of the facts has been provided are rebuffed with an assertion that describing the facts ('merely' describing the facts) is a quite unimportant, even demeaning task; what is important is the glimpse provided of the grand, universal principles of the mind. It is hard to get across to people who think they have glimpsed a principle of the linguistic faculty of the human mind that they will have to substantiate that by exhibiting descriptions of linguistic phenomena which both appeal to the putative principle and equal or surpass previous descriptions in breadth or depth of insight.

It is worth keeping in mind, for comparison, that major synthetic works on language that deserve a permanent place in intellectual history have existed in the past, especially in the pre-generative era. *Roget's Thesaurus* is an example. The *Oxford English Dictionary* is another. And more recently, the 1630-odd pages of description produced by Quirk, Greenbaum, Leech, and Svartvik in their *A Comprehensive Grammar of English* qualifies. In generative grammar, nothing of this sort has been attempted since Stockwell, Schachter, and Partee (1973) published the results of the late-sixties research on English grammar funded by the United States Air Force.

When one reflects upon the expenditure of time and energy that has occurred during the generative period, the lack of substantive products is utterly amazing. The number of linguistics programs in North American linguistics departments grew from about 30 in 1963 to about 140 in 1984 (Newmeyer 1986, 45), much of the growth unquestionably being driven by the increasing influence and dynamism of the generative movement. The active membership of the Linguistic Society of America (LSA) roughly tripled between the publication of *Syntactic Structures* (1,354 in December 1957) and the publication of Ross's dissertation by Indiana University Linguistics Club (4,000 by the end of 1968, remaining there ever since; Newmeyer 1986, 44). About a thousand people now attend a large annual meeting of the LSA (like the one in Washington D.C. in December 1989) or a summer Linguistic Institute (like the one at Stanford in 1987), many of those people being younger faculty and graduate students, generally the most active and engaged members of the profession.

Suppose we assume, very conservatively, that about 100 full-time linguists world-wide were interested in generative grammar by 1960; that it was 200 by 1970 (177 linguistics PhDs were awarded in 1972-73), and that by the end of the 1980s 1,250 people world-wide were devoting their working hours to research in generative linguistics in various ways (the number of abstracts received for the 1990 West Coast Conference on Linguistics meeting was around 250, and surely not more than 20% of the world generative linguistics community sent in abstracts). No matter how you count it up, it amounts to a probable number of person-years of research in generative grammar that rises above 10,000. What do we have to show for that 10,000+ person-years of work, in terms of

substantial scholarly achievements that will stand the test of time as regards their results about language? I find the question rather spine-chilling, and I suggest that a minimal answer is that we cannot show enough.

One symptom of this is that major controversies in syntax still go completely unresolved for decades. In the earth sciences during the same period the theory of plate tectonics (continental drift) has gone from being radical to being solidly established fact about the structure of the planet (Tarling and Tarling 1971). In cosmology the Steady State theory has confronted the Big Bang theory, been tested, failed the test, and quietly given up the ghost, the Big Bang now being universally accepted as the correct conception of how the universe started. Meanwhile, in generative linguistics? Central questions both large and small stand unanswered, to be debated and redebated repetitively without closure.

Consider a fairly major issue in syntax: are there orphan VPs (complement VPs that do not have a unique dominating S at any level of syntactic structure) or not? The work of Brame, Bresnan, Culicover, Emonds, Fiengo, Gazdar, Lasnik, Wilkins, Williams, and many others since the early 1970s has argued that there are; these authors have analyzed at least some surface VPs (*try* complements, *tough*-movement complements, conjoined VPs, etc.) as orphan VPs. But some syntacticians have resisted this conclusion absolutely — notably Chomsky and Postal, plus all those who have followed Chomsky most closely, all generative semanticists, and all relational grammarians. Some have attempted to argue for closure of the issue (Koster and May 1981), but others have answered those arguments from a broadly Chomskyan perspective (see especially Culicover and Wilkins 1986). Nonetheless, the matter remains completely open, and the field remains obdurately split. Those who are ideologically committed to sticking with PRO subjects ignore the issue, and those who see no need for PRO subjects continue to employ orphan VPs. Neither side sets a high priority on determining which kind of analysis is optimal.

The related issue of whether auxiliaries are main verbs likewise never got settled. The arguments of Ross (1967), Pullum and Wilson (1977), and Gazdar, Pullum and Sag (1982) address some specific details of contemporary analyses, but in general are remarkably independent of changes of assumptions within generative grammar. They have not been answered. Instead, the AUX constituent of the *Syntactic Structures* has been quietly renamed INFL (in Chomsky 1981) and subsequently I (in nearly all current transformational work) without any clear changes in the basic character of the assumptions made about auxiliaries. The English modals, for example, are still regarded (in discussions that give decreasing amounts of detail) as some kind of nonverbal tense-related particle, in defiance of what is universally maintained by traditional grammarians like Otto Jespersen (1949), more recent descriptive grammarians like Harold E. Palmer (see e.g. Palmer and Blandford 1939), contemporary grammarians acquainted with generative analyses like Rodney Huddleston (in numerous publications) and so on, namely that the modals are verbs like all the 'auxiliaries' in English.

The problems posed by items like *would rather*, *had better*, *ought to*, *is to* (brought to the attention of the generative grammar community by Huddleston 1978) are forgotten in current work. These items are main verbs that have modal morphosyntactic properties. They are the only candidates for verb in the

topmost clauses in sentences in which they appear (for example, *would* is surely the only candidate for verb of the matrix clause in *I would rather you told him*). They lack third person singular present tense inflectional -s, but in current terms they must originate in VP and raise into I position. It follows that the alternative source (base generation in I?) assumed for modals is redundant, since all modals could be given the analysis that the *would* of *would rather* calls for. But instead, current analyses just skirt the issue uneasily. For example, Baker, Johnson, and Roberts (1989, 245, n23) say:

We are setting aside the exceptional, and somewhat archaic or dialectic, instances of main verb raising with *need*, *have*, and *dare*. See Pullum and Wilson (1977) for discussion.

The Pullum and Wilson discussion presses the case that the 'exceptional, and somewhat archaic or dialectic, instances of main verb raising' have to be taken very seriously as clear evidence that all auxiliaries can and must be treated as main verbs, but Baker, Johnson, and Roberts are content to be 'setting aside' this crucial dilemma. The issue remains unresolved.

Putting the auxiliaries problem together with others, we find, astonishingly, that simply saying what categories the words belong to in a short English sentence is a task that remains utterly beyond today's generative grammar community. Take a sentence like *We ought to be near the rich*.

- Is *we* entered in the lexicon as an N (though it doesn't take articles)? An NP (though phrase nodes are not usually found in the lexicon)? Or a special Pronoun category? And didn't Postal (1966)
- argue that pronouns are really definite articles?
- What is *ought*? A member of V? Or of M (modal)? Or of I (INFL)? What about *to*? Another base-generated I element — or did *ought* take up that slot? Is it under a T (Tense) or Agr (Agreement) node, perhaps (see Pollock 1989)? Or some kind of funny modal? Or a complementizer (that's what Postal and Pullum 1978 called it)? Or is it a preposition, like dictionaries say? Or even a verb, as Pullum (1982)
- and one or two other people have argued? Is *be* a verb? Or is there a special label COP for the copula? Or is *be* always generated in I?
- How about *near*? It takes a straight NP objects, so is it a P (Preposition)? But it inflects for comparison, so is it instead an A (Adjective)?
- Is *the* labelled Art (a lexical category for articles) or Det or Spec:N' (phrasal categories that can also dominate possessive NP determiners)? What about Sommerstein 1972, where it was argued that articles were really pronouns?
- Is *rich* a head noun here (since otherwise there isn't one in this noun phrase), or is it an adjective?

This brief consideration of live alternatives implies over four thousand analyses for the *flat* structure analysis of *We ought to be near the rich* (4,032, to be precise); then we start asking about constituency (and there are hundreds of logically possible bracketings, of which the number that linguists have considered for an example of this sort is larger than you think). What I am pointing to is that there is absolutely no sign of generative grammar reaching the point where

randomly selected practitioners will give approximately equivalent answers when asked for the syntactic surface structure of simple English sentences. The diversity of opinion is remarkable.

Take another big issue: the Projection Principle, and the question it was designed to short-circuit, namely whether there is such a thing as Subject-to-Object Raising (SOR), or more generally, whether there are any constructions which are correctly analyzed in terms of movement-derived or nonthematic objects (or the analogs of these in any other theoretical terms). The split here goes right across the whole field. It is now by no means the case that all relational grammarians believe in SOR and no Chomskyan transformationalists do. At the January 1990 conference on grammatical relations at the University of California, San Diego, there were three consecutive papers on whether there is SOR in Japanese (there isn't), Javanese (yes there is), and Korean (no there isn't), so the split begins to look as if it is by language (or language family) rather than by sociological subgroup within the syntax community.

But the big question is whether English has SOR. Since the feud between Chomsky and Postal on this issue began in 1969 (when Chomsky cast aspersions on the existence of raising into object position in his remarks at the conference on the Goals of Linguistic Theory at Texas (Chomsky 1972, 86) and Postal began to compose his book *On Raising* (Postal 1974) as a response), there has been no hint of a generally accepted resolution of the issue. In fact, things have spiraled downward from direct argument (as when Postal 1974 was answered by Bresnan 1976 and the latter was promptly rebutted by Postal 1977, and so on) to the much lower level of bluster and dishonesty (as when van Riemsdijk and Williams (1986, 33) assert that the dispute over SOR was a 'battle, which basically had already been won with the appearance of Chomsky (1973)', i. e. had already been decided by right-thinking people in Chomsky's favor before Postal 1974 even appeared). The generative grammatical community is quite unable to say univocally whether or not it agrees with traditional grammarians, who without exception describe English constructions like *We hold these truths to be self-evident* (the 'accusative and infinitive' of Latin grammar) as involving an object and a complement verb phrase — and it remains unable even after thirty-five years of research in English syntax and a major argument on the specific question that has been raging in the literature for nearly twenty years.

This kind of inability to achieve consensus or establish any general acceptance of results, even internally, does not betoken a field in good health. And the vanishingly small array of clear and uncontested results in generative grammar makes an investment of 10,000+ person-years look excessive.

One other observation I would make about generative grammar so far is that it is showing a retrogressive tendency to replace well-understood concepts by less well-understood ones. The strict cycle concept in syntax (Thompson 1975) was much better understood than the obscure conditions on argument structure with which Freidin (1978) began to replace it. The structure claimed for AUX was elaborated (e. g. by Akmajian, Steele and Wasow (1979) and Steele (1981) in much more detail than what has been suggested for I and its successors T and Agr (Pollock 1989). The classical transformational idea that expletive (dummy) NPs were those not present in deep structure but present in

surface structure was much clearer than the obscure present accounts, where nothing is clear about where expletives come from. The idea of deep structure or 'logical' grammatical relations was clearer than the notion 'theta-marked,' which apparently now does duty for it; and the idea of grammatical relations in cycle-final structure clearer than the notion 'Case-marked,' which has led to a succession of equivocations about what Cases there are, at what stage the marking takes place, how Case marking is authorized, and what elements are allowed to Case-mark NPs.

3. Generative grammar today

The central thing about the study of syntax as we enter the 1990s is that work in the 'principles and parameters' or 'government-binding' style (which for convenience I shall go on calling GB) dominates it as no variety of theory has ever dominated it in all the previous history of linguistics. The domination is almost total. The few conferences at which other approaches to generative grammar are featured (the bi-annual peripatetic Conference on Grammatical Relations, for example, primarily a relational grammar event) are like small specialist workshops. At the big conferences syntax means GB. And even the Conference on Grammatical Relations gets GB papers these days.

Much less widely recognized is a fact about the nature of GB work: it is moving ever closer to revealing itself as simply a reincarnation of Generative Semantics (GS). The parallels are striking, and numerous. I will give a round dozen.

- (A) No orphan VPs. As mentioned above, a one-to-one correspondence between deep subjects and deep verbs was a non-negotiable principle of the abstract syntax of the late 1960s that led to GS. Chomsky has never relinquished this principle, which entails many key similarities between GS and GB analyses.
- (B) Movement passive. If no VPs are orphans, a movement analysis of the passive construction can be maintained. The concern to have a simple object-movement operation to relate actives to passives motivates Chomsky (1981) — with its 'Move α ' — as much as McCawley (1970) — with its movement of object NPs one constituent to the left.
- (C) Traces. Traces begin not with Thomas Wasow's dissertation (Wasow 1972) as is often suggested, but with Postal's ideas about DOOM marking, developed around 1968. In Ross (1969) a structure is given for a raising example in which a pronoun bearing the feature [+DOOM] is left in the pre-movement position (and an argument is given for the presence of the trace thus indicated).
- (D) The Universal Base Hypothesis. Linguists like Emmon Bach, George Lakoff, James D. McCawley, and others were talking about the idea that the base component might be a substantive universal — identical for all languages — from some time in the late 1960s. GB has remained true to this quixotic hypothesis, and since Stowell (1981) has maintained a somewhat attenuated version of it in which the universal base is in effect an infinite set of phrase structure rules (see Kornai and Pullum 1990 for discussion).

- (E) Abstract constituent order. The tradition of deep structure constituent orders distinct from surface orders (recall McCawley 1970) today lives on solely in GB work, where it is a major consideration. No other line of research in syntax retains anything like it; RG abandoned the idea of linear order at pre-surface levels in 1974, and in GPSG, HPSG, and LFG the notion cannot even be expressed.
- (F) High node-to-word ratios. The ratios of deep structure nodes to surface structure words in the abstract syntax of the late 1960s was astonishing then; some linguists laughed openly at the Lakoff and Ross structure for *Floyd broke the glass* (see Newmeyer 1986, 84, where it is recorded for posterity, apparently from lecture notes; it was never published by Lakoff or Ross). There were 7.75 nodes per surface word if we ignore the performative hypothesis (the top-level 'I declare to you'). Yet structures of the sort posited by Pollock (1989) and others have at least 8.25 nodes per surface word (also without assuming the performative clause). To save paper and artwork costs, I will not reproduce complete structures here to demonstrate this; the reader may easily verify it.
- (G) Quantifier lowering in the syntax. The quantifier lowering of generative semantics work from the later 1960s to the mid 1970s (Postal 1974 gives a more explicit account than is found almost anywhere else) is not really a different analysis from what is argued for in May 1977 and subsequent works. For May and other GB linguists the quantifiers are in fact raised, because the rules derive 'Logical Form' configurations (GS deep structures) from 'S-structure' representations (shallow structures) instead of the reverse; but this makes no substantive difference, as was well understood in the heyday of the arguments between generative and interpretive syntax. The important thing is that quantifier scope is being treated as a syntactic phenomenon, to be handled with movement rules and tree configurations, not a semantic one.
- (H) Predicate raising. Transformational raising of verbs and amalgamation of their clauses (and sometimes their morphological identities) by means of derivational steps was a hallmark of GS work. Despite the remark of Chomsky (1972, 86) that predicate raising 'surely is not' motivated, verb raising in syntax is today the hallmark of GB. Many of the applications (e. g. causative constructions, or combining verbs with their tense properties) are the same as the ones for which GS used predicate raising.
- (I) Tense and Neg as a higher verbs. The main content of the GS claim that elements like Tense and Neg were verbs of higher clauses was not so much that they were verbal (they were uncontroversially a bit different from most verbs in morphosyntactic behavior) but that they were higher: they represented whole separate domains of predication asymmetrically commanding the verbs they superficially appeared on or adjacent to or above. GB today represents this by having such elements as bases for complete maximal projections superordinate to the VP domain. The inspirations of this idea come from Ross (1967) and McCawley (1971), though these are not cited in works like Pollock (1989).
- (J) Cyclic and postcyclic rules. Developing throughout the GS period was the idea that cyclic and postcyclic rules were very different, the former being

local and involved with grammatical relations like subject and object, the latter being nonlocal and never concerned with the creation of subjects or objects. This typology of rules was rendered more explicit in the early stages of the presentation of relational grammar by Perlmutter and Postal. In GB the GR-changers are called A-movements (movements to argument positions) and the formerly postcyclic rules like *wh*-movement have been named A'-movements (movements to the set-theoretic complement of the A-positions, i. e. non-argument positions). Cyclic application of transformations has been jettisoned, but the typology is the GS/RG one; the obscure new names seem to have been chosen to disguise the conceptual debt.

- (K) Deep case participant roles. The theta-roles of GB are of course essentially a rebirth of Fillmore's deep cases. In recent work of Fukui & Speas (cited by Pollock), Fillmore's 'subjectivalization' transformation has been brought back quite explicitly.
- (L) Transderivational constraints. There can be no question that if Pollock's (1989, 420) notion of an English-specific 'Avoid *Do* principle' (supposed to guarantee minimal use of the lexeme *do* and thus block auxiliary *do* where it needs to be blocked) were made precise instead of being left in hand-flailing mode, it would have to surface as a transderivational (i. e. interderivational) constraint of the sort that George Lakoff was being ridiculed for in the early to mid 1970s. Pollock seems to mean that the grammar contains a constraint preventing use of *do* where there is an equivalent derivation of a different sentence that has the same meaning and lexemic content except that it lacks *do*.

This list can be continued almost indefinitely. GS is a rich lode to mine, and GB work is expanding at great speed. But one feature of GB links it to GS even more deeply and at a more general level than any of the small theoretical borrowings listed above: the irredeemably informal character of rule and principle statements and the avoidance of grammar fragments recalls irresistibly the days when once Postal (see Peters 1972, 168, n. 50) announced 'a no doubt never-to-be-written paper' by Lakoff, Postal, and Ross, to be called 'What to do till the rules come,' which would argue against the construction of generative grammars — the formalization of rules or constraints — and in favor of general argumentation surrounding what character rule systems (if they ever arrived) would need to have.

The vagueness of the general principles given in GB has been alarming for some time; this is a framework in which 'Move α ' was supposed to be the key explicit statement of the theory of movement rules (which is something like suggesting that set theory should be based on the axiom 'Pick element' and leaving everything else undelineated). Yet it has slid downward from there toward even more obscure 'principles' like 'Affect α ', 'Assume Grammatical Function', and 'Avoid Pronoun'. (Note the imperative mood of these, and consider this question: who is the addressee?)

The 1980s end with the vagueness and sloppiness of GB work reaching levels that seem all the more ludicrous because they lack the self-conscious whimsicality of later GS work. The flavor is of solemn self-parody. Consider again the note in which Pollock (1989, 420) suggests an 'Avoid *Do*' constraint:

Perhaps there is an 'Avoid *Do*' principle in the grammar of English falling under some version of Chomsky's (1981) 'Avoid Pronoun' principle, itself conceivably the by-product of some more general 'least effort' principle.

Perhaps this, conceivably that, maybe grammars are trying to save energy ... It is just astonishing that such maundering should be turning up in what purports to be a refereed journal of a field with formal underpinnings and technical content.

Again, consider the furtive treatment of features and feature percolation found in current GB, with its impressionistic diagrams of arrows pointing in the direction of some flow of features that the theory does not actually determine (some examples from recent issues of *Linguistic Inquiry* are reproduced in Figure 1). GB desperately needs a serious theory of features, if only to save the costs of extra paper and charges for artwork which are called for at present. The level of precision has gone dramatically downward from works like Lakoff (1970), and at the same time works like Gazdar et al. (1985), which attempt to make some progress on developing an explicit theory of syntactic features, are rigidly ignored in GB, for ideological reasons. Where ideas from such work are needed, as when Abney (1987, 236) finds use for the idea of making bar level a syntactic feature, they are simply reinvented or lifted without remark (Abney proposes his

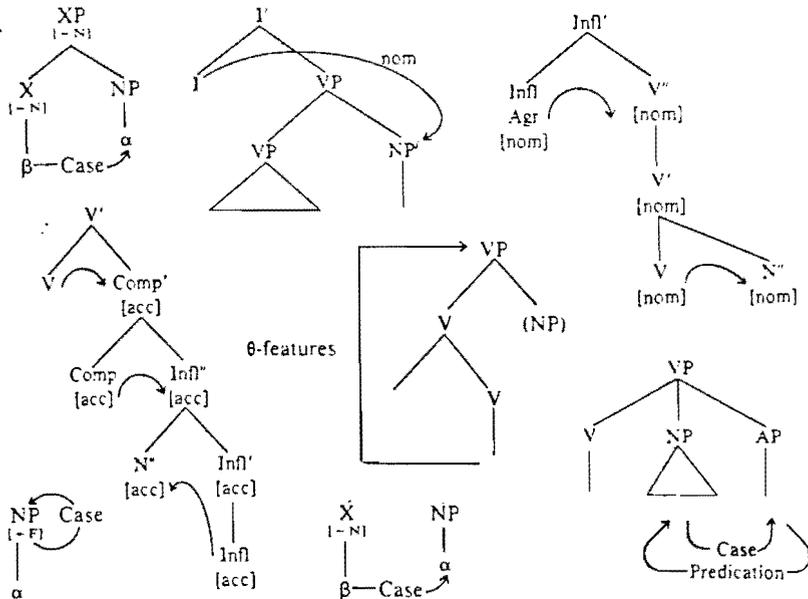


Figure 1: Some recent impressionistic pictures of feature migration, taken from issues of *Linguistic Inquiry* during the late 1980s.

bar level feature with no apparent realization that it is already five years old in the GPSG literature).

We should distinguish at least four properties of grammatical work here: *detail*, *breadth*, *precision*, and *formalization*. These are mutually independent (the third is not the same as the fourth, for example). The remarkable thing about current GB work is that it lacks all of them at once: it is not detailed like Jespersen (1949), or wide-ranging like Greenberg (1963), or precise like Bloch (1946) or formalized like Montague (1973). It deals in a selective way with narrow ranges of facts and provides accounts that are both vague and informal.

In fact, there are some signs of undervaluing of work exhibiting improved descriptive coverage and thoroughness. It is clear, for example, that the thoroughness and responsiveness to the relevant facts of a work like Kuno (1987) merits considerable influence in the field, but instead this work is hardly ever cited. Something similar could be said about Gunji (1987) and about the many papers published (mainly in *Linguistic Analysis*) by Kunihiro Iwakura. (I hope it is just an accidental fact that these scholars are all Japanese. We have seen before, in a variety of industries, the consequences of American assumptions that their sloppy work will always rank higher than more careful craftsmanship from Japan.)

In sum, I am skeptical about the chances of today's generative grammar lasting very long in the intellectual history of linguistics. I am not asserting that nothing of it will survive; doubtless, some concepts of current generative grammar are here to stay. The idea of analysis in terms of categories and features is likely to be robust. Headedness, the fundamental concept explicated by X-bar theory, is doubtless quite important. But these amount to little that is new; both were implicit or even explicit in context-free phrase structure grammar and dependency grammar some thirty years ago.

The most important developments of the generative period were perhaps the recognition of the notion of unbounded dependencies and the discovery of syntactic constraints on anaphoric relations. But the analysis of unbounded dependencies into local domains, either GB-style, in terms of subadjacency, or the even more local analysis of GPSG (introduced in Gazdar 1981) has altered the status of the former; it falls into place as an oversight of traditional and structuralist grammarians, but not a fundamentally new kind of grammatical phenomenon.

The discovery of syntactic constraints on anaphoric relations seems truly new; there just isn't anything on the subject in traditional grammars as far as I am aware. But in this area the problem for generative grammar is that despite an enormous amount of work on the subject, the widely accepted, precisely delineated description of how those constraints are framed, even for one language, has yet to appear. Problematic examples have shifted back and forth across the grammaticality line for years, and highly salient counterevidence has been casually described as on the 'marked periphery' of the syntactician's purview. Even the most fundamental elements of the paradigm that is to be used seem still to be up for grabs; Zribi-Hertz (1989) provides a good example of a recent contribution that makes it clear how far the field is from being able to say where you use what kind of pronoun in standard English.

In a review article published in the last issue of *Language* in the 1980s, Stephen Anderson (1989) laments the failure of linguistics to make enough of an impact on cognitive science to show up centrally in a textbook like Johnson-Laird (1988). But it is not surprising to me that Johnson-Laird pays scant attention to the generative syntax of the last ten years when introducing students to cognitive science in a book entitled *The Computer and the Mind*. There is very little in such work that can be genuinely (as opposed to rhetorically) connected to what is known about either the computer or the mind. It would be good if there were more work that did make the connection, to be sure; but in berating Johnson-Laird for including little recent linguistics in his book, Anderson is merely shooting the messenger.

As a reference to support his claim that 'The list of linguistic constructs and results that have been subjected to concrete empirical examination is truly massive,' Anderson cites the papers in Newmeyer (1988). But amusingly, only one page of *Language* need be turned to find the review of Newmeyer's collection by Richard Hudson (1989). Hudson (p. 814) quotes Newmeyer's introductory claim that 'the prestige of generative grammar among psychologists, neurologists, computer scientists, and so on has reached an all-time high' (a claim that would contradict Anderson's main complaint), but comments that in actuality Newmeyer has simply 'managed to bring together a fair number of scholars who share his enthusiasm for GB,' and that he is 'scraping the bottom of the barrel too hard in his search for motivation for GB.'

I think Hudson is correct on this: linguistics does *not* currently have much prestige in psychology or computer science. Anderson sees a sign of that in Johnson-Laird's book, and lays the blame mainly on the book. Hudson sees, as Anderson apparently does not (though he may suspect it just a little), that linguistics has yet to show that it would merit such prestige. Hudson notes, for example (p. 813), that Lightfoot's theory of 'trigger experiences,' to which Lightfoot accords a central place in the chapter he contributes, is not supported or mentioned by any of the acquisition-related chapters elsewhere in Newmeyer's survey; he notes that Weinberg's argument for GB grammars on grounds of their brevity is fatuous, since no GB grammars have ever been exhibited; and so on. It is the vacuity of generativist works such as these that lies at the root of generative grammar's low profile in mainstream cognitive science.

What generative grammar should undertake during the 1990s is a program of work that would have enough substance to potentially earn it a place in cognitive science. There are many possible directions to take in future developments (not by any means mutually exclusive alternatives). One might imagine pursuing work on the mathematical foundations of linguistic theory, either in grammatical descriptions or in studies of formal learning theory as applied to human languages;¹ or on increasing breadth and depth of treatment of languages, either through comparison across a wide range of languages or through detailed description of a particular language; or insight into psychological or biological capacities, either by experiments on language use in mature users or by observation of language acquisition; or an understanding of change and variation, either via the historical evolution of languages or via variation in and between dialects; or the development of practical applications, either in terms of pedagogical applied linguistics or in terms of industrial applied linguistics

(linguistic engineering).

All such types of investigation have some value. What worries me about the current scene in generative grammar is to see signs of work that cannot be regarded as advancing toward *any* of these goals. Formal rigor is neglected in favor of buzzwords and speculation; breadth is maintained neither in the range of languages made relevant to the inquiry nor in terms of the array of constructions marshaled within one language. Experimental psycholinguistics and developmental acquisitions studies are both neglected as sources of data. Change and variation are rigidly idealized away. Practical applications are spurned. It is unfortunately all too easy to find examples of work satisfying this checklist of negatives, in any recent issue of *Linguistic Inquiry*, *The Linguistic Review*, or other journals publishing contemporary syntax.

It is particularly strange to watch generative linguistics drift away from the psychological and biological goals it claims to have set itself. For example, anyone who truly believed that the biological capacity for language was a genetic attribute, transmitted through the gene plasm in a quite detailed form, would surely (at least, if they understood genetics) look for biological differences between breeding populations of human beings that correlate with differences in UG. This is a point that was made very explicitly by McCawley (1978, 216). It was also made independently by Sampson (1979, 142ff).² The strange thing about generative linguistics of the Chomsky school is that it maintains the geneticist line for rhetorical purposes (as a way to connect linguistics to discourse about biological studies of cognition), but pays no attention at all to the questions that McCawley and Sampson raise.

There are in fact works in the literature that seem to suggest genetic variability in the language faculty, but they are resolutely ignored by generative linguists. A clear recent example is the work of Cowart (1987) and the work of Bever and associates cited there. Cowart's result is that right-handed people with left-handers in their family process anaphoric linkages differently from right-handed people who have no left-handers in their family. This really does suggest something genetic that connects with syntax and semantics. But as far as I know, syntacticians and semanticists have paid not the slightest attention to it.

That generative linguistics apparently never gets serious about its purported psychobiological kinship structure is seen in the way it does not allow evidence from experimental or observational psycholinguistics to intrude on its hypothesizing. No generative linguist ever sets up crucial experimental or observational opportunities for falsification of purely grammatical hypotheses. As far as I know, there is no instance in the literature of a generative grammarian accepting an *unwelcome* conclusion (as opposed to the one their theoretical arguments incline them to anyway) on the grounds that experimental or observational data from psycholinguistics forced it.³

Generative grammar stands today, then, in a rather isolated and unstable position, little of its methodological stance finding real support from its practice. It is in this context that I attempt, rather uneasily, to glimpse something of the likely future of the field in the 1990s.

4. The 1990s: some predictions and an appeal

What can be predicted about generative grammar in the 1990s? It is rather easier to discern developments related to the profession, of course, than to the intellectual drift of things. For example, one thing that seems clear about the profession is that after the rising profile of work that applied generative grammar to symbolic computational linguistics in the 1980s, there will be a decline in such work in the 1990s. The withdrawal of military funding in the late 1980s will hit hard, virtually destroying the active groups at SRI International, Bolt Beranek and Newman, and the Information Systems Institute in Southern California. The highly statistical research that has begun to take over natural language research at sites like IBM Research (Hawthorne and Yorktown Heights) and AT&T Bell Labs (Murray Hill) will find scant use for linguists. The main jobs for linguists will continue to be in academia.

Within the academic profession in the United States, there is likely to be an increase in the number of academic jobs for linguists, especially west of the Mississippi. The increase will be slow, but will continually pick up during the 1990s unless slowed by serious economic disasters (stock exchange crashes; great earthquakes; or the kind of penny-pinching kitchen-table legislation seen in California's proposition system, where voters attempt to deal with highly technical State budget issues by referendum).

Demographics will be driving this growth in the number of academic jobs. In some areas (California being an example) there are signs of an upsurge in the number of young people who will be wishing to enter universities (especially State universities); but there is also a coming wave of faculty retirements. During the 1990s, people who got their PhDs in the early 1960s, and took university jobs in the expansion of universities that was then going on, will be coming up to an age when, despite laws ensuring that they cannot be required to retire, they will nonetheless be thinking about retirement. A linguist who received the PhD at the age of 30 in 1960 will be 60 now, and looking at retirement within five years or so. And by the year 2000, the pace of retirements will be quickening.

Another predictable element in the linguistics profession of the 1990s is that there will be essentially no Blacks or Chicanos in general and theoretical linguistics at all. I know of three African-American US citizens in linguistics graduate programs in the whole of the United States today. This is as close to nonexistence as makes no difference; the likelihood of a Black candidate turning up in theoretical linguistics faculty searches in the next few years can confidently be set at approximately zero. This should be reflected upon in the context of a country at large in which there are increasing cries from minority students in all subjects for more representation of their groups in university faculties. This generally neglected feature of linguistics — the fact that the profession is about as ethnically diverse as physics — is something that linguists should be considering more seriously.

In terms of theoretical framework, it is quite obvious that GB-style theorizing is set to dominate the whole field of linguistics throughout the 1990s. The present graduate students, after all, will be the research-active junior faculty of tomorrow. Just as we can see that hardly any of them are Blacks or Chicanos, we can see that nearly all of the non-phonologists do GB syntax. But there is a problem for GB's internal development. For the last few years, GB has been

developing through rediscovery of generative semantics ideas from about ten to fifteen years before. The problem is that the vein being mined is about to run out. By 1975, generative semantics had collapsed and disappeared. Once 1975 is reached, there is nothing more to borrow. It is hard to see what GB can do about this. Perhaps its best bet would be to work on mining the history of RG, borrowing and recasting the work that Perlmutter, Postal, and others have been doing over the last decade and a half. To some extent this is already happening.

There are other developments that I would predict with more or less confidence: some continued popularity for 'functionalist' syntax (there has to be something to say about grammar at Berkeley Linguistics Society meetings); further interest in the lexicon (fueled by the mutually reinforcing trends of GPSG/HPSG syntax, GB lexicon/morphology interests, and lexicographical work in computational linguistics); a return to some extent of corpus-based linguistics (the Association for Computational Linguistics has a Data Collection Initiative aimed at amassing 100 million words of machine-readable text, and something will have to be done with it); and just possibly, the beginnings of some serious grammar-testing by computer (machines powerful enough for this are now reaching linguists' desks).

In phonology, of which I have said very little here, I would predict that phonetics will receive some renewed interest (there is positive pressure from the speech industry as well as from military funding agencies, which continue to be interested in speech analysis and synthesis, as well as a variety of reinforcing phonology-internal developments and a rash of forthcoming efforts to formalize nonlinear phonology in various ways that involve explicit phonetic representation).

But beyond these hints, I have great difficulty in fulfilling the terms of my mission. Switching from the future indicative to the optative, I will close with an appeal rather than a prediction. If I had to name one thing that I felt would be most valuable for the health of linguistics in the 1990s, I would say that what was needed was a large-scale theoretical synthesis and description effort on the syntax of a single language studied in depth — probably English. What is needed is an effort that would combine the energy and consciousness of detail seen in the best work of Bresnan, Emonds, Postal, Kayne, McCloskey; the organization and cooperative spirit seen in the team of Quirk, Greenbaum, Leech, and Svartvik; the exhaustive coverage seen in the finest dictionaries of the English language. Linguists are not pulling together the ideas they entertain. The discipline of a team effort to lay out a serious reference grammar of English has been lacking for too long. The task will be a large one, and difficult to organize, but it would be worth it.

I disagree diametrically with Anderson (1989, 809) on the risk that linguists might be 'sent back to the narrowly humanist ghetto from which the field managed to emerge in the 1960s.' A lot of discipline and scholarship was left behind during the flight from that ghetto; linguists have plenty to gain from rediscovering their roots. And they will not lose whatever respect they might have in the cognitive science community by doing their descriptive work more thoroughly (any more than the computer science community has lost prestige from supplying us with the UNIX operating system and windowing environments). Anderson bemoans the fact that linguists at the moment are not convincing

cognitive scientists and the like of their claim to be heard. They will do much better, I believe, if they can point to a basis for their elaborate conjectures about mental structure in a comprehensive and widely supported description of the syntactic structures found in even a single language.

Those who are inclined to dismiss such encyclopedizing work as relatively dull when compared to the exploration of the origins of the universe or the probing of human cognitive capacities and their genetic basis should reflect on the fact that astronomers and cosmologists have spent the better part of the last decade constructing detailed maps of the universe, and scientists who work directly on the foundations of genetics have decided that they will spend several billions of dollars over the coming decade or two constructing a complete map of the human genome. An exhaustive account of what we now know about the syntax of English will be a small job by comparison with these giant cartographic endeavors. We can spare a few hundred person-years, surely.

Footnotes

* My remarks here, though adapted to the written medium to some extent, are largely in the form of the lecture I gave at the conference, and some informality with respect to documentation and citation may be evident; I hope the reader will forgive these. Thanks to those in the audience at Tempe who argued with me; they will note in reading what follows that unfortunately their arguments had very little effect.

¹ The view that linguistics is actually a part of human cognitive psychology or even human brain neurobiology is one that I cannot accept (or see as fully coherent). But since I take cognitive science to be a field much broader than human psychology or brain biology, concerned with the abstract structure of entities of any sort (computing machines included) that might be said to be capable of cognitive activity, I do not have trouble with the idea that formal grammar (or parts of logic or theoretical computer science) fall within cognitive science.

² Sampson's remarks are part of a highly iconoclastic argument in which he links the geneticist position on universal grammar to Jensen's racist views on IQ and intelligence. I do not accept the whole of Sampson's argument, for reasons relating to the concept of IQ and the use Jensen makes of his data on race and intelligence, but this is irrelevant here. Sampson is perfectly correct that if universal grammar (or 'intelligence') is inherited genetically we should expect genetic variation in it.

³ This claim, which I have enunciated before in various lectures, finds an echo in Hudson (1989, 819), discussing Newmeyer (1988): 'The survey does not contain a single example of a grammatical analysis that has been rejected (or adopted) because of some facts other than informant judgments.'

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AN UNEXPLOITED RULE FOR MORPHOLOGICAL NATURALNESS

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Jakob Grimm, namesake of one of linguistics' most notable rules identifying English, German and all Germanic languages, viz., the First Sound Shift, considered Modern High German as immediately derivable from Middle High German (MHG, c. 1100-1500), which, in turn, is derivable from Old High German (OHG, c. 700-1100). Of course, for Grimm Modern High German was early nineteenth century German. By the end of the century Wilhelm Scherer (1878) proposed consideration of a transitional phase between MHG and New High German (NHG), called Early New High German (ENHG), which he placed at about 1350-1650. While Grimm's division is accepted as a venerable gross approximation, Scherer's division elicits multiple challenges. The terminus a quo for ENHG is pinpointed at fifty-year intervals between 1250 and 1500, while the terminus ad quem is postulated as somewhere between 1500 and 1700. Of the two outer limits, the time boundary shared with MHG is less disputable, since an array of linguistic rules deriving ENHG from MHG can be documented. However, no such array of rules exists in the literature for the derivation of NHG from ENHG, because the ENHG rules are seen as valid for Modern New High German as well.

In Rauch 1988, I propose that the restructuring of the nominal *ǣ*-plural desinence in the seventeenth century yields incontrovertible evidence for the outer limit in the periodization of ENHG. The massive destruction by ongoing Germanic end-syllable weakening of the genetic (OHG) noun stem classes in MHG leads to the proliferation of the noun plural marker in ENHG. Most productive is the *ǣ*-suffix as a plural marker: In the fourteenth century seven of ten dialects show at least more than 50% genetic *ǣ*-plural statistics; in the fifteenth century only four dialects show more than 50% genetic *ǣ*-plural, the rest yielding to end-syllable weakening, i.e. zero suffix; and in the sixteenth century only two of the ten dialects show a more than 50% genetic *ǣ*-plural suffix, the rest falling to apocope. Within a century's time (seventeenth

century) a remarkable reversal occurs; contrary to the tendency to end-syllable weakening, six of the ten dialects show better than 50% genetic g-plural (Wegera 1987: 184). Non-genetic g- plurals, i.e., where the g-suffix is ahistorical, show parallel statistics (187).

In the present paper I pursue the question of how the proliferation of the plural desinence, in particular the reemergence of -g, is possible in spite of the drift of end syllable weakening and/or loss. There is general agreement on a broad cause-effect answer to this question. Indeed, the weakening itself leads to homophony; thus, e.g., Werner (1969: 114) notes: 'Durch diese Enttonung sind, vorerst noch allgemein gesprochen, viele phonemische Unterschiede innerhalb der Substantivflexion aufgehoben worden'. Accordingly, the resultant phonological neutralizations are untenable for required semantic distinctions, as Augst (1975: 9) writes: 'Der Grund für diese vielen Morpheme für den Plural liegt, synchron gesehen, darin, daß alle genannten Möglichkeiten gleichzeitig auch polysem, d.h. andere sprachliche Funktionen erfüllen'. Both of these observations are codified by Natural Morphology. The apocope of -g leads to damaged or unstable inflectional classes, a condition which runs counter to the system-dependent naturalness principle of class stability defined by Wurzel (1987: 92) as favoring 'inflectional systems whose inflectional classes are independently motivated and whose paradigms follow implication patterns that are as general as possible'. On the other hand, the polysemy of which Augst speaks refers, among other morphemes (cf. -er below), to plurals which are homophonous with their singulars, such as ENHG tag 'day'. Universal Grammar discourages homophony, synonymy, polysemy by the principle of isomorphism which underlies all linguistic iconism. From this principle Universal Grammar derives the requirement that plurality 'be encoded by means of "something" not just by "nothing"' (Mayerthaler 1987: 28), which requirement accedes to the system-independent naturalness principle of constructional iconicity, in that a marked semantic feature such as plural (in distinction to singular) exhibit corresponding additional morphological material.

To be sure, phonological drift is subverted by semantic iconism in the restoration of the ENHG e-plural, yet we must ask why specifically this 'apocopated' -e rather than umlaut, -er, -en or combinations thereof proves so productive? Data for ENHG tag, e.g., show a g-plural, an umlaut plural, an en-plural, in addition to an e-plural plus or minus umlaut. Again, we find some sort of general consensus, with, however, incomplete specific and convincing systemic insights. Thus, for example, in pitting the ENHG e-desinence against the ENHG er-plural Gürtler (1913: 84) observes: 'Ich erblicke den grund für die ablehnende haltung mancher der ... wörter dem -er-plural gegenüber ... vielmehr in der wirkung des a-plural's', i.e., Gmc. ā-stems, viz., ENHG e-plural. Natural Phonology, too, would judge the ENHG e-desinence as tending toward stability, in which 'dominant paradigm structure conditions tend to effect a strict linking of inflectional class membership to the phonological and/or semantico-syntactic properties of words', as Wurzel (1987: 81) writes. The polysemy of the er-desinence is well-known; besides serving as a noun plural suffix, it functions also as a derivational noun agent/instrument suffix, e.g. Schreiber 'writer', the adjective comparative suffix, e.g. besser 'better', and as an adverb building suffix, e.g. lauter 'only'. Nevertheless, the e-suffix itself is not unambiguous. Phonological constraint is invoked by Gürtler (1913: 83): 'Es ist zuzugeben, daß bei neutralen stämmen mit auslautendem -r (jahr, rohr u.a.) der -er-plural möglicherweise nur aus gründen des wohllautes vermieden wurde'. Yet in exhaustive research on the er-plural theory through the centuries of ENHG and on reflexes in earlier centuries, Gürtler (1912, 1913) is able to document ample data of stems in -r which attest to an r-plural, e.g., OHG harir 'hair'; OHG tiorir, MHG tierer 'animals' (1912: 502, 509); ENHG feurer 'fires', röhrrer 'reeds' (1913: 71). Most interestingly, but susceptible to challenge, Molz (1906: 348) suggests a counter-iconicity, counter-naturalness explanation for the low functional load of r-plural for stems in -r, attributing it to 'das bemühen, seltener wörter durch die pluralische form nicht zu sehr von dem sing. zu trennen'.

Upon taking into consideration the wealth of ENHG data, as well as labyrinthine theories concerning the proliferation, in

particular the intricate paradigm structure conditions of the ENHG plural morpheme, Wegera (1987: 283-4) appeals to the prestige of the so-called Luther -e propelled by socio-political factors of the East Middle German speech area. Admittedly such teleological explanations are but one of Wegera's foci, yet they are reminiscent of extralinguistic approaches rather than internal linguistic rules delimiting the ENHG period. To be sure, ENHG is a composite of competing dialects as compared to prescribed Standard Modern German; the extrapolation to dialect-general or supradialectal features is especially recalcitrant, thus feeding the appeal to the insights of universal and natural grammar. It would seem that a natural pragmatics ought also be explored, both language-dependent and language-independent, which might, for example, consider possible discourse distinctions within and across ENHG dialects. As a case in point, the East Middle German, specifically Upper Saxon ENHG ø-plural kunig beside köig 'kings' ought be investigated for the discourse implications of the umlaut (and/or) the non-umlauted alternative. Similarly, Upper Saxon ENHG plural tag beside tage may evince language-general as well as language-specific pragmatic effects of clipping, contraction, ellipsis and the like in various levels of usage and text situations.

We return to a phonological why for the resurgent strength of ENHG -e plural. Natural Phonology, in particular syllable theory, relies on the Jespersen-Saussure sonority scale, which rates the phones of language from the most sonorous (a) to the least sonorous (p, t, k). How does the ENHG weakly stressed plural -e compare phonologically with the weakly stressed plural -er and -en suffixes? In terms of sonority assignment, it is not necessary to reconstruct the exact possible phonetic value of these weakly stressed suffixes, i.e., whether the set [ə, ɔr, ɔn] or the set [ə, ʌ, p-ɔn] are at stake. Both sets of alternatives exhibit the same relative sonority relationships. Thus, the nasal closes the suffix syllable with less sonority than the liquid, both being less sonorous than the open suffix syllable in bare schwa. The relative sonority of [ə] compared with [ʌ] under weak stress can be established by the syllable producing ability of [ə]; German [ʌ] never displays epenthesis.

In dialect, [ʌ] can be heard in strongly stressed syllables, e.g., hAbAt 'Herbert' [personal communication Gerald F. Carr], while [ə] does not occur under strong stress. General phonetics and cross-linguistic evidence corroborate its unmarked sonority among weakly stressed vowels as well. Thus Heffner (1952: 109) writes: 'The vowel [ə] is as nearly an unarticulated [neutral] vowel sound as is to be found in human speech' and Ladefoged (1982: 30) claims 'By far the commonest unstressed vowel is [ə]'. To cite but one contrasting language, English tolerates [ʌ] but not [ə] in strongly stressed syllable.

The Cə syllable which emerges from the ENHG e-plural is particularly felicitous in terms of syllable theory. The Cə-suffix represents the instantiation of the Jakobsonian preferred syllable structure, CV. In Natural Phonology CV can be abstracted to a level of WS (weak strong) in which strong represents not only the V but also the coda (including phantom consonants such as the possible interpretation of ɪ as VC). In distinction to all strongly stressed syllables of German, Wiese (1986: 6) maintains that 'der Silbenkern, der [ə] dominiert, (besitzt) nicht VC-Struktur, sondern (besteht) nur aus einem V-Element'. The favorable unmarked status of the CV e-plural suffix is further corroborated by preferred syllable structure laws such as Vennemann's (1986) 'Endrandgesetz' and Hooper's (1976) 'Optimal Syllable Principle'.

It is necessary to exploit the preferred CV status of the ENHG e plural suffix in another direction. The language-specific articulation basis of German is characterized, among other features, by relatively strong muscular tension and air pressure (cf. Rauch 1975). This is at least partly accountable for the possibility of weakly stressed CV syllables which are [ʔə] (e.g., in Gauē 'regions', Rehe 'deer' pl.) feeding the unmarked syllable preference CV. Indeed, according to Giegerich (1985: 46), syllabification generally in German is without syllable overlap and is perceived as such by the native speaker.

The phonological basis, and thus the prosodic system, of German certainly supports non-monosyllable forms; this is evinced by the inflection and derivation habits of German, which interdigitate with the entire grammar including, of course, the suprasegmentals. Simply illustrated, German

supports the bisyllabic Tage, as opposed to, say, the English monosyllabic days. German is still, as was its ancient Indo-European ancestor, a suffix language. In fact, Wurzel (1975: 228) analyzes the German noun stem singular in such a way that he can derive 'die wichtige Generalisierung, daß alle normalen nativen Morpheme einsilbig sind'. Augst (1975: 36) characterizes German nouns constrained in the selection of a plural marker neither phonologically nor by gender as 'die am häufigsten gebrauchten Wörter in der nhd. Sprache. Weitaus die Mehrzahl dieser Wörter ist einsilbig...'. Just as affixation is primary in Indo-European morphology and ablaut ancillary to it, suffixation retains primacy over vowel alternation/modification in the inflectional number morphology of German throughout its history (cf. further Rauch 1972).

Neither umlaut nor vowel length plays the decisive distinguishing role in Standard High German noun number. With relatively few exceptions, umlaut acts as a redundant feature in NHG noun pluralization. Although still productive in ENHG as a plural marker on monosyllable without suffix, e.g., Täg, Bänck 'bench', umlaut is germane to dialects which favor e-apocope, viz., the Upper German dialects. The German system-favoring suffixation can be another factor, then, in helping to explain why neither the ə-plural (which is least iconic) nor the umlaut-plural, is individually exploited to the extent that the -e plural marker is in ENHG. We reiterate, the suffixation is suprasegmentally system-congruent. Parallel apocope-like phenomena, such as contracted forms of verbs like haben 'have', stehen 'stand', gehen 'go', are also familiar to e-apocope favoring dialects. Thus, Giessmann (1981: 34) shows the dialect groups, West Middle German, East Middle German, and North Middle German (as opposed to East Upper German and West Upper German) to overwhelmingly favor bisyllabic stehen and gehen by the seventeenth century. Certainly, the various speculations attempting to predict the future path of the NHG plural marker need to recognize the articulatory basis of German as a factor to be reckoned with, in particular relative to the native monosyllabic morphology. The parsimony of language, i.e., linguistic iconism, aims for one plural marker; it must, however, also satisfy the often theoretically neglected

suprasegmental structure of German at the given time in history.

The -e morpheme dominance of the masculine and neuter (-en the feminine) plural in NHG (Augst 1975: 38) is no reason to reconstruct the restored ENHG e-plural as an internal linguistic hallmark delimiting the ENHG language period; the NHG situation is a result, not a cause. ENHG differs from both MHG and NHG in that the plural marker is by far less constrained; this is indeed the source of the -e plural phenomenon. As shown above the language-specific naturalness of ENHG, its articulatory basis, segmental and suprasegmental phonology, morphology, syntax, semantics, aided by principles from universal grammar, yield teleological evidence for the emergence of the ENHG e-plural.

Linguistic naturalness finds a strong basis in the phenomenological category of the philosopher/semiotist Charles Sanders Peirce most commonly called iconism, but deriving, in fact, from Peircean Firstness. From Firstness obtains the factual similarity between the sign and its object which is the dynamic of iconicity as in Mayerthaler's (1987: 48-9, 52) 'principle of iconicity'. However, Firstness is no less represented in Mayerthaler's other system-independent morphological markedness principles of 'uniformity' and 'transparency'; in essence both of these work iconically and should be understood as such. Linguists tend to seize upon Peircean iconicity without fully exploiting his paradigm. In particular, Peirce's Secondness and Thirdness, which have correlations in the index and the symbol, respectively, are unfortunately by and large ignored or perhaps overlooked (cf., however Rauch 1983). While Firstness phenomena represent mere possibility, Secondness involves compulsion and Thirdness convention. None of these categories exists in isolation in a semiotic system; they are, nevertheless, isolable by their predominance in a particular linguistic sign.

Within this framework the ENHG -e which undergoes widespread apocope in consonance with the Germanic drift to end-syllable weakening displays Firstness. Although the apocope is system-congruent with the stress accent, it is the stress accent which coerces the e-loss and accordingly evinces the factual contiguity of Secondness. The

reinstatement of the so-called apocopated-e in the seventeenth century is certainly a matter of Firstness and of Secondness, as argued by means of the naturalness rules discussed above. Yet, its return is so astounding because the e-plural is not the 'apocopated -e' at all; the phonological end-syllable weakening has, in fact, not reversed itself. The phonology of the e-apocope is subverted in that the seventeenth century -e noun element becomes conventionalized as a plural marker, if not the plural marker. The restructured ENHG e-plural marker thus embodies, in particular, the unexploited rule of the imputed contiguity of Peircean Thirdness, i.e. convention or law, which provides the necessary condition for the motivation of restructuring.

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Wide Content: a Cognitive Illusion

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Abstract. This paper argues against Putnam's thesis of Wide Content. Section 1 summarizes Putnam's first argument for Wide Content (the Twin-Earth argument), and then criticizes it for its consequences concerning universally quantified sentences. Section 2 summarizes Putnam's second argument for Wide Content (based on the Sociolinguistic Hypothesis), and then criticizes it for ignoring facts about synonymy. Section 3 concludes on a constructive note. It suggests that, if the thesis of Wide Content seems to have any appeal at all, it is because language-users cognize in terms of a certain principle of polysemy.

0. Introduction

Traditional theories of meaning used to assume the internalist thesis of Narrow Content:

(NC) Knowing the meaning of a term is just a matter of being in a certain psychological state.

However, in the extremely influential "The Meaning of 'Meaning'", Hilary Putnam argues for the externalist thesis of Wide Content:

(WC) Knowing the meaning of a term is not just a matter of being in a certain psychological state.

According to Putnam, the extra-mental, or wider, environment enters into meaning.¹

Putnam's arguments for Wide Content involve the celebrated "Twin Earth" scenario and the Sociolinguistic Hypothesis. I shall address each argument in turn, explaining why I think they fail. In the final section I suggest that if Putnam's position may initially seem plausible it is because of an illusion produced by the way human beings tend to cognize polysemy.

1. Indexicality

Putnam starts off with a thought-experiment involving "Twin Earth": Imagine a planet exactly like Earth except that instead of H₂O it contains XYZ. XYZ and H₂O have the same appearance, taste, and nutritional value

under normal conditions, but they are chemically distinct. In a universe where Earth and Twin Earth both exist:

- (1) The word water as uttered by us has a different meaning from the word water as uttered by our counterparts on Twin Earth.

Assuming (1), it follows that the thesis of Wide Content is right: by hypothesis, you and your counterpart on Twin Earth have identical mental states, and yet your utterances mean different things. Therefore non-psychological factors must contribute to meaning.

However, it is by no means obvious that intuition (1) is well-founded. For example, an alternative to accepting (1) is to say that the meaning of an utterance of water depends solely on the beliefs of the speaker. For someone who associates water with the concept "H₂O", water refers to H₂O and only to H₂O. For someone who associates water with (say) "clear tasteless healthy drink", water is a general concept whose extension includes H₂O, XYZ, and any similar substance in the universe. In either case, if you and your counterpart on Twin Earth have identical mental states, then you and your counterpart mean exactly the same things.

Putnam rejects this narrow-content approach because he holds the following:

- (2) Water, along with most other terms, is an indexical (its meaning contains an implicit indexical element).

By this, Putnam means that the extension of water depends on an ostensive reference to a canonical sample:

Suppose I point to a glass of water and say "this liquid is called water"... My "ostensive definition" of water has the following empirical presupposition: that the body of liquid I am pointing to bears a certain sameness relation ... to most of the stuff I and other speakers of my linguistic community have on other occasions called "water". [141]

Putnam has two kinds of motivation for holding this. One has to do with realism in the philosophy of science; this I address elsewhere (Saka 1988b). Putnam's second motivation for holding (2) involves the psychological state of the speaker. This is clear because Putnam talks about intentions:

The rigidity [and presumably the indexicality] of the term "water" follows from the fact that when I give the ostensive definition "this (liquid) is water" I intend [a rigid reading¹ and presumably an indexical reading as well].² [149; see also 156]

Furthermore, Putnam talks about the properties that people are interested in:

x bears the relation sameL[liquid] to y just in case (1) x and y are both liquids, and (2) x and y agree in important physical properties ... Importance is an interest-relative notion ... thus, in one context "water" may mean chemically pure water, while in another it may mean the stuff in Lake Michigan. And [molecular] structure may sometimes be unimportant; thus one may sometimes refer to XYZ as water if one is using it as water. [157]

But when Putnam points out that one can intend to use a term as an indexical, he doesn't demonstrate that they succeed in using it as an indexical. In short, these passages do not provide an argument for maintaining (2).

At the same time, there are many cogent reasons for denying (2). (For some that are already established in the literature, see Kent Bach 1987: ch 13.) My first argument involves the "sameness" relation. Putnam construes the sameness relation in terms of scientific properties (in terms of microstructure); consequently, the extension of a term does not change just because our theory of its referent changes. (Thus, according to Putnam the extension of water was exactly the same in the Dark Ages as it is now.³) This picture of the sameness relation drives Putnam to the following theory of why certain terms designate natural kinds while others do not: sodium chloride must refer to a natural kind because the canonical samples of sodium chloride happen to constitute a natural kind; in contrast, jade refers to a disjunctive kind because the canonical samples of jade come in two chemically distinct forms, jadeite and nephrite. (For an argument charging this account with circularity, see Bach 1987:282f.)

However, Putnam's account is contradicted by facts pertaining to sugar. Sugar has at least two senses: it can refer to sucrose specifically and it can refer to a family of substances including both sucrose and glucose. Glucose, in turn, has at least two senses: it can refer to a right-handed stereoisomer (known as dextrose or D-glucose); and it can refer to a left-handed stereo-

isomer, which does not naturally occur. Left-handed glucose tastes sweeter than D-glucose, and is less easily absorbed by the human digestive system (Gardner 1964: ch 13). Since glucose naturally occurs only in the right-handed form, D-glucose constitutes the canonical sample, the variety that most other speakers have traditionally referred to. Therefore Putnam's theory predicts that glucose must refer specifically to D-glucose. Yet in actuality glucose is a cover term encompassing both "dextrorotary" and "levorotary" varieties.

My second reason for rejecting the claim about indexicality (2) is that Putnam's theory leads to false claims about universal quantification. First I shall explicate Putnam's formulation of the content of "This is water" and then I shall show how the approach fails when it's extended to other sentences.

For the simple sentence "This is water", Putnam explicitly provides an extensionally equivalent sentence in formal logic:

- (3) (For all possible worlds W) (For all x in W) (x is water iff x bears the relation "same [type]" to the entity referred to as this in the actual world). (cf 149)

For the sake of brevity, I will disregard the bit about possible worlds and I will use the constant c to designate the referent of this. Thus, (3) abbreviates to (4):

- (4) $(\forall x) (x \text{ is type-identical to } c \text{ iff } x \text{ is water})$

As it stands, (4) circuitously characterizes "This is water" by using the word water. According to what Putnam says elsewhere (131), we can eliminate the reference to water by replacing "x is water" with "x is of the same type as canonical sample w", where w is determined indexically/ostensively. Therefore the meaning of "This is water" amounts to "Whatever this is, it's the same as water":

- (5) $(\forall x) (x \text{ is type-identical to } c \text{ iff } x \text{ is type-identical to } w)$

(The reason that [5] contains a biconditional is that [5] is meant to represent "This is water" in its definitional sense. In its predicational sense, "This is water" would be equivalent to the one-way conditional $(\forall x) [x \text{ is type-identical to } c \rightarrow x \text{ is type-identical to } w]$.)

Now let's consider the case of an early chemist

like Antoine Lavoisier. Lavoisier does not initially have "H₂O" as part of his concept of water, but eventually he finds a sample c, which he takes to be water. He thus believes (5) "This is water" ($(\forall x) (x \text{ is type-identical to } c \text{ iff } x \text{ is type-identical to } w)$). After performing electrolysis on sample c he concludes that it consists of H₂O, thus believing of c "This is H₂O":

(6) $(\forall x) (x \text{ is type-identical to } c \rightarrow x \text{ is H}_2\text{O})$

From the predicate-calculus sentences (5) and (6) it follows that all water is H₂O:

(7) $(\forall x) (x \text{ is type-identical to } w \rightarrow x \text{ is H}_2\text{O})$

Yet (7) "All water is H₂O" does not follow from the original sentences, in natural-language, that (5) and (6) are supposed to represent. Certainly the beliefs "This is water" and "This is H₂O" might suggest that "All water is H₂O", just as "This is jade" and "This [same sample] is jadeite" might suggest that all jade is jadeite. But the inference about water requires an inductive leap which may or may not prove justified, just as the inference about jade does not demonstrably follow. By no means is it the logical consequence that Putnam's theory makes it out to be."

Another way of looking at it is the following. Suppose that Lavoisier claims all water is H₂O. If he means "all H₂O is H₂O", then the claim is empty. If he means "all substances that are identical to the sample here in my lab is H₂O", then he would have said "This sample of liquid is H₂O"; and clearly we take his claim as having wider significance than that. It's only if Lavoisier means something like "all clear tasteless healthy drinks are H₂O" does his claim have particularly interesting content.

The same problem occurs for "This water is H₂O", which seems to break into two claims: (i) this refers to water and (ii) it consists of H₂O:

(8) $(\forall x) (x \text{ is type-identical to } c \rightarrow x \text{ is type-identical to } w) \ \& \ (\forall x) (x \text{ is type-identical to } c \rightarrow x \text{ is H}_2\text{O})$

Once again, according to Putnam's theory "This water is H₂O" means all water is H₂O. Therefore the theory cannot be right.

Conducting Putnam's thought-experiment with constructions like "This is water" in mind naturally leads one to believe (1). But that is due to the explicit article in the sentence and not to any implicit

indexical built into the noun. Otherwise universally quantified statements would also have an indexical element, and they do not.

2. The Sociolinguistic Hypothesis

Another argument for holding Wide Content comes from the following reductio ad absurdum about flora:

- (9) One can be in the same mental state regarding beech and elm (one can have identical mental representations for beech and elm). This is true for individuals who simply think of beeches and elms as common deciduous trees and who do not know the difference between the two.
- (10) According to the thesis of Narrow Content, it follows from (9) that beech and elm mean the same thing.
- (11) But beech and elm have different meanings. Therefore the thesis of Narrow Content is false.

If one's mental state does not determine meaning, what then does? Again, says Putnam, it is the wider environment, in this case the social environment. Thus, the Sociolinguistic Hypothesis:

Every linguistic community ... possesses at least some terms whose associated "criteria" are known only to a subset of the speakers who acquire the terms, and whose use by the other speakers depends upon a structured co-operation between them and the speakers in the relevant subsets. [146]

However, the argument for the Sociolinguistic Hypothesis falls through because Putnam fails to establish premise (9).

Suppose that the definition of elm is something like "common deciduous tree that others (experts) call elm" and that the definition of beech is "common deciduous tree that others (experts) call beech". Then the narrow content for beech and elm would in fact be slightly different from each other, just different enough for one to know that they may have distinct extensions without knowing precisely what the extensions are. (By the way, these definitions are not circular. The left-hand sides of the definitions refer to the meaning of a word while the right-hand sides refer to its form. See also Bach 1987:159f.)

Not only does this "nominal-description theory"

contradict the assumption that (9) is possible, but there is empirical support for it. According to the received view in linguistics, languages do not tolerate absolute synonymy. If two synonyms somehow come to co-exist in the same language, at least one of them will diverge in meaning. For example, the Norman Conquest brought French speakers to Britain, along with the word pork. As Anglo-Saxon and Norman merged, the synonyms pig and pork quickly acquired differences in meaning (Breal 1897). The reason why this happens is explained by Eve Clark (1987). Clark points out that children acquire words only by a gradual process of hypothesis-formation and hypothesis-revision. For example, while a child might initially conceptualize dog as "dog" and animal as "animal" the child might just as well hypothesize the opposite, or infer that both mean "dog", or infer that both mean "animal". But of these four logical possibilities, children follow only the two non-synonymous patterns. In this way, the absence of synonymy facilitates the acquisition of vocabulary. But it does so only if humans cognize non-homonyms as non-synonyms. Clark calls this The Principle of Contrast: "Every two forms contrast in meaning... [It] has been stated or assumed by virtually every linguist over the years" (p2; see also p24 for references to other linguists). Thus, contrary to Putnam's assertion, there is empirical evidence that any two formally distinct words must have different concepts. (Also see Saka 1988a for a theory of how form contributes to non-truth-conditional content.)

It's true that there are a couple of possible arguments for maintaining (9) as against the Principle of Contrast. However, they do not pose genuine difficulties.

The first one centers on the notion of translatability. In a nutshell:

- (12) The Principle of Contrast entails the general impossibility of translation.
- (13) Translation is not impossible.
- (14) Therefore the Principle of Contrast is incorrect.

If the Principle of Contrast is correct, then translation would be impossible (except where words from two different languages happened to share the same form as well as the same extension). For example, according to the Nominal-Description Theory as applied to natural-kind terms, a monolingual speaker of German might conceptualize Ulme as "common deciduous tree that others (experts) call Ulme". Thus, the definitions of elm, beech, and Ulme would be on a par with each other: they are equally similar in that they mean "common deciduous

tree" and yet their nominal descriptions make them ultimately distinct. Since there is nothing to show that elm and Ulme are in fact coreferential, they cannot be translations of each other.

The problem with this argument is that it equivocates over the notion of translation. If translation means "good enough for practical communication" or "good enough for government work" then (12) is false. The reason we believe that communication is usually successful is that in many cases our words have the same reference even when they have different senses. Just as Hesperous and Phosphorous are related to each other intralinguistically, so elm and Ulme are related cross-linguistically. Moreover, the conceptual content of elm and Ulme are so similar that -- excluding the meta-linguistic content "which others call x" -- even in opaque contexts they are intersubstitutable. Therefore, if translation is meant in a weak sense, the argument is unsound.

If translation is meant in a strong sense, the argument fails because, contrary to (13), it is in fact impossible to replicate one's concept in another's mind perfectly. For example, a native German speaker's concept of Ulme differs from the concept a non-native speaker would have. In the case of a non-native speaker, (15) or even (16) would represent the best definitions ([15] for a fluent speaker and [16] for one who still "thought in" English).

(15) Ulme = "common deciduous tree that others (experts) call Ulme, equivalent to what yet others call elm"

(16) Ulme = "equivalent to what others call elm"

Perhaps the point is more obvious if we stick to English. It's possible for someone to think of a wildcat as "a small undomesticated kind of cat called wildcat" and to think of a bobcat as "a small undomesticated kind of cat called bobcat". However, this person would assume that the two were distinct unless given specific reason to think otherwise. Maybe this has happened to you, when you suddenly learn that two terms refer to the same thing. The surprise you feel shows that you initially presumed the terms had distinct referents. Therefore the argument about translatability does not contradict the Principle of Contrast. In conclusion, assertion (9), the key to maintaining the Sociolinguistic Hypothesis, is false; hence the argument for Wide Content breaks down.

There is another possible argument in support of (9), this one due to Putnam. Suppose that some speaker S associates elm with "a common deciduous tree that

experts call elm". Suppose also that there are two different communities; in one the experts say that elm refers to elms, while in the other the experts say that elm refers to beeches. According to Putnam, the meaning of elm, as uttered by S, depends on which community S is from (or in). However, Putnam gives no argument for preferring his analysis over an analysis which simply takes the reference of elm as indeterminate. Unless speaker S has beliefs about which experts to rely on, it doesn't matter whether the two communities are overlapping, adjacent, or separated from each other by the interstellar void; S simply doesn't know the complete meaning of elm.

3. Intuition

A final motivation for holding (1) is that, for many people, it seems so intuitively true. Putnam says:

what have been pointed out in this essay are little more than home truths about the way we use words ... [the topic deals with] matters concerning which we have, if we shed preconceptions, pretty clear intuitions. [193]

However, it is far from intuitively true for others. (For discussion, see Unger 1983.) Now I would like to sketch an account of why (1) might have intuitive appeal. Specifically, I propose that (1) is just a cognitive illusion that is a special case of the following general principle.

Principle of Polysemy: Suppose that a general concept C is superordinate to concepts c1, c2, c3... such that c1 is cognitively the most important of the subordinate concepts to speakers of language L. Suppose furthermore that language L lexicalizes either C or c1 in the form F. Then when speakers of language L come to find that C is a useful concept that subsumes some useful distinctions, the following will happen: (a) F will become polysemous so that it refers both to C generally and to c1 specifically; (b) c2, c3, etc will be referred to as kinds of F's; and (c) c1 will be thought of as the real F.

Let me illustrate. The concept "screwdriver" is general in that its extension is superordinate to the extension of the following concepts:

c1 = "tool for adjusting standard-slit screws"

c2 = "tool for adjusting hexagonal screws"
 c3 = "tool for adjusting phillips screws"

Let's suppose that at some initial time the meaning of screwdriver was C, "tool for tightening/loosening screws". Given the actual environment of the early users of screwdriver, for all practical purposes C and c1 were coreferential. For this reason, the early users never considered it important to distinguish between C and c1. Therefore, screwdriver meant C and only C. Once hexagonal and phillips screws were invented, however, the general and specific concepts all became more commonly cognized. Since flat screwdrivers are the most frequently encountered kind of screwdriver, the word screwdriver all by itself can refer to flat screwdrivers specifically. Therefore, in accordance with the Principle of Polysemy: (a) the label screwdriver became attached to two concepts, C and c1; (b) c2 became lexicalized as hex-nut driver and c3 became lexicalized as phillips screwdriver; and (c) a screwdriver for standard-slit screws may be referred to not only by the technical name flat screwdriver, but also by the locutions ordinary screw-driver, regular screwdriver, and real screwdriver. The important point is that many speakers consider flat screwdrivers the only genuine kind of screwdriver.⁵

(The Principle of Polysemy partly follows from the Gricean principles of informativeness and brevity. In general [unless, say, you are pointing], if you say screwdriver when you want a phillips screwdriver, you are not being informative enough. On the other hand, in general [unless, say, you are drawing attention to some contrast], if you say flat screwdriver when you mean the most salient kind of screwdriver, you are being too verbose: it suffices to say screwdriver.)

Now I am proposing that water initially meant something like C, "clear, tasteless, healthy drink". Those of us who have been exposed to the barest of chemical theory also cognize c1, "H2O". However, so far as most of us are aware, C and c1 are coreferential. Thus we do not lexicalize C and c1 differently. Those of us who have been exposed to the further concept c2, "XYZ", learn that C is a superordinate concept of c1 and c2; and if we want to talk about the difference, the Principle of Polysemy applies: (a) water comes to refer to either C or c1; (b) c2 acquires a new name, perhaps Twin-Earth water, waterTE, or XYZ; and (c) only c1 will be thought of as real water.

4. Summary

Putnam has argued that the meaning of a term may depend on one's wider environment. His first argument

was that the meaning of water depends on the nature of one's surrounding fluids. In reply to this, I offered two major counterarguments. First, if Putnam is right about the role of canonical samples, his theory would predict that glucose refers specifically to right-handed sugar. However, it refers to both stereoisomers. Second, if Putnam is right about the meaning of "This is water", then the meaning of "This is water and it's H₂O" would be identical to the meaning of "All water is H₂O". Of course, it is not.

Putnam's second argument was that the meaning of elm depends on the knowledge possessed by other speakers in one's linguistic community. Contrary to Putnam's bald assertion, I pointed out that practicing linguists have found reason to posit the Principle of Contrast: the form of a word contributes to its meaning.

Finally, I have proposed that there is a general cognitive/linguistic tendency by which, given a concept C and a subordinate concept c₁, speakers think of c₁ as the only legitimate concept for a label when in fact the label is polysemous between c₁ and C.

Notes

For valuable discussions of wide content, I would like to thank Rob Cummins, Bernard Kobes, Shaun O'Connor, and especially Jean Kazez.

1. In Putnam's terms: wide content is the content that you can specify by talking about mental states only in the wide sense, ie non-solipsistically.

2. Strictly speaking, indexicality and rigidity are independent properties. Putnam's characterization of the meaning of "This is water" ([3] in this paper) can actually be separated into distinct claims about (i) rigidity and (ii) indexicality:

(i) (For all possible worlds W) (for all x in W)
(x is water iff x has property such-and-such
in the actual world).

(ii) (Vx) (x is water iff x has the property of bearing
the relation "same" to the denotatum of this).

The property mentioned in (i) may but need not be indexical, and the indexical in (ii) may but need not be rigid.

It is because Putnam equates the two (152) that I interpret this passage from page 149 in the way I do.

3. Putnam says: "the extension of the term water was just as much [restricted to] H₂O on Earth in 1750 as in 1950"

(141). If this claim has any appeal whatsoever, I think it is due to equivocation. On the one hand, "on Earth" can restrict the universal quantifier:

($\forall x$: x on Earth) (x is water iff x is type-identical to H₂O)

According to this interpretation, Putnam's claim is true in the same sense in which "the extension of the term bear is restricted to polar bears in the Arctic" is true. For this reason Putnam's claim seems reasonable.

On the other hand, "on Earth" can appear as part of the biconditional:

($\forall x$) (x is water iff x is type-identical to H₂O on Earth)

It is only this reading that is relevant to Putnam's position, and it is by no means obviously true.

Note, incidentally, that if the microstructure of an entity determines its membership in a term's extension, then saying "That's a cow" should be true when you point to a petri dish with a single bovine cell -- a most peculiar consequence.

4. While my sentences in the predicate calculus are extensionally equivalent to the corresponding sentences of natural language, they do not fully capture the meanings of the natural-language sentences because they omit stereotypes. They omit stereotypes for two reasons. First, it's impossible to express defeasible properties (as Putnam conceives of stereotypes) in the predicate calculus. Second, omission of the stereotypes does not affect my argument.

It's true that stereotypes are generally important in evaluating belief-sentences. However, my story about Lavoisier stipulates that Lavoisier believes c belongs to the same type as w. Thus, even if he associates different stereotypes with c and w, in ascriptions of belief to him c and w should remain intersubstitutable *salva veritate*.

5. There is an apparent exception to the Principle of Polysemy: animal may refer either to all members of the animal kingdom or to beasts as opposed to humans. Yet "human" is the most important subordinate category. Doesn't the Principle of Polysemy predict that the expressions a real animal, a normal animal, and animal simpliciter should refer specifically to human beings? I think not, for the following reason. The Principle of Polysemy refers to a time when speakers come to find that a general concept subsumes some useful distinctions; but there was probably never a time when humans decided/discovered that the distinction between humans and non-humans was useful.

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Prepositions in Persian
and the Neutralization Hypothesis

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0. Introduction

This paper discusses the categorial nature and the syntactic function of prepositions in Persian. Based on their syntactic characteristics, in particular Case-assigning and Case-receiving properties, it is argued that, in Persian, two subgroups of prepositions must be distinguished in terms of a syntactic feature classification. The first group, (P1), is truly prepositional in nature and can be characterized as [-V, -N] given Chomsky's (1970) syntactic feature system; P1s are direct Case-assigners, must be strictly subcategorized for an object NP, and cannot occur in Case positions. The second group, (P2), displays some nominal properties and is distinct from P1 in a number of ways; P2s cannot assign Case directly and require the occurrence of a dummy case marker before the right branching object NP, they subcategorize for an optional object NP and can occur in Case-positions. Using Van Riemsdijk's Neutralization Hypothesis (1983), I propose that, in Persian, P2s are neutralized in their [-N] feature.

1. Differences Between the Two Groups of Prepositions

1.1. The first group of prepositions, (P1), are strictly subcategorized for an object noun phrase, illustrated in (1-3). In general, these are prepositions which precede the dative, benefactive, locative, goal, and source arguments to the verb. In languages with a rich morphological case system, these arguments often receive overt case marking.

1. a. be 'to'
- b. az 'from'
- c. bar 'on'
- d. dar 'in/on/at'
- e. tā 'to/until'
- f. bi 'without'
- g. bā 'with'
- h. barāye 'for'

2. a. raft be N.Y. '(he) went to N.Y.' 3. a. *raft be

- b. āmad az N.Y. '(he) came from N.Y. b. *āmad az

The second group of prepositions, (P2), on the other hand, subcategorizes for an optional object noun phrase. When occurring without an object NP, the construction closely parallels the verb-particle construction in English, illustrated in (4-6).

- | | | |
|------------|--------------------------|----------------|
| 4. a. bālā | 5. raft bālā-ye deraxt | 6. raft bālā |
| 'up' | '(she) went up the tree' | '(he) went up' |
| b. tu | āmad tu-ye otāq | āmad tu |
| 'in' | '(she) came in the room' | '(he) came in' |
| c. zir | boro zir-e āb | boro zir |
| 'under' | 'go under the water!' | 'go under!' |

1.2. Another important difference between the two groups is that P2s display some morphological nominal characteristics. For example they may take an NP specifier (7) and a plural marker (8), while P1s may not.

- | | | |
|----------------|---------------|------------------|
| 7. a. in ru | | |
| | this top | 'up here' |
| b. *in dar | | |
| | this in | |
| 8. a. un zir-ā | | |
| | that under-Pl | 'way down there' |
| b. *un bar-ā | | |
| | that on-Pl | |

It should be pointed out that semantically the plural morpheme functions as an intensifier with group 2 prepositions rather than a marker of plurality.

1.3. However, the most significant difference between the two groups of prepositions regards the occurrence of a dummy case marker, Ezafe, in the prepositional phrase. Ezafe refers to a morpheme, -e, which occurs before the right branching complement or modifier in the noun phrase (9) and in the adjective phrase (10). In the noun phrase, when there are more than one complement and modifier following the head noun, each is preceded by a separate occurrence of Ezafe, illustrated in (9.d).

- | | | |
|--------------------------------|----------------|---------------|
| 9. a. xune-ye ² Sam | | |
| | house-Ez Sam | 'Sam's house' |
| b. xune-ye kučik | | |
| | house-Ez small | 'small house' |
| c. xune-ye kenār-e-daryā | | |

house-Ez on-the-beach 'house on the beach'

9. d. xune-ye kučik-e kenār-e-daryā-ye Sam
'Sam's small house on the beach'
10. montazer-e Sam
waiting Ez Sam 'waiting for Sam'

As expected, all prepositions in group 1 do not allow the occurrence of Ezafe, evident in (11).

11. a. be N.Y. b. *be-ye N.Y.

However, group 2 prepositions require --in some cases optionally so-- the occurrence of Ezafe before the object noun phrase.

12. a. ru-ye miz
on-Ez table 'on the table'
- b. pošt-e miz
behind-Ez table 'behind the table'
- c. zir-e miz
under-Ez table 'under the table'

Given an analysis of Ezafe as a dummy Case assigner, Samiiian (1986), we can conclude that P1s are direct Case assigners while P2s are not.

1.4. Furthermore, a cluster of differences between the two groups emerges as a direct result of their Case assigning properties and can be explained by using the Case Resistance Principle, proposed by Stowell (1980).

13. CRP: a phrasal category headed by a Case assigner cannot occur in Case positions.

Given the CRP, we would expect that prepositional phrases headed by P2s occur in Case positions but the occurrence of prepositional phrases headed by P1s in case positions result in ungrammatical sequences. This prediction is indeed borne out in nominative, accusative and genitive Case positions, illustrated below.

1.4.1. Nominative Case positions: Prepositional phrases headed by P2s can occur in subject position but prepositional phrases headed by P1s cannot.³

14. a. tu-ye ganĵe kasif-e
in-Ez closet dirty-is

- 'It is dirty inside the closet'
 b. *dar ganĵe kasif-e
 in closet dirty-is

1.4.2. Accusative Case positions: Prepositional phrases headed by P2s can occur as object of the verb or object of a P1 preposition. P1s, on the other hand, cannot occur in such positions.

15. a. [tu-ye ganĵa]-ro tamiz-kard-am
 in Ez closet-AccM cleaned-1st Sg
 'I cleaned inside the closet'
 b. *dar ganĵa-ro tamiz-kard-am
 in closet-AccM cleaned-1st Sg
 16. a. be [kenār-e darya] resid-im
 to next-Ez sea arrived-1st Pl
 'I arrived on the beach'
 b. *be dar manzel-emun resid-im
 to at house-our arrived-1st Pl

1.4.3. Genitive Case position: Assuming that Ezafe is a dummy Case assigner, in the position traditionally associated with the genitive Case, only the occurrence of prepositional phrases headed by P2s leads to grammatical strings.⁴

17. a. aks-e ru-ye miz
 picture-Ez on the table
 'the picture on the table'
 b. *aks-e bar miz
 picture-Ez on table

The cluster of distinguishing characteristics between the two groups of prepositions discussed above indicates that they should not be classified in the same category. In fact, it seems that P1s possess all the properties associated with prepositions, including the ability of direct Case assignment. The question remains as to the categorial nature of P2s. If they are distinct from P1s, what category do they belong to?

2. On the Nominal Status of P2s

A possible attempt at explaining the distinctive characteristics discussed in the previous section would be to assign a nominal status to P2s. Such a proposal

was put forth by Palmer (1971) and more recently by Brame and Karimi (1986). Assuming a nominal status for P2s could provide an explanation for most of the distinctions discussed above. However, P2s display a number of syntactic properties which make them quite distinct from Nouns. In fact, the evidence presented below points out the syntactic and structural similarities between P2s and P1s, which has led traditional grammarians to treat the former as prepositions. In what follows, I will also present a number of arguments focusing on the differences between P2s and Nouns and will conclude that P2s cannot be considered nominal.

2.1. P2s do not take any attributive adjectival or nominal modifiers that nouns take in Persian and most languages (c.f. discussion of Ezafe in the noun phrase 9a-d).

18. a. *ru-ye tamiz-e miz
top-Ez clean-Ez table
'the clean table top'
b. *tu-ye kasif-e ganje
in-Ez dirty-Ez closet
'the dirty inside of the closet'

2.2. Moreover, P2s do not allow the full range of specifiers, such as quantifiers (19), numerals (20), and partitives (21), that is possible in the specifier system of the noun phrase.

19. a. *čand-tā tu-ye ganje
how-many in-Ez closet
b. tu-ye čand-ta ganje
in-Ez how-many closet
20. a. *se-tā zir-e miz
three under-Ez table
b. zir-e se-tā miz
under-Ez three table
21. a. *bištar-e ru-ā-ye miz-ā
most-Ez on-Pl-Ez table-Pl
b. ru-ye bištar-e miz-ā
on-Ez most-Ez table-Pl
'on most tables'

2.3. Unlike nouns which can be followed by a relative clause, P2s do not allow the occurrence of such sentential modifiers (23).

23. a. *tu-i-ro [ke tamiz kard] na-did-am
 in-RM-Acc that cleaned neg-saw-1stSg
 b. tu-ye gan^ye-i-ro [ke tamiz kard]...
 in-Ez closet-RM-Acc that he cleaned

The above arguments indicate that the internal structure of prepositional phrases headed by P2s is radically different from the internal structure of noun phrases. In fact the structure of these phrasal categories (P2P) is totally isomorphic with prepositional phrases headed by P1s --only one NP complement in both cases.

2.4. Finally, it should be pointed out that the occurrence of Ezafe in prepositional phrases headed by some P2s is optional (24). In the noun phrase, however, Ezafe is always obligatory before the phrasal complements under the first bar level. In fact, the deletion of Ezafe in the noun phrase leads to the formation of compound nouns (25).

24. a. tu-(ye) gan^ye
 in (Ez) closet 'in the closet'
 b. ru-(ye) miz
 on (Ez) table 'on the table'
 25. a. kolah-e sabadi
 hat-Ez straw 'a hat of straw' [NP]
 b. kolah sabadi
 hat straw 'a straw hat' [Compound N]

3. Neutralization Hypothesis: An Account of P2 as a Category in Transition

The arguments presented in the previous two sections suggest that P2 is a sort of in-between category, sharing some syntactic properties with 'true' prepositions, P1, and some with nouns. Whenever certain syntactic constructions share the properties of two syntactic categories, the question arises as to how these in-between constructions can be accounted for without introducing excessively powerful mechanisms or relaxing the principles of X-bar theory unnecessarily.

The Neutralization hypothesis has been proposed by Henk van Riemsdijk (1983) to account for a similar situation that arises with regard to the isomorphism between verb phrases and some adjective phrases in German. The heads of these adjective phrases are true

adjectives, morphologically quite distinct from verbs, but the syntactic structure in which they occur is like a verb phrase. Van Riemsdijk proposes that German adjectives are neutralized in their [+N] feature. Under this proposal, rather than fully specified [+N, +V], German adjectives are degenerate constituents of the type [+V], and as [+V] constituents, they are non-distinct from the [+V, -N] category, i.e. verbs. The resulting feature specification for major categories in German following Van Riemsdijk (83) is given in (25).

(25) N: [-V, +N] V: [+V, -N]
 A: [+V] P: [-V, -N]

In this way the similarities between adjectives and the [-N] categories --their case assigning property-- can be accounted for.

Similarly, the Neutralization hypothesis can be used to explain the syntactic behavior of P2s in Persian. In this case, we propose that P2s are neutralized in their [-N] feature, which leaves them with the only feature specification [-V], resulting in the following feature specification for major categories in Persian:

(26) N: [-V, +N] V: [+V, -N]
 A: [+V, +N] P1: [-V, -N]
 P2: [-V]

This provides a principled basis for a categorial distinction between P2s and categories with the [-N] feature, i.e. verbs and prepositions. Subsequently, the inability of P2s to directly assign case can be accounted for since it is only the [-N] categories that can directly assign structural case. On the other hand, since P2s are neutralized in their [-N] feature, they become compatible with the category Noun, with which they share their only designated feature [-V]. This explains those properties that bring P2s closer to nouns, in particular the seeming violation of the Case Resistance Principle. Thus we can conclude that in Persian the Case-assigning categories are the [-N] categories, V and P1; Case-receivers, on the other hand, must be headed by categories that are compatible with nouns, i.e. A and P2.

4. Conclusion

We can conclude that, in Persian, P2 is an in-between category, a category in transition between nouns

and prepositions. Historical data further confirms the nominal source of members of this group of prepositions. The Neutralization parameter provides a principled account of this phenomenon and explains the idiosyncratic behavior of the transitional group of prepositions.

Notes:

1. There are a few prepositions in group 1 that have a final -e: *baraye* 'for', *sare* 'at', and *bedune* 'without'. Historically these originated in group 2, the final -e being the Ezafe marker before the right branching object noun phrase.

2. The Ezafe morpheme (-ye when following vowels) is phonologically adjacent to the preceding element even though its occurrence is dependent on phrasal complement following the head.

3. Safir (1983) and Fabb (1984) point out that in English, while most PPs observe Stowell's Case Resistance principle (i. a-b), some prepositional phrases may occur in case positions, in violations of the CRP, (ii a-b).

- i. a. *Is in June the best time to ski?
b. *from on Monday to on Saturday
- ii. a. Is under the stars the best place to sleep?
b. from behind that tree to beyond that house

4. An exception presents itself with some strictly subcategorized arguments in the noun phrase, just in case no other modifier or complement appears under N-single bar. So, while (i.b) is ungrammatical, (ii) is possible.

- i. a. *bahs-e* *Ali ba Hasan*
 discussion-Ez Ali with Hasan
 'Ali's discussion with Hasan'
- b. **bahs-e* *Ali-ye ba Hasan*
 discussion Ali-Ez with Hasan
- ii. *bahs(-e)* *ba Hasan*

These cases can be accounted for in terms of restructuring (Heny & Samiian 89).

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Echo Question Formation in English*

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0. Introduction. Within the Principles and Parameters Theory, there have been a couple of common but perhaps incorrect assumptions about the analysis of questions which I hope to shed some further doubt on here. The first of these assumptions is that all binder-variable relationships in *wh*-questions are due to *wh*-movement to Comp (in syntax and in LF). The second of these assumptions is that the *wh*-movement analysis completely replaces the earlier unselective binder, or *Q* analysis of Baker (1970).

Contrary to these assumptions, Pesetsky (1987) argues that natural language syntax allows both binding by *wh*-movement, which I will henceforth refer to as M(ovement)-binding, and binding by a somewhat revised unselective binder, much like the *Q* posited by Baker (1970). I will refer to this as Q-binding.¹ In this paper, I will argue that echo questions (EQs) in English provide further evidence for the availability of both M-binding and Q-binding in natural language syntax. First, I will briefly illustrate Pesetsky's modified Q-binding in non-EQs. Then I will turn to EQs and show how Q-binding helps explain some otherwise anomalous-looking but fully grammatical EQs.

1. Two types of binding. As is widely known, while sentence (1a) is well-formed, sentence (2a) is not, at least as a non-EQ:

- (1) a. Who bought what?
 b. [who(1) +WH [t1 bought what]]
 c. [what(2) [who(1) +WH [t1 bought t2]]]
- (2) a. *What did who buy?
 b. [what(2) +WH [who bought t2]]
 c. [who(1) [what(2) +WH [t1 bought t2]]]

(Here and elsewhere, the innermost brackets are IP, and the others are CP, following Chomsky (1986).) Sentence (2) exemplifies what Chomsky (1973) has labeled a Superiority violation. Assume that (1a) has the S-structure (1b) and the (rough) LF structure (1c), and likewise for (2). To account for the Superiority violation in (2), Pesetsky invokes the Nested Dependency Condition (NDC) in (3):

- (3) **Nested Dependency Condition (NDC) (Pesetsky 1987, 105)**
 If two *wh*-trace dependencies overlap, one must contain the other.

(This is only one way of describing Superiority; an alternative would be an ECP account, which I will not deal with here.) Noteworthy here is that the LF structure of (2) violates (3), but that of (1) does not, and hence the Superiority effect. A problem arises, however, with sentences like (5a):

- (4) Which person saw which film?
 (5) a. Which film did which person see?

If we assume that *wh*-movement applies in (5a) as it does in (2), then we have an NDC violation; however, (5a) is grammatical. Pesetsky's proposal is that (5) involves not M-

binding, but Q-binding of the subject NP (and in this case, of the object also), so that its LF structure is (5b):

(5) b. [Q1,2 **which film(2) +WH [which person(1) saw t2]**]

With Q-binding in (5b), there are not two *wh*-trace dependencies. Hence, (5) does not violate the NDC as (2) does; if Superiority violations amount to NDC violations, a Q-binding analysis explains the grammaticality of (5) vs. (2).

At this point, an issue that deserves some attention, more than I will give it here, is this: when may a phrase be Q-bound? What allows the subject in (5) to be Q-bound (avoiding the NDC) but blocks the subject of (2) from Q-binding, which forces the NDC violation? If the subject in (2) may be Q-bound, then the analysis is lost. Pesetsky argues that Q-bound phrases are ones which are D(iscourse)-linked; they are phrases which presume some discourse context for their felicitous use. Thus, a *which*-phrase like the subject of (5) seems to presuppose a discourse context in which a group of people is already under active discussion/consideration. Generally, *which*-phrases, as in (5), have this character, and (following Pesetsky's claims here) ordinary *wh*-phrases as in (1-2) do not. Thus, the *which*-phrases in (5) (and (4)) may be Q-bound, but not the *wh*-phrases in (1-2). *Which*-phrases may undergo syntactic *wh*-movement, as the fronted *which*-phrases in (5a) and in (6) attest. However, in LF, they do not *wh*-move, and are Q-bound instead; hence the in-situ *which*-phrase in (5).

(6) **Which film did Mary see?**

Now, we turn to EQs.

2. EQs. Just as there are two types of binding, I want to claim that there are also two (and possibly more) types of EQs (see also Sobin (1978)). Recognition of distinct types of EQ and the interaction of each with the possibilities of M- and Q-binding facilitates the analysis of English EQs as a product of normal syntax. The EQs which are dealt with below divide into what I will call PSEUDO EQs and SYNTACTIC EQs.

2.1 Pseudo EQs. The type of EQ which I will call a pseudo EQ is illustrated in (7b):

- (7) a. U: **Mary likes chocolate worms.**
 b. E: **What does Mary like?**
 c. E: **Mary likes what?**

(Here, U = an utterance, and E = an EQ to that U.) A pseudo EQ is a "normal" question to which the U would be a felicitous, declarative response. The declarative character of U is important here. Sentence (7b) is a good EQ to (7a), in part since (7a) is declarative. But (7b) is not a good EQ to (8), since (8) is not a declarative utterance, and hence admits no pseudo EQs.

(8) **Does Mary like chocolate worms?**

As is thought characteristic of EQs, pseudo EQs have final upward intonation or a derivative of it. Also noteworthy of pseudo EQs is that their Comp structure, which is

+WH and which may include *wh*-phrases, is distinct from the simple -WH Comp structure of the U being echoed.

2.2 Syntactic EQs. The second type of EQ is what I have labeled a syntactic EQ. An example is (7c). The formation of a syntactic EQ is quite different from that of a pseudo EQ. Syntactic EQ formation involves first, a discourse strategy which I label as Comp-freezing, and second, the Q-binding (rather than M-binding) of any newly-introduced in-situ *wh*-phrases, such as the *what* of (7c). I will now deal with each of these characteristics in turn.

2.2.1 Comp-freezing. Comp-freezing is a discourse strategy, a strategy necessarily involving more than one sentence in some normal language use context such as a conversation. Comp-freezing works roughly as follows: a syntactic EQ is formed, in part, by copying exactly the Comp structure (i.e., the SpecCP, C sequence) of the U in its +/-WH character and in any *wh*-phrases which may be present in Comp, including *wh*-phrases moved in LF.² To return to (7), an approximate LF structure of (7a) is (9a):

- (9) a. [-WH [**Mary likes chocolate worms**]]
 b. [Q1 [-WH [**Mary likes what**]]]
 [+F]

Structure (9b) is the LF structure of the syntactic EQ (7c). LF (9b) preserves the Comp structure of (9a), marked here with a conventional feature [+F] to denote that it is frozen. The Q-binder is CP-adjoined to the left of the frozen Comp. What of (9b) is a questioned element because it is (Q-)bound in the highest Comp -- a newly-created one.

That the Comp structure of a syntactic EQ is a frozen version of the Comp of the echoed U can best be appreciated by considering a range of data. Item (10) contains various utterances, along with a characterization of the Comp structure of each. Some EQ responses are given in (11). Following each EQ in (11) is a set of four markings, indicating the (un)acceptability of that EQ as a response to each of the utterances.

- (10) a. U: **Frieda likes chocolate worms.** [-WH [...
 b. U: **Does Frieda like chocolate worms?** [+WH [...
 c. U: **Who likes chocolate worms?** [who(1) +WH [...
 d. U: **What does Frieda like?** [what(1) +WH [...

contains a *wh*-phrase, the subject *who*. The same considerations which bar pseudo EQs as responses to (10b) also bar them here. Thus the only EQ responses to (10c) are (11h) and (i) analyzed as syntactic EQs. As such, each contains a Comp structure identical to (10c) in regard to the +/-WH character of Comp, and to *wh*-phrases present in Comp.

Last, consider (10d). It too is non-declarative, and so it admits no pseudo EQs. Its Comp structure is +WH and includes the object *what*. In (11), the only admissible syntactic EQs are (11g) and (j). Note that (11i), a perfectly well-formed question in other circumstances, is out as an EQ here. Further, it is rejected in favor of (11j), a structure which in most other contexts would be considered ill-formed (c.f. (2) above).

Having discussed Comp-freezing, let us now turn further attention to Q.

2.2.2 Q-binding. LF structure (9b) contains a Q-binder which is CP-adjoined to its sentence and which binds an in-situ *what*. This is a controversial move. I claim here that such in-situ *wh*-phrases are D-linked, and thus Q-bound rather than M-bound in LF. The following question might be posed: Why invoke Q here? Why not instead claim that in (7c) Comp is frozen, an in-situ *what* is introduced, and *what* is moved in LF, resulting in an LF like (12)?

(12) [*what*(1) [-WH [*Mary* likes *t*1]]] (M-binding hypo.)

That the right answer is Q-binding as in (9b), and not M-binding as in (12), is indicated by the data in (13-15):

- (13) a. U: What did Mozart bake?
 b. [*what*(1) +WH [*Mozart* baked *t*1]]
- (14) a. E: What did who bake?
 b. [Q2 [*what*(1) +WH [*who*(2) baked *t*1]]]
 [+F] [+F] (Q-binding hypo.)
 c. [*who*(2) [*what*(1) +WH [*t*2 baked *t*1]]]
 (M-binding hypo.)
- (15) a. *E: Who baked what?
 b. [Q2 [*who*(1) +WH [*t*1 baked *what*(2)]]]
 (Q-binding hypo.)
 c. [*what*(2) [*who*(1) +WH [*t*1 baked *t*2]]]
 (M-binding hypo.)

EQ (15) correctly fails under either hypothesis due to the lack of Comp-freezing with respect to (13). However, under the M-binding hypothesis, (14) also fails, since its M-bound LF (14c) is an NDC violation. This prediction is wrong, of course, since (14) is a possible EQ to (13). The correct result is predicted by the Q-binding hypothesis, since LF structure (14b) with Q-bound *who* is not an NDC violation. We can therefore conclude that the in-situ *wh*-phrases of syntactic EQs are Q-bound rather than M-bound.

2.2.3 Non-frozen dimensions of syntactic EQs. In striking contrast to the freezing of the Comp/Move-*Wh* dimension in syntactic EQs, other dimensions of these structures are not frozen. For example, particle movement is free, as the U-E set (16) shows.

- (16) a. U: *Mary gobbled the chocolate worms up.*
 b. E: *Mary gobbled up what?*

Perhaps more significantly, while the Comp/Move-*Wh* dimension is frozen, the complementary part of Move-Alpha, namely, Move NP, appears not to be frozen. Thus we have the U-E set (17).

- (17) a. U: *Mozart has been arrested by the police.*
 b. E: *The police have arrested who?*

Important here is that while U and E differ in voice, they do not differ in Comp structure: both are -WH. The demand for matched Comp structure between U and a syntactic EQ, despite differences in derivation in regard to Move NP, is demonstrated further in (18).

- (18) a. U: *Has Mozart been arrested by the police?*
 b. E: *Has who been arrested by the police?*
 c. E: *Have the police arrested who?*
 d. *E: *Who have the police arrested?*

Utterance (18a) has a simple +WH Comp. So do syntactic EQs (18b) and (c). However, in (18d), *who*, since it precedes the inverted auxiliary verb *have*, is clearly in Comp (SpecCP). Hence, (18d) fails as a syntactic EQ to (18a).

3. Some other syntactic oddities explained. The system proposed here makes some other nice predictions about data which are otherwise simply problematic. U-E set (19) shows what ordinarily would be a subjacency violation or a doubly-*wh*-filled-Comp violation.

- (19) a. U: *What does Mary think Mozart baked?*
 b. E: *What does Mary think who baked?*

The present analysis predicts that (19b) is grammatical, however, since *what* has not been moved out over *who*, but rather it is *who* which is newly-introduced and Q-bound in-situ.

In (20), (a-b) show that *think* selects a -WH and not a +WH complementizer. If this is so, then whence the *who* in (20d)?

- (20) a. *Max thinks that Mary ate the pie.*
 b. **Max thinks who ate the pie.*
 c. U: *Max thinks that Mozart ate the pie.*
 d. E: *Max thinks who ate the pie?*

Again, with Comp-freezing and Q-binding, the Comp in (20d) is still -WH, and *who* is not indicative of a +WH complementizer, but is simply an in-situ Q-bound phrase. So the complementizer selection problem is solved.

In (21), the verb *know* may select -WH or +WH, but not both, and further, *that* plus a *wh*-phrase is barred. (In Chomsky's (1973) early account of these, if a *wh*-phrase moves to a -WH Comp, the result is simply uninterpretable unless the structure is a relative clause.) Then why is (21e) possible?

- (21) a. *Mary knows that Max ate the pie.*
 b. *Mary knows who ate the pie.*

- c. *Mary knows that who ate the pie.
- d. U: Mary knows that Mozart ate the pie.
- e. E: Mary knows that who ate the pie?

A Q-binding analysis resolves the difficulty here too, since the *wh*-phrase in (21e) never gets to Comp, but remains in-situ.

4. Conclusions. There is a number of other curious aspects of EQ syntax and semantics that time limitations force me to ignore here (and which I address elsewhere); however, what I have presented here points strongly toward the coexistence of two types of binding: M-binding and Q-binding. This is the major point that I wanted to make here. Further, just as sentences with a declarative intonational pattern show syntactic variety, sentences with EQ intonation are not all of the same syntactic type. The preceding has dealt with two distinct syntactic types: pseudo EQs and syntactic EQs. There may well be others. For example, it seems that there is an EQ strategy of using a *wh*-word to replace material to be questioned from some point in the linear string of words rightward, irrespective of constituency. Hence, it is possible to echo (22a) as (22b), but not as (22c):

- (22) a. U: Mary gave Mozart a kiss on the nose.
- b. E: Mary gave what?
- c. *E: Mary gave what on the nose?

This type calls for further investigation.

Another important question has been raised, namely, what are the independent determinates of the status "Discourse-linked" for a *wh*-phrase? I have claimed here that, in addition to *which*-phrases being D-linked, any *wh*-phrase newly introduced into an EQ is D-linked, and hence Q-bound rather than M-bound. This may somehow be due to the strongly discourse-related nature of syntactic EQs. Clearly, the determining factors are not just the internal structure of the NP in question. Much more work is needed on the independent characterization of D-linking in *wh*-phrases.

Footnotes

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1. In a more comprehensive article on echo questions (Sobin forthcoming), Q-binding is referred to as B-binding, following Pesetsky's (1987) characterization of unselective binding as Baker-style binding.

2. While the noted Comp characteristics are frozen, phonetic form of Comp is not. Thus, *that* may alternate with *0*, or *whether* with *if*.

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HPSG CANNOT AVOID HEAD WRAPPING IN THAI

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1. **INTRODUCTION.** I show in this paper that Head-Driven Phrase Structure Grammar (HPSG) by Pollard and Sag (henceforth, P&S) (1987) cannot avoid head wrapping in Thai. The procedure of my analysis is as follows. First I present Head Wrapping Operation by Pollard (1984). Next I show how P&S avoid Head Wrapping in their HPSG framework. Then I illustrate some background of sentence structures and temporal verbs in Thai. Following this section is a brief presentation of my analysis on temporal verbs, using HPSG framework. To construct a VS(C) sentence in Thai, I propose a linear precedence rule as a head wrapping operation. Finally I discuss an advantage of my analysis over HPSG framework and point out a crucial problem of this theory.

2. **HEAD-WRAPPING OPERATION.** Pollard (1984) handles the so-called "discontinuous constituent" phenomena with head-wrapping operations. Pollard proposes RL2: the function of two (headed string) arguments which wraps the (underlying string of) the second (right-hand) argument around the (underlying string of) the first (left-hand) argument, at the same time designating the head of the second argument as the head of the output. The operation of RL2 is stated formally as (1).

$$(1) \text{RL2}(\sigma, I) = t_1 \dots t_j \text{ s } t_{(j+1)} \dots t_m$$

*

The designation 'RL' is mnemonic for "wrap the Right-hand argument around the other argument, placing the head of the former directly to the left of the latter; the function is referred to as right-left head wrap.

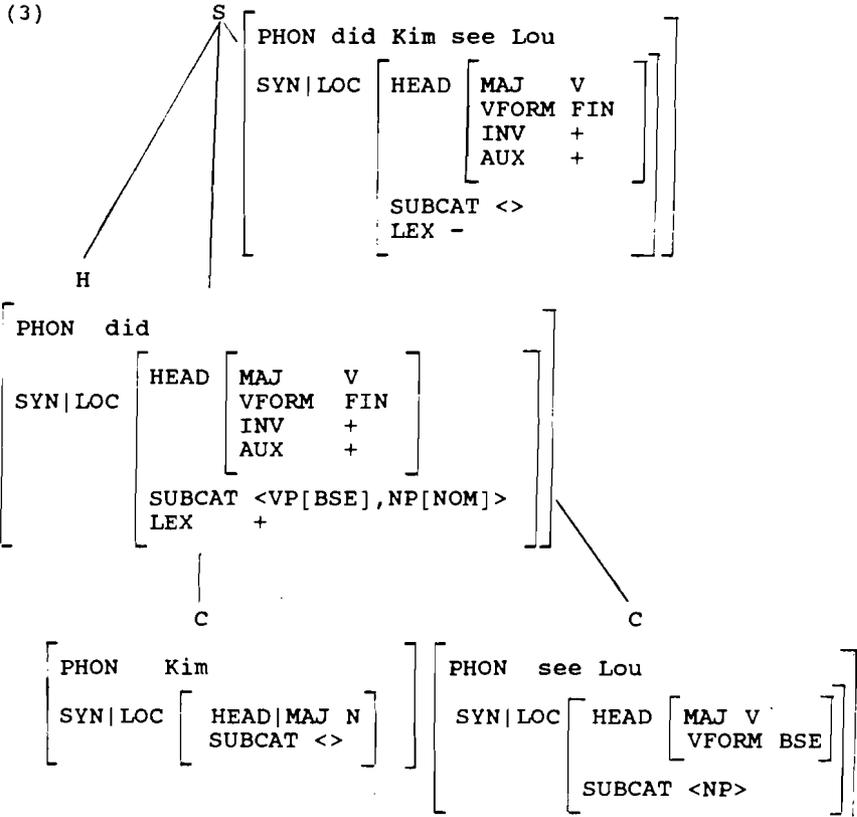
(2) shows Subject-Aux Inversion rule, which exploits function RL2.

$$(2) S[+INV] \rightarrow \text{RL2}(NP, VP[+AUX])$$

For instance, given that **Kim** is an NP and **must go** is a VP[+AUX], the rule in (2) allows us to generate the S[+INV] **must Kim go**.

3. **HPSG AVOIDS HEAD WRAPPING IN ENGLISH.** P&S (1987) avoid Head Wrapping as an operation in English. The Aux-

Subject inversion is constructed by means of the feature-values [INV(ERTED)+], [AUX+] and [LEX+] of a verb head. In this manner, a verb head combines with its subcategorized complements including a subject, forming a flat sentence structure, as illustrated in (3).



The resulting sentence: Did Kim see Lou?

4. **Background in Thai.** Thai is a noninflectional language, which has been described by Thai grammarians and linguists as a SVO language (Wartammasikkhadit, 1963, Hawkins 1985).

4.1. **Sentence Structures.** In my analysis, there are two sentence structures in Thai: SV(C) and VS(C) (Sookgaseem, 1989). "C" here means "complement".

The SV(C) structure is illustrated in (4).

- (4) SV(C)
 [s mxxw kat nuu]
 cat bite rat
 "A cat bites/ bit a rat"
 *[s kat mxxw nuu]
 *[s nuu kat mxxw]
 *[s kat nuu mxxw]

The VS(C) construction is illustrated in (5).

- (5) VS(C)
 [s mii [np phaayu]]
 exist storm
 "There is/was a storm"
 *[s [np phaayu] mii]

4.2. Temporal Verbs. In my analysis following HPSG, temporal elements in Thai are of grammatical category type V[SUBCAT<VP,NP>], whereas regular or main verbs are of type V[SUBCAT<...NP>], where three dots "..."
 can be of any grammatical category.

Some temporal verbs occur in the pre-VP position only; for example, rEm "Inchoative". Others occur in the post-VP position only; for instance, set "Completive" and lxxw "Perfective". Some others which normally occur in the pre-VP position can also occur in the post-VP position when triggered by post-VP verbs set "Completive", dai1 "Permission" and dai2 "Ability".

The positions of these verbs are illustrated in (6a-c). (6a) shows the position of a temporal verb which occurs in the pre-VP position only.

- (6)a. [s mxxw rEm kin nuu]
 cat V:Inchoative eat rat
 "A cat starts/started to eat a rat"
 *[s mxxw kin rEm nuu]
 *[s mxxw kin nuu rEm]

A temporal verb which occurs in the post-VP position only is illustrated in (6b).

- (6)b. [s mxxw kin nuu yuu]
 cat eat rat V:Perfect-Progressive
 "A cat has been eating a rat"
 *[s mxxw yuu kin nuu]

*[s mxxw kin yuu nuu]

The syntactic behaviour of temporal verb ja, which can occur in both positions, is shown in (6c-d). Both (6c) and (6d) hold the same meaning.

- (6)c. [s khun ja kin nuu dai2 lEE]
 you V:Future eat rat V:Ability yes/no Q
- d. [s khun kin nuu ja dai2 lEE]
 you eat rat V:Fut V:Ability yes/no Q
 "Will you be able to eat a rat?"

5.0. Application of HPSG to Thai. Using HPSG framework, I propose the feature [INV+,-] for main or regular verbs, and the feature [BACKWARD-INVERTED (BV)+,UNSP(ECIFIED)] for temporal verbs. The head features for both verb types are presented in (7).

(7) The Head Features

<u>Feature</u>	<u>Value</u>
INV	{+,-}
BV	{+,-,UNSP}
LEX(ICAL)	{+,-}

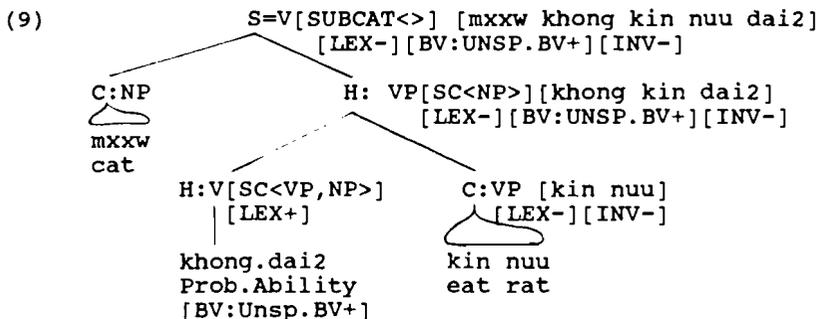
Some examples of verbs with the features and feature-values in (7) are shown in (8).

<u>Verbs</u>	<u>Feature-values</u>
kin "eat"	[INV-]
mii "exist"	[INV+]
kEEt "occur"	[INV+]
ja "Future"	[BV:UNSP]
khong "Probability"	[BV:UNSP]
yuu "Perfect-progressive"	[BV+]
dai2 "Ability"	[BV+]
rEm "Inchoative"	[BV-] etc.

In my analysis, I propose that temporal verbs which occur in the same sentence, a simple sentence or a clause, constitute a lexical verb compound. My argument for this proposal is not presented here. Under this treatment, ja.dai2 "Future.Ability" is thus a lexical temporal verb compound. They have the same feature [BV] but different values: the former holds the value [UNSP], while the latter [+].

Assuming that we have the correct grammar rules, I show in (9) how to construct a SVO sentence in Thai. Here lexical verb compound khong.dai2 is a verb head subcategorizing for VP and NP subject complements. khong.dai2 combines with its VP complement first, yielding khong kin nuu dai2. Next, VP khong kin nuu dai2,

functioning as a phrasal verb head, combines with its NP complement *mxxw* "cat", yielding a SVO sentence *mxxw khong kin nuu dai2* as a result.



Resulting sentence

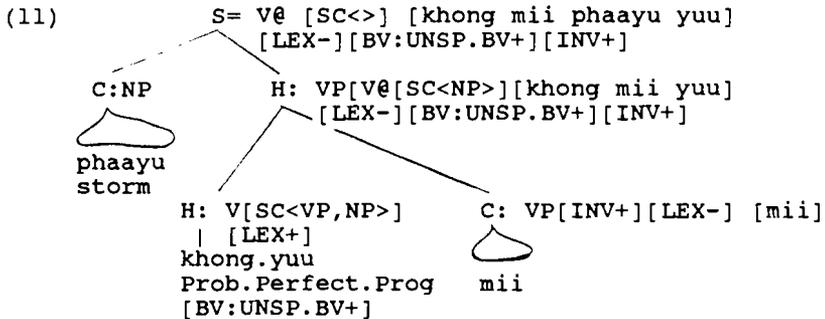
[s mxxw khong kin nuu dai2]
 cat Prob eat rat Ability
 "A cat may be able/may have been able to eat a rat"

To construct an inverted sentence, I propose a linear precedence rule (LP), as shown in (10).

- (10) LP: H: V@ [LEX-,INV+] < C [NP v s v S[COMP]],
 where "@" is for V[INV+] or "parts of inverted VP"

This LP rule requires that a verb head which is nonlexical and inverted, or parts of an inverted VP precede its subject complement which is a noun phrase or a sentence or a sentence with a complementizer.

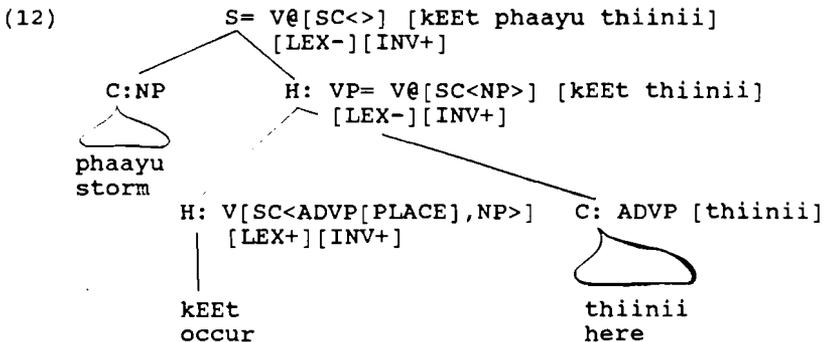
(11) shows, as an example, a construction of a VS sentence. Here *khong.yuu*, a lexical verb compound head, subcategorizes for <VP,NP> as its complements. *khong.yuu* combines first with its VP complement *mii*, yielding VP *khong mii yuu*. This VP then combines with its NP subject complement, yielding a VS sentence *khong mii phaayu yuu* as a result.



Resulting sentence:

[s khong mii phaayu yuu]
 Prob exist storm Perfect-Progressive
 "There may have been existing a storm" or
 "A storm may have been existing"
 ??[s phaayu khong mii yuu]

(12) shows an example of a VSC sentence. Here regular verb of occurrence **kEET** is an inverted lexical verb head which subcategorizes for an adverb phrase and a noun phrase as its complements. It first combines with its ADVP complement, yielding **kEET thiinii** "occur here". VP **kEET thiinii**, as a verb head, then combines with NP as its subject complement, yielding a VSC sentence **kEET phaayu thiinii** as a result.



Resulting sentence:

[s kEET phaayu thiinii]
 occur storm here
 "A storm occurs/occurred here"
 ???[s phaayu kEET thiinii]

6.0. Conclusion. My analysis has shown that to construct a VS(C) sentence in Thai, we require a linear

precedence rule which functions as a head wrapping operation, in addition to the feature [INV+,-]. One advantage of this operation is to maintain uniformity of a sentence construction. That is, a lexical verb head must combine with its nonsubject complement(s) first, and then a phrasal verb with its subject complement. In this manner, we can avoid a flat sentence structure when a VS(C) sentence is constructed.

From this analysis, we have also found that there is a crucial problem about the head feature principle in HPSG framework, which need more analysis.

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THETA-MARKING AND SUBJECT EXTRACTION IN CAUSATIVES

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0. INTRODUCTION*

It will be proposed in this paper that a language independent property of causative verbs is to enter into both Direct and Indirect Causality relations, but that the manner by which these relations are syntactically realized is language-specific. It is further argued that there is a Semantic Constraint which conditions the possibility for a Causative construction to yield a Direct Causality Interpretation. To support our hypothesis, English Causatives and Spanish 'Subjunctive Causatives' are discussed, and shown to resort to a 'Movement' strategy in order to disambiguate and render the Direct Causality Interpretation both at S-structure and LF.

1. THE ASYMMETRIC EFFECT

Consider the asymmetry exhibited by the pairs of sentences in (1) and (2) vs. (3) and (4), in which the a sentences represent canonical Causative constructions, and the b sentences their passivized versions:

- (1) a. Mary made John win that prize
b. John was made to win that prize
- (2) a. Tom made her apologize publicly
b. She was made to apologize publicly
- (3) a. She made me forget the documents
b.*I was made to forget the documents (by her)
- (4) a. She made him love her
b.*He was made to love her (by his mother)

Notice that while in (1a) and (2a) passivization can readily apply, it results in ungrammaticality if applied to (3a) and (4a). It is interesting to note that this contrast runs parallel to a semantic difference between the a and the b sentences. For example, take sentence (1). While (1a) has the two interpretations represented informally in (5), (1b) has only that corresponding to (5b):

- (5) a. Mary created the situation so that John would win that prize. [For example, by bribing the judges.]
b. Mary forced John to win that prize. [For example, by forcing him to manipulate the final results.]

In (5a) there is a meaning of Indirect Causation in relation to the embedded subject, so that John does not play an active and deliberate role to win as a consequence of Mary's actions. However, in (5b), there is a sense of Direct

Causation: John is interpreted as having been actively involved in his winning a specific prize as a consequence of Mary's actions. Thus, while in (1a) both the Indirect and the Direct Interpretations are obtained, movement of the subject in (1b) –by means of passivization– leads, interestingly, to an unambiguous interpretation.

It is intended here to explain why subject extraction restricts the interpretation of sentences like (1a), to allow only the unambiguous sense of Direct Causation present in the b sentences. Although our focus here is mainly on movement strategies, it will be shown that this is not the only strategy by which the same effects can be obtained across languages. Along with explaining the disambiguation phenomenon, we will also try to account for the fact that (3a) and (4a) cannot be passivized. It can now be observed that in these two sentences, the only available interpretation is that of Indirect Causation parallel to that of (1a), represented in (5a). The general intuition seems to be that passivization is only allowed if the Direct Interpretation is present.

1.1 SPANISH SUBJUNCTIVE CAUSATIVES

A very common and productive construction in Spanish, although rarely mentioned, is that of 'Subjunctive Causatives' (i.e. constructions with Subjunctive Causative complements). These exhibit a similar asymmetric effect as that described thus far, in relation to English, and as the pairs of sentences (6) and (7) vs. (8) and (9) illustrate:

- (6) a. Juan hizo que Pedro terminara la tarea
 b. Juan hizo a Pedro que terminara la tarea
 a' Juan made that Pedro finish the homework
 b' Juan made to Pedro that finish the homework
 'Juan made Pedro finish the homework'
- (7) a. Ella hizo que Mario admitiera el error
 b. Ella hizo a Mario que admitiera el error
 a' She made that Mario admit the error
 b' She made to Mario that admit the error
 'She made Mario admit his mistake'
- (8) a. Hizo que su ayudante recibiera los insultos
 b.*Hizo a su ayudante que recibiera los insultos
 a' (He/she) made that his aide receive the insults
 b' (He/she) made to his aide that receive the insults
 'He/she made his aide take the flak'
- (9) a. Ella hizo que Mario supiera la verdad
 b.*Ella hizo a Mario que supiera la verdad
 a' She made that Mario know the truth
 b' She made to Mario that know the truth
 'She made Mario learn the truth'

We will argue below that in these examples the b sentences are related to their a versions by movement of the embedded subject into a position

where it enters into a closer relation with the causative verb. The bulk of this paper will not, however, be concerned with the syntactic technicalities as much as with the semantic factors at play. Notice again, as with the English examples (1–2), that the a examples of (6–7) allow both Direct and Indirect Causation Interpretations while the b sentences, where our analysis treats the embedded subject as an extracted subject, allow only a reading of Direct Causation, as (10) illustrate:

- (10) a. Juan got Pedro to finish his homework. [For example, because he promised to take him to the game].
 b. Juan forced Pedro to finish his homework. [For example, because he sat right beside him and forced him to finish].

While (6a) has the two readings in (10), (6b) has only the reading (10b). The ungrammaticality of (8b) and (9b) –parallel to that of English (3b) and (4b)– will be shown to follow from a violation of a Semantic Constraint.

1.1.1 SUMMARY. Although the distinction between Direct and Indirect Causation hasn't been previously discussed within the GB framework, the phenomenon was been noted for French Causatives, among others, by Bailard (1982), Dorel (1980), Hyman and Zimmer (1976), Kayne (1975) and Morin (1978). For Spanish Causatives with Infinitival complements, Strozer (1976) noted this property. In essence, a case alternation in the marking of the NP interpreted as the 'logical subject' (LS), yields a concomitant distinct interpretation. This is mostly evidenced when the LS appears cliticized: a LS cliticized in the Accusative is said to render a Direct Interpretation while a LS cliticized in the Dative renders an Indirect Interpretation. This phenomenon, however, has been largely maintained within the domain of Romance Causatives where a distinction in Case produces a distinct semantic interpretation.¹ We claim first, that it is a general property of causative verbs to enter into Direct and Indirect causality relations, and that there is a direct connection between this semantic property and its syntactic representation both at S-structure and LF. Second, language-specific properties determine the particular manner by which an unambiguous interpretation of Direct Causation can reveal itself at S-Structure. And third, that this Interpretation is semantically constrained to apply to NPs bearing a semantic/thematic feature 'compatible' with the semantic properties of the causative verb.

1.2 NON-MOVEMENT STRATEGIES

We have seen that to convey a Direct Causation Interpretation unambiguously, some languages have recourse to subject movement: English raises the LS (as shown in the b sentences of (1–2)), and in Spanish Subjunctive Causatives, the LS appears to the left of the complementizer as the sentence pairs in (6–7) show. However, there are non-movement strategies available in language to obtain the disambiguation under discussion.

1.2.1 SPANISH INFINITIVAL CAUSATIVES. Spanish has also a non--movement option which we can briefly discuss in relation to 'Infinitival Causatives', (Infinitival causative complements), such as those presented in (11) and (12):

- (11) a. Juan le-Dat hizo entender las cosas
 b. Juan lo-Acc hizo entender las cosas
 Juan to--him made understand things
 'Juan made him understand the situation'
- (12) a. Juan le-Dat hizo saber que llegaría tarde
 b.*Juan lo-Acc hizo saber que llegaría tarde
 Juan to--him made know that (he/she) would arrive late
 'Juan let him know that he would be late'

Where the Dative clitic le --representing the cliticized LS-- may be used, both Direct and Indirect Interpretations may be obtained, as is the case in (11a). In contrast, whenever the Accusative clitic lo can be used, the interpretation is that of Direct Causation only, as evidenced by (11b). Notice also that in examples like those in (12), if only the Indirect Causation Interpretation is present as in the le sentence (12a), then the lo version is barred.

1.2.2 FRENCH CAUSATIVES. Standard French exemplifies yet another type of strategy to carry out the disambiguation effect. We must notice that this language differs from both English and Spanish in that it does not have recourse to a movement strategy to disambiguate the two causative interpretations. In this language, the effect is achieved by means of the alternation between, what Kayne called, the *Faire-à* and the *Faire-par* constructions, whereby the LS may appear in an à-phrase, or in a par-phrase. When the embedded subject appears in an à-phrase, as in (13a), an ambiguous reading, including that of Direct Causation, may obtain:

- (13) a. Jean a fait lire un livre à Pierre
 b. Jean a fait lire un livre par Pierre
 a' Jean has made read a book to Pierre
 b' Jean has made read a book by Pierre
 'Jean has made Pierre read a book'

However, (13b) --with the LS in a par-phrase-- conveys an Indirect Interpretation only. We may remember that Kayne noted that examples such as those in (14a) vs. (14b) were ungrammatical, yet he also noted that there was no obvious explanation for the source of this ungrammaticality:

- (14) a.*Il a fait torturer le prisonnier à la police
 b. Il a fait torturer le prisonnier par la police
 a' He has made torture the prisoner to the police
 b' He has made torture the prisoner by the police
 'He has made the police torture the prisoner'

Within the present proposal, it will become clear that the reason why sentences like (14a) are ungrammatical, is due to a violation of what we will call the Causative Compatibility Constraint.

2. THE CAUSATIVE COMPATIBILITY CONSTRAINT

Recall that the English sentences (3) and (4) bear only an unambiguous interpretation, that informally represented in (15) and (16) respectively:

- (15) She caused him to love her. [For example, because she talked in such a way about her that he fell in love with her without even knowing her.]
- (16) She caused me to forget the documents. [For example, she made me nervous to a point where I forgot the documents].

Likewise, the Spanish sentences (8) and (9) can only be unambiguously interpreted with the Indirect Causation reading, as (17) and (18) show respectively:

- (17) X created the situation whereby his aide would get the insults. [For example, since he knew that the clients would get very cross, he send his aide to get insulted.]
- (18) She created the situation whereby Mario would learn the truth. [For example, she managed somehow for Pedro to know what had really happened.]

In contrast, sentences (6a) and (7a) are ambiguously interpreted; (6a) with the two meanings in (10), as presented above, and (7a) with those in (19) below:

- (19) a. She created the situation whereby Mario would admit his error. [For ample, she is so clever that she tricked him into admitting the error was his.]
- b. She forced Mario to admit his error. [For example, she tortured Mario until he would admit the error was his.]

Sentences (6b) and (7b) however, allow only the Direct Interpretation. Thus, the generalization seems to be that if an ambiguous Direct/Indirect Interpretation is present, the LS is allowed to move, as was the case with the English examples (1b) and (2b), as well as with the Spanish examples (6b) and (7b). We also see that if only the Indirect Interpretation is available, then movement by the LS is not permitted, as the ungrammaticality of sentences (3b), (4b), (8b) and (9b) attest.

Notice that in the cases where the Direct Interpretation results, the LS is thematically interpreted as being predominantly 'agentive' and semantically as 'someone with the ability or capacity to cause an event knowingly, deliberately, consciously, willingly, and with the capability of having control over causing such event'. For convenience, let's treat this particular set of

semantic properties as a semantic feature. Let us call it [active participant] or [ap]. Concurrently, a subject will be a [+ap]-subject if it has this feature, and it will be a [-ap]-subject if it lacks it. In brief, a subject bearing the [+ap]-feature will be able to surface under the syntactic conditions portrayed here, with a Direct Causative reading. While a [-ap]-subject will always surface with the Indirect Interpretation. Thus, for example, in (1a) and (2a), where the subject is [+ap], and the two meanings are available, passivization is possible: after the movement applies, (1b) and (2b) –with the feature [+ap]– surface with the Direct Interpretation. In contrast, the examples of (3) and (4) are not allowed to passivize because the LS lacks the relevant feature [+ap], a fact which is evidenced by the presence of the Indirect Interpretation only.

2.1 THETA-MARKING

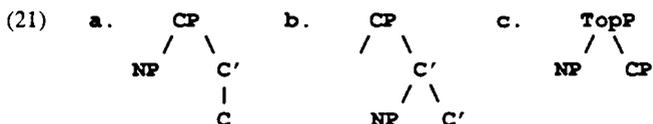
It is clear that the LS is Theta-marked by the complement verb, more precisely, by the embedded VP, and that the causative verb is not the element which Theta-marks it in any manner. First, notice that the thematic content of the LS is determined by the subcategorizational framework of the embedded verb. For example, the LS in (1) and (6) bears a predominantly 'agentive' role, as is expected with verbs like *win* and *terminar* 'to finish'. In (8), the subject bears the role of 'goal', assigned by *recibir* 'to receive', and in (3) and (9), the subject bears the 'experiencer' role assigned by *love* and *saber* 'to know', respectively. Thus, it is also the embedded verb which is responsible for the specification of the [ap] nature of the LS. As a point in fact, sentences such as (8b), which are ungrammatical because a [-ap]-LS has been extracted, can be made grammatical by changing the composition of the embedded VP, and thus of the thematic role that it assigns to this subject. See the example (20b) where adding the modal *con entereza* 'with courage' –a manner adverbial which 'charges' the NP with an extra semantic feature– renders (8b) grammatical:

- (20) a. Hizo que su ayudante recibiera los insultos con entereza
 b. Hizo a su ayudante que recibiera los insultos con entereza
 a' He made that his aide receive the insults with courage.
 b' He made to his aide that receive the insults with courage.

Second, in the English sentences (1b–2b), we are dealing with passive structures, and therefore, the syntactic subject is in a non-theta position. This shows that the chain headed by the moved NP (the LS), has only the thematic/semantic role transmitted by the trace in the complement Theta position. In Spanish, the same thematic/semantic relationship obtains in both types of sentences where the relevant subject appears either to the right or to the left of the complementizer, showing that the thematic composition of the LS is not altered by its position in the sentence. That is, the LS will not acquire, e.g. the [ap]-feature, or an extra Theta-role by virtue of being in one or the other position, relevant to the point in question. The change that is brought about is a disambiguation phenomenon whereby the LS may

come to have scope, so to speak, over the caused event. Similarly, the French sentences evidence that the same thematic/semantic relationship holds between the embedded VP and the LS, whether it appears in an à-phrase or in a par-phrase.

We conclude from the above discussion that Spanish Subjunctive Causatives with extracted subjects are not Control constructions. That is, we are not dealing with chains capable of receiving more than one thematic role. This is manifested by the fact that the thematic/semantic content associated to the subject NP under discussion, remains constant regardless of being inside or outside the Subjunctive complement. Details of the specific analysis of Spanish subject extraction, still remain to be fully worked out. However, it can be proposed that the subject moves to Spec of CP –(21a)– since in Spanish there are no “that–trace” effects. A proposal along the lines of that of Rizzi and Roberts (1988) with a subject adjoined to C’ –as in (21b)– may also be adapted to our purposes. Still, a third possibility is to analyze the subjects in the *b*-type sentences as Topics in the specifier of a TopP –as in (21c), a configuration much in the spirit of Rivero’s (1980) proposal–:



Notice that the possibility of subject extraction in Spanish Causatives, with the concomitant semantic interpretation, must be a property of *hacer*. (22a) below, shows a construction where the perception verb *ver* ‘to see’ subcategorizes for a Subjunctive complement in a manner parallel to sentences *a* of (6–9) above, and whose interpretation also parallels that of the Subjunctive Causatives. Thus, (22a) is interpreted with a causative meaning:

- (22) a. Pedro vio que María terminara la tarea
 b. *Pedro vio a María que terminara la tarea
 a’ Pedro saw that María finish the homework
 b’ Pedro saw to María that finish the homework
 ‘Pedro saw to it that María finish her homework’

Nevertheless, (22b) with the LS extracted results in ungrammaticality, confirming the view that Causatives under *hacer* exhibit the ambiguity/movement property while ‘causative’ *ver* does not. However we account for the occurrence of the NPs (=LS) to the left of the complementizer in (6–7), we must still account for the asymmetries in grammaticality and interpretation.

2.2 THE COMPATIBILITY CONSTRAINT

The predicates ‘to make’ and ‘*hacer*’ have an intrinsic ‘active’ sense in

their meaning, as compared to verbs like 'to let' and 'dejar'. This 'activity' meaning, conveyed by their semantic content, is 'carried out' by their agents or subjects whether in a simplex or complex syntactic environment. But, when 'to make' and 'hacer' appear in a Causative structure, these verbs display the following two-fold property, a) the Matrix Subject can directly cause an event, in which case the subject of the event –the LS–, appears as its 'target'. The LS is considered to be equivalent to a beneficiary or to an experiencer –as argued in Treviño (1989)–. b) the Matrix Subject is the indirect causer of the event, in which case the LS is caused (by the former) to act as the direct causer; the LS is then, considered to be an active agent of the event. Concurrently, in the (a) case, the LS is said to undergo Indirect Causation, whereas in the (b) case, it is said to undergo Direct Causation. Nevertheless, this 'two-fold property' seems to be contingent upon the thematic/semantic nature of the embedded event, including its LS, as conformed by its own predicate.² In any case, it can be observed from all the ungrammatical examples provided above, that the LS intended to bear a Direct Causation Interpretation (at S-structure), lacks the relevant [ap]-feature. That is, the LS bears a rather passive role, lacking thus the ability to control the event of which it is the LS. Put in different words, the LS appears to be incompatible with the (active) thematic/semantic features projected by the causative verb.³ As a result, the generalization seems to be that for the embedded LS to enter into a 'direct relationship' with the causative verb –by means of being a direct causer–, it has to "match" a relevant semantic definitional feature of the causative verb. What is clear is that for the verbs 'to make' and 'hacer', the proposed feature [+ap] satisfies this matching requirement. We suggest that this Semantic Constraint can be informally defined as in (23), and that it applies at LF:

- (23) The logical subject of the causative complement can be direct causer
if it bears a [+ap] feature.

First, notice that (23) has nothing to do with some property of Passives such as that generally referred to as the Affectedness Condition –as proposed by Anderson (1979) or more recently by Jaeggli (1986)–. In fact, the embedded LS is always affected regardless of whether it bears the relevant [ap] feature. Second, it is worth mentioning that (23) is independent of the syntactic nature of the embedded verb: whether it is transitive, unergative or unaccusative as the sentences in (24) and (25) illustrate:

- (24)
- a. transitive
John was made to sign the papers
 - b. unergative
John was made to run for hours
 - c. unaccusative
John was made to arrive on time

- (25) a. transitive
 Hizo a Juan que firmara los papeles
 'John was made to sign the papers'
- b. unergative
 Hizo a Juan que corriera durante horas
 'John was made to run for hours'
- c. unaccusative
 Hizo a Juan que llegara a tiempo
 'John was made to arrive on time'

We suggest that disambiguation takes place at LF necessarily, and that it may also occur at S-structure depending on language specific properties. Examples (1a–2a) above, show that the underlying Direct/Indirect Causation ambiguity can only pertain to the level of LF, and it is there that disambiguation takes place. Examples (1b–2b) on the other hand, tell us that disambiguation has occurred at S-structure. Thus, the English and Spanish constructions here examined, allow disambiguation at both levels of syntactic representation, whereas languages like French, Italian or Japanese –among many others– may only undergo disambiguation at LF. The disambiguation phenomenon does not seem to be motivated by any particular extra syntactic or semantic factor external to the Causative constructions. Its presence depends on inherent semantic properties of (certain) causative verbs and on the thematic/semantic conditions the embedded predicate may cast over an event, including the Subject of that event. In this sense, disambiguation at LF is not carried out optionally, although, it may be so at S-structure; hence our claim that the disambiguation phenomenon appears to be a property of (certain) causative verbs. If this is correct, (23) might seem superfluous. However, the difference between sentences such as (8a,b) vs. (20a,b), with the same embedded V but different VP composition, has to be explained. Clearly (23) is not a condition on representations but a semantic condition on interpretations. We claim that (23) is a Constraint that applies at LF, that is, at a level subject to semantic interpretation.

NOTES

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1. However, previous to the study of the distinction between Direct and Indirect Interpretation in Romance, Shibatani (1973, and subsequent work), had already noted this phenomenon in languages like Japanese and Turkish where a case alternation in the embedded subject –allowed only with embedded intransitive verbs– yields one or the other interpretation.

2. Notice that the logical subject of the causative complement must be [+Animate, +ap]. Nothing in the lexical property of verbs like *entrar* (to enter), or *escribir* (to write), specifies that the NP acting as subject or external argument has to meet the abovementioned characteristics, as in (i); but, for a LS to bear Direct Causation, it has to meet them. Therefore, a [-Animate] LS may appear as direct causer iff it is interpreted as being [+Animate, +ap], as shown in (ii):

- (i) a. El coche entró por la ventana
The car entered through the window
- b. La pluma escribe muy grueso
The pen writes very thick
- (ii) a. Hizo al coche que entrara por la ventana
(He/she) made the car that enter through the window
- b. ?Hizo a la pluma que escribiera grueso
(He/she) made the pen write thick

Sentences in (ii) are grammatical only if it is interpreted that someone has the ability to command a car or a pen to do something, and if the car and the pen acquire the ability to act by themselves, that is, if they become [+Animate, +ap]. On the other hand, [+Animate] NPs are endowed with semantic properties of their own, and thus irrespective of the elements that theta-marks them.

3. For an alternative account of verbs that seem to semantically affect NPs not directly subcategorized by them, see e.g., Emonds (1985). He proposes that the direct object of perception verbs, which is also interpreted as the subject of the complement clause, is actually assigned a theta-role by both the matrix and the embedded verb in bare-VP configurations. For a different approach, see Raposo (1989) who suggests an analysis of perception verbs as non-Control verbs. According to him, as a lexical property, perception verbs assign a 'secondary theta-role' to an embedded subject which is also interpreted as the matrix object.

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The Distribution of Tone in Taiwanese⁽¹⁾

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1. Introduction

The main claim of moraic theory (Hayes 1989, Hyman 1985, McCarthy & Prince 1986, 1988) is that the only real prosodic units within a syllable are moras. In this paper, I show that this claim holds for Taiwanese. In addition to moras, we also need the concept of minimal word (McCarthy and Prince 1988) and the melodic tier to account for prosodic phenomena like tone patterns and compensatory lengthening in Taiwanese. I will show that there is a constraint on the assignment of tone from the melodic tier (i.e. moras alone are not enough to account for the distribution of tone) and that the distribution of tone and the moraic structure of the syllable together provide evidence that the minimal word of Taiwanese is a bimoraic syllable.

Taiwanese, a Chinese dialect belonging to the Southern Min family, has two kinds of syllables. One ends in a sonorant segment (including vowels and sonorant consonants); the other ends in a non-sonorant segment. The former is called a "free syllable" and the latter a "checked syllable" in the traditional terminology.

There are seven surface tones in Taiwanese which can be divided into two types. One type I call "long tone" and the other "short tone" since the former is phonetically four times longer than the latter according to spectrographic analysis done by myself.

Interestingly, the free syllables always have long tone, and the checked syllables the short tone. By using moraic theory, we can account for this fact.

This paper is organized as follows. In Section 2, the tone pattern in Taiwanese is introduced. In Section 3, the syllable types are presented. In Section 4, the correlation between syllable structure and tone is demonstrated. Then, the evidence from compensatory lengthening is given in Section 5. Finally, in Section 6, the seven surface tones are reduced to five underlying tones.

2. The pattern of tones in Taiwanese

Taiwanese, like other Chinese dialects, is a monosyllabic tone language, that is, each word is one syllable and every word has a tone. The data cited in this paper are from my own dialect.

Taiwanese has seven tones: high level, rising, falling, low level, mid level, low short, and high short.⁽²⁾ Following Chao (1930), I use digits to denote

the Taiwanese tones; "5" denotes the highest pitch, "1" the lowest. For example, "55" is a high-level tone; "53" is a high-falling tone (a contour tone). Only one digit is used for short tones. For example, "2" is a low short tone.

The seven tones are given in (1), together with examples.^[3]

(1) I.	55 (high-level)	ts ^h ya	55	'car'
		swa-	55	'mountain'
II.	24 (rising)	lay	24	'come'
		i	24	'aunt'
III.	53 (falling)	lyu	53	'button'
		ban	53	'pluck'
IV.	11 (low-level)	p ^h wa	11	'break'
		se	11	'small'
V.	33 (mid-level)	bin	33	'face'
		twa	33	'large'
VI.	2 (low-short)	pit	2	'pen'
		kut	2	'bone'
VII.	4 (high-short)	ap	4	'box'
		kut	4	'slip'

Note that the short tones appear only with words that end with a voiceless stop, and the other tones appear with any words except those ending with a voiceless stop. We will come back to this point later.

3. The structure of Taiwanese syllables

3.1. Some generalizations

The maximal number of segments per syllable is four, with at most two prevocalic segments and one postvocalic segment. There is only one vowel per syllable. This vowel is the only segment that is obligatory within a syllable, e.g. /e/ 24 'shoe'. All consonants except /ʔ/ can appear in onset position, but only nasals and some voiceless stops (i.e. /p, t, k, ʔ/) can appear in coda position. Glides can either precede or follow the vowel, and are always adjacent to the vowel. The structure of Taiwanese syllables is summarized in (2).

(2)	(C)	(G)	V	(G)
				(C) (/m, n, ng, p, t, k, ʔ/ only)

3.2. Syllable types in Taiwanese

The examples given in (3) exhaust all syllable types in Taiwanese. Syllables are classified into two major types, free and checked, according to the sonority of the last segment in the syllable. Free syllables are sonorant-final, including three subtypes: vowel-final (3a), glide-final (3b), and nasal-final (3c). Checked

syllables are stop-final, i.e. nonsonorant-final (3d).

(3) **Free syllables** -- [+son]-final syllables

a. Vowel-final syllables and syllabic nasals

e	24	'shoe'
te	11	'take'
yu	55	'worry'
ɲ	33	'no'

b. Glide-final syllable

ay	11	'love'
t ^h aw	24	'head'
kway	53	'cheat'

c. Nasal-final

im	11	'shade'
swan	55	'sneak'

Checked syllables -- [-son]-final syllables

d. Stop-final syllable

yap	2	'hide'
saʔ	2	'push'
kyɔk	4	'situation'
geʔ	4	'moon'
sit	4	'real'

So far, we have seen the tone pattern and the syllable structure of Taiwanese. We will see the correlation of tone and syllable structure next.

4. The correlation of tone and syllable structure

4.1. Tones in complementary distribution

As mentioned above, the short tones only appear with checked syllables. On the other hand, free syllables never have short tones. This is shown in (4).

(4) Tone	Free syllables					Checked syl	
	Long		Tone			Short	Tone
	I	II	III	IV	V	VI	VII
	55	24	53	11	33	2	4
Free a	e 'sift'	e 'shoe'	e 'short'	ke 'marry'	e 'able'	/	
Free b	taw 'home'	taw 'cast'	taw 'Chinese peck'	taw 'fare'	taw 'bean'		
Free c	tam 'taste'	tam 'wet'	tam 'scared'	tam 'nod'	tam 'mild'		
Checked d							

In summary, the correlation between tone and syllable structure is three-fold as shown in (5).

- | | | | |
|-----|------------------|-----|--------------------|
| (5) | "Free syllable" | vs. | "Checked syllable" |
| | ==> [+son]-final | vs. | [-son]-final |
| | ==> "Long tone" | vs. | "Short tone" |

4.2. Two hypotheses

In the surface representation, there are two types of tones, long and short. Likewise, there are two types of syllables; free syllables end with a [+son] segment while checked syllable end with a [-son] segment. There are two hypotheses about this phenomenon.

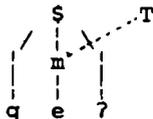
4.2.1. Hypothesis I -- there are two moras in free syllables and one mora in checked syllables.

Since the checked syllables always have short tone, we may suppose that it is a light syllable which has only one mora. In other words, a nonsonorant stop in coda position is not moraic. The free syllables, then, are heavy and have two moras. This can be seen in (6) and (7).

- (6) Free syllables (two moras - heavy)
(T = tone, m = mora, \$ = syllable)

- | | | |
|-------------|-----------------|---------------|
| a. (3a) | b. (3b) | c. (3c) |
| e 24 'shoe' | kway 53 'cheat' | im 11 'shade' |
| | | |

- (7) Checked syllables (one mora - light)
(=(3d)) ge? 4 'moon'



4.2.2. Hypothesis II -- all syllables have two moras.

Suppose that all syllables have two moras, that is, that the minimal word in Taiwanese is a bimoraic syllable [\$ mm].

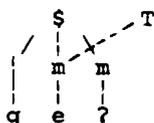
- (8) Minimal Word in Taiwanese
[\$ mm] wd

In other words, all consonants in coda position are moraic. Now we need to explain why checked syllables get only short tones and why free syllables never get short tones. The explanation can be obtained if we assume that moras linked up with a [-son] segment in the melodic tier cannot be linked to a tone. In other words, the checked syllables, which end with a [-son] segment, have only one mora that is linked up with the tone.

These two hypotheses give us the same results with respect to tone linking. Since in both hypotheses free syllables have two moras and the two moras are linked up with the tone, there is no distinction between them.

For checked syllables, the difference that the two hypotheses make is that there is only one mora in a checked syllable according to Hypothesis I, while there are two moras in a checked syllable according to Hypothesis II. But if we make the reasonable assumption that moras linked to a [-son] segment cannot link to a tone, the result in both hypotheses is the same: there is only one mora linked up with the tone in checked syllables. This is shown in (9) (cf. (7))

- (9) Checked syllables (two moras -- but only one is linked up with the tone)
 (= (3d)) ge? 4 'moon'



Since both hypotheses work equally well, how do we choose between them? Fortunately, there is an additional phenomenon that can help us make the decision, namely, compensatory lengthening.

5. Compensatory lengthening

5.1. Tone Sandhi⁽⁴⁾

Before discussing compensatory lengthening, we should look at the tone sandhi which accompanies it.

In Taiwanese, a base tone changes into a sandhi tone when it is not the last member within a tone group. (See Chen 1987 for detailed discussion.) The Tone Sandhi Rule is given in (10).

- (10) Tone Sandhi Rule (Chen 1987)
 T --> T' / ___ T [within a tone group]

Sandhi tones from my own dialect are given in (11).

(11)	I	II	III	IV	V	VI	VII
T (Base tone)	55	24	53	11	33	2	4
T' (Sandhi tone)	33	33	55	53	11	53/4	11/2

There are alternate sandhi tones for short tones (Tone VI and Tone VII). Note that the sandhi tones of Tone IV and Tone V have the same tone values as one of the two alternate sandhi tones, i.e. 53 and 11. We will come to this later.

For checked syllables not ending with a glottal stop /ʔ/, the sandhi tones are (4) for Tone VI and (2) for Tone VII. (See (12)). (Sandhi tones are put in curly brackets to distinguish them from base tones.)

(12) a.	kut 4 +	tɔ: 53	---	kut (2)	tɔ: 53
	'slip'	'fall'		'slip-fall'	
b.	kɔk 2 +	ka: 55	---	kɔk (4)	ka: 55
	'country'	'home'		'country'	

For checked syllables ending with /ʔ/, the sandhi tones are (53) for Tone VI and (11) for Tone VII together with glottal stop deletion. (We will talk about the glottal stop deletion in next section.) That is, the sandhi tone shows up long instead of short as we would expect. Examples are given in (13).

(13) a.	peʔ 4 +	tswa 53	-->	pe: (11)	tswa 53
	'white'	'paper'		'white paper'	
				(* pe: (2) tswa 53)	
b.	peʔ 4 +	sa- 55	-->	pe: (11)	sa- 55
	'white'	'clothe'		'white clothe'	
				(* pe: (2) sa- 55)	
c.	peʔ 2 +	tyu- 55	-->	pe: (53)	tyu- 55
	'eight'	'piece'		'eight pieces (of paper)'	
				(* pe: (4) tiu- 55)	
d.	peʔ 2 +	ni- 24	-->	pe: (53)	ni- 24
	'eight'	'year'		'eight years'	
				(* pe: (4) ni- 24)	

5.2. Compensatory lengthening

A syllable-final glottal stop is dropped when followed by another syllable, as shown in (14).

(14)	geʔ 4 +	niu- 24	-->	ge: (11)	niu- 24
	'moon'	'lady'		'moon'	

The Glottal Stop Deletion Rule is given in (15).

(15) Glottal Stop Deletion Rule
ʔ --> 0 / ___ C.V

If nothing follows, then the final glottal stop is not dropped and the tone remains the same, as shown in (16).

- (16) a. swan 11 + pe? 4 --> swan {53} pe? 4
 'garlic' 'white' 'garlic sprout'
 b. >55 + pe? 4 --> >{33} pe? 4
 'black' 'white' 'black and white'
 c. te 33 + pe? 2 --> te: {11} pe? 2
 'order' 'eight' 'number eight, eighth'

The change of a short sandhi tone to long sandhi tone in (13) can be understood as a case of compensatory lengthening (CL) if we assume that a tone linked up with two moras is long. The process is as follows: When the final glottal stop drops, it leaves a mora. The preceding vowel then spreads to that mora and gets lengthened. Since the second mora is not linked to a [-son] segment anymore, the tone can link to both moras and surfaces long. The derivation is given in (17).

- (17) pe? 4 -----> pe: (11) 'white'
 (Sandhi)
-

If there weren't two moras in the checked syllables, as is claimed in Hypothesis I, compensatory lengthening shouldn't have happened and we shouldn't have got a long tone. This is shown in (18).

- (18)
-

Thus we conclude that Hypothesis II is more desirable than Hypothesis I. By adopting Hypothesis II, we claim that there is a constraint on tone linking, as given in (19): a tone cannot link to a mora that dominates a [-son] segment.

- (19) Tone Linking Constraint



By proposing this constraint, we also claim that the tonal tier is able to look down to the melodic tier.

6. Returning to tones in complementary distribution

As we recall from (11), we notice that Tone IV and Tone V are in complementary distribution with Tone VI and Tone VII, respectively.

(20) (= (11)) Sandhi tones

	I	II	III	IV	V	VI	VII
T (Base tone)	55	24	53	11	33	2	4
T' (Sandhi tone)	33	33	55	53	11	53/4	11/2

Considering the same sandhi behavior in these two groups, we may suppose that these four tones are underlyingly two tones. We can assign IV/VI as L (low) and V/VII as M (mid), as in (21).

(21)

	IV	V	VI	VII
T	11 (L)	33 (M)	2 (L)	4 (M)
T'	53	11	53 / 4	11 / 2

The difference between IV and VI is neither a difference in underlying tone (they are both L), nor a difference in the moraic structure of the syllables (all syllables are bimoraic, as we have argued), but rather it is a result of the Tone Linking Constraint given in (19) above. In other words, it's due to the different values of some segmental feature in the melodic tier, the feature [son]. This is supported by the fact that after the /ʔ/ is dropped, Tone VI gets the same sandhi tone as Tone IV. In (22), it is shown that Tone IV and Tone VI have the same underlying tone L, and that the Tone Linking Constraint applies to Tone VI, not Tone IV. Thus they surface as short tone and long tone respectively.

(22) a. Tone IV (non-sandhi) b. Tone VI (non-sandhi)

--	--

In (23) we see how these two tones get the same sandhi tone.

- (23) a. Sandhi of Tone IV b. Sandhi of Tone VI
- (ke: 53) 'marry'
(as in ke: 53 ts'wa 33 'marriage')

(ke: 53) 'separate'
(as in ke: 53 k'wi 55 'separate-open')

The same is true for Tone V and Tone VII. We thus reduce the seven surface tones to five underlying tones.^[5]

(24) Underlying tone system of Taiwanese

	I	II	III	IV/VI	V/VII
Underlying	H	LH	HL	L	M
Base tone	55	24	53	11/2	33/4
Sandhi	33	33	55	53/4	11/2

7. Conclusion

I have argued that the minimal word in Taiwanese is a bimoraic syllable. This is supported by the phenomenon of compensatory lengthening. I have also shown that the seven surface tones in Taiwanese can be reduced to five underlying tones. Thus Hayes's model of compensatory lengthening (Hayes 1989) works for Taiwanese, and the concept of minimal word as proposed by McCarthy and Prince (1986, 1988) is necessary in prosodic phonology.

NOTES

- [1] I would like to thank Diana Archangeli, David Basilico, Dick Demers, Rosa Garcia, Mike Hammond, Masahide Ishihara, Jorge Lemus, James Myers, Douglas Pulleyblank, Robin Schafer, and Wendy Wiswall for helpful comments on previous versions. Any errors are my responsibility.
- [2] The tone values vary slightly among sub-dialects. (Ting 1970, Weingartner 1970)
- [3] Phoneme Inventory of Taiwanese
- a. Consonants
- | | | | | |
|---------------|----|-----------------|---|-------|
| Labial | p | p ^h | b | m |
| Dental | t | t ^h | l | n |
| Alveo-palatal | ts | ts ^h | s | dz(z) |
| Velar | k | k ^h | g | ng |
| Glottal | ʔ | | h | |
- (The alveo-palatals are palatalized when followed by

/i/ or /y/. /ts/ /ts^h/ /dz/ are affricates. /^h/ marks the aspiration. /p, t, k/ are unreleased word-finally.)

b. Vowels (The vowel in an open syllable is phonetically long. However, there is no phonemic vowel length contrast in this language.)

i e a u o ɔ
i- e- a- ɔ-

c. Glides

w y

d. Syllabic consonants

ʙ ɲ

[4] I thank Robin Schafer for reminding me to look at the tone sandhi data.

[5] A question can be raised about the lack of a corresponding short tone for Tone I, the high tone. This is interesting both diachronically and synchronically.

Diachronically speaking, the seven surface tones are the result of the tone split of the four tones in Middle Chinese due to the devoicing of the initial consonants. Thus :

Middle Chinese	Level I	Rising II	Falling III	Entering IV
Modern Taiwanese	I , II	III	IV , V	VI , VII

(N.B. Some Middle Chinese Tone II words changed to Modern Taiwanese Tone V.)

If Middle Chinese Tone III and Tone IV were in complementary distribution, it is not an accident that Modern Taiwanese IV/V and VI/VII are also in complementary distribution.

Synchronically speaking, Cantonese does have three short tones. I am not sure at this point if they correspond to three long tones in Cantonese.

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Arizona Regional Usage of Lexical Items:
 Roller Shades, Submarine Sandwich
 Swamp Cooler, and Arcadia Door
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Following the model of previous submarine sandwich lexical item studies done by Edwin Eames and Howard Robboy in 1967 and William Labov in 1988 in which telephone directory yellow pages were sampled, Phoenix Yellow Page directories were studied to determine usage changes and variations of four lexical items: roller shades, submarine sandwich, swamp cooler, and arcadia door.

My study begins with 1949, the year Hans Kurath first studied "roller shades," and ends with the last available directory for Phoenix, 1989. The sample years correlate as closely as possible with those of previous lexical studies on two of the items. Of the four lexical items and their variants, two of them--"roller shades" and "submarine sandwich"-- were chosen on the basis of previous studies of their transregional usage and two of them--"swamp cooler" and "arcadia door"--on the basis of their intraregional usage.

"Roller shades" and its variants have been studied in the Eastern United States by Kurath in 1949, in California by David Reed in 1952, in Colorado by Clyde Hankey in 1960, and in eight Southern states by George Wood in 1970. These studies, based on Kurath's original research, deal with dialect regions that have an influence on Arizona's dialect patterns.

"Submarine sandwich" and its variants such as "hero," "poor boy," and "hoagie," have been studied by Eames and Robboy in 1967. Their research includes 100 cities in the United States where they study not only lexical variants, but also frequency of use. Their samples come from newspaper articles, questionnaires, direct observations by local informants, and telephone directories. In 1988, Labov also studies distribution of "submarine sandwich" and its variants by direct observation, questionnaires, and Yellow Page listings. Although his research reports on such cities as San Francisco and Los Angeles, he focuses on Northeastern cities, including Philadelphia, Boston, and Pittsburgh.

Following these two models, I have used the Metropolitan Phoenix Yellow Page listings to study not only the local historical development of "roller shades," and "submarine sandwich," but also the two Southwestern items, "swamp cooler" and "arcadia door." I made follow-up telephone calls to businesses in the 1988-89 Yellow

Pages who have either ambiguous advertisements or who use both a lexical item and its variant in their listing.

When Kurath does his study in 1949, he calls the "roller shade" a "recent invention" (52). This new thing is a device for regulating the amount of light that enters through a window" and consists of "a piece of stiff cloth attached to a roller" (Wood 49). Kurath finds that the device is called "curtains," "roller shades," or "blinds," depending on the dialect region. "Blinds" is a Midland term also used in Pennsylvania, although in Delaware, Maryland, and Virginia, it is used interchangeably with "curtains." The variant, "curtains," is used in the Northeastern settlement area that includes Philadelphia and the Chesapeake Bay region, as well as in northeastern N. Carolina, although "curtains" also has scattered usage in the Midland region.

In 1952, Reed's study of Eastern dialect words used in California is based on origins of his informants, and his research corroborates Kurath's earlier findings. In addition to the terms used in Kurath's study, Reed finds that a different variant, "(window) shades," is used by 86% of the informants from the Hudson Valley and Southern dialect regions as well as those who have urban origins. "Curtains" is used by 5% of the informants who are from Virginia and coastal N. Carolina while 27% of the informants with Midland origins use "(window) blinds" (Reed 14).

Hankey's 1960 study of dialect usage in Colorado classifies the informants according to the protocol of I folk, II popular, III cultured, and A for older and B for younger. He reports that "(window) blinds" is used by Midland and Southern informants classed as IA and IIIB while "(window) shades" is used by informants classed as IIIB as well as IIA and B (70). Hankey discusses Colorado's position as a "transition area" for regional dialects and his findings fit the migration patterns described by Burkett (68).

One factor that enters into Hankey's study that is not mentioned in either Kurath's or Reed's study is the influence of "trade usage" on lexical choice; for example, window shades becomes the "fashionable cultured expression" (Hankey 60). Trade usage can be an important factor in lexical choice as Arthur Kimball shows when he traces the influence of the Sears-Roebuck catalogs on privileging one term over a local variant. The catalog copywriters, aware of regional variants, demonstrate a "consistent effort to differentiate and standardize" lexical choice (213). The 1911 catalog cross-indexes all variants such as "window blinds" to the main heading Shades and includes the parenthetical "subtle reminder

to the customer" that "[a]ll roller-type items on the merchandise pages are described as shades, while blinds appear as the "Venetian" type" (212).

Wood's survey in 1970 of the southern states that include Tennessee, Georgia, Florida, Alabama, Mississippi, Louisiana, Arkansas, and Oklahoma reflects commercial influence not only on lexical choice, but also on item definition. By this time, "curtains," "blinds," and "roller shades" refer to completely different products and are not used interchangeably. However, the term "(window) shades" is used in Arkansas and Oklahoma while informants in Mississippi used "roller shades." The term "blinds" is used for a product that differs from the one on the questionnaire which is based on earlier definitions, and many informants note that the questionnaire term refers to "Venetian" blinds (2).

Wood's survey shows the diachronic change in usage that Troike reminds the researcher to be aware of when dealing with "old" information (153-4). Although items from old studies may have already changed in meaning and usage, such studies are still useful for sampling future trends, especially if younger, rather than older, speakers are sampled. Just as Hankey's and Wood's studies show the effects of trade usage, my own research on the lexical item, "roller shades" and its variants shows this effect also.

Between the years 1949 and 1965, there are no significant number of listings in the Metro Phoenix Yellow Pages and Classified Section. In 1965, there is one listing under "blinds" for "Venetian blinds," but 36 listings for "Curtains and Draperies" of which two are for window shade dealers. In 1969, there are 30 listings under "Curtains and Draperies" of which three are for window shades, but no "Blinds" listings. From 1969 on, there are no listings for "Blinds" as a separate heading. In 1976, of the 30 listings under "Curtains and Draperies," three are for window shades just as it was in 1969. Four years later, in 1980, there are 75 listings under "Curtains and Draperies with nine listed under the variant, "Draperies and Curtains." Of these listings, one is for blinds, two are for shades, and one includes both items. In 1988-89, there are 91 retail listings and 26 advertisements. Ten of the ads listed "blinds" and referred to either vertical or "Venetian" type window coverings, while six of the ads not only included both "blinds" and "shades," but also differentiated between the two.

Three of the ads were ambiguous, so I telephoned to ask about the company's products. All three, Sears-Roebuck, Montgomery Ward, and Sam Bixler Carpet and Drapery, differentiate between "blinds" as vertical,

"Levelor," or "Venetian," and "shades" as the roller-type shade that includes such variations as pleated shades and Duette shades. One informant at Soft Decor Draperies and Slipcovers would not commit herself to making a distinction between blinds and shades. She said that, due to the wide range of available options, it is impossible to define the lexical items over the telephone since what one person calls a "blind," someone else may call a "shade," and vice-versa.

I find that trade usage also effects the Southwest regional items, "swamp cooler" and "arcadia door," rather than the dialect influence which has affected early roller shade terms and still seems to affect variations of sandwich terms.

For example, according to an informant at the Baca Door Company, the term "arcadia" refers to a sliding-glass aluminum patio door manufactured by the Arcadia Door Company in Arcadia, California. Another informant at the Sun Valley Door Service also said that "Arcadia" is just a brand name. Use of the term seems to correspond with the manufacturer's market area. In the 1988-89 Yellow Page listings, from a total of 113 listings and 15 ads, the item is identified as "Arcadia" only three times. Previous Yellow Page listings from 1949 through 1980 do not list the specific brand name, but, rather, the generic term, "sliding glass patio doors."

"Swamp cooler" is a folk term for an Arizona invention, but it is known by the more prestigious term, "evaporative cooler," in all of the publications surveyed, except the Home Depot advertisement flyer, Tucson Shopper, and Pennysaver. The cooler, described by Leonard Simes of the Arizona Republic, was originally a large wooden box with double layers of chicken wire for sides that held excelsior pads. A water hose was hooked to the top and, as the water trickled down, an ordinary fan inside the box blew cooled air outward. The unit was commercially manufactured by the Palmer Manufacturing Company in 1934 (102). Sime's Arizona Republic article and all of the Yellow Page listings refer to the unit as either "evaporative cooler" or its variant, "evap cooler." The units are also referred to by this term in the Want Ad section of the Arizona Republic, although a classified ad clerk at the Pennysaver said that they use whatever term the customer uses.

My study does suggest that trade usage has an influence on these two lexical choices as either preferred or dispreferred based on Kimball's discussion. In contrast, my research on the lexical choice for submarine sandwiches seems to show some influence of regional dialect in-migration patterns, although, again,

my research is limited by the market influence inherent in Yellow Page listings. The item under observation is described by Eames as, "a sandwich served on a large Italian roll containing a variety of Italian meat and cheese, lettuce, tomato, onion, and garnished with olive oil and assorted spices" (279). Meats may also include tuna, roast beef, and boiled ham.

In addition to 100 urban areas included in their 1965 study, Eames and Robboy also studied sandwich terms used in Philadelphia between 1938-1943 and 1945-1946. They localized the term "hoagie," and its variants, "hoggie" and "hokie" to Pennsylvania, specifically Pittsburgh, and New Jersey as early as World War I (283).

Their study in 1965 that focuses on national usage shows no responses from the Northwest, Midwest, or Southeast. Arizona is mapped as a "low concentration" area along with California while the only term listed for Phoenix is "submarine" with a density factor listed as "some subs" (281). There are 13 different variants listed for "submarine sandwich," including the Pennsylvania term "hoagie," the Southern term "poor boy," and the New York City-Newark term, "hero." "Grinder" is listed for such diverse cities as San Francisco, Des Moines, and Philadelphia, as well as for the state of Ohio.

Labov's study lists seven variants for submarine sandwiches, including the Philadelphia term "hoagie," the Southern "Po' Boy," the New England "grinder," the New York city "hero," and the New York terms "wedge," "torpedo," and "zep" which are also used in Norristown, Pennsylvania. He finds that the national distribution patterns include areas that had not responded to Eames and Robboy's study. Northern Midland regions uses "hogie" while Southern Midland uses "submarine," as do Utah, New Mexico, and Colorado. He reports that New Jersey uses both "hero" and "hoagie," Chicago uses both "submarine" and "hoagie," and Pennsylvania is evenly distributed in its use of "hoagie" and "submarine." New York State and Los Angeles use "submarine," while New York City uses "hero," "submarine," and "hoagie." His findings on San Francisco show "submarine" and "torpedo" where Eames and Robboy show "grinder."

Before 1960, the Metro Phoenix Yellow Pages do not include any Italian-type restaurants or pizza businesses that sell either submarine sandwiches or its variants. It is not until the sample year 1976 that there are two submarine sandwich listings out of 66 pizza listings, one identified as "New York" style pizza, in the restaurant section of approximately 900 listings, and, in the same section, of the five sandwich shop listings, one uses the term "poor boy." In 1980, the restaurant

section of approximately 1300 listings has 101 pizza stores, one "New York" style, and four of them offer submarine sandwiches. There are seven submarine sandwich shops, one hoagie shop that identified itself as "Philadelphia," one hero shop, and seven sandwich shops that also include three which also offer submarines.

The 1988-89 sample year reflects a change in the Yellow Pages format. There is now a separate section for "Pizza," as well as "Sandwiches," and restaurants are cross-listed, although few of the restaurants are listed in both sections. Advertisements are now included in with the listings instead in a separate section of the directory.

I telephoned some of the stores listed either to clear up an ambiguity about their product or to ask what they offered. Of the seven pizza stores that offer generic sandwiches, all of them identify their product as "sub(marine)." One store, Uncle Sam's Pizza which identified itself as "Philadelphia," offers both hoagies and grinders, but according to an informant at the store, the hoagies are "cold" and the grinders, served with ground pepper, are "hot."

Under "Pizza," there are 183 listings, and 38 ads, of which 32 include sandwiches, 18 were identified as "submarine," with seven noting they were "New York" and one "Chicago." A telephone call to Showbiz Pizza Palace elicited the variant, "torpedo," which is submarine-sandwich fillings wrapped in pizza dough and baked. Of the 49 listings in the "Sandwich" section, 15 are "sub(marine)."

Under "Restaurant" listings, there are 165 pizza stores. Two of the listings using "submarine" note they are either "Chicago" or "New York." Of the five ads in this section, one ad identifies "sub" with "Chicago," and one with "New York," although two mention only "submarine." The crosslisting under "Pizza" has 27 stores, but only three with "submarine." Of the sandwich shops listed under "Restaurant," 14 list no specific type, 48 are "(sub)marine," one noting "Philadelphia," 23 are "Subway Sandwich" shops, two are "hero" shops, and two are "hoagie" shops, including one that identifies itself as "Philadelphia."

The correlation between self-reported origin and variant used follows not only in-migration patterns, but also dialect origins. For example, both "hoagie" and "grinder," used in North Midland Philadelphia, are used more frequently than "submarine" by Phoenix businesses that note they are "Philadelphia." Chicago and New York, both in the Inland Northern dialect region, use "submarine," as do Phoenix businesses that identify the two cities in their ads.

Other submarine sandwich variants that are used in dialect regions with influence on Arizona dialects appear in the Phoenix directories. For example, the Southern term, "poor boy," appears once, and "torpedo," used in both New York and San Francisco, is offered at one pizza store. In general, the term, "sub(marine)," used in New York, Pennsylvania, and California, is the most frequently used term in Phoenix, with "hoagie" and "hero" also mentioned. Such variants as "wedge" and "zep" do not appear.

One single factor does not influence change and variation in a lexical item, but, rather, multiple influences working together cause change and variation. The influences that affect variation either over time or within a region can come from trade usage, as in the case of "roller shades" and "swamp cooler," where one variant achieves dominance over another. A marketplace factor similar to trade usage may cause a specific term to be used as if it were public domain, as in the case of "Arcadia" door. Migration patterns influence variation when in-migrants carry variants with them, as in the case, again, of "roller shades" where the terms used in the new areas correspond to the terms used in the regions of origin, at least until commercial activities cause the referent item to change. The variants used to refer to submarine sandwiches in Phoenix show that the in-migrants have not only brought the lexical variants, but also the product with them, as exemplified by the absence of pizza or submarine sandwich listings before 1960.

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THE SEMANTICS OF ADVERBS AND THE PERCEPTION PROBLEM*

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0. Introduction

In this paper, I apply a semantic analysis for adverbs developed in Wyner (1990) to the semantics of perception verbs. The reader is referred to that paper for more detailed motivations as well as the formal semantics of the theory. In the first two sections, I will provide very brief sketches of the Perception Problem and the semantics of adverbs. Then I'll turn to a more detailed analysis of the Perception Problem within this semantics.

1. The Perception Problem

Parsons (1987) advances a theory of adverbial modification, adopted from Davidson (1967), based on the notion of an underlying event in the argument structure of the predicate. He argues that the event argument is directly attributed the property of an adverb. (1a) shows Parsons's translation of (1b).

- (1) a. Brutus stabbed Caesar violently with a knife.
 b. $\exists e$ [stabbing(e) & subject(e, Brutus) & object(e, Caesar) & violent(e) & with(e, a knife)]

(1b) should be read as: there is an event which is a stabbing event; the subject of the event is Brutus; the event is violent, etc.. The role of the event argument is to tie together the logical representations of predicates and adverbial modifiers.

However, an events theory runs into a problem when the event argument is used to represent the semantics of perception verbs. Parsons follows the account of perception verbs in Higginbotham (1983). Presumably, there is a parallel between the sentences in (2):

- (2) a. Mary saw Brutus.
 b. Mary saw Brutus stab Caesar.

Leaving details aside, Parsons represents the sentences in (2) as (3a) and (3b) respectively.

* Thanks to Professors McConnell-Ginet, Chierchia, and Landman for pointing out the issues and helping me develop this approach. Thanks also to Leslie Porterfield and Veneeta Srivastav for discussion and encouragement.

- (3) a. $3e$ [seeing(e) & subject(e , Mary) & object(e , Brutus)]
 b. $3e$ [seeing(e) & subject(e , Mary) & $3e'$ [stabbing(e') & subject(e' , Brutus) & object(e' , Caesar) & object(e , e')]]

The important point is that just as 'Brutus' is the object of perception in (3a), so is the event argument of 'Brutus stab Caesar' an object of perception in (3b). Note that (3b) has e' - an event of stabbing - as the object of the event e which is an event of seeing. What Mary sees in (3b) is an event.

There is a problem, however, given in the argument in (4) where (4a) and (4b) are the premises and (4c) is the conclusion.

- (4) a. Mary saw Brutus stab Caesar violently.
 b. Brutus stabbed Caesar with a knife.
 c. There was only one event of Brutus's stabbing Caesar.
 d. Mary saw Brutus stab Caesar violently with a knife.

Clearly, the argument need not go through in a situation where Mary did not see the knife which Brutus stabbed Caesar with. However, under an events theory the argument would go through. Therefore, the events theory licenses an illicit inference. This problem will be referred to as the Perception Problem.

It is worth pointing out that the Perception Problem arises, in part, because the event argument is being used as both an object of perception and as an argument for adverbial modification. Another reason is that the event argument is presented as a single unanalyzable argument. Presumably, a solution to the Perception Problem should not make use of the event argument in these ways.

2. The Modification Set Theory of Adverbial Modification

As proposed in Wyner (1990), I suppose that predicates have, as part of their basic argument structure, an additional argument A which is a set of unordered individuals. This is shown in the abstract for the predicate 'stab'.

- (5) $\lambda y \lambda A \lambda x$ [stab (x , y , A)]

This additional set A , called the modification set, may contain individuals such as manner or instrument. In this approach, adverbs are functions from predicates

into predicates and attribute a property to an individual argument. However, it is required that the individual argument must also be an element of the modification set of the predicate.

With this theory, (1a) can be represented as (6) where all the arguments have been existentially instantiated. (Please note again that the method of building these representations is to be found in Wyner (1990).) The elements of the modification set, m_1 and i_2 , represent individuals of manner and instrument.

(6) [stab (Brutus, Caesar, (m_1 , i_2)) &
 $[m_1 \in \{m_1, i_2\} \& \text{violent } (m_1)] \& [i_2 \in \{m_1, i_2\} \&$
 with (knife, i_2)]]

In virtue of the modification set, I'll name this theory the Modification Set Theory (MST) of adverbial modification.

Let me point out one other feature of the analysis: the representation in (6) can also be taken to represent 'Brutus stabbed Caesar with a knife' and 'Brutus stabbed Caesar.' That is, given the way the modification set is selected, it must have at least the arguments which are predicated by an adverb, but it may have more. Therefore, we can admit implicit arguments of adverbial modification. As we'll see, this characteristic of the theory is crucial in the analysis of perception verbs.

3. The Perception Problem and the Modification Set Theory

Here I will turn to the treatment of the Perception Problem. I will assume, contrary to Davidson, Parsons, and Higginbotham, that there is not always an event argument in the argument structure of action predicates over which adverbs are predicated. The MST allows us to logically represent adverbial modification without such an event argument. The event argument e is then free to be used as an object of perception only rather than as an argument of adverbs. Following some ideas suggested by Gennaro Chierchia and Fred Landman (personal communication), I suppose that a perception verb can take as an object the event argument e . Furthermore, I suppose that the relation between the event argument and the Naked Infinitival (NI) clause 'Brutus stab Caesar violently' is taken to be that event which supports the truth of the NI; that is, e is the event in which the NI is true. Schematically, this is given as in (7).

(7) $\exists e \exists x [\text{see } (x, e) \& e \approx Y]$

Where e is an event argument, x is an individual experiencer, and Y is the semantic representation of the NI. (7) should be read as 'x is in the see relation to the event e which supports the truth of Y '. This approach is similar to Situation Semantics (Barwise (1981)).

The MST allows us to have partial representations of events considered intuitively as 'things which happen' rather than as the event argument. That is, the same event can support several representations which have different degrees of specificity. For example, take (8a) and (8b). As discussed in Wyner (1990), the logical representation of (8a) and (8b), shown in (9a) and (9b) respectively, are such that (8a) entails (8b).

- (8) a. Brutus stabbed Caesar violently with a knife.
 b. Brutus stabbed Caesar with a knife.
- (9) a. [stab (B, C, { m_1 , i_2 }) &
 [$m_1 \in \{m_1, i_2\}$ & violent (m_1)] &
 [$i_2 \in \{m_1, i_2\}$ & with (knife, i_2)]]
 b. [stab (B, C, { i_2 }) &
 [$i_2 \in \{i_2\}$ & with (knife, i_2)]]

(Note that B and C simply abbreviate Brutus and Caesar.) Moreover, the event which supports the truth of (8a) will also serve to support the truth of (8b), though not vice versa. This observation can be generalized:

- (10) An event which supports a logical representation ϕ is the same event which supports a logical representation Ψ iff ϕ entails Ψ .

There is partiality in the sense that the logical representation of the event in (9b) is not as specified as in (9a) for it is missing a manner argument in the modification set. If (9a) were taken as a 'full' representation of the event, then (9b) would only be a partial representation of it. Partial representations of events will be used to solve the Perception Problem for I'll claim that in (4), while Mary sees an event of Brutus's stabbing Caesar, the representation of the event which Mary sees is only partial.

Given this framework, the argument in (4) could receive a translation as in (11):

- (11) a. [see(Mary, e) & e \approx [stab(B, C, {m₁}) & m₁ ∈ {m₁} & violent (m₁)]
 b. [stab(B, C, {m₁, i₃}) & i₃ ∈ {m₁, i₃} & with(knife, i₃)
 c. [see(Mary, e) & e \approx [leave(B, C, {m₁, i₃}) & i₃ ∈ {m₁, i₃} & with(knife, i₃) & m₁ ∈ {m₁, i₃} & violent (m₁)]

The premise in (4c), that the events are the same, is implicit in (11) for the event which supports (11b) also supports the representation in (11a) of 'Brutus stab Caesar violently'; that is, the argument in (11) meets the condition in (10). The Perception Problem remains in (11) as there are no clear limits on substituting different representations of the same event. In other words, since (11b) is supported by the same event as in (11a), why can't it be introduced into (11a) to yield (11c)?

A solution is suggested by considering the following sorts of arguments. The argument in (12), where (12d) is the conclusion, is good presumably because the arguments which represent 'unhappily' and 'with a knife' in the modification set are both objects which Mary reports seeing.

- (12) a. Mary saw Brutus leave unhappily
 b. Mary saw Brutus leave with a knife.
 c. Brutus left once.
 d. Mary saw Brutus leave unhappily with a knife.

In addition some pragmatic factors may be crucial here. For instance, while a knife is small and can be overlooked, an elephant is large and can't be overlooked, so perhaps 'visibility' is a factor in some of these judgements. The example in (13) illustrates the point for it seems the argument goes through where (13c) is the conclusion.

- (13) a. Mary saw Brutus leave.
 b. Brutus left with an elephant.
 c. Mary saw Brutus leave with an elephant.

What (12) and (13) share is the sense that the objects in (12b) and (13b) are visible: in (12b), the knife is reported to be visible; in (13b), the elephant is pragmatically visible. I would like to pin the account of why (4) is out to a 'Visibility Condition' on perception verbs; that is, (4) is a bad argument not because of a problem with how adverbial modification is done, but because of the semantics of the perception verb.

Suppose there is a meaning postulate on perception verbs roughly along the lines of (14). Intuitively, the idea is that part of the truth conditions for perception sentences is that a perception sentence with an NI is true if and only if it is true that the perceiver sees each of the individual arguments of the NI clause. One complication in the condition is that the perceiver perceives an event which supports the truth of the NI. Therefore, a 'bridge' must be built between the perceiver, via the event argument, to the individual arguments.

(14) Visibility Condition for Perception Verbs

For a perception verb P, the following condition holds between the experiencer $\mathbf{1}$, the event e which is the object of perception, and the arguments of the NI ϕ which the event supports:

- $\mathbf{1}$ P e is true iff
- i) for ϕ which e supports,
 - ii) for all arguments x_1, \dots, x_n of ϕ other than the modification set A,
 - iii) for all arguments y_1, \dots, y_n which are elements of the modification set A of ϕ ,
- $\mathbf{1}$ P x_1 & \dots & $\mathbf{1}$ P x_n & $\mathbf{1}$ P y_1 & \dots & $\mathbf{1}$ P y_n is true.

This is a strong condition as it stipulates that the perceiver directly sees all the individual parts of an event and does not see individuals which may be part of the event, but not directly visibly to the perceiver. This seems intuitively attractive: if there is a piece of paper with the letter A marked on one side, the letter B marked on the other side, and all one is shown is the side with the letter A, then I believe one could report only "I saw a piece of paper with an A printed on it" and not "I saw a piece of paper with a B printed on it" despite the fact that she saw the paper which has both an A and a B printed on it. With respect to direct perception, we are following in the spirit of Barwise (1981) and Higginbotham (1983). An additional aspect is that since manners and instruments are individuals and included in the Visibility Condition, then they too must be directly perceived.

I will demonstrate the effect of the Visibility Condition for the representation of the argument in (11). The Visibility Condition can be applied to the premise (11a). The question boils down to whether 'Mary see i_3 ' is true or not; that is, did Mary see the knife? There are two ways to answer the question. First, the answer depends on the absence of the instrument argument from the modification set. Given

the representation in (11) and the Visibility Condition, the answer is the the argument doesn't go through since there is no instrument argument in the modification set of the NI. Therefore, since we can't infer Mary saw the instrument in the premise, we can't draw the inference in the conclusion. Another way to look at the question is in terms of the semantics of the perception verb itself.

Suppose that the instrument argument were in the modification set of (4a). Instead of the argument in (11), we would have (15).

- (15) a. [see(Mary, e) & e \approx [stab(B, C, {m₁, i₃}) & m₁ ∈ {m₁} & violent (m₁)]]
 b. [stab(B, C, {i₃}) & i₃ ∈ {m₁, i₃} & with(knife, i₃)]
 c. [see(Mary, e) & e \approx [leave(B, C, {m₁, i₃}) & i₃ ∈ {m₁, i₃} & with(knife, i₃) & m₁ ∈ {m₁, i₃} & violent (m₁)]]

These will still be supported by the same event according to (10), but in this case the representation of the NI entails (15b). The instrument argument in (15a) is an implicit argument. According to the Visibility Condition, 'Mary saw i₃' must be true for the whole statement to be true. If Mary did not in fact see the instrument, then I would claim that (15a) is false: Mary did not see an event of stabbing with a knife for she did not see a knife. This makes intuitive sense for how could Mary have reported having direct perception of a knife stabbing without having directly perceived the knife. After all, if she didn't see the knife, how could she know Brutus didn't stab Caesar with a fork? On the other hand, if Mary directly saw the knife as part of the stabbing event, then (15a) would be an accurate representation and 'Mary saw i₃' would be true. Intuitively, it seems to me that if Mary saw the knife, then the argument in (15) should be good. Indeed, the argument appears to be sound for the premise (15a) and the conclusion in (15c) are virtually the same.

While there are undoubtedly questions and problems with the proposal so far, I believe that the framework accounts for the Perception Problem in a principled way. The issues of the identification of events and the precise sense in which one perceives all or only some parts of an event are not entirely clear matters in and of themselves, so it is not surprising that a treatment which makes reference to them leaves certain issues open. Nonetheless, the capability of the modification set theory to differentiate events in

virtue of the particular individuals in the modification set seems to afford us a more refined analysis of the issues.

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PHONOLOGICAL DESCRIPTIONS OF DIGLOSSIA: A LABORATORY
FOR DECIDING HOW TO AVOID HOMONYMS AND NONSENSE FORMS

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Two kinds of solutions have been proposed for blocking the derivation of homonyms and nonsense forms in grammars. The first, exemplified by Wilkinson 1974, 1975a, 1975b, 1976, is phonological. Homonymy, it is claimed, will be reduced by maintaining underlying contrasts, and this is accomplished by a general principle, the "minimization of homonymy principle" (MHP), which gives precedence to one order of a set of well-motivated rules over a competing, equally valid order whenever objectionable outputs would result from applying the latter. The second solution, presented by Kisseberth and Abasheikh 1974, assumes a greater relationship between morphology and phonology. Certain grammatical processes, e.g. past tense formation, typically occur in a limited number of ways. Choosing the wrong alternative can result in homophonous or ungrammatical derivations. The phonological rules for past tense formation in this language are thus transderivational and require the addition of a general principle, the "avoidance of homonyms principle" (AHP), which specifies that only the derivation which would not result in undesirable mergers should be taken. In this paper we will present a convincing case from Persian (Farsi) which supports the morphological solution. Specifically, we will show that a unified account of speech varieties requires a theory which emphasizes the role of the lexicon in phonological derivations. Lexical phonology, as developed by Kiparsky 1982, Mohanan 1982, Mohanan and Mohanan 1984, and Halle and Mohanan 1985, provides just such a theoretical model. Our example, which is supported by psycholinguistic investigations of how lexical items are stored in the brain, will permit us to refine the AHP in a linguistic as well as a cognitive sense.

The diglossic nature of Persian has been recognized by a number of scholars, e.g. Hodge 1957, Beeman 1974a, 1974b, 1977, Modarressi 1978, Zamir 1982. Although the distinction between Formal Style (FS) of speech, used in radio and television broadcasts, speeches, lectures and most written communications, and Informal Style (IS), used in all informal conversations, is to some extent characterized by choice of lexical items and morphophonemic alternation, e.g. the third person singular morpheme FS - æd vs. IS - e, it shows up primarily as phonological variation in specific lexical items.

A major difference between FS and IS is the [æv-ow] alternation shown in (1).

(1)	FS	IS	
	mi+ <u>ʃæ</u> v+æ m,	mi+ <u>ʃow</u> +æ m	I chew/am chewing
	mi+ <u>ʃæ</u> v+æ m,	mi+dow+æ m	I run/am running.
	mi+ <u>ʃenæ</u> v+æ m	mi+ <u>ʃnow</u> +æ m	I listen/am listening
	dæ v+an	down	galloping
	ræ v+ante	{ ræ vune }	gone
		{ rowune }	

In addition to these alternations there are numerous nouns containing the [ow] sequence which were borrowed from Arabic and in present-day Persian have no corresponding form with [æv], e.g. towših 'writing' (Ar. tæwših), towzih 'explanation' (Ar. tæwzih), towhin 'insult' (Ar. tæwhin), towlid 'production' (Ar. tæwlið), mowred 'case' (Ar. mæwred), mowj 'wave' (Ar. mæwʃ), ʔowza? 'position/posture' (Ar. ʔæwza?). There is good reason to believe that [w] is not a segmental phoneme of Farsi and must therefore be derived from an underlying /v/. That /v/ is a segmental phoneme is attested from numerous [f-v] contrasts in all environments. However, unlike all true consonants and glides in Persian, [w] never appears word initially. Furthermore, there is a gap in the system with regard to the attested phoneme /v/. It does not appear as the first member of a consonant cluster, whereas all true consonants do. However, the examples above show that [w] occurs only after /o/ in which position that /v/ doesn't occur and that it can be an initial member of a consonant cluster, but only when preceded by /o/. The gap in the system is filled and all the above data are accounted for if [w] is derived from an underlying /v/: /v/ after /o/ is changed to [w] except when geminate, since nouns like næbovvæt 'prophecy', golovv 'transgression', modævvær 'round' and tælvvovon 'capriciousness' do not become *næbowwæt, *goloww, *modowwær, and *tælowwon respectively.

Frequently there is only one form for both FS and IS, e.g. in the case of the words for 'barley', 'vine' and 'new' in (2). There is no strong evidence that all three examples can be derived from underlying representations with [æv]. The strongest case can be made for mow which has a related FS noun mæviz 'rasin'. But no strong synchronic or diachronic grounds can be advanced for deriving ʃow from underlying ʃæv or now from næv, so we must posit underlying representations of ʃov, nov and mæv respectively for these examples.

(2)	IS & FS	FS
	ʃow 'barley'	ʃovin 'from barley'
	now 'new'	novin 'new type'
	mow 'vine'	

The data can be handled by the two rules shown as (3) and (4). Assuming that both styles are derived from a single representation, a phonological solution in the sense of Wilkinson would require two orders, i.e. (3)-(4) for IS and

(4)-(3) for FS.¹ But this is not very satisfactory since it still entails a totally arbitrary block on underlying nov and jov when the productive suffix - in 'made from' is attached to them to prevent the derivations *nowin and *jowin, even though this restriction must be removed when no suffix is attached.

(3) $v \rightarrow w / \left\{ \begin{array}{c} \text{æ} \\ \text{o} \end{array} \right\} \text{ —]}_{\text{syl}}$

(4) $\text{æ} \rightarrow \text{o} / \text{ — } w$

A parsimonious account of the blocking necessary to account for all of the data is provided by lexical phonology, which is supposed to handle cases exactly like this. We will assume the validity of the Strict Cycle Condition, which stipulates that feature changing rules of this type operate only at the level of the word. Furthermore, we will assume that every lexical entry is assigned an initial syllabic structure. IS and FS may then be viewed as two dialects that differ in terms of the point at which the above rules apply in derivation. IS forms arise as a result of (3) and (4) operating on underlying representations prior to resyllabification, whereas FS forms undergo resyllabification before they are subject to these rules. (5) illustrates how this works at stratum 2, derivational morphology, and (6) shows a sample derivation at stratum 4, inflectional morphology, in Persian.²

(5)	IS/FS	FS
	nov	nov+in
resyllabification		no vin
rule 3	now	BLOCKED
Output	(now)	(novin)

(6)	IS	FS
	mi+dæ v+æ m	mi+dæ v+æ m
resyl.		mi dæ v æ m
rule 3	mi+dæ w+æ m	BLOCKED
rule 4	mi+dow+æ m	BLOCKED
resyl.	mi do w æ m	
Output	(midowæ m)	(midæ v æ m)

The other major alternation in Persian that distinguishes FS from IS is [æv-ø]. This is almost exclusively restricted to verb stems, cf. (7). The one notable exception we are aware of is the FS noun hendævane 'mellon', which may have obtained its IS form hendune by analogical extension.

(7)	FS	IS	
	mi+ræv+æm	mi+r+æm	I am going/I go
	mi+šæv+æm	mi+š+æm	I am becoming/I become
	mi+tævan+æm	mi+tun+æm	I can/am able

The problem here is to block IS derivations like *mirowæm, *mišowæm, etc. and to insure that above outputs are generated. A phonological solution would be to posit an æv truncation rule or two separate rules, the first of which elides the /v/ and the second the remaining /æ/ in just those verbs where the alternation occurs. An alternative morphological solution, which achieves the necessary blocking and at the same time is more intuitively correct in light of the demonstrated validity of rules (3) and (4) is to posit two additional rules which are fed by them. The first, (8) elides the off glide and the second, (9), the stem final vowel.

(8) $G \rightarrow \emptyset / \text{---}]$

(9) $V \rightarrow \emptyset / \text{---}]v$

Support for the morphological solution could come in the form of a general tendency in Persian to elide glides in IS. There is certainly evidence of this with the other glide in the system, /y/, witness FS bu 'smell' → IS bu, FS pay 'foot' → IS pa, FS guy 'sphere' → gu, FS ruy 'face' → IS ru etc. Cross-morphemically, when two vowels are juxtaposed one is usually elided unless this would produce ambiguity. With non-verbs the second vowel is normally elided in IS except when the first is /e/ e.g. FS baečče 'child' + æm 'my' → IS baeččaem. With verbs the first vowel is usually elided e.g. FS košt 'killed' + æ (participle) + æm 'I' → IS koštæm. Crucial cases of elision of verb stem final glides followed by elision of the remaining vowel can be found for /h/ and /y/.

(10)	FS	IS	
	mi+dæh+æm	mi+d+æm	I give/am giving
	mi+jæh+æm	mi+j+æm	I leap/am leaping
	mi+guy+æm	mi+g+æm	I say/am saying

The above evidence plus the fact that all other deletions in Persian leads us to believe that (8) and (9) are in fact well motivated. They apply after (3) and (4) to derive the IS forms as shown in (11).

(11)		mi+ræv+æm	mi+šæv+æm
	rule 3	mi+ræw+æm	mi+šæw+æm
	rule 4	mi+row+æm	mi+šow+æm
	rule 8	mi+ro+æm	mi+šo+æm
	rule 9	mi+r+æm	mi+š+æm
	Output	(miræm)	(mišæm)

The analysis exemplified in (11) is similar although more complex than the problem discussed in Kisseberth and Abasheikh, where the avoidance of homonyms was shown to play an important part in the formation of the past tense in Chi-Mwi:ni. In order to derive the appropriate IS verb forms in Persian, some verbs like ræv 'go' must be submitted to rules (3), (4), (8) and (9), but others like yæv 'chew' and dæv 'run' must be blocked from undergoing (8) and (9) to avoid creating homophonous forms with the derivations which arise as a result of the latter two rules operation on the stems like jæh 'leap' and dæh 'give'. As Kisseberth and Abasheikh noted, there seems no way of handling this except by means of a transderivational constraint, the AHP.

Our treatment of the phonological alternations which characterize stylistic variation in Persian is clearly in the morphological tradition first proposed by Kisseberth and Abasheikh, and it leads us to conclude that phonological solutions to the problem of constraining homophonous derivations such as the MHP are probably insufficient and should be reexamined to determine what morphological factors may be interacting with phonological rules. Considering Kisseberth and Abasheikh's data from Chi-Mwi:ni together with the evidence from Persian, it appears that the most general formulation of the AHP would be something like (12).

(12) The Avoidance of Homonyms Principle

Before submitting any underlying form to the phonological rules at the level of the word, check the derivational output of all other forms to determine whether homonyms will arise.

The above formulation is couched in terms of a correlary to the rules of a linguistic grammar. It should be noted that the AHP is intended to make very specific claims with respect to acquisition and lexical storage. The morphological solution to the problem of describing speech variation in Persian which we have advanced here accords with the psycholinguistic research of Mackay 1978 into the nature of lexical storage. In an experiment designed to test how complex derived forms are accessed in the brain, Mackay obtained consistently longer latency times for the retrieval of words which contained phonological changes as well as resyllabification when subjects

were asked to add affixes to stems as opposed to items where no phonological changes were required. The cognitive implication of Mackay's results for our analysis is that verb stems in Persian are stored as separate lexical entries with different sets of rules which yield different outputs; as part of these rules are instructions for when resyllabification should occur in derivation.

Notes

1. Rule (3) is stated in its most general form. It is a "structure-dependent" rule in the sense of Hayes 1986 and is thus subject to inalterability constraints. It will therefore not be able to affect geminates. For a formalization of the principle on which inalterability is based as well as an illustration of how it applies to this rule in Persian, see Hayes 1986:347.
2. We are assuming that a description of Persian requires at least four strata, although this has not been conclusively demonstrated. We have not yet found evidence supporting the need for five strata, as Halle and Mohanan have posited for English.

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Sentence Types: A Look at Imperative Constructions

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O. Introduction

Schmerling (1975, 1982) suggests that imperatives should be taken as a distinct third clause type, as opposed to the binary distinction between tensed and untensed clauses in generative grammars, for example as in the current government and binding theory (Chomsky 1981). The basis for this proposal is the findings that imperatives exhibit certain syntactically arbitrary properties and are primitive relative to indicatives with respect to formal elaboration. Akmajian (1984), however, shows that the formal properties noticed in imperatives are also shared by a class of exclamative sentences (Mad Magazine sentences, henceforth MMs). Hence he proposes that imperatives are simply a functional sentence type and play no role in a syntactic theory.

In this paper, I argue that there are non-neglectable formal properties associated with imperatives in English which are distinct from MMs. I show that imperatives, cross-linguistically, tend to be associated with specially marked formal properties. These properties serve as arguments for treating imperatives as syntactically interesting. I then show that not only do we have independent imperative constructions but also we have dependent or embedded imperative constructions. The paper is organized as follows. In Section 1, I argue that a distinction must be drawn between imperatives and MMs, where attention is directed to the particular use of elements like do, do not and don't special to imperatives in English. In Section 2, I focus on formal markers for imperatives with an emphasis on negative imperatives, establishing the hypothesis that negators are a formal property for judging imperatives from non-imperatives. In Section 3, it is shown that imperative negators in languages occur in complement clauses and it is argued that these complements are embedded imperatives. In Section 4, I conclude the paper by addressing some consequences of the hypothesis put forward in Section 2 and of the results reached in other sections in terms of sentence types.

1. Imperatives and MMs

1.1. Similarities

Akmajian (1984) observes a class of exclamative sentences (MMs) given in (1) which he argues to be syntactically equivalent to imperatives.

- (1) a. What, me worry?
 b. What! John get a job! (Fat chance)
 c. My boss give me a raise?! (Ha)
 d. Him wear a tuxedo?! (Sure)

Akmajian (1984, 2)

The similarities noticed include the following. (i) Subjects are optional in MMs as in (2a,b) and must form the the intonation center, since a reduced pronoun is not allowed as in (2c). The same is true with imperatives as shown in (3).¹

- (2) a. (You) get a job at IBM (Fat chance)?!
 b. What! (Her) call me up?! Never.
 c. Him/*im get a job?!
- (3) a. (You) leave!
 b. *Ya leave

(ii) Neither MMs nor imperatives allow tense/modal elements, i.e. AUX (Akmajian et al 1979), as illustrated in (4) and (5) respectively.

- (4) a. Him get a job?!
 b. *Him gets a job?!
 c. Her call me up?!
 d. *Her might/will call me up?!
- (5) a. Leave!
 b. *Must leave!
 c. Be nice!
 b. *Are nice!

(iii) MMs do not allow sentential adverbs as in (6), neither do imperatives as in (7).²

- (6) a. What! Her lose her job?!
 b. *What! Her unfortunately lose her job!?
- (7) a. *Certainly drive the car!
 b. *Perhaps open the door!

(iv) Syntactic operations such as topicalization are difficult to apply to MMs as shown in (8). Often it is case that topicalization is not acceptable in an imperative with the subject you present, as in (9).³

- (8) a. What! Us read that trashy novel by tommorrow?!
 b. *What! That trashy novel, us read by tommorrow?!

¹. As pointed out to me, British English allows sentences in (4) with a nominative subject, for instance, She might call me up?! Fat Chance.

². Notice that sentences in (7) would become grammatical if an intonational break were allowed after the adverbs. But (6b) cannot be accepted even if there is a break after the adverb.

³. Sentence (9) seems acceptable to some speakers with you.

- (9) The first forty problems, (*you) solve by tomorrow!
Akmejian (1984)

Based on the above, Akmejian concludes that imperatives and MMs share the same formal structure (except intonation which distinguishes them) and that this is simply an instance of one sentence type having two distinct pragmatic functions. Therefore, he concludes that it makes no sense to have an "imperative sentence" type.

1.2. Dissimilarities

Despite the above similarities, there exist many dissimilarities between MMs and imperatives, which are crucial and sufficient enough to separate them. Firstly, MMs could have subjects in either accusative case or nominative case and subjects are not restricted to second person.⁴ Imperatives, however, require only second person subjects and NPs that are second person in nature as addressees, such as somebody/anybody/everybody etc. Secondly, subjects precede the negator not in MM as in (9), whereas the subject must follow the negator in an imperative as in (10).

- (9) What! Mary not clean the room?! Nonsense.
(10) Don't you make a mess in the room!

Thirdly, MMs and imperatives use different negators, not for the former as in (11), don't or do not for the latter.⁵

- (11) a. What! Not leave early?! That is unthinkable.
b. What! *Don't leave early?! That is unthinkable.

Fourthly, imperatives are compatible with the element do whereas MMs are not:⁶

- (12) What! *Do leave early?!

The element do plays an important role in imperatives, since do in imperatives differs from do in non-imperatives. While imperative do allows aspectual words such as have (13), non-imperatives do not (14):

⁴ Akmejian (1984) reports that only subjects in accusative case is allowed. British English also allows subjects in nominative case (Keith Allan, personal communication).

⁵ Sentences like (11b) is acceptable to some speakers with an echoic and emphatic effect. According to Akmejian (1984), (11b) is ungrammatical if used as a MMs.

⁶ Again some dialects accept (12) for emphatic use.

- (13) a. Don't you have eaten all the cookies before I come back.
 b. Do have tasted the fish before you say you don't like it.
- (14) a.*We do have loved Chinese food.
 b.*We don't have eaten all the cookies before you come back.

Last but not least, both do and don't are useable as single words with their own rules of use in imperatives:

- (15) A: (About to take the last cookie in the jar)
 B: Don't!

Given these empirical facts, it seems that Akmajian's conclusion cannot be accepted. Instead, what seems clear is the fact that the particular properties associated with imperatives in English cannot be ignored on syntactic grounds. In the next section, I will look at imperatives from a micro point of view, focusing on the negative imperatives and their negators which systematically differ from the negation in non-imperative constructions.

2. Cross-linguistic tendency: imperative markers and form of negation

As a rather surprising and general phenomenon across languages, there are negative forms as a pair in imperative and non-imperative constructions. Apart from the well-known facts in Latin, Greek and Hindi, I have chosen a number of languages to conduct this investigation (16), which all turn out to have a separate negation form for imperatives.

- (16) Chinese, Thai, Indonesian, Romani, Japanese, Korean, Welsh, Yaqui, Papago, Yidin, Dyirbal, Fijian, Kusiean, Tiwi,

The results I have reached in this study coincides with that of Jelinek (1979) where out of 30 languages under investigation 25 have special negators for imperatives (Old Irish, Berber, Papago, Pawnee, Cree, Quechua, Tagalog etc). The results of the research can be summarized into two respects: (i) most languages have special markers for both positive and negative imperatives; in some languages, if the positive imperatives are not formally marked, then their corresponding negative forms are; (ii) if the negative imperatives do not have a special imperative negator, they usually make use of other negation forms which are different from the regular negation, for instance, negative imperatives use the subjunctive negation such as in Hebrew, Classic Arabic and Egyptian Arabic.

2.1. Tiwi

As a few illustrations of the imperative markers and negators, let's first look at Tiwi (Osborne 1974). Positive imperatives in Tiwi require the prefixation of the imperative morpheme Ca- to the verb stem, as shown in (17).

- (17) a. ta-jakupauli!
imp-go back
'Go back!' (you sg)
- b. na-ra-jakupauli!
you-imp-go back (you pl)

Negation of non-imperative constructions involves placing the negative adverb kaɪlu at the beginning of VP and changing the verb form into the subjunctive ma, as in (18).

- (18) a. awunu-pa-kupauli
man-future-come back
'he'll come back'
- b. kaɪlu jini-ma ta-kupauli
not he subjunctive-fut
he won't come back

Negation of imperatives, however, uses the negator natiti and requires a change of mood in the verb from imperative to future incomplete a-, as in (19).

- (19) a. ta-kaɪmi
imp-fut-do
do it!
- b. natiti n m-p-a-ta-kaɪmi
not you-np-ic-imp-fut-do
'Don't do it!'

2.2. Yidip

The next example is from Yidip (Dixon 1977), where imperatives can be formed from any verbal stem by the imperative inflection. These are shown in (20) and (21).

- (20) (nyndu:ba) buna wawa
you-all woman look-at-IMP
'(All of you) watch the woman!'
- (21) nanda wiwin wangal
I-DAT give-IMP boomerang-ABS
'Give me (your) boomerang'

Negative imperatives use a verb in regular imperative inflection preceded by the imperative negative particle giyi:

- (22) (nundu) bulmba giyi wawa
you camp IMP-not look at
'Don't you look around the camp!'

- (23) **giyi** wanga:diŋ
 IMP-not get up
 'Don't get up!'

In negative imperative constructions, the particles giyi/guni must precede the verb. As a contrast, the non-imperative negator uses a different particle nudu, which can either immediately precede or follow the verb, as shown in (24) and (25).

- (24) **nayu** **nudula** bugan
 I not eat
 'I am not eating now'
- (25) **nayu** **dina** **budi:linu** **nudu**
 I foot put down not
 'I couldn't put my foot [in the water]'

2.3. Kusaiean

Our third example comes from Kusaiean (Lee 1975), where negative imperative formation resembles that in English in having an obligatory subject-negator inversion (cf. You are not silly. Don't you be silly!). Kusaiean has SVO word order and a regular sentence can be negated by any of the following negative words tiyac (not), tihlac (not any more, not any longer) or soenna (not yet). As a rule, the negator follows the subjects, as in (26) and (27).

- (26) **El ac** **tiyac** tuhkuh
 he tense not come
 he will not come
- (27) **Kun el** **tihlac** sismohk
 Kun S-marker not smoke
 Kun does not smoke any longer

In Kusaiean, positive imperative constructions usually do not have subjects, just like that of English, but only in negative imperatives must the subject kom (you) occur and invert with the imperative negator nik:

- (28) **Nik kom** (komtacl) ahkams
 don't you kill (pl)
- (29) **Nik kom** (komtacl) pihsrapasr
 don't steal (pl)

2.4. Form of negation as a formal property

To conclude, the overall cross-linguistic evidence suggests that the negative form for imperatives differs systematically from that for non-imperatives. A question arises as to whether English has its special imperative negation. A detailed argumentation for a positive answer is far beyond the scope of this paper (see Zhang forthcoming), but I will cite

here two pieces of evidence regarding the English imperative negators don't and do not. First, as mentioned earlier, both are compatible with the aspectual have, whereas regular don't and do not are not; moreover, none of the two imperative negators allow subjects to precede them, whereas the non-imperative negators do. Secondly, imperative don't is an unseparable unit and differs from non-imperative don't which is separable. Additionally, you can be contracted to non-imperative don't as in (30a) but cannot be to imperative don't, as in (30b):

- (30) a. Dontcha wanna go now?
 b. *Dontcha hit me! Akmajian (1984, 16)

Imperative do not is even more peculiar in that it does not permit any overt subject following itself.

Thus, I propose a hypothesis in its strongest form: negation serves as a formal criterion for typing sentences as imperative and non-imperative constructions.

3. Dependent Imperative constructions

Given the above hypothesis of treating negative form as a formal property for typing sentences, I would like to suggest that there is such a thing as dependent/embedded imperative construction. I present evidence from three languages to show that imperative negators observed in independent imperative constructions also occur in embedded clauses that are complements to lexical items such as ask, tell, order etc. I then draw evidence from English to support this conclusion.

3.1. Mandarin Chinese

As given in (31), bie (IMP-not) is the negator for independent imperative constructions in Chinese.

- (31) a. Zhangsan bu chi lajiao.
 Zhangsan not eat hot pepper
 Zhangsan does not eat hot pepper.
 b. (Ni) bie chi lajiao!
 you IMP-not eat hot pepper
 Don't (you) eat hot pepper!

Bie also occurs as the negative element in embedded clauses as in (32).

- (32) a. Ta jiao/reng wo qu nar
 he ask me go there
 He asked me to go there
 b. Ta jiao/reng wo bie qu nar
 he ask me IMP-not go there
 He asked me not to go there

Given the fact that Chinese does not syntactically mark infinitives from noninfinitival clauses, it is necessary to show that the embedded clause is an indirect speech rather than a direct speech, equivalent to the corresponding English infinitive in the gloss. There is both phonological and syntactical evidence. Phonologically, there cannot be a pause between the matrix clause and the complement clause. Syntactically, the referential relations between the pronouns or reflexives in the complement and the two nominal arguments of the verb in the matrix clause, as given in (33), must be consistent with the referential relations between the speaker, addressee and the target in a direct speech, just as required for the English counterparts as in (34).

- (33) a. Zhangsan dui Lisi shuo "Bie kan wo de xin"
Zhangsan to Lisi say IMP-not read my DE letter
Zhangsan said to Lisi "Don't read my letter"
b. Zhangsan rang Lisi bie kan *wo/ta de xin
Zhangsan ask Lisi IMP-not read my/his DE letter
Zhangsan asked Lisi not to read his letter
- (34) a. Bill said to Mary "Don't read my letter to your father"
b. Bill told Mary not to read *my/his letter to *your/her father

3.2. Romani

Similar syntactic evidence for the existence of embedded imperatives is also observed in Romani, a gypsy language. The regular negator is na (not) and the imperative negator is ma (don't):

- (35) a. Ja !
go
Go
b. Ma ja !
IMP-not go
Don't go

The embedded clause in (36) must use the imperative negator ma but not na, for verbs such as rakerja (ask).

- (36)
- | | | | | | | | |
|-----|-----|---------------------------|---------|-----|------|---------|-----------|
| a. | O | John | rakerja | e | Bill | te | ja |
| | NOM | John | ask | ACC | Bill | SUBJUN | go |
| | | John asked Bill to go | | | | | |
| b. | O | John | rakerja | e | Bill | ma | te ja |
| | NOM | John | ask | ACC | Bill | IMP-not | SUBJUN go |
| | | John asked Bill not to go | | | | | |
| c.* | O | John | rakerja | e | Bill | te | na jal |
| | NOM | John | ask | ACC | Bill | SUBJUN | not go |
| | | John asked Bill not to go | | | | | |

If the complement is for verbs such as perol (try), it must be negated by na but not by ma.

- (37) a. Ov probinal te na perol
 he try Subjun not fall
 He tried not to fall.
- b. * Ov probinal ma te perol
 he try IMP-not Subjun fall

3.3. Indonesian

Indonesian provides further interesting evidence for embedded imperatives. Not only does the negative imperative particle jangan (as opposed to the regular negative particle tidak) occur in embedded clauses, but also the prefix of the verb in an embedded clause drops, as the prefix of the verb does in an independent imperative sentence. In (38a), the prefix mem- is present in non-imperative sentences. However, it disappears in imperatives, as shown in (38b).

- (38) a. John membaca buku itu.
 John read book the
 John reads the book
- b. Baca buku itu!
 read book the
 Read the book!

The same disappearance of the prefix mem- is also found in embedded clauses, optional in the positive clause in (39a) but obligatory in the negative clause in (39b).

- (39) a. Saya minta John (untuk) (me-)baca buku itu.
 I ask John to read book the
 I asked John to read the book.
- b. Saya minta John jangan baca buku itu.
 I ask John IMP-not read book the
 I asked John not to read the book.

3.5. Summary and some English facts

To sum up, the above fact that syntactic properties of negation noticed in independent imperatives are observed in certain embedded clauses in Chinese, Romani and Indonesian suggests that it is plausible to think of the embedded complements to verbs such as ask, tell etc as embedded/dependent imperatives.⁷ There is also some syntactic evidence in

⁷. Compare Sadock & Zwicky (1985) for a cross-linguistic study in which they stated that there are no dependent imperatives in terms of imperative morphology.

English which points to such a conclusion.

Imperatives in English freely allow the word please. As noticed by McCawley (1988), those that report requests (as opposed to orders) allow a preverbal please:

- (40) a. I asked Mary to please help me
 b. Fred screamed for someone to please help him
 McCawley (1988)

Furthermore, independent imperatives can form imperative conditionals (pseudo-imperatives) such as (41).

- (41) a. Be quiet or I'll call the police.
 b. Work harder and I will give you a bonus.

Surprisingly, embedded infinitival clauses may also allow "pseudo-imperatives" as substitutes, as shown in (42).

- (42) a. Sam told us [to be quiet or he'd call the police].
 b. The boss asked us [to work overtime and he'd give us a bonus].
 c. He asked me [to buy him one more beer and he'd leave]

4. Some Consequences

4.1. Parallel sentence types

If the hypothesis established in Section 2 is reasonable, the above analysis and conclusion in Section 3 follow. As a result of the conclusion reached in Section 3, the study suggests that the imperative really does constitute a separate sentence type. It has been generally recognized that there are such pairs of clause types as independent and dependent declaratives, and independent and dependent interrogatives, examples of which are given in (43) and (44).

- (43) a. Bill is a good mathematician.
 b. John thinks (that) Bill is a good mathematician
 (44) a. Is Bill a good mathematician?
 b. John wonders whether Bill is a good mathematician

Is there also a pair of independent and dependent imperatives? This question has received a negative answer from Davies (1986) and has been raised by McCawley (1988). Given the previous conclusion of embedded imperatives in Section 3, the answer in this paper is positive. The complement in (45b) is the dependent imperative on a par with the independent imperative in (45a).

- (45) a. Go to the party !
 b. John told Bill to go to the party.

4.2. A distinct third clause type?

Having arrived at the above sentence types, one must ask the question of the relationship between infinitives and dependent imperatives. Dependent imperatives in Chinese, Romani and Indonesian are complements to lexically selected verbs such as *ask*, *tell* etc. Although no clear evidence is presented in this paper to argue that independent imperatives in Chinese and Romani are infinitives, Indonesian does show that the independent imperative is syntactically marked as infinitive as in (39). The same thing can be seen clearly in the English sentences in (40) and (45). If independent imperatives are treated as infinitives, it then follows that embedded imperatives are not restricted with respect to the person form of the subject NP. Subjects can be first, second or third person, which is the case in Chinese and Romani.

Now, given the tentative conclusion that dependent imperatives, cross-linguistically, are best treated as a part of infinitives, it seems that Schmerling's suggestion for a third clause type has to be reconsidered. The result of the present study seems to indicate that imperatives could be a special type of infinitive constructions, but not necessarily a distinct third clause type in a grammar.

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