Papers

from the

Eighth Annual Meeting

of the

Atlantic Provinces Linguistic Association

Dalhousie University

Halifax, Nova Scotia

9-10 November 1984

Actes

du

Huitième Colloque Annuel

de

l'Association de Linguistique des Provinces Atlantiques

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Outre les communications qui paraissent dans le présent volume, les suivantes furent également présentées à la Sixième Réunion de l'ALPA:

L'Estrie: Modèle réduit du Québec linguistique.
Normand Beauchemin (Guest speaker/Conférencier invité)

Ambiguous Pronoun Reference as an Artistic Device in Russian.
John Barnstead.

Two Samplings of Newfoundland Words--historical in the DNE vs. regional in the SAVINE (Survey of Areal Variations in Newfoundland English).
Harold Paddock.

Acknowledgements

This volume and the November 1984 conference were made possible through the generous assistance of Dalhousie University.

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L'ALPA tient à remercier l'Université Dalhousie d'avoir subventionné le colloque annuel ainsi que la publication du présent volume.

La Syntaxe de *en* et la structure des tours *Nom de Nom*.

James R. Black
Memorial University of Newfoundland

Abstract

La syntaxe X-barre permet aux linguistes d'utiliser dans leurs analyses grammaticales des catégories synatxiques intermédiaires entre les catégories syntagmatiques et les catégories lexicales terminales. Elle rend possible la création d'une catégorie nominale N', par exemple, qui exclut du syntagme NP ses déterminants, mais qui en contient le nom et ses compléments.

Dans leurs discussions des constructions *Nom de Nom*, Elisabeth Selkirk et Jean-Claude Milner proposent une double analyse pour la suite *de + Nom*. Ce qui suit la préposition n'est un syntagme nominal entier que si un déterminant précède le nom; s'il manque de déterminant, on a affaire à une catégorie N'.

Dans cet exposé, je propose qu'un sous-ensemble seulement des suites *de + Nom* sans déterminant constitue une catégorie non-syntagmatique *de N'*; d'autres suites, qui ressemblent superficiellement aux premières, sont en effet des syntagmes prépositionnels contenant une catégorie nominale entière *de NP*. Cette idée découle des observations sur la possibilité qu'a la proforme *en* de remplacer *de + Nom*; la substitution ne peut s'effectuer qu'au cas où *en* est contrôlé par *de NP* ; là où le groupe nominal suivant *de* n'est pas un syntagme NP entier, *en* se trouve exclu.

Cette conclusion sur l'analyse structurale des constructions *Nom de Nom* se voit renforcer par des arguments relatifs à des faits de pronominalisation, de topicalisation et de relativisation.

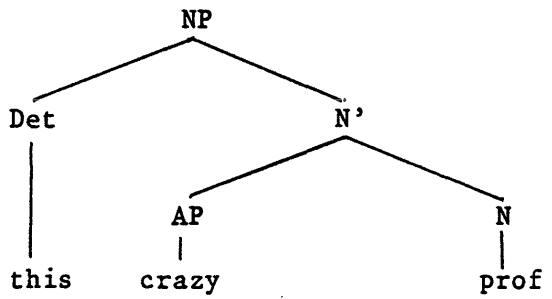
Introduction

Je me propose dans cette communication de me pencher sur la question de la structure interne des syntagmes substantivaux comportant deux éléments nominaux liés par *de*. En m'appuyant sur les données que fournit la syntaxe de *en*, j'avance une double analyse de ces syntagmes, qui s'oppose aux analyses de certains autres linguistes. Puisque ces analyses sont faites dans le cadre de la syntaxe X-barre, je commence mes propos par esquisser quelques notions de base de cet outil d'analyse.

La Syntaxe X-Barre.

Si on utilise la notation X-Barre, c'est pour représenter l'idée qu'il existe des catégories intermédiaires entre les syntagmes entiers et les constituants lexicaux terminaux. Il est donc possible de reconnaître, au sein du syntagme nominal par exemple, un élément nominal dépourvu de déterminants, mais qui comporte toujours d'autres modificateurs - adjectifs ou compléments relatifs. Cette possibilité est exploitée à profit dans la double représentation de la proforme one en anglais, qui semble remplacer la catégorie lexicale N ou la catégorie intermédiaire N' dans les phrases formées à partir de la structure arborescente de 1.

1(a)



(b) I don't like this crazy prof, but I do like that crazy one.
(Ici, one se substitue à prof, donc à la catégorie N.)

(c) I don't like this crazy prof, but I do like that one.
(Ici, c'est crazy prof qui se voit remplacer par one, donc N'.)

Les Analyses d'Elisabeth Selkirk et Jean-Claude Milner.

Ces linguistes ont étudié la structure des syntagmes qui ressemblent superficiellement à ceux de 2: ils arrivent, grosso modo, à la même conclusion. Ce qui suit de appartient soit à la catégorie N', soit à la catégorie NP (Syntagme Nominal). S'il manque de déterminant, l'élément nominal n'est qu'un N'; la présence d'un déterminant indique que la catégorie qui domine le nom est NP, et que de + NP forment ensemble un syntagme prépositionnel.

2 (a) un nombre de langues ergatives

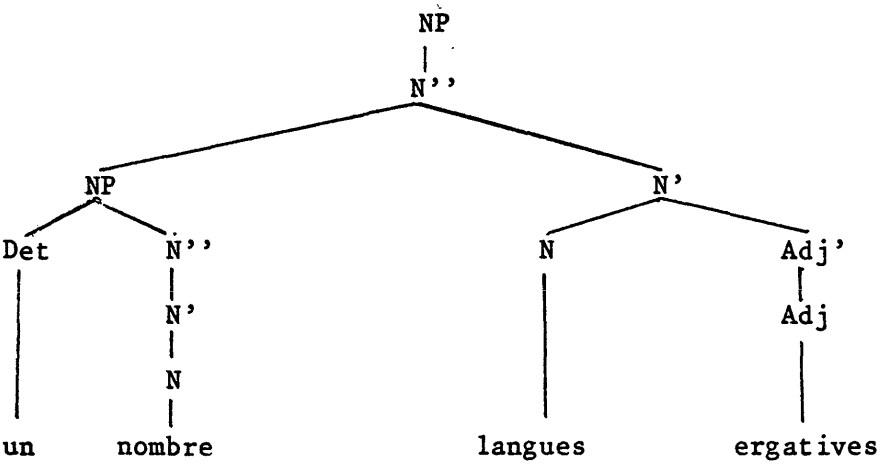
(b) un nombre de ces langues ergatives

Les structures proposées de part et d'autre pour représenter les syntagmes en 2 (a) et (b) sont exposées en 3 et 4. Elles ne se ressemblent pas tout à fait dans les détails, mais l'essentiel ne varie pas: chez Selkirk (1977:312) et Milner (1978:47-48), seule la présence superficielle d'un déterminant indique qu'on

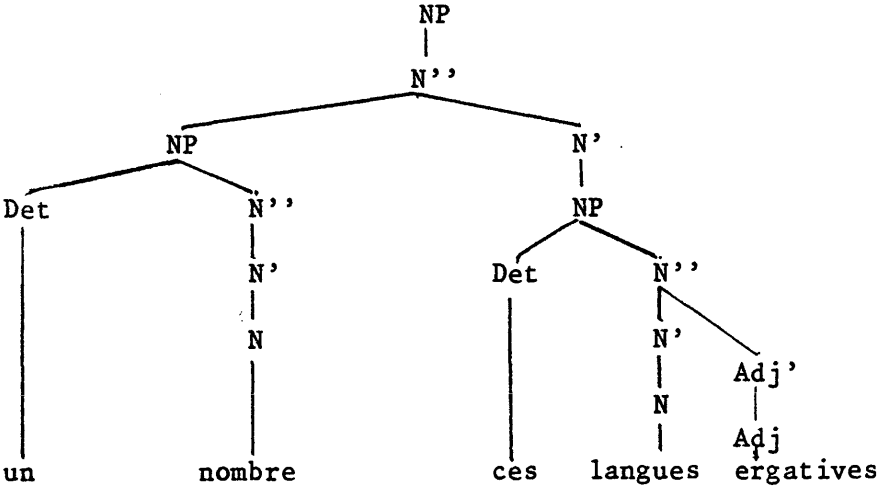
a affaire à NP. Son absence signale l'adhésion à la catégorie moindre N'. (Pour l'analyse de Selkirk, une transformation de DE-INSERTION, déclenchée par la contiguïté de NP et N', assure l'apparence en surface de la préposition.)

3. L'Analyse de Selkirk

a. Structure 'pseudopartitive'

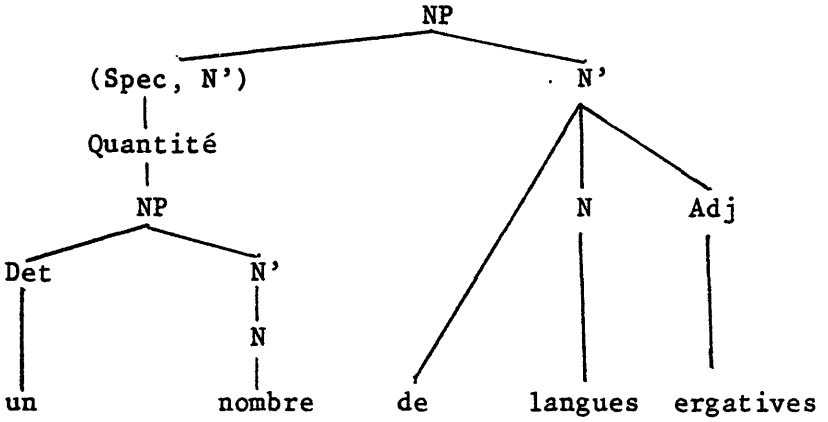


b. Structure 'partitive véritable'

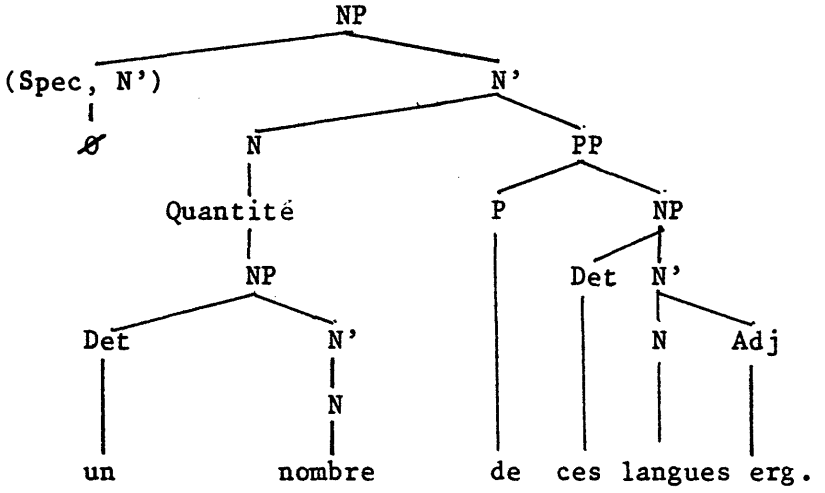


4. L'Analyse de Milner

a. Tours 'quantitatifs'



b. Tours 'partitifs'



J'affirme, par contre, que les suites de mots soulignées en 2 font partie de la même catégorie NP, l'absence de déterminant dans le premier cas n'étant que le reflet de la non-réalisation facultative d'un déterminant potentiel.

Les Antécédents de la proforme <u>en</u>.

Cette affirmation découle des observations sur ce qui peut contrôler la proforme <u>en</u>. Dans les phrases suivantes, <u>en</u> correspond à N, N', et PP. Dans le numéro 5, <u>en</u> peut remplacer un nominal tout seul, sans déterminants et sans autres modificateurs, dans la version 5(b).

5(a) Il est sur le point de rédiger des articles bouleversants sur la paléosociolinguistique.

(b) Il est sur le point d'en rédiger des bouleversants sur la paléosociolinguistique.

On est donc en droit de dire qu'<u>en</u> correspond à la catégorie N du syntagme nominal analysé en 5(c).

5(c) $_{NP}[_{Det}[des]_{N'}[_{N}[\underline{articles}]_{Adj}[bouleversants]_{pp}[sur$ la paléosociolinguistique]]]

Dans le numéro 6, <u>en</u> peut remplacer la quasi-totalité du syntagme nominal quantifié: ici, on a choisi de retenir le seul quantificateur dans la phrase 6(b).

6(a) Mon collègue vient de publier plusieurs poèmes scandaleusement érotiques.

(b) Mon collègue vient d'en publier plusieurs.

6(c) $_{NP}[_{QP}[plusieurs]_{N'}[\underline{poèmes\ scandaleusement\ érotiques}]]]$

Et dans le numéro 7, c'est le syntagme nominal entier avec la préposition qui est pronominalisé en 7(b).

7(a) J'étais époustouflé du discours récent du président.

(b) J'en étais époustouflé.

(c) $_{pp}[_{p}[\underline{de}]_{NP}[\underline{le\ discours\ récent\ du\ président}]]$

La Structure interne des séquences DE + NOM sans déterminant à l'intérieur des NPs.

Si, dans ce dernier cas, <u>en</u> correspond à une suite <u>de + NP</u>, comment peut-on expliquer le fait qu'il existe des <u>de</u> suivis d'éléments nominaux qui ne correspondent pas à <u>en</u>? C'est à dire, comment différencier des syntagmes tels qu'<u>un verre de vin</u> des syntagmes comme <u>un toit de maison</u>, pour rendre compte de leur comportement opposé vis-à-vis la pronominalisation de leurs seconds éléments? Dans les phrases de 8, c'est uniquement le premier groupe de mots qui se laisse substituer par <u>en</u>

8(a) Je veux un verre <u>de vin</u> .....J'en veux un verre.

(b) Je vois un toit <u>de maison</u>....*J'en vois un toit.

Il est possible de résoudre ce problème en postulant une différence structurale entre ces deux syntagmes basée sur des tests sur le statut catégorial du deuxième élément nominal. Si on considère qu'il faut un syntagme nominal entier pour effectuer une substitution par <u>ça</u>, ou pour attacher des relatives appositives et restrictives,

on voit tout de suite, d'après les résultats de 9, que ce qui suit de dans des syntagmes comme un toit de maison n'est pas une catégorie nominale pleine.

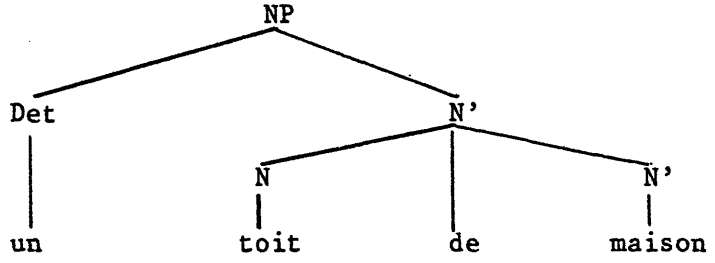
9. **Diagnostique**	**Verre de vin**	**Toit de maison**
Substitution par ça	un verre de ça	*un toit de ça
Non-restrictives	J'ai bu plusieurs verres de vin, qui est très bon pour la santé.	*Un toit de maison, qui a normalement quatre murs, est difficile à construire.
Restrictives	J'ai dégusté un verre de vin qui a été fait en Italie.	*J'ai vu un toit de maison dont les murs étaient construits de pierre taillée

Je propose donc d'engendrer des syntagmes comme un toit de maison à partir de la règle de base suivante:

10(a) NP ⟶ Det N'
(b) N' ⟶ N de N'

Ceci donne la structure arborescente 10(c).

10(c)



Le premier élément se voit dépourvu de barre en raison du fait qu'on ne trouve jamais, après cet élément, de complément, adjectival ou autre.

10(d) *un toit rouge de maison

S'il y a une modification précédente, c'est à la catégorie N' supérieure qu'elle est attachée.

10(e) un petit toit de maison

Toute modification suivante s'attache à l'une ou l'autre des catégories N'.

10(f) un toit de maison ancien
(g) un toit de maison provençale

L'accord masculin dans 10(f) indique une qualification du syntagme entier, l'accord féminin dans 10(g) indique que seul le deuxième élément est modifié.

Dans ce type de syntagme donc, l'absence de en s'explique facilement: en ne correspond à de + nom que si le nom est un NP (syntagme nominal); là où de précède un élément appartenant à une catégorie moindre N', la pronominalisation par en est refusée.

Or, si cette conclusion est vraie, et si l'analyse de Selkirk et Milner est également vraie, on devrait constater un contraste net dans le comportement de en relativement aux constructions en 2, étant donné que les mots soulignés en 2(a) et (b) font partie de deux catégories différentes. Mais ce n'est pas le cas; sans ou avec déterminant, il est possible d'anaphoriser par en dans les phrases de 11.

11(a) Elle connaît à fond un grand nombre de (ces) langues.
(b) Elle en connaît à fond un grand nombre.

Il m'incombe donc d'examiner les arguments de Selkirk et Milner en faveur d'une distinction syntaxique à l'intérieur de tels syntagmes, et de démontrer que toute suite de de + nom qui correspond à en fait partie de la même catégorie syntaxique NP. Je me limite à une discussion de trois des arguments apportés par ces linguistes.

Arguments contre l'analyse de Selkirk et Milner.

Premier Argument : La Topicalisation des séquences de + nom

Les tours Nom de Nom avec et sans déterminant après de se différencient quant à la mobilité de leur second élément. La règle d'EXTRAPOSITION DE NP crée des topicalisations en permettant le mouvement vers la gauche des syntagmes accompagnés de déterminant; les éléments nominaux qui n'en ont pas semblent plus limités dans leurs pérégrinations. Milner souligne le contraste suivant.

12(a) De ces livres, je n'ai lu aucun.
(b) *De livres, j'ai lu beaucoup.

Ce contraste, d'après Milner, s'explique du fait que l'EXTRAPOSITION ne déplace que les syntagmes prépositionnels; si la suite de livres dans 12(b) est exclu de cette catégorie en raison du statut N' du mot livres, il n'est pas étonnant que la tentative de scission du syntagme beaucoup de livres soit vouée à l'échec.

Mais cette conclusion paraît prématurée, vue l'acceptabilité des phrases de 13:

13(a) De lait, je n'ai toujours bu que très peu.
(b) De musique électronique, je n'ai jamais écouté beaucoup.

C'est Marie-Thérèse Vinet (Vinet 1977:146) qui trouve ces phrases bonnes: elle suggère que les formes restrictives ne...que ou négatives ne ...jamais contribuent à un effet de mise en relief. Dans ces conditions, la séquence de + nom sans déterminant est capable de recevoir une interprétation sémantique. Ce n'est donc pas une contrainte syntaxique qui explique la nonacceptabilité de 12(b), c'est plutôt l'absence dans le reste de la phrase d'autres éléments susceptibles de produire un effet de mise en vedette. Il reste certes la question de l'immobilité relative des suites sans déterminant; sa solution relèvera sans doute de la sémantique, et non pas de sa structure catégoriale.

Deuxième Argument:L'Interprétation des relatives non-restrictives.

Selkirk trouve que l'interprétation des relatives non-restrictives vient appuyer son analyse syntaxique des tours Nom de Nom, puisque de telles relatives ne sont associées qu'avec un noeud NP, et non avec un noeud moindre N'. Elle constate donc une ambiguïté dans la phrase 14.

14 Elle lui a acheté des douzaines de ces jonquilles, dont deux seulement étaient flétries.

L'une des deux interprétations, c'est que les deux jonquilles flétries ont fait partie de la gerbe achetée; l'autre, c'est qu'elles se trouvaient dans le groupe désigné par ces jonquilles qui n'étaient pas forcément choisies. Ces possibilités découlent de l'appartenance de chacune des deux suites soulignées à la catégorie NP.

Mais dans la phrase 15 le manque de déterminant correspond à une réduction dans le nombre d'interprétations disponibles.

15 Elle lui a acheté des douzaines de jonquilles, dont deux seulement étaient flétries.

Ce qu'on comprend ici, c'est que deux quantifie les fleurs achetées, parce que la relative ne peut s'associer qu'avec le syntagme nominal entier douzaines de jonquilles, et non avec l'ensemble des fleurs en vente. Ces considérations confirment pour Selkirk le bon sens et le bien-fondé de sa distinction structurale.

Cependant, cette conclusion ne me semble pas solide, puisque chaque phrase est capable de recevoir une interprétation supplémentaire. S'il est plausible d'associer deux dans 14 et 15 avec des fleurs individuelles dénotées par le terme jonquilles, il n'est pas moins plausible de voir aussi une association avec le mot douzaines. Le nombre de fleurs fanées s'élève donc ou à deux,

ou à vingt-quatre. Si vingt-quatre des fleurs sont abîmées, on peut supposer un rapport syntaxique avec le syntagme entier des douzaines de (ces) jonquilles; mais si deux fleurs individuelles sont flétries, c'est avec le mot jonquilles que la relative s'associe, malgré son manque de déterminant.

L'interprétation supplémentaire de 14 est en fonction des facteurs pragmatiques: l'adjectif démonstratif ces indique, sans autres indices contextuels, les fleurs choisies ou les fleurs desquelles elle a fait sa sélection. Sans déterminant, on ne parle que des jonquilles en générale.

Mais si les relatives s'associent facilement avec un nom sans déterminant, c'est que ces nominaux sont en effet dominés par le même noeud NP que les nominaux accompagnés de déterminants. Cette conclusion est appuyée par le double accord singulier et pluriel des verbes de 16.

16 J'ai acheté un kilo de pommes { qui valait trois francs / qui étaient toutes gâtées }

Troisième Argument: EN correspondant à un NP sujet.

Milner fait remarquer que si en a une origine préverbale, le NP associé doit contenir un déterminant. Ainsi s'explique la différence entre 17 et 18.

17(a) Le prix de l'or est trop élévé.
(b) Le prix en est trop élévé.

18(a) Des tonnes d'or sont sur le marché.
(b) *Des tonnes en sont sur le marché.

Sa conclusion, c'est qu'il existe une distinction syntaxique entre les éléments après de suivant qu'ils ont un déterminant ou non.

Mais un examen des contraintes sur l'extraction d'en d'un NP sujet révèle que ce deuxième élément doit être défini: si le déterminant est indéfini, en n'est pas possible. C'est ce qui ressort des phrases suivantes 19 et 20.

19(a) La porte de cette maison doit être fermée à clé.
(b) La porte doit en être fermée à clé.

20(a) La porte d'une maison doit être fermée à clé.
(b) *La porte doit en être fermée à clé.

Il s'ensuit donc que si l'antécédent de la proforme en doit être défini, et si le nom dans une construction sans déterminant est forcément indéfini, on ne trouvera pas une version grammaticale de 18(b). Ces tours sont dérivés semblablement d'une forme sousjacente de + article partitif ou article indéfini au pluriel + nom, d'où l'article a été supprimé, en raison de la règle dite de CACOPHONIE. C'est donc la nature indéfinie de telles séquences, et non pas une différence structurale, qui explique le comportement de en dont la source est préverbale.

Conclusion

L'appareil analytique fourni par la notation de la syntaxe X-Barre constitue une invitation à faire des distinctions très fines et à trouver dans la syntaxe la solution d'une foule de problèmes. Pour ce qui est des constructions qui correspondent à en, il paraît que la disponibilité d'outils grammaticaux a amené à la suranalyse. Ce n'est pas la présence superficielle d'un déterminant qui détermine la structure syntaxique, mais la possibilité de contrôler en. Là où en peut pronominaliser de + nom, nom doit être un syntagme nominal. Et inversément, si en n'anaphorise pas de + nom, nom doit appartenir à une catégorie moindre N'.

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Conversational Strategies in a Bilingual Context: Code-Switching in L'Anse-à-Canards, Newfoundland

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York University

ABSTRACT

The methodology employed in a study-in-progress of French-English code-switching in L'Anse-à-Canards, Newfoundland is outlined and preliminary results are presented. They reveal high frequency of occurrence of certain code-switch types as well as within-group variation which may be related to social networks and individual social histories.

Introduction

This paper will report on the preliminary phase of a study of code-switching in the bilingual (French-English) community of L'Anse-à-Canards situated on the Port-au-Port Peninsula of western Newfoundland.[1] Although in the latter half of the nineteenth and the early twentieth century French was the majority language in this geographically-restricted area, it is today the minority language. As a result of intermarriage between French speakers and English monolinguals and of lack of prestige associated with the local variety of French, the number of fluent French speakers has been on a steady decline for some decades (see Clarke and King (1983)). L'Anse-à-Canards is one of three communities which still have substantial French populations. Sixty of its 110 residents are fluent French speakers, several others would be classed by Dorian (1981) as 'semi-speakers', others have only passive knowledge of French, and still others (particularly the young) are English monolinguals. All residents of the community speak English, with younger speakers in general more fluent than older speakers. English is the primary language of instruction in schools of the area, today as in the past, and even those L'Anse-à-Canards residents who have studied French have only studied it briefly in high school as a second language.

In a study of covert languages attitudes using an adaptation

of the 'matched guise' technique developed by Lambert et al (1960, etc.) Clarke and King (1983) found French Newfoundlanders to be linguistically insecure and to have less than positive attitudes towards the local variety of French. It is not surprising, then, that in the presence of English monolinguals, L'Anse-à-Canards residents will almost always speak English. Language choice in conversations involving bilinguals is complex, being influenced by such factors as topic, interlocutors' status, degree of bilingualism, and degree of linguistic insecurity. Conversational code-switching, we have found, is also influenced by social and linguistic factors.

The Code-Switching Literature

John Gumperz (1982:66) defines code-switching as "the juxtaposition within the same speech exchange of passages of speech belonging to two different grammatical systems or subsystems".[2] It is distinct from the use of borrowings, either single words or frozen expressions. Thus words and expressions such as anyway, alright, and I guess so, fully integrated into many varieties of North American French, cannot be considered code-switches. Neither is the speaker's ignorance of or momentary forgetting of a lexical item in one language and substitution with the term in another language to be code-switching, as in J'ai acheté un nouveau vacuum cleaner. and Ce sont des...euh... des white clams. Nor would we consider the use of words from another code due to language loss to be code-switching since speakers have no option.

The code-switch itself may take the form of an independent sentences(s) within the same turn, as in this example in which the speaker repeats his statement:[3]

(1) S.T.: ...et la vache donne plus de lait. Now I don't know if it happened or not. Je sais pas si ç'a arrivé.
('...and the cow doesn't give any milk...I don't know if it happened.')

The code-switch may be made in reply to another speaker, i.e., at a turn boundary, as in the example below:

(2) S.T.: ...pis nous-autres, c'est pareil comme des dummies.[4]
('...now us, we're like dummies.')

G.B.: (laughs) Je pense pas, je pense pas.
('I don't think so, I don't think so.')

S.T.: Well now there you are.

In cases of intrasentential code-switching, the code-switch may take the form of a single word or phrase:

(3) J.C.: Quand j'arrive à quasiment le même endroit-là, la même affaire...i tombe dans l'eau encore. Je revire de bord encore pis je vais voir. No sir! J'ai rien vu. "Ben," j'ai dit, "tu peux sauter si tu veux mais je reviens, je reviens plus voir." J'ai parti à m'en venir...et pis ça resaute encore, c'était le same splash dans l'eau.
('When I get to just about the same place, the same thing...he falls in the water again. I turn around again and go to see. No sir! I didn't see anything. "Well," I said, "you can jump again but I'm not coming back to see." I left to come home...and then he jumps again, it was the same splash in the water.)

In the literature one finds three basic approaches to code-switching. Very recently, generativists have turned their attention to code-switching data as a source of evidence for syntactic theory (e.g., Brown (1983), Woolford (1983)). The sociolinguistic study of code-switching includes both quantitative and social interactional approaches. The quantitative approach is typified by typological classification of code-switches and by the examination of syntactic constraints on code-switching (.e.g., Pfaff (1979), Poplack (1980), (1984)). The social interactional approach, on the other hand, is concerned with code-switching as a communicative device for conveying meaning (e.g., Gumperz (1982), Heller (1982)). The function of code-switching in a particular context is believed to be dependent upon the social roles and expectations of interlocutors. Both the quantitative and social interactional approaches have influenced the methodology we have used in the L'Anse-à-Canards study, as well as our interpretation of the preliminary results.

Methodology

A high degree of familiarity with the speech community, including intimate knowledge of the language variety, is a prerequisite to the study of code-switching. Otherwise, true code-switchers could not be distinguished from integrated borrowings. In the L'Anse-à-Canards context at least, code-switching occurs

most often between and in the presence of insiders. The person without a well-established social role in the community (outside of his/her role as researcher) will not be privy to much code-switching (with the possible exception of flagged code-switches, to be discussed below). The researchers in this case have implemented participant-observation, as well as sociolinguistic-survey, methodology in L'Anse-à-Canards over a period of several years and have studied its linguistic and social systems in some detail. Moreover we have established social roles in the community quite apart from our roles as researchers.

For a preliminary analysis of code-switching in L'Anse-à-Canards, we have randomly selected from our corpus of over 100 hours of field recordings (representative of each family group and consisting primarily of free conversation) fifteen hours, the only stipulation being that fifteen different speakers be represented. We then searched each tape transcript for code-switches, ignoring loan words which have been fully integrated into Newfoundland French. We found a total of 300 potential code-switches of which we could positively identify 199 as true code-switches. The 101 problem tokens consisted mainly of one word which may have been idiosyncratic borrowings from English (e.g., one informant used so, quite rare in Newfoundland French, extensively; another had an occurrence of still, etc.). In the remaining cases we were unsure of whether a particular informant knew the French equivalent of an English token, although we knew other speakers to use the French. The 199 code-switches we could positively identify were then classified typologically according to the categories used by Shana Poplack (1984) in her study of code-switching in Ottawa-Hull French. We have supplemented these data with examples of code-switching recorded on field survey cards. These code-switches occurred in natural contexts outside of interview sessions.

Results

Looking first at the relative frequency of code-switching, we find that the average number of code-switches per hour is 13.2. This seems to be fairly high, given that these data come from taped interviews. We might expect an even higher frequency in more natural situations. These data, of course, would be quite difficult to quantify.

Typologically, we have found that many L'Anse-à-Canards code-switches involve repetition of single words, phrases, and whole sentences. In the example given below, we might interpret

the code-switch as a stylistic device used for emphasis; it might also be used to ensure the comprehension of members of the group who are English dominant:

(4) F.G.: Je crois pas anyway c'est vrai. J'ai jamais cru dans les ghosts,[5] moi. Je crois pas c'est real, ça.
('I don't think it's true. I never believed in ghosts. I don't believe it's real.')

In example 5, the code-switch ensures the comprehension of an outsider:

(5) M.M.: Oh bien, ça, ça c'etait fait avec des patrons, ça. C'est carraté barré, le herring-bone qu'i l'appelont.
('Well, that was made with patterns. It's herring-bone,...that they call it.')

In this example, the code-switch is announced by qu'i l'appelont. Some 8% of the corpus were marked by such bracketing, largely in early tapes made by the authors. Given the less than positive language attitudes held by French Newfoundlanders towards the local variety of French such bracketing is not surprising in the presence of strangers.

The second most frequent code-switch type (19%) involved the insertion of le mot juste, as in the following example:

(6) J.C.: Pis j'ai pris une de ces bouteilles-là, après ça c'était number one.
('I had one of those bottles and after that I was ...)

It is difficult with such a small sample to analyze variation within the group. Even so, one informant stands out since he differed greatly in his code-switching behavior from the other informants. Removing his data from the total number of code-switches, we find that the mean number of code-switches per hour is lowered from 13.2 to 10. S.T.'s one-hour transcript contains 62 true code-switches, with an additional 31 possible switches. He moves fluidly from one language to another with virtually no hesitations and no English bracketing.

The obvious question is why S.T.'s behavior differs markedly from that of the other informants. We believe that the answer may lie in his social history and his social role in the community.

Unlike many French Newfoundlanders who had a French father and an English mother (and two English grandmothers), S.T. is a fluent French speaker. As a child, he would have had more contact with English than most francophones of his generation. His wife is an English monolingual from another part of the province. At age 67, S.T. is an extrovert who is accepted as a member of older (basically French-dominant) and, unlike most older people, younger (basically English-dominant) social groups. There would appear, then, to be a link between his social role and linguistic behavior which warrants further study.

Future Research

We also need to extend the study to include a larger sample and to incorporate more data from naturally-occurring situations. To date we have looked only briefly at typological categories for code-switches and not at all at syntactic constraints on code-switching. We have identified S.T. as a star code-switcher; it would be equally interesting to investigate who does not code-switch. The influence of topic and the possible relationships between the use of loan words and code-switches are also worth pursuing. Our preliminary study indicates that code-switching is related to other linguistic and social variables in L'Anse-à-Canards. More precise analysis of the nature of the relationship may be done with further research.

FOOTNOTES

[1]See King (1984a) for discussion of the settlement history of L'Anse-à-Canards.

[2]There is some debate in the literature as to what constitutes code-switching. Gal (1979) refers to all single-word tokens as borrowings whereas Poplack (1980) refers to single-word tokens which are non-integrated as code-switches. There is also variation in terminology. Fasold (1984) uses the term code-mixing for what Gumperz refers to as code-switching.

[3]All examples given in this paper are drawn from our L'Anse-à-Canards corpus.

[4]We regard dummies [dʌmiy] as a borrowing into Newfoundland French.

[5]Ghosts [go:s] would appear to be an idiosyncratic borrowing.

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The Role of Linguistic Feature Types in Dialect Stereotyping

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ABSTRACT

The matched-guise technique and its derivatives have been used extensively as a means of discovering dialect stereotypes held by particular speech communities. Little attention has been paid, however, to the precise nature of the linguistic variables on the basis of which attitudinal evaluations are elicited in such studies. The aim of this paper is to demonstrate that speaker evaluations are in fact differentially affected by the particular type of linguistic feature (phonological, morpho-syntactic or lexical) incorporated in taped matched-guise samples. Moreover, although linguistic feature type appears to play a significant role in attitudinal judgments, subjects of different social and linguistic backgrounds may differ significantly in their evaluations of taped samples involving phonological, grammatical or lexical non-standardisms. Such findings have obvious implications for the methodology employed in language attitude studies, and suggest in particular that greater control must be exercised over the selection of linguistic features used to represent dialect types. [1]

Introduction

In recent years, language attitude studies have been conducted in a number of speech communities in order to discover those stereotypes associated with speakers of various standard and non-standard dialects with which a given community is familiar (see, for example, Giles and Powesland 1975). In such studies, which typically involve a genuine or modified matched-guise methodology, it is clear that the dialect stereotypes elicited are triggered by various linguistic and vocal features of the taped speech samples under evaluation. Obviously, through the use of a matched-guise methodology, many language attitude studies attempt to control for bias in judgments which might result from vocal parameters. Yet most of the attitude studies involving non-standard regional dialects have been content to gloss over the precise content of the speech segments used to elicit dialect stereotypes. Even though such studies may

involve a single reading passage, they typically do not itemize the differences in linguistic features that exist among the various standard and non-standard dialects under evaluation.

For some years, however, the literature has suggested that certain types of linguistic features serve to stigmatize speakers to a greater degree than do others. For example, a study conducted in Montreal by Chiasson-Lavoie and Laberge (1971) used essentially a direct questioning approach to draw two conclusions. First of all, the authors observed that non-standard phonological features were considerably easier to identify, on an overall basis, than were grammatical non-standardisms. Secondly, perception of non-standardisms was affected by subject variables: specifically, subjects from higher social classes were better able to recognize non-standard grammatical features, and those from lower classes were more successful at identifying phonological features.

As no statistical testing was conducted by Chiasson-Lavoie and Laberge, however, the significance of these observations remains unclear. Both conclusions, nevertheless, find some support from sociolinguistic variability research. Thus certain studies (e.g., Ash and Myhill 1983) have noted that individuals tend to integrate themselves into social groups which speak a dialect different from their own primarily by the adoption of phonological and lexical rather than grammatical features, presumably owing to the greater salience of the former two types. As for the second conclusion, that pertaining to social group differences, it has often been observed (e.g., Wolfram and Fasold 1974) that the social stratificational patterns associated with phonological variables differ considerably from those relating to grammatical features, in that social classes are more sharply differentiated with respect to the latter.

If then the literature suggests that features belonging to the same linguistic category may share similar sociolinguistic functions, there is, nonetheless, also considerable evidence that linguistic variables of one and the same type may differ considerably, within a given speech community, from a sociolinguistic perspective. Urban sociolinguistic variability studies reveal that while certain features, serving as mere social indicators, may not be associated by the community at large with the social classes which use them, other linguistic features may function as social markers, the social significance of which is recognized by the community, in the form of stylistic shifting between more standard and less standard variants. Such an observation would suggest that the category to which a linguistic variable belongs (that is, phonological, morpho-syntactic or lexical) is of less sociolinguistic relevance than is the status of the individual variable as a social diagnostic measure.

It is the aim of the present study to investigate the issue of whether, indeed, there is any foundation to the claim that linguistic

feature types or categories are sociolinguistically significant. It approaches this issue with three hypotheses in mind, namely:

1. Linguistically naive subjects are capable of recognizing non-standard linguistic features in the speech of others, and regularly stereotype speakers on the basis of the occurrence of even small numbers of such features in their speech.
2. Recognition of non-standard phonological features may be considerably easier than recognition of non-standard morphological or lexical features. Further, the degree of stigmatization associated with non-standard phonological features may be greater than that associated with the other two types.
3. Recognition of non-standard linguistic features, and stereotyping on the basis of these, is nonetheless to some degree dependent on such social variables as educational and dialect background.

Method

Taped Stimuli

To test these hypotheses, a set of taped stimuli was constructed, consisting of individual sentences, each designed to incorporate a single linguistic variable. The set included twelve non-standard sentences - that is, four sentences to represent each of the three non-standard linguistic feature types investigated - phonological, morphological (henceforth "grammatical"), and lexical. These are listed in Table 1. Each of the twelve sentences was matched with a standard version, and each such pair was read by a single male Newfoundland bi-dialectal speaker. The resulting sentences were arranged in random order on the test tape.

Subjects

In order to ensure a subject sample representative of a range of dialect and educational backgrounds, the questionnaire was administered to final-year high-school students from four Newfoundland communities. These communities were selected so as to provide maximal contrast among subject regional backgrounds, and therefore ranged from the largest urban centre of the province (St. John's), through a town of under 10,000 inhabitants (Carbonear), to two very small "outport" settlements (Avondale and Long Island, the

former of these located only some forty to fifty miles from St. John's). St. John's students, then, represent one extreme of the sample, in that they can be expected to have had extensive contact with standard speech varieties; Long Island students represent the other, in that they come from the geographically most isolated of all these communities, and may be expected to speak the least standard dialect of all the groups.

The four student groups were complemented by a fifth or teacher group, randomly selected from several hundred teachers from all over the province who were attending summer university courses in St. John's. Teachers were included in the sample to provide an indication of the rating patterns of an educated adult group, one which would have had a reasonably high degree of exposure to standard forms of English. In all five of the groups, males and females were approximately equal in number. All groups were represented by sixteen subjects, with the exception of the teacher and the Long Island student groups, which contained fifteen subjects each.

Procedure

All 78 subjects of the sample were asked to rate each speaker on seven-point scales representing four different personality traits: two status-assessment adjectives (INTELLIGENT, EDUCATED), and two solidarity-assessment scales (FRIENDLY, KIND). Two further evaluations were also conducted. The first - designed to provide additional information on the estimated status assigned each speaker - consisted of the choice of one of five occupations (doctor, accountant, welfare officer, fisherman, janitor), occupations which had been ranked as to socio-economic status by previously conducted pilot studies. The second evaluation involved identification of the speaker's regional background in terms of one of four options: Mainland Canada, St. John's, a Newfoundland city or town other than St. John's, and a Newfoundland outport (i.e., a small rural fishing community).

Once subjects had indirectly evaluated the linguistic variables incorporated in the taped segments, the 12 non-standard sentences were replayed one by one in a new random order, along with four filler sentences judged to contain no non-standard linguistic items. While subjects had previously been instructed to concentrate uniquely on the personality, occupation and background of speakers, they were now questioned directly on the actual speech forms of the taped samples. Thus subjects were asked, again on a seven-point rating scale, how "standard" the speech of each of the segments sounded to them; they were also requested to write down any non-standard items of vocabulary, pronunciation or grammar which they might have noted

in each individual taped segment. This section of the questionnaire was designed to discover whether subject opinions elicited through a direct questioning technique of this nature correlated in any significant manner with those stereotypes indirectly elicited in the first section of the questionnaire, there being mixed results on this issue in the language attitude literature.

Results

Overall Sample Results for (Linguistic) Variable Type

A first set of statistical analyses was run on the indirect or matched-guise section of the questionnaire for each of the two status and two solidarity scales. This assumed the form of an analysis of variance, conducted by means of the Bio-Medical or BMDP computer package. It involved the independent variables Subject Group (i.e., the four student and one teacher groupings noted above), Subject Sex, and the repeated measures linguistic variable which will here be called Variable Type. This consisted of the six groups or levels of stimulus sentences used in the study, namely the three non-standard sentence types (phonological, grammatical, lexical), and their three standard counterparts.

On each of the two status and two solidarity scales, significant main effects were found for Linguistic Variable Type at the .001 level of significance. In other words, speakers were evaluated significantly differently, depending on the category of linguistic feature which their taped segment embodied. Means and F-ratios are presented in Table 2 below. As this table demonstrates, all three variable types were rated lower on the two status scales in the non-standard guise; the standard/non-standard opposition is particularly marked in the cases involving the controlled phonological variables under examination (on a 1 to 7 scale for which the value 7 represents the maximum of the characteristic being evaluated, the mean of 3.38 on the characteristic "intelligent" for phonologically non-standard or PNS speakers stands in marked contrast to the corresponding mean of 4.51 for phonologically standard or PS speakers). The contrast is somewhat less apparent in the case of the lexical variables, and is least obvious with respect to the grammatical variables. In short, status scale results suggest that the overall sample does indeed stigmatize speakers on the basis of non-standard speech items, and that this stigmatization is most fully triggered by phonological rather than by grammatical or lexical features.

Solidarity scale results, though significant, are less important for the present study, given the fact that previous dialect attitude

investigations within Newfoundland have shown that, while subjects of various regional backgrounds are largely in agreement with respect to their status evaluations, such is not the case for solidarity scales. It might simply be noted here that mean differences between standard and non-standard speaker guises were considerably less apparent on solidarity than on status scales. Further, while in all but one case speakers received higher solidarity ratings in their non-standard rather than their standard guise, this difference is of magnitude only with respect to phonological variables.

On the questions dealing with regional background identification and occupational assignment, percentages were calculated for the overall sample responses on each standard/non-standard sentence type. The percentage data for regional identification are to be found in Table 3, while the occupational percentages are presented in Table 4. These results confirm those already reported, since once again the non-standard speaker guises - in particularly the phonological non-standard - are downgraded. That is, in Table 3, speakers who used phonologically non-standard variants were identified as coming from Option 4, a Newfoundland outport, to a considerably greater degree than were any other speakers (in fact, almost 75% of the time). In Table 4, these same speakers were judged to be fishermen or janitors in over 84% of cases, while this was true of speakers who used grammatically or lexically non-standard variants only approximately 55% to 58% of the time.

Interestingly, results on the second or "direct" section of the questionnaire largely confirm those emerging from the matched-guise section. In the direct question section, a three-way analysis of variance was conducted for the dependent variable "degree of standardness" attributed by subjects to each of the non-standard taped sentences. This also revealed a highly significant ($p < .001$) main effect for (Linguistic) Variable Type. On a scale of 1 to 7, on which the value "1" represented maximum non-standardness, and the value "7", maximum standardness, the overall sample mean for the phonologically non-standard sentences was only 2.27, which contrasted with a mean of 3.69 for grammatically non-standard and 5.04 for lexically non-standard speech segments. Thus on both direct and indirect attitudinal elicitation, non-standard phonological features would appear to be more stigmatized than are either grammatical or lexical non-standardisms.

As previously pointed out, after rating each sentence in terms of its perceived degree of standardness, subjects were also asked to identify directly any non-standard features that they had noted. Percentages of correct and incorrect identification are presented in Table 5 . Once again, the non-standard phonological features were much more successfully identified than were the grammatical, and non-standard lexical features were recognized as such least of all. Clearly, the results of the present study suggest that non-standard

phonological features are often salient enough to be overtly recognized as such by a speech community, whereas grammatical and lexical features may not display the same degree of salience.

Effects of Subject Variables

While the highly significant main effects which have resulted for (Linguistic) Variable Type on the various rating dimensions outlined above indicate that all subjects clearly discriminate among the various types of standard/non-standard linguistic features under investigation, the present study also confirms the hypothesis that subject variables may have significant effects on rating patterns. Of the two subject variables under examination - namely, Sex and Subject Group - it is the latter which produced by far the greater number of significant evaluational differences, on both indirect and direct measures of attitude elicitation.

Effects of age and regional dialect background

The matched-guise or indirect section of the study produced significant differences among the five subject groups on three of the four adjective scales used in this investigation. This took the form of significant interactions between Group and Linguistic Variable Type. These results are presented in Table 6. It is interesting to note from this Table that all five groups agree, by awarding lowest ratings on the two status scales to phonologically non-standard speakers (such speakers, that is, are ranked 6th or last by all groups on the scales intelligent and educated). Clearly, then, the significant differences among groups in rating patterns lie elsewhere. Indeed, for the status scales they appear to reside in the fact that the teachers tend to differ from most student groups in discriminating least between corresponding standard and non-standard speakers. The results presented in Table 6 for the status scale intelligent reveal that the teachers' highest mean rating (for LS or lexically standard speakers) is 4.94 on a scale of 7, and their lowest is 4.32 (for PNS or phonologically non-standard speakers), a difference of only .62 . Among the student groups, it is Long Island students who most resemble the teachers, while the remaining groups are characterized by a greater degree of downgrading on the most noticeably non-standard variable type, namely phonological - that is, there is a greater amount of separation between the highest and lowest means for the first three student groups in the Table. The results for the solidarity scale friendly, in the same Table, reveal that teachers rate speakers displaying phonological and to a lesser

extent grammatical non-standardisms considerably higher than do any of the student groups. Note that teachers ranked phonologically and grammatically non-standard speakers as most embodying the quality of friendliness (they award these a first and second ranking, respectively), while the same speakers obtained a much lower ranking from the student groups. Indeed, the teachers and the Carbonear student groups are the only two to award consistently higher solidarity ratings to speakers in their non-standard rather than their standard guise. In short, then, the adult teacher group appears to display greater tolerance towards non-standard speakers than do the teenage high-school groups; among the latter, it is the most non-standard subjects (the Long Island group) who tend to least discriminate, on status-related assessment scales, between standard and non-standard speakers.

All of these observations on subject group differences have emerged from the indirect measures of attitudinal elicitation used in the present study. As it turns out, they are better understood in the light of intergroup differences resulting from the direct measures. First of all, evaluations of the "degree of standardness" associated with each of the three non-standard feature types revealed a highly significant main effect (again at the .001 level) for Subject Group. Here, the chief opposition is between the adult teacher group and the Long Island student group. For the teachers attribute, on an overall basis, the least amount of standardness to the non-standard speech samples: that is, this group may be judged the one most able to perceive the non-standard linguistic features under examination. At the opposite extreme are the most non-standard subjects, the Long Island students, who - since they award the non-standard sentences the highest means on the "degree of standardness" scale - must be judged to perceive these non-standard linguistic features to a lesser extent than does any other group.

This basic pattern is confirmed by a significant interaction on the "degree of standardness" scale just discussed between Subject Group and Linguistic Feature Type. It is obvious from Table 7 that Long Island subjects differ considerably from other groups in the degree of standardness they attribute to lexically non-standard, and even more so to phonologically non-standard, sentences. Thus the Long Island mean of 3.51 for PNS (Phonologically Non-Standard) speakers contrasts sharply with the other PNS means, which are mostly in the 1.8 range. Incidentally, the general lack of linguistic perception on the part of the Long Island student subjects is corroborated by this group's relative lack of capacity to correctly identify the regional backgrounds of speakers, and failure to award low occupational status to non-standard speakers to the extent that other groups do (although neither of these trends is formally represented here by means of a Table). In short, the somewhat aberrant rating behaviour displayed by the Long Island students on both direct and indirect evaluations seems due in large measure to this group's relative lack of ability

to discriminate between the standard and non-standard features incorporated in the present study.

Effects of subject sex

The subject variable Sex produced few significant results on either the direct or the indirect measures of attitude elicitation. The absence of such results is somewhat surprising, in the light of the many observations in the sociolinguistic literature to the effect that males and females differ considerably with respect to speech production. It is nevertheless quite in keeping with results from several other language attitude studies conducted in the Newfoundland context (e.g., Clarke 1980).

Conclusions

A number of conclusions emerge from the research into linguistic feature types described above:

1) Linguistically naive subjects do recognize non-standard features in the speech of others, and do stigmatize speakers on the basis of these features; in the present study, this is evident from the low ratings attributed to the non-standard speech guises on status scales. It is clear that such judgments can be made on the basis of very few occurrences of non-standard linguistic variables - in this study, usually from one to three such occurrences per sentence.

2) In general, non-standard phonological features are more perceptible, and at the same time more stigmatized, than are either non-standard grammatical or lexical features. This seems to be true of all subject groups used in the study, irrespective of their divergent backgrounds.

3) Stereotyping on the basis of non-standard linguistic features is not nevertheless invariable throughout a population, but is to some degree dependent on subject variables. While other language attitude studies have suggested that socio-economic status is a factor in this regard, the present study indicates that the variables of age/educational level and dialect background may also play a role; subject sex seems to be relatively insignificant. To summarize, adult teachers seem to be both more aware and more tolerant than are teenagers of non-standard features in the speech of others. It would also appear that those subjects who use least standard varieties of English are in general least perceptive of non-standard features, and

consequently downgrade speakers less on the basis of such features than do more standard subject groups. Lest this observation appear a statement of the obvious, it should be noted that it seems to conflict with certain of the conclusions drawn by urban variability studies, to the effect that speakers who are most sensitive to prestige features in the speech of others typically themselves use few such features in casual speech (e.g., Labov 1972).

The conclusions of this paper have important implications for attitudinal research involving standard and non-standard dialects. For unless researchers exercise rigorous control over the non-standard linguistic variables which they incorporate into taped samples used for testing purposes, and are fully aware of the degree of prestige or stigmatization associated with these variables by the speech community under investigation, they may unknowingly inject a bias into their work. Conclusions deriving from the many attitudinal studies which do not contain precise information as to the linguistic variables involved may be somewhat misleading, to say the least.

FOOTNOTES

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Features Under Examination	Taped Sample Sentence
I. Phonological (Sentences provided in standard orthography only)	
1. [θ] pronounced [t], [ð] pronounced [d]	If it wasn't <u>th</u>is <u>th</u>ing, it was <u>th</u>at <u>th</u>ing, or <u>th</u>e other <u>th</u>ing
2. [h] deletion	Put your <u>h</u>at on the <u>h</u>anger over <u>h</u>ere
3. [ɔj] pronounced [aj]	Ro<u>y</u> and Mary's two bo<u>y</u>s left their to<u>y</u>s all over the place
4. [ɔr] pronounced [ar]	There are fo<u>ur</u> clean fo<u>r</u>ks on the table, and two dirty fo<u>r</u>ks too
II. Grammatical	
1. <u>some</u> used as an adverbial intensifer	Bob looked <u>some</u> stupid! That was <u>some</u> stupid thing to do!
2. use of the <u>after-</u> perfect	He's <u>after talking</u> to her twice already and she's <u>after refusing</u> both times
3. <u>me</u> instead of <u>my</u> as a possessive adjective	I thought I put <u>me</u> tickets in <u>me</u> wallet, but I'm not really sure
4. <u>either</u> used instead of <u>a</u> or <u>any</u> as a generalized indefinite adjective	We didn't have <u>either</u> radio or <u>either</u> T.V. set in those days
III. Lexical	
1. <u>beat out</u> instead of <u>tired out</u>	After walking through the woods for more than two hours, he was all <u>beat out</u>.
2. <u>get a lend of</u> instead of <u>borrow</u>	I've got to <u>get a lend of</u> a hammer if you want me to finish this up
3. <u>lunch</u> instead of <u>snack</u>	We had a little <u>lunch</u> before going to bed: some bread and tea
4. <u>handy</u> instead of <u>close</u>	He was so <u>handy</u> to me I could see the whites of his eyes

Table 1. <u>Linguistic Variables Used in the Present Study</u>

VARIABLE TYPE	'STATUS' SCALES		'SOLIDARITY' SCALES	
	INTELLIGENT	EDUCATED	FRIENDLY	KIND
PNS	3.38	2.94	4.43	4.51
GNS	4.25	4.06	4.66	4.63
LNS	4.37	4.24	4.70	4.78
PS	4.51	4.45	4.07	4.15
GS	4.50	4.38	4.59	4.57
LS	5.03	5.11	4.72	4.74
F-Ratio	66.13	115.56	14.90	11.90

Table 2. <u>Mean Ratings for "Variable Type"</u> (df = 5/340, $p < .001$ in each case. P(N)S = 'phonologically (non-)standard' sentence, G(N)S = 'grammatically (non-)standard' sentence, L(N)S = 'lexically (non-) standard' sentence)

REGION	VARIABLE TYPE					
	PNS	GNS	LNS	PS	GS	LS
1. Mainland Canada	2.71	12.68	12.01	15.30	17.80	28.53
2. St. John's	9.43	22.10	25.78	37.18	31.70	29.75
3. A Newfoundland city other than St. John's	13.33	24.00	30.03	31.62	34.40	29.93
4. A Newfoundland outport	74.53	41.22	32.18	15.90	16.10	11.79

Table 3. Percentage Identification of Regional Background, per Variable Type

OCCUPATION	VARIABLE TYPE					
	PNS	GNS	LNS	PS	GS	LS
1. Doctor	1.30	6.83	6.50	14.86	11.27	15.60
2. Accountant	4.76	16.90	16.25	19.67	35.10	25.01
3. Welfare Officer	9.57	21.13	18.85	28.40	26.43	28.92
4. Fisherman	62.20	33.04	31.52	8.73	10.73	8.42
5. Janitor	22.17	22.10	26.88	28.34	16.47	22.05

Table 4. Percentage Assignment of Occupational Status per Variable Type

IDENTIFICATION	TYPE OF NON-STANDARDISM		
	PNS	GNS	LNS
1. Correct identification	79.13	46.19	21.45
2. Correct identification of non-standardism type, but non-specific	4.91	10.40	0.65
3. Incorrect or no identification	15.96	43.41	77.90

Table 5. Percentage Identification of Non-Standard Features of Taped Samples

VARIABLE TYPE	GROUP				
	St. John's	Avondale	Carbonear	Long Island	Teachers
PNS	1.80	1.84	2.46	3.51	1.81
GNS	3.69	3.32	3.96	3.87	3.67
LNS	4.96	4.96	5.08	5.89	4.34

Table 7. Group Means per Variable Type on the Degree of Standardness Scale
($F = 4.48$, $df = 8/136$, $p < .001$)

VARIABLE TYPE	GROUP				
	1. St. John's (SJ)	2. Avondale (A)	3. Carbonear (C)	4. Long Island (LI)	5. Teachers
	INTELLIGENT (F = 3.09)				
1. PNS	3.15(6)	2.64(6)	3.24(6)	3.61(6)	4.32(6)
2. PS	4.45(2)	4.34(3)	4.49(2)	4.48(5)	4.86(3)
3. LNS	4.06(4)	4.13(4)	4.49(2)	4.54(3)	4.69(5)
4. LS	4.73(1)	5.35(1)	5.06(1)	4.76(1)	4.94(1)
5. GNS	3.91(5)	4.09(5)	4.20(5)	4.53(4)	4.78(4)
6. GS	4.09(3)	4.47(2)	4.25(4)	4.72(2)	4.91(2)
	EDUCATED (F = 2.77)				
1. PNS	2.60(6)	2.46(6)	2.71(6)	3.53(6)	3.46(6)
2. PS	4.38(2)	4.22(3)	4.39(3)	4.54(2)	4.78(3)
3. LNS	3.88(4)	4.00(4)	4.50(2)	4.48(3)	4.37(5)
4. LS	4.66(1)	5.53(1)	5.10(1)	5.04(1)	5.23(1)
5. GNS	3.80(5)	3.77(5)	4.05(5)	4.11(5)	4.59(4)
6. GS	3.91(3)	4.44(2)	4.28(4)	4.41(4)	4.87(2)
	FRIENDLY (F = 2.92)				
1. PNS	4.23(5)	4.55(5)	4.08(5)	3.98(5)	5.33(1)
2. PS	4.22(6)	4.04(6)	3.92(6)	3.83(6)	4.30(6)
3. LNS	4.39(3)	4.88(2)	4.80(1)	4.59(2)	4.83(4)
4. LS	4.89(1)	5.07(1)	4.38(2)	4.53(3)	4.51(5)
5. GNS	4.41(2)	4.82(4)	4.53(3)	4.39(4)	5.15(2)
6. GS	4.32(4)	4.86(3)	4.33(4)	4.60(1)	4.85(3)

Table 6. Means on Scales Involving Significant Group/Variable Type Interactions (df = 20/340, $p < .001$ in each case. Ranking of Variable Type per Group is provided in brackets after each mean)

Regional Varieties in English in Nova Scotia

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ABSTRACT

This report will present a selection of data obtained from a fifty-item questionnaire distributed to English-speaking residents of Nova Scotia. The questionnaire was aimed at identifying regional preferences rather than at finding unique Nova Scotia words or expressions. The items selected for this report will reflect some of the patterns emerging from this survey, such as the presence or absence of marked regional preferences.

Background

This paper reports on one part of a survey project begun in the Fall of 1983. The project consists of several parts, including personal interviews as well as a check of written sources, both printed and other. The part reported here deals with the first eleven questions of the written questionnaire.

The questionnaire consisted of fifty questions, most of them offering multiple choices for answer, and some open-ended ones. This was sent out province-wide through a variety of contacts to fifty respondents. The following information was obtained about the Respondents:

1. All were native residents of Nova Scotia and had spent most of their life in one community.

2. All were native speakers of English.

3. Their ages ranged from 17 to 95.

4. 23 were male, 27 female.

5. Although a large number were now retired, farming, fishing, and carpentry were predominant occupations among the men, while homemaking and teaching were predominant occupations among the women.

Ages were represented as follows:

	below 30	30-39	40-49	50-59	60-69	70 & over
Women	1	3	7	4	6	6
Men	5	3	2	6	6	1

Representation of places in Nova Scotia was as follows: fourteen respondents from Cape Breton, representing ten communities, and thirty-six from Mainland Nova Scotia representing twenty-five communities.

The questionnaire was divided as follows:

Questions 1-11 referred to the house, the rooms, and common furnishings.

Questions 12-29 referred to names of foods and meals.

Questions 30-34 referred to words descriptive of likeable people, bright people, unlikeable people, dull people, and playful children.

Questions 35-38 referred to assorted words and expressions.

Questions 39-50 concluded with a quick check on preferred words for dandelions, inchworms, green gages and the like.

Uniform Responses

One of the main results observable in the replies is a rather marked division between questions which drew uniform replies and those that drew divided replies. For this purpose 40 or more answers will count as uniform replies. In the questions in unit 1-11 those which drew uniform replies are given below.

Ques. 1. What do you call the biggest room in the house?

a. the living room b. the front room c. the parlour d. other

Reply:			
a.	40	20M	20F
b.		3M	3F
c.		___	2F
d.		___	2F

The above result is similar to Scargill 1974.

Ques. 4. What do you call a porch at the back of the house?

a. back porch b. back stoop c. other

Reply:			
	a.	47	22M 25F
	b.		--- ---
	c.		1M 2F

Question 4 with its uniform reply, contrasts sharply with Questions 2 (Front Porch) and 3 (Basement) where the answers are divided.

Ques. 5.4 What do you call the low table to put things on? (No alternatives were given.)

Reply:			
	coffee table	43	18M 25F
	stand		1M 1F
	table		2M ---
	tea table		1M ---
	omitted		1M 1F

Ques. 5.5 What do you call a table to put an ashtray or lamp on? (No alternatives were given.)

Reply:			
	end table	43	20M 23F
	table		3M ---
	stand		--- 2F
	occasional table		--- 2F

Questions 5.4 and 5.5 contrast with 5.1 (chesterfield), 5.2 (love seat) and 5.3 (chair) where the answers were not uniform.

Ques. 7.2 What do you call the appliances in the area where people wash their family laundry?

a. dryer b. drying machine c. other

Reply:			
	a. dryer	46	21M 25F
	b. drying machine		1M ---
	c. outdoors		--- 2F
	omitted		1M ---

This contrasts with Ques. 7.1 (washer) were there was a variety of answers.

Ques. 9.1 What do you call the main appliances and equipment in the kitchen?

a. stove b. range c. kemac d. other

Reply:		21M	21F
	a. stove 42		
	b. range	---	4F
	c. kemac	1M	---
	d. cookstove	1M	---
	" electric stove	---	1F
	" woodstove	---	1F

Ques. 9.2 a. fridge b. refrigerator c. cooler d. icebox e. other

Reply:		19M	24F
	a. fridge 43		
	b. refrigerator	4M	3F
	c. cooler	---	---
	d. icebox	---	---
	e. other	---	---

One woman said "I call my fridge a refrigerator" and another one said "refrigerator" when questioned, but in a later conversation about newlyweds said "Now they have to have fridge and a washer." The word fridge is so widely used in Nova Scotia, that is hardly seems local, yet it contrasts with frigidaire in Montreal and possibly icebox in New York. At least icebox is used by Sigurney Weaver in the film Ghostbusters when she enquires what is a Bablylonian god doing in her icebox.

The responses for fridge and stove contrast with those for big pot and small pot which were not uniform.

Non-Uniform Responses

Several questions in section 1-11 yielded non-uniform responses.

Ques. 2. If a house has a porch at the front, what do people call it usually?

a. front porch b. patio c. verandah d. other

Reply:		13M	12F
	a. front porch		
	b. patio	1M	--F
	c. verandah	9M	10F
	d. porch	---	3F
	" sunporch	---	2F

Ques. 3. What do you call the part of the house under the main part?

a. basement b. cellar c. other

Reply:			
	a. basement	14M	16F
	b. cellar	8M	11F
	omitted	1M	---

Two persons metioned a difference between older people, who would say cellar and even use the expression downcellar and down cellars and younger people, who would say basement.

Ques. 5.1 What do you call a piece of furniture for three people to sit on? (No choices were given.)

Reply:			
	chesterfield	10M	17F
	sofa	4M	3F
	couch	9M	3F
	cot	---	1F
	studio couch	---	1F
	settee	---	2F

Ques. 5.2 What do you call a piece of furniture for two people to sit on? (No choices were given.)

Reply:			
	love seat	14M	15F
	settee	3M	8F
	couch	3M	1F
	bench	1M	---
	cot	1M	---
	sofa	1M	2F
	omitted	---	1F

Ques. 5.3 What do you call a comfortable chair for one? (No choices were given.)

Reply:			
	armchair	5M	7F
	easy chair	4M	7F
	chair	6M	3F
	rocker	6M	3F
	recliner	1M	2F
	rocking chair	1M	1F
	lazy boy	3M	1F
	lounge chair	2M	1F
	chesterfield chair	1M	2F

Ques. 6. What do you call the kind of cabinet where people keep their best dishes?

a. buffet 2. dresser c. other

Reply:			
	a. buffet	8M	12F
	b. dresser	1M	---
	c. china cabinet	14M	13F
	" china closet	---	1F
	" cupboard	---	1F

Ques. 7. What do you call the appliances and equipment in the area where people wash the family laundry?

7.1 a. washer b. washing machine c. other

Reply:			
	a. washer	12M	19F
	b. washing machine	10M	8F
	omitted	1M	---

7.3 a. sink b. tub c. other

Reply:			
	a. sink	17M	19F
	b. tub	4M	4F
	c. sink-tub	---	1F
	" rinsing tub	---	1F
	" washtub	---	1F
	" set-tub	---	1F
	omitted	2M	---

7.4 a. scrubboard b. washboard c. other

Reply:			
	a. scrubboard	7M	11F
	b. washboard	13M	15F
	c. washing board	1M	---
	omitted	2M	1F

Ques. 8. In the bedroom, what do you call these items?

8.1 The little table next to the bed.

a. night table b. bed-table c. nightstand
d. bedside table e. other

Reply:			
	a. night table	11M	17F
	b. bed-table	3M	1F
	c. nightstand	1M	2F
	d. bedside table	6M	6F
	e. small table	1M	---
	" bedstand	1M	1F

The preference for night table can be contrasted with nightstand in the short story Neighbors by Raymond Carver, an American born in Oregon.

8.2 A piece of furniture with drawers, without a mirror.

a. dresser b. chest of drawers c. chiffonier d. other

Reply:			
a. dresser	12M	9F
b. chest of drawers	9M	13F
c. chiffonier	---	3F
other	2M	2F

8.3 A piece of furniture with drawers and mirror:

a.dresser b. bureau c. dressing table
d. chiffonier e. other

Reply:			
a. dresser	5M	13F
b. bureau	16M	14F
c. dressing table	1M	---
d. chiffonier	---	---
omitted	1M	---

Ques. 10. What do you call a really large pot?

a. kettle b. Dutch oven c. big cooker d. stewpot
e. other

Reply:			
a. kettle	3M	5F
b. Dutch oven	2M	6F
c. big cooker	---	---
d. stewpot	10M	10F
e. large pot	4M	2F
" canner or blancher	--	1F
" big pot	3M	2F
" cauldron	---	1F
omitted	1M	---

Ques. 11. What do you call a really small pot?

a. small saucepan b. small pot c. dipper d. other

Reply:			
a. small saucepan	8M	12F
b. small pot	4M	8F
c. dipper	11M	6F
d. milk pot	---	1F

In addition, two men said "dipper" as an alternate form after making their choice.

Uniform choices were obtained in six instances in questions 1-11 that is, in six items out of twenty-one. In the remaining part of the questionnaire, uniform choices were obtained in fourteen instances. Together they indicated preferred usage with respect to the following items:

Ques. 1 the living room
Ques. 4 back porch
Ques. 5 coffee table, end table
Ques. 7 dryer
Ques. 9 stove, fridge
Ques. 12 sick to my stomach
Ques. 14 can of pop
Ques. 15 temperature in Fahrenheit
Ques. 16 distance in miles
Ques. 18 a snack
Ques. 27 this is a lot of snow compared to ...
Ques. 28 garbage
Ques. 31 good-for-nothing
Ques. 40 dandelion
Ques. 44 inchworm

Items 45, 46, 48, 49 received a negative reply, indicating that they are not used or not known.

Ques. 45 otter
Ques. 46 hoar's eggs
Ques. 48 sungob
Ques. 49 loaf-bread

All other answers were non-uniform.

Comments

Although the answers shown above reflect only one part of the questionnaire, and an analysis of the remaining questions may suggest a different interpretation, it is possible to make several inferences from the results reported above. First of all, the division between uniform and non-uniform responses is likely to occur in any one lexical group (such as parts of the house, furniture, appliances, utensils). With regard to parts of the house there seems to be the problem whether the question conveys "the same thing" to most respondents, or not. Thus "living room" apparently conveyed "the same thing" whereas basement was "finished" and cellar had "dirt floor and stone walls." This distinction would account for division in responses. "Basement" was chosen by 30 speakers, and "cellar" by 19. It appeared from additional questions, that cellar was still used by a few older speakers to denote what younger family members called basement. The expression going down cellar and down cellars still seems to be used occasionally.

As for back porch it seemed to convey the same thing to most respondents, whereas verandah or patio or front porch would relate to size, and whether the space was open or enclosed.

In Ques. 4 coffee table and end table were uniform, perhaps as being rather recent items, but chesterfield, love seat and chair showed variety. Of these, chesterfield seemed favored by women (17:27) more than by men (10:23). These results differ from Scargill 1974 where Male and Female Parents in N.S. had a much higher preference for chesterfield, and there was no difference between the sexes in this respect. Conversely, only a few women chose couch (3:27). Choice of word for comfortable chair varied, partly in relation to shape and function of the article, but also indicating that there is no strong preference for any one of the variants.

In Question 7 dryer was uniform and washer was not. If one suspects an influence of commercial usage, this would not apply here, as Sears (Summer 1982) sells only washers and only dryers. Commerical influence must be also rejected in the case of fridge and stove as Sears sells only refrigerators and only ranges. (Stoves are sold, but not for kitchen use.) Conversely, by consulting only a commercial catalogue, one cannot predict what would be the preferred word in Nova Scotia.

The part of the questionnaire examined in this report indicates, mainly, that not only is it possible to observe prevailing lexical preferences in Nova Scotia, but that these may contrast with U.S. use or with other parts of Canada, that patterns of preference may vary between men and women, and that some lexical items resist both change and commercial influence. Whether similar trends will prevail in the remainder of the questionnaire, remains to be seen.

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ETUDE COMPARATIVE DES PARLERS ACADIENS EN NOUVELLE-ECOSSE: METHODOLOGIE ET RAPPORT SUR LA PREMIERE PHASE

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Résumé

Dans cette étude nous exposons les objectifs et la démarche adoptée dans le projet "Etude comparative des parlers acadiens de la Nouvelle-Ecosse" subventionné pour deux ans par le Conseil de Recherche en Sciences Humaines. La methodolologie de l'enquête, ainsi que les modalités de choix d'informateurs sont présentées. Les expériences de la première phase du projet sont discutées; pendant cette phase, cent dix interviews ont été effectuées dans une double optique sociolinguistique et dialectologique, dans cinq régions acadiennes de la Nouvelle-Ecosse.

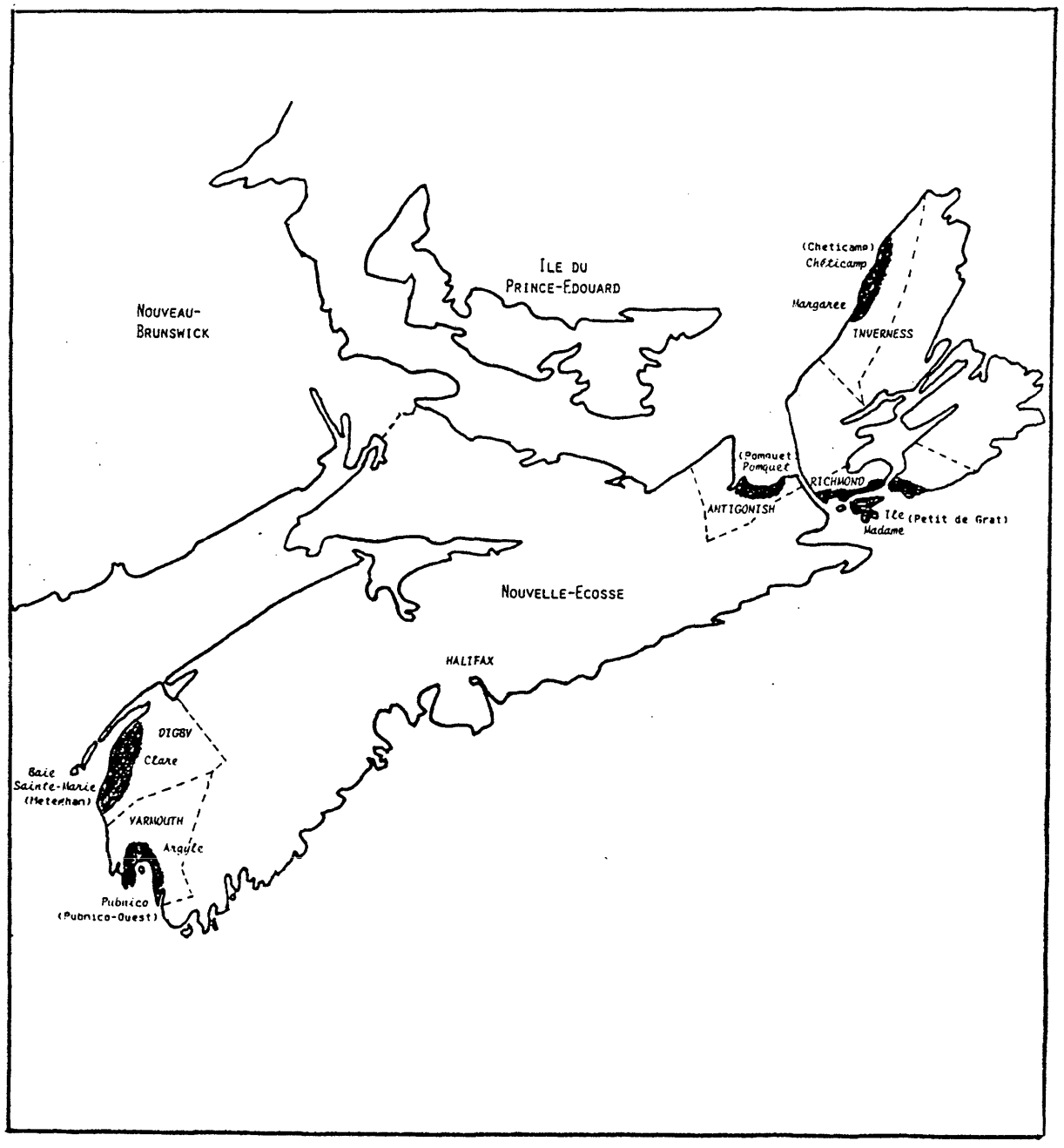
1. Introduction

Chacun sait que la linguistique acadienne connaît un essor assez extraordinaire depuis une dizaine d'années. L'on pensera tout de suite aux recherches de Ruth King, de Robert Ryan, de George Patterson, de Louise Péronnet, de Pierre Gérin père et fils, de Maurice Holder, de Moshé Starets, et de bien d'autres. Les recherches de tous et de toutes sont fort intéressantes mais, pour ne prendre que le cas des parlers acadiens néo-écossais, ce qui manque toujours, c'est une description complète du système linguistique du franco-acadien. Les études déjà menées sont consacrées soit à une region, soit à un aspect particulier d'analyse linguistique, soit à un seul groupe d'âge. Une région en particulier, la Baie Sainte-Marie, a déjà fait l'objet de plusieurs très bonnes monographies, mais on ne peut pas comparer les systèmes linguistiques des autres régions avec celui de la Baie Sainte-Marie, car les données correspondantes n'ont jamais été recueillies. Donc, la somme des études partielles est loin de constituer une description complète du franco-acadien.

2. Objectifs du projet de recherche.

En lançant le projet de recherche dont il est question dans ce court exposé, nous nous sommes proposé un triple but; nous entreprenons en quelque sorte une recherche à trois volets, un volet purement descriptif, un volet socio-linguistique et un volet dialectologique ou géolinguistique. Notre idée de base serait de faire une étude systématique de cinq parlers franco-acadiens néo-écossais, et ceci dans une optique comparative: nous voulions incorporer une perspective identique dans la constitution du

corpus et dans l'approche analytique pour les parlers des cinq régions où sont concentrés les Acadiens de la Nouvelle-Ecosse, voire la Baie Sainte-Marie, Pubnico, Chéticamp, l'Ile Madame et Pomquet (voir la carte ci-dessous).



Carte 1

Arrêtons-nous quelques instants sur l'aspect démographique de notre recherche (voir le tableau 1).

Population des principales subdivisions par rapport aux comtés

Comté	Population d'origine ethnique française	Population de langue maternelle française (L.M.F)	Principale subdivision à concentration francophone	Population de langue maternelle française (L.M.F.)	Pourcentage de la population francophone (L.M.F.) du comté
Digby	8,715	7,270	Clare	6,850	94.2%
Yarmouth	9,430	7,050	Argyle	5,425	77.0%
Inverness	4,155	3,595	Chéticamp-Magré	3,295	91.7%
Richmond	5,570	4,005	Ile Madame	2,670	66.7%
Antigonish	2,170	880	Pomquet-Tracadie	630	71.6%
Total	30,060	22,800		18,870	82.8%

Source: Statistiques Canada, Recensement de 1981, Publication E-573.

Tableau 1

Rappelons d'abord que les Franco-acadiens dont la langue maternelle est le français constituent environ 5% de la population de la Nouvelle-Ecosse. C'est dans cinq comtés, Digby, Yarmouth, Inverness, Richmond et Antigonish que l'on retrouve la plus forte concentration de francophones néo-écossais et, à l'intérieur de chaque comté, une seule des subdivisions de recensement contient la très grande majorité de ces mêmes francophones. Prenons le cas du comté de Digby. D'une population d'origine ethnique française de 8,517 âmes, 7,270 sont de langue maternelle française, et parmi ceux-ci, 6,850 personnes, ou 94.2% de la population de langue maternelle française habite dans la subdivision de Clare. La population des cinq subdivisions retenues pour notre enquête représente presque 83% de la population francophone des cinq comtés. Il n'est guère surprenant de constater que plus la population de langue maternelle française constitue une proportion importante de la population totale de la subdivision, moins le taux d'assimilation est grand. Quant aux parlers des cinq régions, il nous semble tout à fait approprié

de parler d'isolats, vu que les francophones de chaque région se trouvent entièrement entourés de populations anglophones. Comment s'explique cet isolement des Acadiens les uns des autres? Il n'est peut-être pas inutile de rappeler qu'après le traité de Paris en 1763, les Acadiens qui avaient été déportés des Provinces Maritimes en 1755 pouvaient revenir s'y réinstaller à la condition formelle, qu'ils ne se mettraient pas en grands groupements. Il est bien évident que moins un groupe est géographiquement homogène moins il aura de pouvoir politique.

Soulignons les objectifs de notre recherche. L'importance de faire une description complète du système linguistique des cinq parlers à l'étude nous semble évidente. La tâche est énorme et l'établissement d'un vaste corpus fiable constitue de toute évidence le premier pas. Quant au deuxième volet, celui de l'analyse sociolinguistique, nous sommes persuadés que des facteurs sociaux pourraient expliquer une bonne partie de la variation linguistique que nous avons déja décelée. Si l'importance de facteurs fondamentaux tels que le niveau de scolarité, le statut social, l'âge et le sexe est déjà bien établie, plusieurs autres facteurs nous paraissent également très pertinents. Il faudra savoir pour chaque individu non seulement le nombre d'années de scolarité; il sera nécessaire de procéder à une quantification de la composante française de sa scolarisation, en termes d'intensité et de durée. Nous voudrions aussi obtenir des renseignements précis sur le degré d'utilisation de l'anglais.

Passons enfin au troisième volet. Si des facteurs sociaux peuvent sans doute expliquer une partie de la variation linguistique, la géographie y contribue également dans une grande mesure. N'oublions pas que nous sommes en présence de cinq véritables isolats. Si certaines différences inter-régionales sont tout de suite apparentes au linguiste, les différences intra-régionales restent en très grande partie inexplorées. Le modèle d'analyse sociolinguistique que nous envisageons devra inclure la variation géographique. Nous postulons qu'il existe dans le cas des parlers acadiens néo-écossais, une situation de diversification dialectale des plus complexes. Cet aspect géolinguistique devra donc retenir notre attention.

3.0 Méthodologie.

3.1 Choix de communautés

Pour représenter à la fois la structure sociale et démographique de la population et la répartition géographique à l'intérieur de chaque région, il aurait fallu un échantillon beaucoup plus grand que ce que nous pouvions envisager même dans un projet de cette envergure. Nous avons plutôt choisi de nous concentrer sur une communauté clé dans chaque région pour l'examen des structures sociolinguistiques, tout en poursuivant une exploration dialectale de l'ensemble des autres communautés acadiennes.

Dans chacune de ces communautés clés, nous avons procédé à un échantillonage par le hasard, avec une stratification par âge et sexe. Douze informateurs par localité était le nombre visé dans cette première phase du projet. Chaque informateur participait à deux interviews différentes,

technique dont nous parlerons plus loin. Soixante informateurs ont été interviewés dans cette optique, repartis sur cinq localités (voir la carte 1): Petit de Grat, Chéticamp, Pomquet, Meteghan et Pubnico-Ouest. L'enquête sociolinguistique complète contiendra au minimum le double de ce nombre.

3.2 L'exploration géographique

La première étape de l'exploration géographique intra-régionale s'est également effectuée cette année. Les enquêteurs, tous originaires de la région même où ils faisaient les enquêtes, avaient comme tâche de choisir un ou deux informateurs de chaque village francophone de la région en question. Par souci d'uniformité nous nous sommes dans un premier temps limités au groupes d'âge de soixante ans ou plus pour cette partie des interviews. Une meilleure représentation des âges dans cette composante de l'enquête sera obtenue par la suite. Cinquante interviews de ce type ont été effectuées, en nombre variable selon les régions.

L'examen des interviews recueillies nous permettra d'ajuster et de planifier notre approche pour la deuxième phase de plusieurs manières, en vue d'aboutir à une représentation relativement complète de la répartition géographique.

3.3 L'échantillonnage

L'échantillonnage s'est fait à partir des listes électorales. Une liste de noms pris au hasard était présentée aux enquêteurs, qui en connaissaient un grand nombre et s'informaient auprès d'autres membres de la communauté pour identifier les autres. Certaines catégories d'individus étaient écartées au départ, par exemple ceux qui étaient originaires d'autres régions où ceux qui parlaient anglais à la maison. Les personnes malades ou ayant un défaut d'articulation étaient également mises de côté. La connaissance intime que les enquêteurs avaient de leur communauté permettait ainsi de réduire de beaucoup le nombre de personnes abordées, par rapport à la démarche nécessaire dans une grande ville. En plus, du fait que les enquêteurs connaissaient à peu près l'âge des individus sur la liste, nous n'avions pas besoin de contacter ceux pour lesquels le quota d'âge et de sexe était déjà rempli. Lorsqu'une personne était contactée et qu'elle n'était pas disponible pour une interview, le remplacement par d'autres membres du même ménage était pratiqué.

Suite à l'examen de la composition de l'échantillon ainsi obtenue, certaines décisions fondamentales restent à prendre avant la deuxième phase du recueillement du corpus. Le but de notre structuration en deux phases était justement de pouvoir ajuster notre approche selon la composition de la première moitié de l'échantillon. Il s'avère que la taille de l'échantillon ne permet pas de refléter de façon complète la composition sociale de chaque communauté par le simple jeu du hasard. Certains groupes occupationnels sont trop restreints pour que nous puissons être sûrs de

les voir représentés en laissant jouer le hasard pur. Ce qui plus est, l'aspect comparatif de notre plan de recherche serait mieux servi si la structure de chacun des cinq sous-echantillons était aussi semblable que possible, à tous les points de vue. Une structuration plus détaillée de l'échantillon s'impose ainsi au moment où nous compléterons notre corpus.

Quant à la sélection des informateurs à l'intérieur des quotas établis, il est également possible que la décision soit prise de ne pas maintenir le principe de la sélection par le hasard. Bien que par ce procédé les sous-échantillons soient statistiquement représentatifs des communautés représentées, la comparabilité directe entre ces sous-échantillons n'est pas assurée pour autant. Nous pouvons comparer les relations sociolinguistiques dégagées quantitativement pour chaque communauté, en contrôlant statistiquement pour la composition des échantillons en ce qui concerne les caractéristiques qui n'entrent pas dans chaque relation, mais nous ne pouvons pas comparer directement les niveaux d'utilisation des phénomènes linguistiques examinés.

3.4 Interviews à deux temps

Un procédé particulier a été adopté pour explorer la dimension stylistique. Les informateurs ont chacun été interviewé à deux reprises, une première fois par un enquêteur local, une deuxième fois par un enquêteur de l'extérieur de la région. Deux situations stylistiques se trouvaient ainsi représentées.

Notre choix d'entraîner des enquêteurs originaires de chaque région était motivé par le désir de voir régner pendant les interviews un climat aussi intime et familier que possible (en autant que la présence du magnétophone puisse le permettre). Les enquêteurs avaient comme consigne d'utiliser la variété de langue qui leur était la plus naturelle compte tenu de l'interlocuteur. Il s'agissait dans la plupart des cas d'étudiants qui avaient été absents du village pendant l'hiver, et le contenu de l'interview consistait souvent tout naturellement en échanges de nouvelles; l'enquêteur se rattrapait sur les événements qui s'étaient déroulés pendant son absence.

La situation était évidemment toute autre lorsque l'intervieweur était un étranger vis-à-vis de l'informateur ainsi que vis-à-vis de la communauté acadienne. Une plus grande formalité était automatiquement ressentie. Notre hypothèse était évidemment que le choix de régistre en serait affecté. Dans le but de rehausser encore l'attention portée au langage, nous avons abordé de façon explicite le sujet de la langue, en interrogeant les informateurs sur leur perception de leur propre situation linguistique.

4.0 Exploitation des données

4.1 Attitudes linguistiques

Le contenu même de cette deuxième série d'interviews fournit des renseignements extrêmement intéressants sur les attitudes linguistiques qui règnent dans les communautés examinées. Ce sondage direct des perceptions linguistiques était destiné à nous aiguiller vers une approche plus indirecte, visant à fournir des mesures quantifiables des attitudes, sur le plan individuel ainsi que sur le plan de la comparaison des communautés entre elles. Parmi les dimensions qui paraissent pertinentes, il y a par exemple la valeur accordée à la langue locale, illustrée par les attitudes exprimées dans les deux extraits suivants:

> J'ai pas vraiment du vrai français comme vous là, pis comme dans les autres endroits qu'a du vrai français.(...)C'est pas le même français. Vous, vous avez une meilleure école que nous autres pis vous parlez mieux.(...)Ils ont à dire que je parlons pas du vrai français, nous autres. Pis c'est vrai.

> Oui, je nous organisons pour montrer aux touristes ça que je sons, tu sais, pis je sons fiers d'être acadiens. (...) Tu sais, j'ai jamais... je m'ai jamais forcé pour... tu sais, au ... mès qu'ils compreniont ça que je voulais dire, de changer mon langage.

Une deuxième dimension intéressante est celle de la conscience de la variation stylistique et de la pression sociale qui s'exerce contre l'individu qui se mettrait à "parler en grands mots":

> On ose pas, on va pas parler avec ses amis pis toute ça pis dire, tu sais, ces... des grands mots là. (...) Je crois que, tu sais, il y a beaucoup, beaucoup, beaucoup de monde de Chéticamp que (...) automatiquement ils peuvent changer, au français, pis ils peuvent parler notre langue, mais ils peuvent se revirer pis dire O.K. on va sur... on va parler... tu sais, on va pas mettre de mots anglais, pas rien.

Nous avons tenté de faire expliciter les éléments qui caractériseraient le style des "grands mots". L'élément le plus souvent cité était l'utilisation de mots français à la place des anglicismes dont l'usage est consacré dans chaque communauté.

Certains informateurs énuméraient des traits phonétiques et morphologiques parmi les caractéristiques qui selon eux séparent les styles, par exemple l'utilisation où non de la prononciation acadienne dans des mots tels que "coeur" ([tʃœr]/[kœr]) ou "quai" ([tʃɛ]/[kɛ]) ou l'emploi de "je" par rapport à "nous" à la première personne du pluriel ("j'avions nos livres"/"nous avions nos livres").

La diglossie superposée au bilinguisme donne une situation où l'individu a l'impression d'alterner entre trois langues:

> C'est très difficile pour eux (les enfants) de distinguer entre la langue qu'ils avont appris, maternelle, acadienne, à la maison et pis ça faut qu'ils apprennent à l'école. C'est comme trois langues. (...) Ils parlent comme ça à l'école, quand ils arrivent chez eux, ils parlent comme leur père et leur mère et leurs petites copines, ou bien ils parlent anglais avec leurs... les autres.

4.2 Analyse sociolinguistique

L'examen direct des changements de registre peut se faire dans notre corpus en comparant les deux séries d'interviews. Une première hypothèse serait que nous trouverions une différence mesurable dans le nombre de formes anglaises selon que l'interview a lieu avec l'enquêteur local ou celui de l'extérieur de la communauté. L'étude quantitative de ce contraste compte parmi nos analyses en cours.

Cette analyse s'étendra également à l'étude de la variation entre individus d'une même communauté, dans une même situation de communication. Les relations avec les facteurs sociaux et démographiques pertinents seront dégagées à travers l'analyse statistique.

Parmi les variables phonétiques sous étude d'un point de vue sociolinguistique, se trouvent les différentes alternances entre des formes acadiennes et des formes du français standard, telles que l'alternance [tʃ] - [k] déjà mentionnée. Une des variables grammaticales que nous étudions en ce moment est l'alternance entre les terminaisons verbales "-iont" et "aient" ("ils chantiont"/"ils chantaient").

L'étape suivante de l'analyse sera de déterminer si les structures de variation décelées se retrouvent ou non de façon semblable dans les différentes régions. Notre hypothèse de départ est que certains processus se révèleront communs à l'ensemble, mais que l'histoire et le développement particuliers de chaque région auront une influence sur les structures de variation.

4.3 L'approche quantitative

Les analyses de type sociolinguistique que nous venons d'esquisser nécessitent une étape préalable de transcription et de dépouillement, qui constitue une des composantes les plus exigeantes en temps et en ressources d'un projet de ce type.

Les étapes de dépouillement et d'analyse se trouveront grandement facilitées par le fait d'avoir au préalable entré les textes transcrits à l'ordinateur. Cette étape de mise en mémoire est actuellement en cours. Nous utilisons l'ordinateur central de l'Université Saint Mary's (VAX 11/780).

La majeure partie des interviews effectuées dans les communautés principales sont déjà transcrites et mises en mémoire. Dès que cette étape sera terminée, nous ferons sortir des concordances des formes linguistiques que nous voulons analyser, ce qui facilitera la recherche de l'ensemble des

occurrences de chaque phénomène, bien que les réalisations doivent ensuite être vérifiées à l'aide des enregistrements.

4.4 Etude descriptive et géolinguistique

Parallèlement à la démarche sociolinguistique, nous entamons l'analyse systématique des différentes variétés régionales sur le plan de la description phonologique, morphosyntaxique et lexicale. Un souci constant lors de l'analyse découle de notre objectif de comparaison systématique entre les parlers des cinq régions. Il sera naturel de prendre comme point de départ les aspects particuliers déjà décrits pour la région de la Baie Sainte-Marie par Ryan (1981, 1982) et d'examiner en premier lieu le fonctionnement de ces aspects dans les autres parlers. Nous augmenterons progressivement les bases de comparaison, en même temps que nous avancerons dans la description systématique de chaque parler.

L'examen de la variation dialectale à l'intérieur de chaque région s'appuie sur les cinquante-cinq interviews effectuées dans les villages entourant les communautés clés. Deux régions sont particulièrement intéressantes de ce point de vue, Richmond et Argyle, et font l'objet d'une étude initiale cette année. Un questionnaire phonologique, administré oralement aux informateurs de cette série d'interviews, permet de dégager dans les grandes lignes certaines divergences d'ordre géographique dans la réalisation des oppositions phonologiques.

5. Conclusion

Cette présentation des différents aspects de la cueillette et de l'analyse des données de notre corpus aura permis de montrer l'ampleur de ce projet de recherche et les multiples étapes nécessaires pour mettre en place tous les éléments dont nous avons besoin pour l'ensemble des analyses envisagées.

Le fait d'avoir discuté séparément des différentes démarches descriptives, sociolinguistiques et géolinguistiques ne doit pas pour autant impliquer que cette séparation sera maintenue au delà des étapes initiales. En dernier lieu, notre analyse vise à intégrer ces aspects. L'alliance nécessaire de la sociolinguistique et de la dialectologie est bien établie, ainsi que la conception que la description linguistique doit comprendre l'étude des éléments variables. Dans la communauté linguistique des Acadiens de la Nouvelle-Ecosse, où la complexité du jeu des variétés géographiques et stylistiques est apparente, mais où l'unité fondamentale se dégage sur plusieurs plans, une approche intégrée nous semble particulièrement appropriée.

Questions of Language Contact and Genetic Kinship of the Languages of Southeast Asia

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ABSTRACT

A phonological and grammatical analysis of Vietnamese, Thai, Laotian, Cambodian and Chinese provides evidence for considering Vietnamese, Thai, Laotian and Cambodian as a group in opposition to Chinese. Within this group Vietnamese, Thai and Laotian form a subgroup in opposition to Cambodian, and serve as intermediate stages between Cambodian and Chinese. The smallest subgroup consists of Thai and Laotian.

At present our knowledge of these languages and their interrelationships permits a claim of genetic kinship only for Thai and Laotian. In spite of the many undoubted similarities among the five languages examined, we cannot claim a genetic kinship between any pair remaining. In making such an attempt it is very easy to mix signs of genetic kinship with the results of language contact.

In this century interest in investigating linguistic regions or areas has significantly increased. Already in the first decades of this century it was established that within a significant number of living languages there may be found similarities of specific grammatical characteristics distributed in such a way that neighbouring languages display striking coincidences, despite basic differences in grammatical structure and in the lexicon. Even then it was impossible to explain this phenomenon other than as a diffusion of linguistic features in language contact zones where the neighbouring languages are not genetically related. A language as a system is located in constant contact with other languages, as a result of which bilateral influence takes place: the language influences its neighbours and is in turn influenced by them. The main object of our investigation is the Vietnamese language; however, in order better to understand its grammatical structure it is useful to compare it with other languages which it directly

or indirectly borders upon. These are Thai, Laotian, Cambodian, and Chinese.

At the present state of our knowledge of these languages it is possible to posit a genetic kinship, apparently, only between Thai and Laotian. Despite the obvious closeness of the five examined languages, we have no right to postulate direct genetic relationships between any other pair of these languages. If we were to try to do so, we might confuse signs of language contact with those of genetic affinity.

The fact of lexical interaction is unquestioned in linguistics, and it would not be difficult to adduce examples of the numerous and varied lexical borrowings to illustrate the results of such interaction. In the middle of the last century Max Müller (1876:86) expressed the thought that no matter what lexical borrowings a language may undertake, the grammar of that language does not undergo any external alterations. The result of this approach was that there existed in linguistics for a considerable period of time that point of view that there could be no "mixed" languages. And although G. Schuhardt already in 1884 demonstrated slavo-germanic and slavo-italic interaction, O. Jespersen (1922:215) and L. Bloomfield (1933:468) succeeded in showing the shortcomings of the approach. The fact that by the 1920s their point of view was supported by far from all linguists becomes clear when one considers that E. Sapir in 1921 in his book Language unambiguously supported the point of view of Max Müller, stating that we have no convincing examples of deep morphological influence via diffusion and that in the final analysis we cannot conclude that a language may easily exercise a significant morphological influence on another language. (Sapir, 1949:203-204) Such an approach made it easier for E. Sapir to examine structurally identical grammatical features as the best evidence of a genetic kinship. E. Sapir thought, for example, that English had borrowed a large number of affixes from French and Latin, but that these were only a simple addition to the system of affixes which already existed, and hardly differed from lexical borrowings. Further, he concluded that although serious morphological influence was not impossible, its realization was so gradual that it was hardly likely to occur in a relatively short period of language history. E. Sapir differentiated between two types of morphological influence: "superficial" and "profound"; however, in practice it is often difficult to draw a distinction between the two types. (Emeneau, M.B. 1956:4) Later investigations which are based on a rich collection of facts about language contact, shed light on the essence of

this problem. Important contributions to the formulation of he conclusion that languages influence each other not only in the domains of phonology and lexicon, but also in pure grammar, were made by U. Weinreich, (1953), E. Haugen, (1953), and M. B. Emeneau, (1962). Working on a whole series of languages belonging to the northern Indian and Dravidian language families, and also on languages of the Mund family, M. B. Emeneau worked out the concept of the "linguistic area", a translation of the term Sprachbund, which was first introduced into linguistics by N. S. Trubetzkoj. Emeneau defines the linguistic area as a defined area or zone including languages belonging to two or more distinctive language families, yet sharing common features which are not to be found in other, non-contiguous members of the language families concerned (1956:16). On the basis of his analysis of counting words widely distributed in many northern Indian and southern Indian languages, as well as in the Mund languages, Emeneau tried to prove the influence of the languages of South-East Asia, where, in Chinese, Vietnamese, Thai, Burmese and others such a system of counting words plays a significant role in the grammatical systems of these languages. The problem of language contact is closely related to the problems of divergence and convergence. By divergence we mean departure from a unified form, a fragmentation such that a single phenomenon, be it phonological or grammatical, as a result of the loss of contact and of mutual distancing of the languages divides into two, such that the common feature ceases to exist, although it can still be reconstructed. Convergence or merging is the reciprocal function, under which two features two units or two models under the influence of more or less prolonged linguistic interaction merge into a single unit, a single feature, or an identical model. In other words, as A. Martinet (1953:VIII) has said, language contact generates imitation, and imitation generates linguistic convergence. Even a fleeting acquaintance with the languages of the Indo-chinese peninsula makes it possible for one to suppose that a systematic examination of the differences and similarities in these languages will lead us to a better understanding of the place of each of the examined languages with respect to all the others. It is possible that as a result of such comparison, several grammatical categories will be clarified.

In the countries of Southeast Asia we encounter cultures which in the course of many centuries have been in close contact and which at the present time exhibit the results of a complex process of synthesis. This is true in the fields of art and religion and, not least, in the languages themselves. When

one compares the data of the phonology, morphology, and syntax and determines the common features of these languages it would be a mistake to immediately come to a conclusion concerning genetic kinship. There are grounds for thinking that a comparison of linguistic systems and subsystems will make a significant contribution towards enriching our knowledge in the corresponding sphere of language, even without supposing or making any hypotheses concerning language kinship. Significant work has already been done in comparing the languages of the Indochinese linguistic area, and important results have been obtained in establishing Vietnamese-Chinese, Vietnamese-Thai, Thai-Chinese and Cambodian-Chinese parallels.

Three basic assumptions must be made as basic to such comparative studies. First, the composition of grammatical categories must be limited by establishing more general systems and subsystems in comparison with those which are formulated for the individual languages. Second, along with coincidence of grammatical elements in the plane of expression and of content we must also be interested in the distributional characteristics of auxiliary elements. Third, one ought not to lose sight of the fact that at the present stage we are studying linguistic systems as established by previous scholars. However, work aimed at establishing all possible ties between systems is an important step on the road to clarifying all possible ties between the languages themselves.[1]

Our comparison of the phonological and grammatical systems is conducted, on the basis of Vietnamese, Thai, Laotian, Cambodian and Chinese. It would also be highly desirable to adduce the data of Indochinese dialects, although the data are highly limited due to the lack of adequate investigation of this question. It would be even more desirable to adduce data from the languages of other peoples of Indochina, especially from those occupying an isolated position, as it is precisely these languages which would be expected to retain archaic traits, since as a result of their isolation they would not have had close contacts with the languages we are examining.[2]

The use of Chinese materials is all the more intentional in that Chinese during the course of many centuries exercised a serious influence on all the languages of Indochina which fell into the zone of contact with it. As statistics evidence, the Chinese form a significant portion of the population of the countries of Indochina, primarily they are immigrants from the southeastern and southern regions of China. The influence which the Chinese language exercised

on the languages of Indochina went in two directions: the predominant influence was that of the Chinese literary language, but along with this the Chinese colloquial language had a significant influence.

The major ethnic groups of Southeast Asia were formed in the first millenium before Christ. At that time many characteristics of their national culture were formulated and intensive cultural interaction began. Scholars have emphasized the independence of the origin and evolution of the cultures of these peoples, and also the fact that they attained a high level of development before they began to experience an influence of the Indian and Chinese civilizations. The linguistic picture of Indochina is quite variegated; here there intertwine three large linguistic families: Sino-Tibetan, Austro-asiatic, and Malayo-Polynesian. Scholars have postulated that the Urheimat of the Indonesian and ancient Indian cultures was in the territory of Western China. The general stream branches out during its movement southward: one, or perhaps several, of its streams, moving West, went to India; the other penetrated Indochina and Indonesia. By the time when the Indian culture began to exercise its influence on the culture of Indochina, the epoch of great prehistoric migrations had already finished. On the islands of the Indonesian archipeligo Indonesians formed the principle mass of the population already from the neolithic period. That part of the continent now occupied by central and southern Vietnam in the Meking Delta region, was settled by Tiams; Cambodia and the central Mekong region were settled by the Khmer, while the Menama valley and the territory now known as southern Burma was occupied by the Mon people; the Malay peninsula was settled by Malayans. Later the Tiam were squeezed out of central Vietnam by the Vietnamese, the Thais conquered the Mon in the Menama valley, while in the Iravadi valley the Burmese conquered. In the fifth to the first centuries before Christ the Dongshon civilization was formed in the territory of North Vietnam, the bearers of which were the population of the ancient state of Aulak. (1958:84) This civilization was connected with the Indonesian and is called the Lakviet after the name of the population of Aulak. The Lakviets are to be considered the direct ancestors of the Vietnamese; in terms of the level of development of their material and spiritual culture they were close to the contemporary Muong. From the beginning of our era there begins a period of intensive synthesis of the Lakviet and Khan cultures. The mixing of the local population of Lakviet

with the immigrant Khan laid the foundation for a new ethnic group: the Vietnamese, while the mixing of these two civilications laid the foundation for the future Vietnamese culture. From the third century B.C. to 938 A.D. Vietnam was under the rule of China. Indian influence came via trade and religion. Buddhism began to penetrate Vietnam during the first centuries of our era and was widely propagated.

There arises an important and difficult question: how may one differentiate borrowings which result from language interaction from forms and features inherited from the parent language? Correspondences with identical phonemic shapes are most likely borrowings; correspondences where there is not complete identity in the plane of expression must be taken as elements inherited from the parent language (Greenberg J. H. 1963:49). The solution to this problem can be facilitated by analogous data from related but distantly-located languages. Although the question of the inclusion of the Vietnamese language into the family of languages under consideration here has not yet been finally confirmed, the tendency to consider it as a language belonging to the Austroasiatic group is intensifying. To this group also belong the Monkhmer languages, the Mund languages, and the languages of peoples living in the Southern part of China.[4]

Concerning the origin of the Thai language there is no universally accepted theory. Some scholars consider it to be a member of the Thai-Tibetan group, while others assign it to the Malay-Polynesian.[5]

Rather than provide the detailed examples to substantiate my proposed scheme of relationships of the languages of Southeast Asia, I will simply state my main conclusions; details and conclusions on phonological, morphological, and syntactic comparisons may be found in my previous works. (Glazova M.G., 1966: 91-96, 203-207; 1967: 274-275; 1979: 100-101; 1980:99).

A phonological and grammatical analysis of Vietnamese, Thai, Laotian, Cambodian and Chinese provides evidence for considering Vietnamese, Thai, Laotian and Cambodian as a group in opposition to Chinese. Within this group Vietnamese, Thai and Laotian form a subgroup in opposition to Cambodian, and serve as intermediate stages between Cambodian and Chinese. The smallest subgroup consists of Thai and Laotian.

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FOOTNOTES

[1]See, for example, Honey, P. J. and E. H. S. Simmons 1963: 71-77. As a result of the comparison of nominal constructions in Vietnamese, Thai, and Chinese, these authors come to the conclusion that in this sphere there are to be found many more correspondences between Vietnamese and Thai than between Chinese and any one of these two languages. The authors emphasize that their conclusions will become reliable if they are confirmed by corresponding results in comparing the verbal constructions of the given languages.

[2]H. Maspéro 1912, for example, indicates the particular importance of comparing the Vietnamese language with the mường language, which is spoken by the population of a series of provinces of northern and central Vietnam, and which has preserved many archaic traits of Proto-Vietnamese. A. G. Haudricourt 1948: 238 speaks of the necessity to investigate languages located in Yunan, on the border with Vietnam -- la-ti, la qua, ke lao, and on the island of Hainan - da-i, which P. K. Benedict 1942 gave the common name ka-dai, and which are the oldest linguistic layer which did not undergo Chinese influence. Examining the peculiarities of ka-dai languages, P. K. Benedict found in them many traits common with Thai and the Indonesian languages.

[3]In the works of anthropologists the term "Indonesians" is applied to the most ancient population of all Southeast Asia.

[4]On the composition of the languages making up the Austro-asiatic family, see for details Pinnow H. J. 1963: 63-81, and Gorgoniev, Ju. A. 1963.

[5]See, for example, Benedict, P. K. 1942; Solncev, V. M., Ju. K. Lekomcev, T. T. Mxitarjan, I. I. Glebova 1960: 13-18.

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Charles Osgood's Place in the History of Semantics

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ABSTRACT

Beginning in the 1950s, psychology felt the impact of increased attention to the study of meaning through the work of one from its own ranks, Charles Egerton Osgood. The breadth of Osgood's scholarship is the first general indication that it is inappropriate to class him categorically as a behavioral psychologist. This presentation attempts to show (contrary to an earlier assessment by Uriel Weinreich) that Osgood's work, though its objectives remain incomplete, is of as much relevance for the linguist's concern with meaning as for the psychologist's. The presentation focuses on parallel developments in psychology and linguistics, Osgood's early work, the semantic differential technique, semantic space, Osgood's model of meaning, the evolution in Osgood's thought and its relation to the contributions of other scholars which may be regarded as turning points in the history of semantics.

Introduction

There are substantial parallels between the history of linguistics in America in the early part of the twentieth century and the history of psychology during the same period. To some extent, the parallels exist because linguistics was influenced by the new paradigm developing in psychology. In particular, Leonard Bloomfield was influenced by behavioral psychology. His reaction against mentalism in European structuralist linguistics, therefore, parallels the reaction of J.B. Watson and other pioneers of behaviorism against the ad hoc postulating of faculties in the human organism, characteristic of nineteenth century psychology. But Bloomfield's reaction is post hoc propter hoc. In a reverse and more complex situation, beginning in the 1950s, psychology would feel the impact of the study of meaning through the work of one from its own ranks, the behavioral psychologist Charles Egerton Osgood, as well as through the work of an outsider, linguist Noam Chomsky. Whereas Chomsky's theoretical work has had its most clearly definable effect within psychology in the work on child language development, the effect of Osgood's work has been much more diverse. There are at least two principle reasons for this diversity: 1) It is a reflection of the diversity within Osgood's work and of the broad-ranging, free-wheeling approach which characterizes his writings and which often makes the reader forget that the words are those of

a behavioral psychologist committed to all the basic tenets of that discipline. 2) Osgood received an appointment in the then newly created Institute of Communications Research at the University of Illinois in 1949, and the students he has trained over three decades have made their own contributions within various divisions of psychology and communication theory which come under the aegis of that institution.

The breadth of Osgood's scholarship is the first general indication that it is inappropriate to class him categorically as a behavioral psychologist. This paper will attempt to show (contrary to an earlier assessment by Uriel Weinreich) that Osgood's work, though its objectives remain incomplete, is of as much relevance for the linguist's concern with meaning as for the psychologist's.

Parallel developments in psychology and linguistics

Parallel developments in the history of psychology and of linguistics, already alluded to, are discussed in Osgood and Sebeok (1954):

"Psychology developed as a branch of philosophy, as did the other sciences, but the weaning was longer than for most. As late as 1900 most psychology departments were subdivisions within philosophy departments; some still are. Along with the mentalistic tradition of the nineteenth century, psychologists and pseudo-psychologists were prone to 'theorize' by sticking into the organism whatever faculties, aptitudes, instincts, etc., seemed to serve their immediate purpose. There were practically as many intervening 'explanatory' constructs as there were things to be explained. This has been aptly entitled 'junk shop' psychology.

In the early part of the present century there was a general revulsion against this kind of theorizing, typified by the writings of such men as Watson, Kantor, Weiss, and more recently Skinner. This stress on objectivity paralleled a similar revolution taking place in linguistics through the same period. These men went to the other extreme, the 'empty organism' position. This view held that the psychologist should concentrate on exploring the many functional relations between objectively verifiable S (stimulus) events and objectively verifiable R (response) events, avoiding intervening variables which involve 'going into' the organism.... If all variations in R were in fact predictable from knowledge of the current stimulus field, then this model would be sufficient".(26-7)

But of course it is not sufficient in either psychology or linguistics. Watson and Bloomfield may be excused as having done

their work too early to have seen this insufficiency, caught as they were in the fervor of pioneering efforts. In the context of this paper, it is important to note further parallels between psychology and linguistics in the somewhat paradoxical positions held by these two scholars with respect to meaning.

Noble (1952) notes that "it is of historical interest that Watson's position on the meaning issue in 1925 was that the term was then unnecessary but that, should the need arise, behavioristic definition and explanation could be given." (421) This parallels nearly perfectly Bloomfield's position in Language (1933) where the study of meaning is said to be undesirable in linguistics, if not completely unnecessary, since human knowledge is not sufficiently advanced to afford a valid basis for its analysis and yet meaning is defined within what Bloomfield takes to be the perfectly adequate stimulus-response framework afforded by behavioral psychology (1933: 139).

Against this background, the inevitability of meaning, as well as the inevitability of assumptions about meaning, which Osgood identified in Bloomfield's work (Snider and Osgood 1969: 3 [Osgood 1952]), it is not entirely surprising that Osgood, trained as a behavioral psychologist, should have focussed from the beginning of his career on the study of meaning and approached the study with some resignation to its snares.

Osgood's Early Work

The earliest Osgood publications (1941a, 1941b) were written in collaboration with one of Osgood's teachers at Dartmouth College--Ross Stagner--and deal with attitude measurement. Osgood's interest, as expressed there, clearly anticipates the development of the semantic differential technique, to receive its first full expression a decade and a half later.

"The process of evaluation of a stimulus object, whether physical or social, always takes place with reference to some standard.... The basic continuum of all reference frames appears to be a positive-negative, acceptance-rejection, or 'good-bad' relationship.... In addition to this basic dimension of acceptance-rejection, each reference frame seems to involve other continua which add qualitatively to a characterization of the conceptual objects judged within that frame." (1941a: 389)

and

"The term 'polarization' has been coming into frequent use in the social sciences. The term has appeared in the writings of political scientists, psychologists, and sociologists. Although the term has been used with slightly varying connotations, the

common essence of meaning seems to be action or thought which occurs in extremes." (Stagner and Osgood 1941: 403)

Generalized scales for the measurement of attitude had first been proposed by psychologists in the 1930s, but by the time Osgood and his associates developed the semantic differential technique (Osgood et al. 1957) such scales had been subjected to extensive and severe criticism, and their use had been largely abandoned. Reviewing this criticism, Osgood et al. conclude that the scales had sufficient fundamental merit to be worth rehabilitating, since they obviate the necessity of developing new scales for each attitude object and since they facilitate comparison among attitude objects. The semantic differential technique is presented not merely as a rehabilitation but as an extension of the original idea:

"When used as a measure of attitude, the semantic differential carries even further the logic used by Remmers in developing his generalized scales. Rather than having different 'master' scales for different classes of attitude objects, exactly the same set of evaluative dimensions would be used for all objects of judgment. Rather than using 'statements' of any sort with which the subject must agree or disagree, scales defined by pure, abstracted linguistic evaluators would be used." (1957: 196-7)

This is coupled with the recognition that the semantic differential technique will meet essentially the same criticisms that were directed at Remmer's work.

In the same year that Osgood's first publications appeared, and in fact in the same journal, Charles I. Mosier applied attitude scales specifically to the study of word-meaning (Mosier 1941). The introductory chapter of Osgood et al. (1957) acknowledges Mosier's work but views his method as too narrow, in a way which the authors explicitly seek to correct with their semantic differential technique.

"The limitation in Mosier's technique is that he tapped only one dimension of meaning, the admittedly important evaluative dimension, whereas we may assume at the outset that meanings vary multidimensionally." (1957: 17)

Semantic Differential Technique

I want to sketch Osgood's main basic notions, those of the semantic differential technique itself, semantic space, and meaning as a mediated reaction, as a background before dealing in the main section with the evolution of Osgood's key ideas and establishing the relationship of his work to linguistics proper.

The semantic differential technique is introduced in Osgood 1952 where it is characterized as "a combination of associational and scaling procedures." (Snider & Osgood 1969: 26 [Osgood 1952] A succinct description of the technique is to be found in Osgood et al. (1957):

"We provide the subject with a concept to be differentiated and a set of bipolar adjectival scales against which to do it, his only task being to indicate, for each item (pairing of a concept with a scale), the direction of his association and its intensity on a seven-step scale. The crux of the method, of course, lies in selecting the sample of descriptive polar terms. Ideally, the sample should be as representative as possible of all the ways in which meaningful judgments can vary...." (20)

The semantic differential technique has been subjected to much critical scrutiny, not all of it to the point, as will be noted in more detail below. Much of this criticism begins with the observation that the technique does not come to grips with meaning as traditionally defined. Brown (1958) makes this point, but with an important concession about a redeeming characteristic of the technique:

"The differential solves none of the problems of meaning posed by philosophers and does not even observe the distinctions of which they feel most confident. This may be just the bold step needed to advance the empirical study of meaning whose movement until now has been glacier-like in its imperceptibility." (Snider and Osgood 1969: 87 [Brown 1958])

An observation of greater insight comes from Carroll (1959), who acknowledges problems with the semantic differential but also sees its strengths in the context of the objectives of Osgood's earlier work:

"It may be said that far from making measurements of meaning more precise, the SD capitalizes on ambiguity and vagueness.... I am not sure whether there is a good remedy for this characteristic of the SD, or whether in fact there is any need of a remedy. Perhaps a certain degree of ambiguity in the experimental situation is requisite for teasing out semantic dimensions possessing a sufficient degree of generality. Indeed, all the well-known figures of speech--simile, metaphor, personification--feed upon ambiguity, and one may suggest that SD dimensions may be useful in describing and accounting for the uses of metaphor. One must call to mind that the SD was a natural extension of Osgood's prior studies in synesthesia." (Snider and Osgood 1969: 113-14 [Carroll 1959])

Semantic Space

Because the associations which a subject makes on the semantic differential scales have 'direction' (1957: 20), because the scales have 'poles' (ibid.), and, more importantly, because of a life-long predisposition to spatial metaphors in Osgood's thinking[1], the semantic differential technique is complemented by the notion of semantic space. While these factors explain the source of the concept of semantic space, it is equally important to stress the function of the concept, which is to relate the learning theory model of meaning as a representational mediation process (to be discussed subsequently here) to the measurement of meaning by the semantic differential technique. The definition of semantic space is inseparable from this function:

"We begin by postulating a semantic space, a region of some unknown dimensionality and Euclidian in character. Each semantic scale, defined by a pair of polar (opposite-in-meaning) adjectives, is assumed to represent a straight line function that passes through the origin of this space, and a sample of such scales then represents a multidimensional space. The larger or more representative the sample, the better defined is the space as a whole." (Osgood et al. 1957: 25)

Osgood's Model of Meaning

The conventional terminology of behavioral psychology's s-r paradigm was rejected by Osgood et al. (1957) for the presentation of their model of meaning:

"A pattern of stimulation which is not the significate is a sign of that significate if it evokes in the organism a mediating process, this process (a) being some fractional part of the total behavior elicited by the significant and (b) producing responses which would not occur without the previous contiguity of non-significant and significant patterns of stimulation. It will be noted that in this statement we have chosen the term 'mediating process' rather than 'mediating reaction'; this is to leave explicitly open the question of the underlying nature of such representational mediators--they may be purely neural events rather than actual muscular contractions or glandular secretions in the traditional sense of 'reaction'." (7, italics omitted)

Nobody has quibbed over the terminology, but the model of meaning as a mediation process, or simply as a mediator, has been challenged, most notably by Fodor (1965). The subsequent debate (Osgood 1966, Berlyne 1966, Fodor 1966) not only illuminates the model of meaning under discussion, but touches on the

development of psychology, the development of linguistics, and the relationship between the two.

As was the case for semantic space, as defined above, Osgood provides a personal note in the debate, revealing what could not even be suspected by reading Fodor (1965).

"[Fodor] identified his adversary as Hobart Mowrer; however, the symbol r_m is characteristic of my discussions of mediation theory.... Perhaps this choice was dictated by the fact that Mowrer's presentation was the first serious attempt in modern behaviorism to deal with the dynamics of sentences. Fodor wrote the original draft of "Could Meaning be an r_m?" during the year that he was a visiting professor in the Institute of Communications Research at Illinois and was sitting in on my graduate seminar in Psycholinguistics--if one can imagine Fodor merely 'sitting in' on anything! In any case, the notion of representational mediation was and continues to be central to my theorizing about language behavior, and it is therefore appropriate for me to write a reply to Fodor's critique." (1966: 402)

In some respects, Fodor and Osgood are less at odds than Fodor would lead readers to believe. Osgood calls attention to this, pointing out at the same time the continuity in the development of his theory from 1957 to 1963 and, incidentally, mentioning the relevance of the theory to semantics:

"Mowrer clearly opened himself to criticism.... I have contributed to this criticism myself, along psychological lines as well as those pointed up by Fodor. However, both in The Measurement of Meaning (1957) and in my own APA address (1963) I have tried to show how principles of cognitive interaction (which in my treatment include r_m's) can apply both to performance (the momentary cognition of phrase and sentence meanings) and to learning, or semantic change (change in r_m due to repeated exposure to co-occurrence of word combinations.)" (ibid)

Fodor criticizes both single-stage learning theories and two-stage or mediation theories of the type on which Osgood's model of meaning is based. In fact, he argues that qualifications of the sort which Mowrer (and Osgood) accept or impose on two-stage theory reduce the theory to the single-stage type and therefore expose it to the same criticisms. (Fodor 1965: 81) Osgood counters this charge by saying that Fodor fails to understand the first distinction between the two types of theory, namely the functional separation of decoding from encoding. (1966: 404) A point which Osgood does not make, but which is highly relevant in countering Fodor's attack, is that Mosier (1941) had provided experimental evidence in support of the two-stage theory. A discussion of such evidence would have been

far more germane than the abstractions into which Fodor ultimately lapses.

In the further defense of his r_m, Osgood cites the work being done by Chomsky in transformational generative grammar, drawing an analogy between his own description of r_m components and the model in Chomsky (1965: chapter 2) for characterizing subcategorization and selectional restrictions. (1966: 406) Previously, he had made a comparison between his description of the r_m and the phonological level of linguistic analysis:

"In a fashion analogous to the notion of a phoneme as a simultaneous bundle of distinctive phonetic features (cf. Jakobson and Halle, 1956), the meaning of a sign is conceived to be a simultaneous bundle of distinctive semantic features -- which I identify with component r's of the total r_m." (1966: 404)

Following tradition, the editor of JVLVB, where the debate appeared, gave the last word to the initiator of the debate--Fodor. There Fodor begins by stating:

"I want to argue that the explanatory power of a mediational account of reference reduces to that of corresponding single-stage accounts." (1966: 412)

This lends some credence to Osgood's view that Fodor failed to grasp the functional separation of decoding and encoding in the mediational model, for that separation, far from reducing explanatory power, increases it, and Osgood explicitly stated his own reservation about mediation theory in that respect:

"In sum, it is the functional separation of decoding and encoding stages--yet both utilizing the full machinery of single-stage theory--that provides the greater flexibility and explanatory power of the mediation model. The real danger, as I see it, is that by proliferating unobservables (albeit of the same kind as overt S's and R's) mediation theory may be able to 'explain' too much and hence become untestable." (1966: 404-5)

Furthermore, Osgood appears to have anticipated Fodor's final argument as stated above, for his conclusion in replying to Fodor (1965) had already shown the possibility of turning the argument completely around:

"It is interesting to note in passing that a recent (and to me convincing) paper by Jakobovits (1966) argues that single-stage theory can be 'reduced' to two-stage theory--that Skinner is really a mediation theorist in disguise!" (1966: 407)[2]

The Evolution of Osgood's Thought

Osgood has recognized deficiencies in his work and has never been reluctant to admit them. As early as 1957, he made one such admission in the final chapter of The Measurement of Meaning:

"It has become increasingly clear that our original conceptions were insufficient, that human semantic processes are very complex, and that problems of meaning are inextricably confounded with more general problems of human thinking or cognition. Certainly, when viewed from some future vantage point, our theoretical notions and measuring operations will seem very crude and inadequate." (318)

This is essentially an admission that the proposed method of measuring meaning will stand improving and only hints at the more serious underlying problem. Osgood revealed the full extent of this problem some years later.

"When this research was begun over a decade ago, I had expected that the dimensions of the semantic space would correspond to the ways in which the sensory nervous system divides up the world, e.g., there would be visual brightness, hue and saturation factors, auditory loudness and pitch factors, olfactory factors, and so on. (This result would have been in flat contradiction to my own mediation theory of meaning--according to which meanings are anticipatory portions of the reactions we make to signs--although this did not occur to me at the time." (Snider and Osgood 1969: 330-31 [Osgood: 1964])

The inevitable conclusion is that either the original model of semantic space or the mediation model of meaning must be scrapped to eliminate the contradiction. Because all the testing done by the semantic differential technique suggests that the recurring dominant factors (sets of equivalent judgment scales) of test results are in the nature of responses to stimuli, Osgood opts for the mediation model. Therefore, the original model of semantic space must be abandoned. If this model must be abandoned, there is at least suggestive evidence for advancing the study of Osgood's other early interest--synesthesia:

"The highly generalized nature of the affective reaction system --the fact that it is independent of any particular sensory modality and yet participates with all of them--appears to be the psychological basis for the universality of three factors of Evaluation, Potency, and Activity, as well as the basis for synesthesia and metaphor. That is, it appears to be because such diverse sensory experiences as a white circle (rather than black), a straight line (rather than crooked), a rising melody

(rather than a falling one), a sweet taste (rather than a sour one), a caressing touch (rather than an irritating scratch) can all share a common affective meaning that one can easily and lawfully translate from one modality into another in synesthesia and metaphor. The labelling of this shared affective response is apparently uncovered in the factor analysis of adjectives." (Snider and Osgood 1969: 331 [Osgood 1964])

A different problem, though not unrelated, since it weakens the concept of semantic space, is also acknowledged at the conclusion of The Measurement of Meaning:

"One of the difficult methodological problems we have faced--unsuccessfully so far--is to demonstrate that the polar terms we now use are true psychological opposites, i.e., fall at equal distances from the origin of the semantic space and in opposite directions along a single straight line passing through the origin." (327)

Mosier (1941) had countenanced this problem in his measurement of meaning when some test results showed a 'precipice' effect--no responses along part of a judgment scale. He speculated briefly and cautiously on the problem:

"A possibility...though as yet without convincing evidence, is that we are dealing with a real 'end-effect'--that favorableness-neutral is one continuum and neutral-unfavorable is another, not collinear with the first. (At present the writer has nothing to offer but the suggestion and the 'precipice' effect to support it.)" (133)

Some twenty-four years later, in a discussion of Osgood's work, the problem is raised again by Edmund S. Howe. Noting studies that report larger responses to unpleasant stimuli than to pleasant ones, he states:

"The Bad half of the Good-Bad bipolar scale must cover greater psychological distance than the Good half; and...positive affect has an upper limit, while negative affect does not. Previous research in this area has not, however, generally come to grips with the basic problem of separating evaluative directionality from psychological intensity." (Snider and Osgood 1969: 216 [Howe 1965])

Howe concedes, in conclusion, that his own proposal for validating this hypothesis requires independent substantiation.

Osgood's own speculation on a solution to the problem when he first recognized it (see 1957: 327 above) does nothing to salvage the original notion of a multidimensional semantic

space. Furthermore, as Osgood also admits, it may prove to be no solution at all:

"The use of unidirectional scales might eliminate this problem, but it would probably involve us in another: if there is a 'natural' human tendency to think in terms of opposites, the so-called neutral point at one extreme of unidirectional scales would probably tend to take on the semantic properties of opposition." (1957: 327-8)

In light of the conundrum discussed here, the inherent problem with the concept of semantic space evident as early as 1957, it is not surprising that Osgood's later work focuses on mediation theory. The mediation process is as much an heuristic device as is semantic space, but it is less problematic (unless one accepts Fodor's view as outlined above) and more compatible with the now vast body of test results derived from the application of the semantic differential technique.

The Relation of Osgood's Work to Linguistics

The first assessment of Osgood's work to have come from a linguist was Uriel Weinreich's review of The Measurement of Meaning (Weinriech 1958, reprinted in Snider and Osgood 1969). It was a review of startling irrelevance, with most of the objections reverting to the chief complaint that the book represented no advance specifically applicable to lexicology. It does not seem to have troubled Weinreich that such was not the purpose of the work. It is true that by virtue of appearing in the concluding chapter, Osgood's references to lexicology and a quantized thesaurus are prominent (as they were in Osgood and Sebeok 1954), but the chapter is titled 'Summary and Prospectus', and the lexicological application is carefully qualified as belonging to the prospectus of future work. With respect to lexicology itself, Osgood and Sebeok (1954) was not, as Weinreich says, a briefer statement; on the contrary, it was a fuller statement and an indication that whatever lexicology might grow out of the semantic differential technique would not be of a sufficiently conventional form to satisfy Weinreich:

"The operations of semantic differentiation allow us to indicate the meaning of any verbal concept as a point in an n-dimensional space; this point can also be defined by a series of index numbers representing locations along the set of factors. Using a sample of subjects, carefully drawn to be representative of the population, the point (and the index numbers) would represent the mean location of the sample and another number would indicate the dispersion of the individual points about this mean (i.e., the variability in meaning of this concept). The concepts in this functional dictionary would be arranged in double

classification: once in ordinary alphabetical arrangement (e.g., NOBLE: 134xxxx, indicating this word to be extremely favorable, somewhat potent, neither active nor passive, and so on for additional factors) and once according to location in semantic space (e.g., under 134xxxx in a distribution running the gamut from 7777777 to 1111111 one would find NOBLE along with all other words having the same connotative meaning, and in their neighborhood would be found similar meanings)." (183)

Furthermore, such a dictionary would not be used in a conventional way:

"There are several ways in which such a functional dictionary could be used. For example, one could look up a particular noun, such as WARRIOR, and find a group of adjectives similar in all respects except one, say evaluation (e.g., if WARRIOR were 322xxxx, one might look under 722xxxx and find words like vicious, savage, and barbaric). In other words, one would move in any desired direction from a given point in the space and find appropriate words. Wishing to choose an adjective which accurately represents for other people one's own meaning for a concept, one could quickly differentiate his own meaning for the concept and then look into the functional dictionary under the index thus derived for words having appropriate connotation. An interesting derivative of this work could be study of semantic isoglosses (e.g. geographical boundaries across which the meanings of common words shift) in much the same manner that linguists have studied phonemic isoglosses. (ibid.)

That these statements are fuller than Osgood's similar statements three years later may have led Weinreich to mistake his personal disappointment in the advance of lexicology offered by Osgood's work for failure on Osgood's part to make such an advance.

Beyond the comments on lexicology, Weinreich makes some valid criticisms of The Measurement of Meaning. He notes, for example, that the explanation offered for the apparently inappropriate proximity of some terms in semantic space, an explanation invoking habits of usage and association, obscures the insufficiency of a three-dimensional model for what is in reality a multi-dimensional space. Osgood remained silent on this matter, though he answered Weinreich's other charges in Osgood 1959 (reprinted in Snider and Osgood 1969).

But Weinreich made other errors. He suggested Korzybski's general semantics as a source of the semantic differential technique (Weinreich 1958, footnote 4). Occasionally, Osgood's writings do have the tone of the pronouncements from the general semanticists, but Osgood and Sebeok (1954), with which Weinreich

was familiar, had already specifically disclaimed any connection with general semantics (177). More seriously, Weinreich charged that Osgood et al. were "helpless in placing their position in a historical framework of a general theory of signification." (1958: 358) This is simply not true, and Osgood (1958) was too modest in replying to the charge. The balance of this paper will deal with the issue as phrased by Weinreich, as well as with the broader question of the relevance of Osgood's work to linguistics.

Weinreich's charge is based on the brevity of the passage in The Measurement of Meaning dealing with linguistic meaning and the fact that references there are confined to scholars who notoriously rejected semantics. Yet the immediately following section deals with psychological meaning, and it is here that Osgood et al. do place their work in a historical context.

"The following conception of the sign-process can, in fact, be viewed as an attempt to make more explicit the behavioral nature of what Morris has termed 'dispositions'." (1957: 5)

Later:

"Returning for a moment to Morris's trichotomy, it can be seen that this view encompasses two of his aspects of meaning, semantical meaning (sign is related to significant via the common properties of r_m and r_t) and pragmatical meaning (signs are related to overt behavior via the mediation function.)" (ibid., 8)

Weinreich complained that Osgood et al. did not relate their work to the "general theory of signification" (my emphasis), but this is surely not the preserve of lexicologists or any other linguists; it is equally the concern of philosophers such as Morris, to whose work, as the preceding quotations show, Osgood et al. explicitly relate their work.

In assessing Osgood's work, Weinreich displays an inclination to wall linguistics off from other legitimately related inquiries into language. This is not in keeping with the assimilating tendencies and breadth of scholarship of Weinreich's other writings. Insisting on narrow definitions would have led Weinreich to view Ogden (1932) as no more of a contribution to lexicology than Osgood et al. (1957), since Ogden's method there belongs to logic more than to linguistics. But the Ogden work was part of his larger project on basic English and intended

precisely as a contribution to lexicology. Interestingly enough, in Osgood and Sebeok (1954), Osgood relates the incipient work on the semantic differential technique to Ogden's work:

"Some twenty or more years ago C.K. Odgen brought out a delightful little book on the nature of opposition. In it he analysed on logical grounds various types of opposites. The semantic differential seems to offer a quantitative way of approaching the same problem." (181)

Osgood characterized himself and Weinreich as belonging to "two disciplines with different traditions and problems (yet which have been interacting fruitfully over the past few years)." (Snider and Osgood: 138 [Osgood 1959]) These words were written when that interaction had barely begun, and in the intervening years (as already noted to some extent above in the discussion of the Osgood-Fodor debate), Osgood would relate his work explicitly to developments in linguistics. Thus, in reprinting Osgood 1963 in Jakobovits and Miron 1967, Osgood prefaces his article with this note:

"If I were writing this paper today, there is much that would be modified.... The general characteristics of my own model of the language user (speaker-hearer) would not be modified to any great extent, but I would now have much more to say about the notion of meaning as a simultaneous bundle of distinctive semantic features--how such features relate to componential r_m's, how they can be inferred from the rules of combination of words in phrases and sentences, and how this notion appears to coverge with the most recent ideas of Chomsky on subcategorization and selection rules in the grammar." (104)

Osgood argues in 1964 against the strong version of the Sapir-Whorf hypothesis (Snider and Osgood 1969: 303, [Osgood 1964], and Osgood et al. (1975), to the extent that it supports the claims of Berlin and Kay 1969) for universals in the color lexicon, is a disconfirmation of the Sapir-Whorf hypothesis.

In his interest in semantic universals, Osgood aligns with Chomsky. This, of course, does not automatically afford him a secure place within linguistics, but the relevance of his work to both current and traditional issues in linguistics must not be ignored.

Returning to The Measurement of Meaning, we can see that some psychological findings (from the application of the semantic differential technique) have obvious linguistic relevance which has not been exploited by linguists:

"When the profiles for GOOD and NICE are compared with those for MALE and FEMALE, we find that wherever MALE and FEMALE separate sharply, so also do GOOD and NICE." (1957: 168)

Here, and in subsequent studies, is empirical evidence for what can only be handled intuitively within linguistics.

With respect to semantic change, Osgood's work offers a way of exploring the explanation of such change, if not in the manner of conventional linguistic semantics:

"Even though the effect of synchronous presentation of two signs like LAZY and ATHLETE may be such as to shift them completely toward a point of mutual congruity, it is certain that this effect is not permanent--word meanings would be as fluid as quicksilver if this were the case! After each such cognitive interaction, we assume that the meanings of the related signs tend to 'bounce' back to their original locus--that representational process elicited in isolation is again much as it was before. However, this does not mean that repeated cognitive interactions have no effect which persists; like the bough that is repeatedly bent in a particular direction, the mediation process characteristic of a sign will gradually change toward congruence with the other signs with which it is associated. This is the basis, we suspect, for semantic change in languages over time...." (1957: 208)

A fundamental disjuncture between the conventional basis of linguistic semantics and the study of meaning by the semantic differential technique is delineated most explicitly in the summary chapter of Osgood et al. (1957):

"In what sense, then, are we measuring meaning with the semantic differential? It is certain that we are not providing an index of what signs refer to, and if reference or designation is the sine qua non of meaning, as some readers will insist, then they will conclude that this book is badly titled. On the other hand, language users do develop representation processes in association with signs and these processes are intimately concerned with their behavior." (325)

Subsequently, Osgood came to the view that this disjuncture could be bridged:

"If a series of concepts, falling into different implicit classes is judged comparatively on single semantic differential scales one at a time, the scales will tend to be used denotatively to the extent that they have denotative properties. By assessing the dimensionality of the scale-space under these conditions we may be able to generate a denotative semantic space. Such a space will certainly contain many more factors than the

affective semantic space. Furthermore, the distances between concepts within this space may provide a usable quantitative index of denotative similarity. The development of a satisfactory quantitative measure of denotative meaning appears to me to be one of the most important problems for contemporary psycholinguistics." (Snider and Osgood 1969: 330 [Osgood 1964])

The final word here is not psychology but psycholinguistics. Osgood has accomplished a personal transformation during his career from psychologist to psycholinguist, and as it appears that the semantic differential technique is applicable to the denotation of linguistic signs, he may still fulfill his original objective of configuring Roget's Thesaurus into "clusters of starlike points in an immense space." (Snider and Osgood 1969: vii)

FOOTNOTES

1 "I have always been a visualizer, which may explain why I did well in geometry but miserably in algebra." (Snider and Osgood 1969: vii) This observation is part of a rare first-hand account of the origin and development of the idea of semantic space, in which Osgood reveals: "I also recall my visual representation of Roget's Thesaurus--a vivid and colorful image of words as clusters of starlike points in an immense space." (ibid.) He goes on to say that when he studied under Theodore F. Karwoski at Dartmouth College, Karwoski, himself given to synesthetic thinking, was doing research on synesthesia, and his ideas on the parallel polarities in dimensions of experience reactivated Osgood's childhood conception of semantic space.

2 Osgood's aside in making this charge shows how his own position on learning-theory differs from Skinner's. "It is the lack of such functional separation that forces Skinner (1957) into such intuitively unsatisfying assumptions as (a) that we must subvocally mimic a speaker in order to comprehend what he says or (b) that having learned to name objects (tacting) we will not therefore be able to ask for them (manding), since they are dependent single-stage operants." (Osgood 1966: 404)

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ON THE COEXISTENCE OF PITCH-ACCENT AND STRESS-ACCENT SYSTEMS

A PRELIMINARY OUTLINE

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Introduction

A useful set of criteria to classify languages according to the use they make of pitch, has been provided by Hyman 1978. His framework consists of three basic types of language, tone, pitch-accent, and stress-accent, and three interactional types involving tone and accent. Hyman does not however consider those languages which combine pitch-accent and stress-accent systems, and this is precisely the object of the present paper.

First we review the basic notions as presented by Hyman 1978.

Basic types

In Table 1, we see that there are three basic categories according to the use a language makes of pitch. This typology is based firstly on the paradigmatic and syntagmatic aspects of language, and secondly on the constancy of the association between a given pitch-level or pitch-contour, and a given tone or accent.

Thus, in Igbo, a basic tone language, every 'slot' or position in a polysyllable takes either a high or low tone, and these are paradigmatically opposed; in two syllable units (for example), this results in a four-way distinction: we obtain four different patterns, and four different words distinguished by paradigmatically opposed tones in different slots (see (1) below, from Welmers 1970, in Hyman 1975:213).

In 'Afar on the other hand which is a pitch-accent language, we can get only a two-way distinction because the contrast is made not vertically, but horizontally, and we oppose either an accented syllable to an uncaccented syllable, or an unaccented to an accented. The accented syllable in 'Afar is always H, and so we get a constant association between pitch & accent, and this makes 'Afar a pitch-accent language (see (2) below, from Carter 1980:15).

In English, the association is not constant, and so English is a stress-accent langauge. This is illustrated in (3) below with the

Table I Basic Types

	Paradigmatic	Constant
Tone (Igbo)	+	+
Pitch-accent ('Afar)	-	+
Stress-accent (English)	-	-

(1) Basic Tone (Igbo)

H-H	ákwá	'crying'
H-L	ákwà	'cloth'
L-H	àkwá	'egg'
L-L	àkwà	'bed'

(2) Basic pitch-accent ('Afar) (CUSHITIC, Voegelin 1977)

H-l	áwka	'boy'
l-H	awká	'girl'

(3) Basic stress-accent (English)

		Citation	Interrogative	Attidudinal	Pre-nuclear
A-u	subject (noun)	˙	˙	˙	˙
u-A	subject (verb)	˙	˙	˙	˙

Table II Interactional types (Hyman 1978: 5-6)

Tone (+ accent): Mandarin
Accent (+ tone): Fasu, Serbo-Croatian, Scandinavian
Tone and accent coordinate : no examples

(4) Accent superimposed on tone (Mandarin)

T_1-t_5
T_2-t_5
T_3-t_5
T_4-t_5

(5) Tone superimposed on accent (Fasu) (New Guinea, Voegelin 1977)

H-m̀	(marginal)	m-H
L-m		m-L

(marginal = unaccented)

(6) Tone and accent coordinate (no examples)
(T and A independent and unpredictable)

H́-H H́-L Ĺ-H Ĺ-L
H-H́ H-Ĺ L-H́ L-Ĺ

Table III

Pitch-accent and stress-accent side by side (Caribbean English) (not in Hyman)

Pitch-accent		Stress-accent		
		A-u	'subject'	(noun)
L-H	'pillow'	u-A	'subject'	(verb)

(7) The bird is a tùrkéy, the country is Turkéy.

(8) The country is Tu̇rkey, the bird is a tùrkéy.

grammatical opposition subject (noun), subject (verb), where the stress opposition is maintained, while pitch is manipulated to suit intonational requirements such as utterance type, attitude, position in the sentence, & so on (O'Connor & Arnold 1961, Carter 1980: 16-8).

Interactional types

Let us now consider languages like Mandarin and Fasu where tone & accent interact (Table II). Here we find that two features are quite insufficient to show the difference between basic & interactional types. We therefore add one feature, syntagmatic, which happily accounts for the difference: whereas [- paradigmatic] previously meant [+syntagmatic], now we introduce types which are both [+ paradigmatic] and [+ syntagmatic]. This accounts for the difference between basic & interactional types, but still fails to distinguish among the various interactional types themselves. Among the latter, we have on the one hand accent superimposed on tone (Mandarin), on the other tone superimposed on accent (Fasu, Serbo-Croatian, Scandinavian - see Hyman 1978). In order to further differentiate these sub-types, it will be necessary to add another set of features. The object here will be to establish paradigmatic oppositions as structurally more important than syntagmatic, & vice versa. A very simple quantitative criterion is in fact available to us: on the one hand, number of paradigmatic oppositions, on the other, number of unaccented syllables.

Mandarin now qualifies as basically a tone language because it has a relatively high number of paradigmatic oppositions, and a low number of unaccented syllables; Fasu on the other hand would be basically an accent language because it has many unaccented syllables, and a single H-L paradigmatic opposition (see (4) & (5) above).

A third possibility - tone & accent co-ordinate and equal - allows eight patterns for disyllables, 24 for trisyllables, and so on (see (6) above). In other words we have exactly double the possibilities of a true tone language like Igbo ((1) above). There seems to be no language on record which exploits this possibility, though apparently certain of the elements are provided in Diuxi Mixtec (Eunice Pike, Phonetica 33 (1976), in Hyman 1978).

Pitch-accent and stress-accent side by side by side

Having considered interaction between tone & accent, it would seem the next logical step to look for 'interaction' between pitch-accent & stress-accent systems. 'Interaction' however does not seem the appropriate term to describe what goes on between these two. Tone & accent may very well interact: we have seen that paradigmatic & syntagmatic oppositions can occur at the same time in a given slot. Pitch-accent & stress-accent however by definition are mutually

exclusive: it is not possible for the association between pitch & accent to be constant as well as non-constant at the same time. What is possible is coexistence of the two, rather than 'interaction' in the strict sense. The English which developed in the Carribbean provides a good example of this. Here we have neither stress-accent superimposed on pitch-accent, nor pitch-accent superimposed on stress-accent, but the two existing side by side. Thus we find that some words in this variety of English bear a pitch-accent pattern, and other words a stress-accent pattern, as in Table III.

As an illustration consider the sentences (7) & (8) above. Here we have a minimal pair Túrkey (the country) with normal H-L pattern, and tùrkéy (the bird) with accent shift, giving L-H in Caribbean English (Guyana, Trinidad, Barbados). These two sentences demonstrate very well the difference between pitch-accent & stress-accent, because we have both words in mid-sentence and in sentence-final position.

First, consider Turkey (the country): in final position there is a definite fall in Standard English as well as Caribbean: '... the country is Tūrkȇy.' In mid-sentence before a pause, there is frequently an upturn: (The country is Tūrkéy, ...).

Secondly, consider turkey (the bird): in Standard English there is no difference at all in the pitch patterns between turkey (bird) & Turkey (country). But in Caribbean we get an upturn in all positions for the bird tùrkéy, therefore this corresponds to a pitch-accent. On the other hand, since patterns are more or less as in Standard English for the country Turkey, this corresponds to a stress-accent in Caribbean.

Note that even in mid-sentence, where L-H occurs for both words, the lexical opposition may still be maintained by a greater pitch interval for tùrkéy (bird), than for Tūrkéy (country). Nevertheless, the possibility of neutralization should not be excluded, but a great deal of testing will have to be carried out to dertermine whether this happens.

As a great many words in Caribbean have the L-H pitch-accent pattern, and as this is also the pattern used for compounding (e.g. lìghthóuse), one gets a steady succession of pitch-accents & stress-accents in the flow of speech. The proportion of pitch-accents increases in Creole speech because the tendency here is towards syllable-timing, and non-reduction of vowels in unaccented position: in words like compel or machine, equal timing and weight will be given to both syllables, the only difference being pitch, i.e. L-H. But in educated speech, the pitch & stress patterns for such words will be practically the same as in Standard English, i.e. u-A, where the unaccented syllable has noticeably less weight than the accented.

Theoretical problems

The theoretical problems associated with this subject are quite considerable; they can be only briefly evoked, as they go well beyond the limited preliminary considerations undertaken here.

The problems which arise in the description of coexistent phonemic systems were first highlighted by Fries & Pike (1949). More recently, the question has been discussed by Tsuzaki (1971) and Labov (1971). Though Fries & Pike were referring to unassimilated loans, and this is certainly not the case we are dealing with in Caribbean varieties of English, nevertheless their observations have a direct bearing on the Carribbean situation. For instance they claim (p. 29) that the observable reactions of native speakers are necessary to determine whether we have a uniform system, or one where two separate systems coexist.

It would be premature to try and give any definitive answers regarding Caribbean English. There does not seem to be any 'conflict' in the sense indicated by Fries & Pike. They point out certain phonotactic restrictions in Mazateco, which apply in some words, but not in others (i.e. the unassimilated loans). In Caribbean on the other hand (e.g. Guyanese), words with the L-H pattern, with equal weight on both syllables, follow the pitch-accent system without exception. If there were words where the identical L-H pattern admitted stress-accent variation, then indeed we would have a conflict parallel to the examples furnished by Fries & Pike. To be sure, we do have some degree of conflict in Caribbean, since it is the H-L pattern where stress-accent is most likely to occur. As for the second criterion regarding native speaker reactions, a great deal of testing will have to be carried out before we can make any kind of valid pronouncement in the matter.

Tsuzaki's position is that Hawaiian English is a set of coexistent systems, partly a pidgin, partly a creole, and partly a dialect (Tsuzaki 1971:328). The last is divisible into (a) a non-standard & (b) a standard variety of English (id. 330). Here again, the meaning given to the term 'coexistent systems' does not correspond to that which we have presented above: our coexistent systems occur within a single language variety, though indeed we have hinted at the possibilty of an increasing proportion of pitch-accents in creole.

Labov (1971:456), commenting on both Fries & Pike's, and Tsuzaki's papers, states that more exacting standards must be set up for establishing the existence of separate systems. Labov's requirement is applicable to our case, since one exacting standard, that of empirical proof, is somewhat lacking and will have to be supplied eventually, in order to establish our claims about coexistent stress-accent & pitch-accent systems in the Caribbean. Labov also mentions

style shifting, and this is pertinent to the Caribbean situation, since, as we have pointed out, pitch-accent probably increases as we go from 'acrolect' (educated speech) to 'mesolect' (intermediate varieties) to 'basilect' (creole).

Another large theoretical question is that of underlying level. In order to best account for surface phenomena, do we posit a single underlying structure, and at some point of our derivation insert a rule which splits the system into stress-accent & pitch-accent? Or do the two systems coexist at all levels? Such problems will be well worth following up in the future.

Conclusion

Hybridization of systems resulting from languages in contact is one of the most common phenomena known to linguists. The aim of this paper has been to bring to light a case which we find specially interesting typologically. Within the classification into tone, pitch-accent, and stress-accent, 'interaction' between tone & accent has been shown to exist in some very well known languages (Mandarin, Scandinavian, Serbo-Croatian). However, coexistence between stress-accent & pitch-accent has never been brought to light, to my knowledge.

As can be seen from the foregoing, the theoretical issues are many and challenging, and will certainly bear much scrutiny in the future. Much more data is needed to support or disconfirm theories, and the theories themselves have to be examined with a view to determining what is important, what trivial, what unsaid or neglected, and what further theories need to be advanced.

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A Note on the Underlying Shape of Present-Tense Morphemes in East Slavic

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ABSTRACT

In the one-stem theory of the Russian verb (Jakobson 1948), which has subsequently been generalized to apply to all East Slavic languages, the present-tense morphemes are broken into two components: a present-tense marker and a person/number marker.

Evidence derived from the development of these morphemes in Byelorussian dialects, however, suggests that native speakers of these dialects treat them as single units, a fact which has broad implications for determining the procedures by means of which morphemes are isolated in a linguistic analysis.

In his important article "Russian Conjugation",[1] Roman Jakobson proposed that the present-tense endings found in Contemporary Standard Russian were actually composed of two independent morphemes: a non-terminal present-tense marker (which can be ∅), and a terminal person/number desinence. There are four overt "present classifiers" (as he called them), which are assigned to different verbs according to the shape of their stem: o, i, u, and a. The last two are used in the third person plural (3p) only, while the first two are found in the 2 s and p, the 3s, and the 1p. Verbs with "open full stems" in i and e -- known traditionally as verbs of the second conjugation -- take the non-terminal markers i and a, while all other stems (i.e., verbs of the first conjugation) utilize o and u. The 1s of all verbs has the present marker ∅. The person/number endings are as follows: 1s -- u; 2s -- s; 3s + p -- t; 1p -- m; 2p -- te.

Now, this bipartite analysis of the CSR present-tense endings is perfectly consistent with the general aims of the so-called "one-stem" theory. As has often been stated,[2] the advantages of this theory over the older view (that at least two stems are needed to generate all the forms of any verbal paradigm[3]) include simplification of the lexicon, its greater predicative power, and the fact that it renders the superficially complex Slavic verbal system quite regular and homogeneous. In viewing the present-tense endings as complex morphemes containing a present marker and a person/number desinence, Jakobson was able to state that all but the most anomolous verbs took the same set of terminal morphemes in the present tense, and that the two major conjugations differed only in which set of present markers is utilized. Furthermore, this analysis unquestionably mirrors the history of the forms: the o of the first conjugation endings ultimately comes from the thematic vowel e of the Indo-European thematic conjugation,[4] while the second conjugation i was probably originally part of the stem of certain classes of thematic verbs.[5] 3p u and a developed from a back and front nasal, respectively, which in turn originated in the combination of theme vowel o/stem-final i plus desinence-initial n of the old 3p ending -ntĭ. The person/number morphemes, too, have their own histories as independent forms.[6]

This tiny bit of Jakobson's theory has generated little debate over the years. Most scholars who have applied the one-stem system to the various Slavic languages have incorporated it without comment.[7] A few have just as quietly chosen to use indivisible global endings in their works.[8] However, while the one-stem theory as a whole retains its power,[9] and most certainly has even psychological reality for speakers of at least some Slavic dialects, there is evidence from Belorussian that Jakobson was wrong to posit a morpheme boundary within the East Slavic present-tense endings. The nature of variation in Central and Southern Belorussian suggests rather that speakers view these endings as indivisible units.

Variations between alternate present-tense endings in Belorussian dialects: an overview.

Belorussian present-tense paradigms are quite similar to their CSR counterparts, and in most cases utilize reflexes of

the same endings. There are three major differences: 1.) first-conjugation endings begin with e, not o, except in the 1p; 2.) 3s and p endings end in a soft, voiceless dental, not a hard one; 3.) the final vowel of the 2p endings receives the stress in verbs marked for fixed end stress, instead of the desinence-initial vowel, as is the case in most Russian dialects.[10]

Belorussian is important for Slavic historical linguistics because so many crucial isoglosses criss-cross its territory. Several of these involve variations between various present-tense endings.[11] The most important of these has to do with the 3s of 1conj verbs: in the northeast, these verbs take -ec', while in the southwest they take -e: v'aʒ'éc' "leads", píšyc' "writes" : v'aʒ'é, píša. This is the most important isogloss in a major bundle which transsects the Belorussian language area from northwest to southeast.

Another isogloss in this bundle separates different reflexes of the 2p endings: NE of the line, the final e has gone to o when stressed, doubtless as part of the well-known East Slavic shift of e to o (whence came the o of the Russian 1conj endings).[12] SW of the isogloss the e remains: 1conj v'aʒ'ic'ó, 2conj s'aʒ'ic'ó "you sit" : v'aʒ'ac'é, s'aʒ'ic'é.[13]

1p endings with -mo instead of -m are a feature of Ukrainian. Such forms have penetrated dialects of southern and western BR. In these dialects, 1p endings in -mo are found mostly with 2conj verbs marked for fixed end stress (s'aʒ'imó), less frequently with 2conj verbs having fixed stem or mobile stress (xóʒ'imo "we walk"), and less frequently still with 1conj verbs (vedémo, písemo; note that these endings are stressed differently in the 1conj, and that the stress pattern a stem is marked for does not play any role here).[14]

In Belorussian, as in all languages, there is never a sharp demarcation between dialects, but rather "hybrid" systems between the major divisions which provide for a gradual transition. The particulars of the variation between present-tense endings in these more dynamic systems are what militate against Jakobson's bipartite analysis. Let us now examine four such cases in detail.

The 3s in Central Belorussian dialects.

Besides dialects with free variation between the two 1conj endings in all 1conj verbs, there are also those in which this variation has become fixed: 1conj verbs with fixed end stress take /-et'/, while those not marked for fixed end stress take /-e/ -- thus v'aʒ'éc', but p'íša. In a large number of central BR dialects, especially towards the east (see map), /-e/ has spread to 2conj verbs displaying the same stress patterns, with end stressed 2conj verbs retaining /-it'/: thus s'aʒ'íc', but xóʒ'a.[15]

Some Soviet linguists have claimed that this situation marks the falling together of all non-end stressed verbs into one paradigm, a state of affairs which would mirror a widely-studied phenomenon found in NE BR and SRus. However, data published in the major works of BR dialectology do not show such a complete coalescence.[16] They perhaps suggest that such a "stem-stressed" paradigm may be taking shape gradually in the area,[17] but there is no compelling evidence that the original 2conj verbs are using any other 1conj endings with any consistency. Therefore, it is safer to say that, at least in most Central BR dialects, the two conjugations remain distinct regardless of stress, and that /-e/ has simply replaced the historical 2conj ending /-it'/ in the paradigms of the non-end-stressed 2conj verbs.

The 2p in Central BR.

In the NE BR dialect area, 1conj verbs with fixed end stress regularly display the 2p ending -ic'ó. There is every reason to believe that these verbs have appropriated the 2conj ending found in this area: /-it'ó/. In virtually all of the different vowel reduction systems found in the NE, an /-e/ in the first pretonic syllable should either reduce to [a] before stressed o, or else remain unchanged.[18] Therefore, it is highly unlikely that a surface form such as v'aʒ'ic'ó comes from underlying /vedet'ó/. (Recall, too, that the speakers of these dialects do display a marked tendency to restructure verbal paradigms according to stress-patterns rather than stem-final suffix).

three villages,[24] but in general the published data confirm her statement.

What is interesting is that speakers in many villages in southern and central BR use lp doublets in -emo and -om: p'íšom : p'íšemo, v'adóm : v'aʒ'éma.[25] While -em will often be found alongside these two endings, -omo never will.

* * *

With all four cases of variation, the Jakobsonian analysis proves too powerful. Consider: according to this theory, in each instance we are dealing with two or three present-tense markers, and two person/number desinences. Simple arithmetic shows that in the first three situations, we should have four different endings to contend with: 3s e, i X Ø, -t' = -eØ, -iØ, -et', -it'; 2p e, i X -té, -t'ó = -eté, -et'ó, -ité, -it'ó; 1pA e, i X -m, -mo = -em, -emo, -im, -imo; and in 1pB, we should expect six: e, i, o X -m, -mo = -em, -emo, -im, -imo, -om, -omo.

However, in each of these cases, one of the allegedly possible endings simply does not exist. There is no trace of 3s forms like *xóʒ'i (/xod-i-Ø/) on Belorussian soil.[26] In several BR dialects, 2p forms in -ité, -eté, and -it'ó will be encountered, but never forms in *-et'ó. Some SW dialects display lp forms in -em, -im, and -imo, but never *-emo, while others have lp -em, -emo, -im, -imo, and -om, but never *-omo.

Obviously, in these linguistic systems, the one-stem analysis starts to lose its advantage of simplicity and economy. The spell-out rules needed to assign endings to stems become excessively complex, since they have to make reference to stem type, stress pattern, and different combinations of present marker and person/number desinence. Furthermore, how can the linguist account for the fact that certain perfectly reasonable endings simply do not exist? Is he expected to discuss these ephemeral forms at all? One would expect so, for the theory, combined with logic and linguistic analogy, fairly cries out for these fictional items (e.g., 1conj end-stress: e + t'; 2conj end-stress: -i + t'; 1conj non-end-stress: -e + Ø; 2conj non-end-stress: X + Y; why not X = i, Y = Ø?).

Clearly, it makes infinitely more sense simply to assume that speakers of these Belorussian dialects view present-tense endings as indivisible global units. The variation that the linguist should have to discuss is that which the data present to him: 3s -et' vs -it' vs -e; 2p -eté vs -ité vs -it'ó; 1pA -em vs -im vs -imo; 1pB -em vs -emo vs -im vs -imo vs -om. Our underlying analysis must not become so abstract as to obscure the true nature of the problem!

I mentioned at the outset that the question of whether present-tense endings in Slavic are bipartite or global has never been an issue for hot debate in the field, and it has hardly been my intention to fulminate a tempest in a tea-pot, or to artificially create controversy over a minor descriptive point. It is interesting, however, how evidence from living dialects refutes an analytical detail of a theory which has otherwise proved so fruitful over the years.

FOOTNOTES

[1]Word IV (1948), 155-67; reprinted in Selected Writings II (The Hague: Mouton, 1971), 119-29.

[2]See, for instance, virtually all of the adaptations of the one-stem theory to other Slavic languages (e.g., Herbert Rubenstein, "The Czech Conjugation", Word VII, no. 2, 1951, 144-54, esp. 144; Morris Halle, "The Old Church Slavonic Conjugation", ibid., 155-67, esp. 155); and generative versions of the theory (Horace G. Lunt, "Russian Verbal Morphology -- A Generative Sketch", (unpublished draft, 1968); Lunt: Old Church Slavonic Grammar (The Hague: Mouton, 1974) 145-214 (the epilogue), esp. 156.

[3]See Scatton 1984, 213-16 for a discussion of the "two-stem" approach and references to the literature.

[4]Meillet 1937, 237-38; Watkins 1969, 59. The thematic vowel was originally o in the 1s and p, and in the 3p; however, in Slavic the o of the 1s and 3p combined with a following nasal to give o, and it is generally assumed that the o of the 1p was

then levelled out in favor of *e* already in Common Slavic.

[5]Meillet 1937, 232-40. Another theory holds that the 2conj was formed on the base of the old Indo-European perfect conjugation: see Kuryłowicz 1964, 79-84, and Watkins 1969, 222.

[6]See, *inter alia*, the appropriate sections of Meillet 1937 and Watkins 1969.

[7]For example, Rubenstein 1951, Halle 1951, Lunt 1968, and Lunt 1974.

[8]E.g., Townsend 1968.

[9]See Scatton 1984, however, for a recent, more negative assessment (esp. 218-223).

[10]Terminally-stressed 2p endings are found in Western Russian dialects contiguous to BR, as well as in Northeastern Russian; see Orlova 1970, 106, 108. Such endings are also a feature of Ukrainian. For a discussion of the history of these forms, see Miller 1980, 3.36-3.38.

[11]See the *Dyjalektalahičny atlas belaruskaj movy* (Minsk: AN BSSR, 1963), maps 148-157, and the accompanying sections in the *Commentary* to the *Atlas*; and Ju. F. Mackevič, *Marfalohija dzejaslova ŭ belaruskaj move* (Minsk: AN BSSR, 1959). Also see the relevant sections in *Narysy pa belaruskaj dyjalektalohii* (Minsk: Navuka i texnika, 1964) and *Linhvistyčna heahrafija i hrupoŭka belaruskix havorak* (Minsk: Navuka i texnika, 1968-69), 2 vols.

[12]A discussion of the change of *e* to *o* and its reflection in East Slavic verbal paradigms (with reference to the literature) can be found in Miller 1980, 2.7-2.24.

[13]DABM, maps 152 and 155.

[14]DABM, maps 150, 151, and (especially) 154. For a discussion of *-mo* in Ukrainian, see Bevzenko 1960, 287-88.

[15]DABM, maps 148, 149, and 153.

[16]See the sample paradigms given in Narysy, 261-62, in which all non-end-stressed verbs are shown taking 1conj endings optionally in all persons but the 3p, where the 2conj ending /-at'/ is used. These paradigms are not discussed in the text; neither is syncretism in non-end-stressed verbs in Central BR, except in the 3s. For the possibility that such a falling-together may actually be in progress, see Mackevič 1959, 57ff; Miller 1980, 6.6-6.11; and Miller 1981.

[17]Miller 1981.

[18]For a discussion of vowel reduction and the 2p in BR, see Miller 1980, 3.9-3.22, esp. 3.14ff.

[19]Data can be found in DABM, Commentary to map 152, nos. 408, 415, 480, 562, 792, and 900; and in Miller 1980, table 3.3, p. 3.19.

[20]Miller 1980, 3.20.

[21]For data, see DABM, Commentary to map 151, nos. 769, 962, 963, and 965; also Miller 1980, table 2.9, p. 2.50.

[22]See Panov 1967, 48-62. The important factor in determining the phonetic quality of any vowel in northern East Slavic is the hardness/softness of the surrounding consonants.

[23]Mackevič, 154, 158.

[24]DABM, Commentary to map 150 (nos. 976 and 978) and map 151 (no. 803).

[25]Data in DABM, Commentary to maps 150 (nos. 495, 806, 966, 1027) and 151 (nos. 975, 978, 990); see also Miller 1980, tables 2.10 and 2.11, p. 2.50.

[26]Linhvistyčna heahrafija (vol. 2) quotes three -iØ forms from old manuscripts on pp. 261-62, but offers no further comment. Such 2conj forms are occasionally encountered in NW Russian dialects: see Orlova 1970, 125-128.

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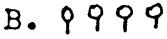
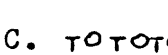
Key to map.

A. 1conj 1p: -em vs -om.

B. 1p: -mo vs -m.

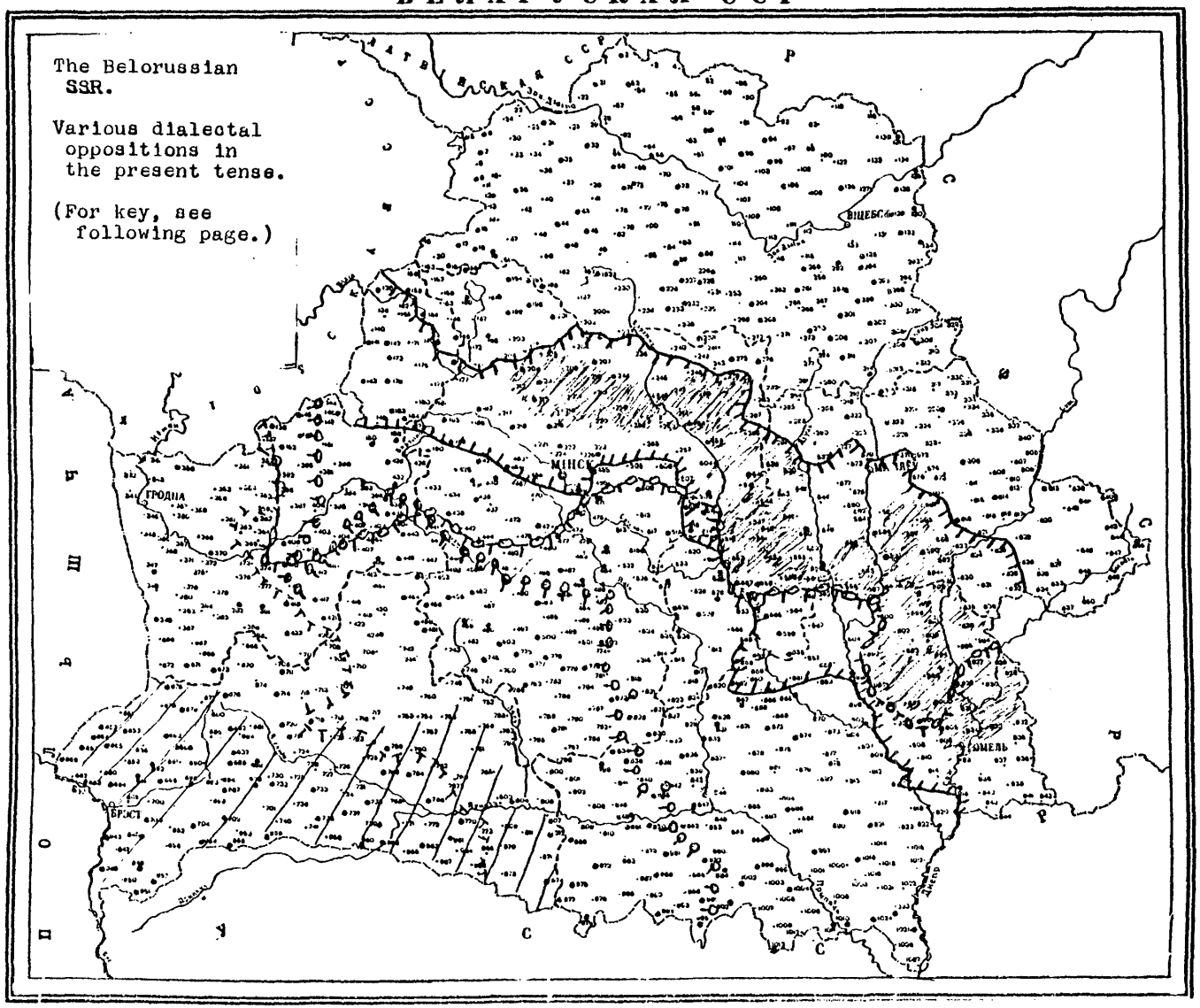
C.  End-str 2p: -te vs -t'o.

D.  3s: NE of isoglosses: 1conj -ec'; SW of isoglosses: 1conj -e; within isoglosses: doublets in 1conj, 2conj non-end-str w/-e.

E.  Area in which 1conj 1p can take endings in -mo.

F.  Area in which 1conj end-str 3s is -éc', 2conj end-str 3s is -íc', and all verbs not marked for end-str take 3s -e.

БЕЛАРУСКАЯ ССР



L'Acadie de bouche en oeil

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La problématique que cache mon titre est un demi-millénaire plus vieille que l'Acadie. Elle me tient donc particulièrement à coeur, et elle a, depuis quelques années, fait couler beaucoup d'encre; je ne mentionne que deux livres capitaux récents: l'*Introduction à la poésie orale* de Paul Zumthor[1] et *The Implications of Literacy* de Brian Stock.[2] Oralité et écriture, l'une méprisée par les critiques littéraires, l'autre délaissée par les linguistes, voilà deux pôles sûrs, me semble-t-il, entre lesquels nous pouvons situer notre sujet d'étude, cette Acadie par ailleurs à jamais insaisissable.

Mais comment étudierions-nous l'Acadie orale, nous, les mordus des mots imprimés? Ne faudrait-il pas, à l'instar de certains linguistes de notre connaissance, aller se promener avec son magnétophone pour écouter les vieux et les moins vieux? Il le faudrait effectivement. Il nous faudrait aussi respecter davantage, et exploiter à fond, les travaux de nos collègues folkloristes qui ont accumulé, comme vous savez, d'impressionnantes archives sonores. Il faudrait enfin prêter attention tout particulièrement aux manifestations orales du pays, au théâtre en premier lieu, mais aussi à la chanson et à la poésie récitée. Nous y découvririons les mécanismes profonds qui ont fait survivre, vivre et rêver les Acadiens; nous y découvririons également (pardonnez-moi la terminologie contradictoire) les structures littéraires profondes de l'oralité acadienne, structures qui, à notre époque, organisent les textes.

Aurions-nous pour autant capté l'Acadie? Une Acadie sans textes est-elle encore imaginable? Il y a cent ans, l'écriture était une aventure; aujourd'hui, l'Acadie, sachant de moins en moins se dire, est condamnée à s'écrire.

Du coup, bien des choses changent. De recroquevillée dans les plis de sa mémoire collective, l'Acadie devient domaine public, identifiable, récupérable et exploitable.

Comme le dit Lévy-Strauss: "le seul phénomène qui...ait fidèlement accompagné.../l'écriture/ est...l'intégration dans un système politique d'un nombre considérable d'individus et leur hiérarchisation en castes et en classes.../l'écriture/ paraît favoriser l'exploitation des hommes avant leur illumination...la fonction primaire de la communication écrite est de faciliter l'asservissement."[3] A l'élite prédicante de jadis se substitue ainsi l'élite littéraire d'aujourd'hui, une élite souvent auto-congratulatoire, encline à un certain nombrilisme et qui risque de refermer bien vite les portes que la création s'est si péniblement ouvertes sur des horizons supra-régionaux.

Pour certains, c'est le cas d'Antonine Maillet et de son oeuvre. Médire d'elle est assez à la mode ces jours-ci, mais c'est méconnaître le rôle fondamental, même s'il est limité et limitant, qu'elle est appelée à jouer dans l'histoire culturelle de l'Acadie. Comme n'importe quelle autre communauté fragmentée en micro-sociétés isolées, dans la disparate de ses parlers, l'Acadie, telle la Neustrie des Francs, a éprouvé le besoin et trouvé l'élan vital pour l'assouvir, de se projeter elle-même dans une image qui la fonde en existence. Et la première affirmation éclatante d'une communauté qui se saisit comme telle, l'irruption dans l'histoire de la première prise de conscience collective, c'est le texte épique qui les véhicule, c'est l'épopée, forme première dans l'ensemble des formes où s'articule la société.[4] Ceci est un fait anthropologique universel, et *Cent Ans dans les bois*[5] et *Pélagie-la-Charrette*[6] le confirment pour l'Acadie: si elle avait vécu au XIe siècle, Antonine Maillet aurait pu écrire *La Chanson de Roland*.

Or, l'histoire nous enseigne également que l'épopée en tant que genre est auto-destructrice: son rôle prend fin dès que, par elle et à travers elle, une communauté s'est reconnue comme telle, dès que chacun de ses membres s'y définit par son appartenance (Ollier:212). Alors, *Pélagie-la-Charrette* ayant joué son rôle, l'épopée n'est-elle pas morte? *Cent Ans dans les bois* serait alors sa nécrologie, commémorant la naissance et la vie du genre dans trois ou quatre textes, et invitant discrètement à adopter d'autres manières d'énoncer l'Acadie.

L'épopée est également condamnée par son statisme: le discours épique, redondant, répète toujours le même modèle (Ollier:209). Il en résulte une certaine prévisibi-

lité dans la création d'oeuvres acadiennes, et une certaine impatience de la part de ceux qui ne veulent plus célébrer la geste devenue mythe et qui s'acharnent, sans trop de succès encore, "à accrocher.../le/ passé à... /l'/avenir" (*Cent Ans*:340).

Ce mythe d'une Acadie héroïque risque enfin d'être récupéré à des fins de propagande. Emerveillé de s'être projeté dans l'histoire (Ollier:211) et ainsi ravigoté, le peuple pourrait finir par abandonner sa docilité. L'épopée sert alors à canaliser et exploiter l'énergie galvanisée par la prise/crise de conscience, en divertissant et édifiant les gens "par l'évocation des tribulations historiques" (Ollier:210) et en les stimulant en même temps au courage. C'est ce que firent les "bonzes d'Acadie" en proposant au pays "une patronne...un hymne et...un drapeau" (*Cent Ans*:342), et c'est pourquoi à la tradition mailletienne succéderont des oeuvres à la *Zélika à cochon vert*,[7] et non des romans.

Pourtant, à longue échéance, le remplacement de l'épopée par le roman est, *sub specie medii aevi*, inévitable. Les premières ébauches romanesques acadiennes montrent d'ailleurs par leur matière, par le public auquel elles sont destinées, et par les ressources de création dont elles se servent, qu'elles ne veulent plus alimenter la tradition épique. L'épopée mailletienne ne saurait rester à jamais le paradigme de tout discours acadien, tout comme *La Chanson de Roland* n'était déjà plus celui du discours médiéval à l'époque de Chrétien de Troyes. Car tout est en train de changer. La matière n'est plus celle d'un passé héroïque, mais celle d'un présent contradictoire et d'un avenir incertain; et le public n'est plus un auditoire collectif, mais un amalgame de lecteurs individuels dont l'horizon d'attente est souvent plus vaste que celui qui suffisait à l'épopée traditionnelle, linéaire et répétitive. Depuis la fin des cent ans passés dans les bois, il y a eu une nouvelle distribution du rapport de forces, un nouvel univers politique s'est formé (Ollier:213), des renouveaux, crises et révolutions se sont succédés, phénomènes que devrait traduire et interpréter le discours romanesque. Car pour donner un sens à l'Acadie moderne, il ne suffit plus, comme le faisait l'épopée, d'imposer ce sens de l'extérieur, *ex cathedra*.

Quel rôle le roman peut-il alors jouer en Acadie? Après que dans l'épopée s'est identifiée la collectivité, le roman, en introduisant des personnages plutôt que des types, répond au besoin d'identification personnelle; aucun lecteur ne penserait à s'identifier à un héros épique, prévisible, sans complexité conflictuelle, prédéterminé. Le personnage romanesque, par contre, est le lieu de changements imprévisibles tout au long d'un itinéraire jalonné des obstacles ou des appuis (des "aventures" médiévales) qu'y dispose la société (Ollier:213), et le lecteur qui se reconnaît en lui pénètre d'autant plus aisément à l'intérieur signifiant de l'oeuvre et du monde.

Autant l'épopée est impénétrable et limitée dans ses manifestations, autant le roman est ouvert et libre dans l'organisation de ses unités de signification et de structure, produisant sens de façon autonome (Ollier: 214), de l'intérieur. Le roman est un système polyvalent de signes multiples, il sécrète sa cohérence (la "conjointure" de Chrétien de Troyes) propre et soustrait le récit au temps du mythe pour créer sa propre temporalité (Ollier:214).

Si nous scrutons l'horizon littéraire acadien, nous trouverons, d'après moi, très, très peu de textes véritablement romanesques. C'est que la parole mythique dont se nourrit l'épopée ne permet pas encore, dans son omnipotence, la polyvalence totale des signes romanesques. La hantise de la parole et l'attrait irrésistible des mots sont tels que certains auteurs ont eu recours à ceux-ci pour tenter de maîtriser celle-là. J'ai étudié ailleurs ces tentatives visant à contrôler l'oralité au moyen de graphies plus ou moins élaborées.[8] Ce phénomène de la parole devenue mot reflète, bien sûr, la controverse opposant écrivains acadianisants et écrivains francisants. Ce n'est plus aujourd'hui une controverse bien violente, comme si le français, tel le francien sur les autres dialectes médiévaux, l'avait emporté sur l'acadien, ou comme si l'on s'était accommodé d'un mélange franco-acadien tel qu'il a été étudié par notre collègue Pierre Gérin.[9]

L'emprise de la parole sur la création littéraire persiste avec plus de tenacité au niveau de la structuration, de l'expression, du style. Il y a onze ans déjà, Antonine Maillet affirma que seuls "les chroniqueurs, les composeux de complaintes, les défricheteux

de parenté, les conteux de contes et de légendes...savaient vraiment rebâtir le monde.... Les romanciers s'y perdraient."[10] En onze ans, rien n'y a changé pour Antonine Maillet, et très peu pour ses épigones. La parole de l'oralité, à moins d'être conjurée, risque d'étouffer le libre flot des mots romanesques.

Nous sommes tous, critiques littéraires et linguistes, envoûtés par la parole magique. A ce que j'observe, et je peux évidemment me tromper, la fleurissante linguistique acadienne est une linguistique de la parole. Avant que nous ne nous en rendions compte, nos études synchroniques de phonétique, de morphologie et de syntaxe appartiendront à la phonétique et à la morphosyntaxe historiques. Car les temps ont changé, et l'écrit s'affirme de plus en plus comme sujet d'étude et comme moyen de transmission du savoir collectif.

Pour réserver enfin à l'écrit et à l'écriture la place qui leur revient aux côtés de la parlure, comment, une fois pour toutes, exorciser la parole? En l'appelant à se manifester cette ultime fois, dans toute sa richesse et avec toutes ses contradictions, dans un texte! J'entends par là bien plus que l'inévitable transformation du parlé en écrit, j'entends par cet ultime exorcisme la littérarisation de la parole, la parole devenue matière, structure et sens du récit.

C'est, bien entendu, Antonine Maillet qui a résussi ce tour de force, dans un texte, entre autres, dont on ne parle pas beaucoup: *Cent Ans dans les bois*. C'est le récit, en effet, non pas de la Gribouille et de Jérôme le Menteux, ni du siècle du grand silence noir, ni même de la chasse au trésor de la race des LeBlanc ou des événements qui préparent la première Convention nationale des Acadiens, non, c'est le récit de la parole acadienne, de sa naissance, de sa vie, de son déclin.

Dans ce XIXe siècle d'Antonine Maillet, le personnage central de *Cent Ans dans les bois*, la parole vernaculaire, naît et assume pour la première fois la "transmutation du vécu" et dégage ainsi l'historicité propre de la collectivité, sur quoi se fonde sa puissance morale (Ollier:209). Quoi qu'en disent les linguistes et les stylicien(ne)s, la langue que (re)crée, chante et défend Antonine Maillet a la dignité essentielle et inexpugnable d'une langue maternelle, née d'une pulsion profonde, com-

munautaire. Elle se distingue de l'anglais (j'allais dire: du français) comme le francien se distingue du latin.

Collective, la parole impose à quiconque veut la manier un acte de (re)création collective. Même si Antonine Maillet en est le scribe, ses individualisations naissent au sein d'une expérience de la collectivité et sont soutenues, étendues et déployées par tous. Elles sont, avant même de faire l'objet d'une rédaction, essentiellement et nécessairement orales ou non "scribales," non seulement en raison du caractère *primaire* du genre épique, mais aussi parce que l'épopée est le lieu de convergence de divers codes sémiotiques où l'élément linguistique n'est pas le seul à signifier (Ollier:210), comme nous verrons tout de suite.

Lorsque "toute la maçoune s'empar(e) encore un coup de l'histoire et cont(e)...collectivement..." (*Cent Ans*: 29), Antonine Maillet est de la partie. Et lorsque Jérôme orne "chacun de ses récits de 'comme disait mon père,' 'au dire du vieux Mathias,' 'je le tiens de la défunte Agnès,' ou 'je m'en vas vous raconter un conte que m'a conté un conteux mort y a passé cent ans'" (*Cent Ans*:14), elle l'écoute attentivement afin d'imiter ensuite ces "dires de menteux" qui contiennent "assez de paroles entières ou fractionnées pour reconstituer le vitrail où se mire le monde" (*Cent Ans*:14). Pour la collectivité et grâce à elle, Antonine Maillet cogne aux portes, "extirpant de chaque gorge un mot à la fois" (*Cent Ans*:13-14), ou met "au monde à rebours /sa/ mère, /sa/ grand-mère, /sa/ grand-grand-mère et trisaïeule, /se/ remémorant le chemin parcouru, repérant les odeurs et les bruits, revivant au creux de leur ventre la vie de ceux qui /l'/ont mise au monde" (*Cent Ans*:11-12).

Création collective, donc, dans un acte de parole collectif. Car "la langue...est...l'affaire de ceux qui la parlent. Et c'est beaucoup l'affaire de ceux qui l'écrivent comme s'ils la parlaient."[11] Et en effet, l'épopée mailletienne s'inscrit largement dans l'oralité des "mots de gorge...et du gueuloir. Là sont les véritables tamis de la langue: les poumons, le gosier, le palais. Les mots qui ne passent pas le gorgoton ou s'accrochent à la luette, n'aboutiront jamais au lexique...un peuple qui durant des siècles a respiré l'air salin de la mer finit par avoir la voix rauque et grave...le geint des goélands dans le suroît, mêlé à

celui des lames qui s'écrasent sur les basses la nuit, finit par s'entortiller dans son oreille et par lui donner une voix traînante, coume j'contont" ("Mots de gorge": 11).

Comment la linguistique acadienne peut-elle ne pas tenir compte, lorsqu'elle décrit la réalité de l'acadien, de telles analyses poético-scientifiques? Je ne prêcherai pas la nécessité d'études psycholinguistiques et sociolinguistiques, j'appelle tout simplement à la collaboration plus étroite entre linguistes et littéraires!

Que faites-vous, par exemple, quand la parole se fait silence, geste ou regard? Antonine Maillet a bien compris et magistralement décrit cet aspect secret de la vie de la parole:

> La réponse de la Gribouille se fit attendre, au point de donner à Dâvit à Gabriel l'illusion qu'il avait eu le dernier mot, et à Marguerite à Loth la corde pour se pendre. Pauvre Marguerite...à tout coup /elle/ se pendait à cette corde-là. A tout coup elle s'enfargeait dans les silences des autres, en gros sabots, toutes voiles au vent, croyant faire avancer le temps en poussant dessus. Trop de zèle, Marguerite à Loth!... La Gribouille, bien entendu, ne faisait rien pour aplanir le sentier sous les sabots de la Marguerite. Même certains prétendent qu'elle y mettait de la mauvaise foi. (*Cent Ans*:17-18)

Et plus loin:

> Jérôme prend une profonde inspiration. Mais...les poumons sont engourdis et la respiration paresseuse. Et Jérôme...doit ajuster son souffle..., ajustement qui accorde à la Gribouille trois bonnes secondes d'avance.... Et voilà Jérôme-le-Menteux qui est forcé d'entendre tout au long et jusqu'au bout, sans remuer les jambes ni cligner des yeux, une histoire...qu'il gardait au fond de son sac pour les plus grands jours et qu'il avait cru, l'innocent, faire avaler à ceux-là même qui l'avaient vécue. (*Cent Ans*:19-20)

Ou encore:

> Une phrase trop longue donne à l'adversaire le temps de l'encaisser de flanc, puis de se retourner pour lancer sa réponse de face. La Gribouille sait d'ailleurs qu'en langue du pays, toute phrase plus longue que son souffle n'est qu'un échafaudage. (*Cent Ans*:273)

Et enfin:

> Toutes les têtes sortent du conte l'une après l'autre, laissant le conteur Bélonie ralentir ses phrases, freiner, puis semer dans l'air du temps trois ou quatre points de suspension, avant de baisser les yeux sur son auditoire qui déjà s'affaire et court aux quatre horizons. (*Pélagie*:77)

Voilà le fonctionnement subtil de l'oralité.

Les "mises en garde, les étapes, les péripéties, les clins d'oeil, les digressions, les commentaires" (*Cent Ans*:27-28), "la redondance...l'une des formes stylistiques les plus chères aux gens des côtes, avec la litote et l'hyperbole" (*Cent Ans*:190), tout passe "devant les douze chevaliers accroupis sur la galerie" de la Gribouille (*Cent Ans*:55). Créant spontanément, il n'est pas étonnant que le protagoniste-auteur, bien avant que l'écriture ne fixe et nivelle son récit, parfois "béga(ie) et s'embrouill(e), enfilant les verbes les uns aux autres sans sujets ni nominatifs...mélang(e) les circonstanciels de temps et de lieux, pren(ne) une causale pour une finale, et accroch(e) des propositions relatives à des complétives à des subordonnées, en parsemant son discours d'imparfaits du subjonctif" (*Cent Ans*:198; voir aussi 17-18)!

S'il n'y avait pas eu Antonine Maillet dans l'histoire de l'Acadie, sans doute aurait-il fallu que l'on l'inventât. En élevant la parole au rang de monument national, elle en a signifié à la fois la vie illustre et le caractère aujourd'hui muséal. Oralité et épopée ont cessé de structurer l'Acadie, elles ne lui donnent plus son sens. Dans l'Acadie post-mailletienne, elles ont été remplacées par les mots du roman. James de Finney l'a bien vu: "dans

le milieu, soumis et exploité, des gens de la Baie... l'épopée n'a pas sa place: 'le roman se caractérise comme l'histoire d'une recherche de valeurs authentiques...dans une société dégradée, dégradation qui...se manifeste principalement par la...réduction des valeurs authentiques...et leur disparition en tant que réalités manifestes.'"[12] Aux veillées collectives du monde sûr et authentique d'antan correspondent aujourd'hui les innombrables lectures individuelles et solitaires d'un monde incertain et contradictoire. Au fur et à mesure que les bouches se ferment, les yeux s'ouvrent pour découvrir et comprendre la nouvelle Acadie.

NOTES

[1](Paris: Seuil, 1983).

[2](Princeton: Princeton Univ. Press, 1983). Voir aussi Walter J. Ong, *Orality and Literacy* (London & New York: Methuen, 1982).

[3]*Tristes Tropiques* (Paris: Plon, 1955), pp. 343-44.

[4]Je paraphrase Marie-Louise Ollier (dans *Mosaic* 8,4 /1975/, p. 208) et J.-M. Paquette (dans *Etudes littéraires* 1 /1971/, p. 11).

[5](Montréal: Leméac, 1981).

[6](Montréal: Leméac, 1979).

[7]Laurier Melanson (Montréal: Leméac, 1981).

[8]"Le français du XVIe siècle dans l'Acadie d'aujourd'hui," XVIe Congrès Intern. de Ling. et Phil. Romanes (Majorque, 1980).

[9]Dans *Atlantic Literature Colloquium Papers* (St. John: Atlantic Canada Institute, 1977), pp. 99-109.

[10]*Ecrits du Canada français* 36 (1973), p. 23.

[11]Antonine Maillet, "Mots de gorge," *Acaditout* 1,5 (janvier 1977), p. 11.

[12]*Revue de l'Université de Moncton* 8,2 (mai 1975), pp. 37-46.

DES MOTS EMPRUNTÉS DU FRANÇAIS AU MALÉCITE

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RÉSUMÉ

Il y a des emprunts français et anglais dans la langue malécite des indiens au Nouveau-Brunswick. Les mots empruntés de l'anglais sont plus nombreaux, les emprunts français sont plus vieux.

L'auteur de cette communication a recueilli douze volumes de contes en malécite et il a publié le premier dictionnaire de cette langues. En y travaillant pendant 14 ans, il a eu l'occasion d'étudier les emprunts en malécite.

Dans cette communication les consonnes seront examinées d'abord: les occlusives, les fricatives, les nasales, les latérales, les vibrants, et quelques conbinaisons de plusieurs consonnes dans les emprunts du français. Ensuite suivront les voyelles, qui ne sont pas si nombreuses en malécite qu'en français. Finalement quelques mots seront examinés qui ont été empruntés du français via l'anglais ou via une autre langue indienne (probablement le micmaque). Il y a aussi des mots qui ont deux versions en malécite, dont l'une venue directement du français (p.e. mihsel 'Michel'), et l'autre via l'anglais (mihcel).

Le malécite est une langue indienne, qui est parlée au Nouveau-Brunswick et au Maine. Elle appartient à la famille de langues algonquienne.[1]

J'ai publié un article sur les mots empruntés de l'anglais au malécite.[2] Les mots empruntés de l'anglais sont plus nombreaux dans cette langue que les mots empruntés du français, parce que les Malécites parlent maintenent deux langues: le malécite et l'anglais. Mais les mots empruntés du français paraissent être plus vieux. Quoique ils ne soient pas si nombreux, leur importance est particulièrement grande linguistiquement. Ils sont devenus des parties "organiques" de la langue malécite, reçoivent des suffixes malécites, et leur structure phonologique est totalement d'accord avec la phonologie de la langue malécite.

1. Premièrement je parlerai des consonnes: les occlusives, les fricatives, les nasales, les latérales, les vibrantes, et quelques combinaisons de plusieurs consonnes.

2. Ensuite je parlerai des voyelles, et des changements des voyelles dans les mots empruntés.

3. Ensuite suivront quelques remarques sur les cas spèciaux.

Il y a des occlusives sonores et sourdes en français. En malécite, il n'y a pas de contraste phonémique entre p et b, t et d, k et g. Pour cette raison nous ne notons que trois occlusives simples: p, t, k. Mais ces trois occlusives peuvent suivre une occlusive glottale, que nous notons comme h. Nous devons noter les conbinaisons ht, hp, hk, parce qu'elles sont en contraste phonémique avec p, t, k.

Les occlusives sonores (b, d, g) des mots français doivent devenir logiquement p, t, k dans les mots empruntés. Je ne peux citer que des exemples avec les changements b>p et d>t:

kēpek 'Québec',
atle 'André',
pōhtay 'bouteille'.

Les occlusives sourdes des mots français (p, t, k) doivent être logiquement dans les mots empruntés p, t, k, ou hp, ht, hk. Nous avons des exemples pour les deux types d'emprunt. Nous avons cité le mot kēpek 'Québec', ou les deux consonnes k sont sans h; et nous avons vu le mot pōhtay 'bouteille', ou le t du mot français est devenu ht en malécite. Un changement pareil se produit avec le p et avec le t dans le nom français Laporte, il devient lāhpolht en malécite.

Les fricatives sont beaucoup plus nombreuses en français qu'en malécite. En malécite il n'y a qu'une fricative alvéolaire (s), qui ne peut pas être dans un contraste phonémique avec une fricative alvéolaire sonore, ou avec un fricative post-alvéolaire, sonore ou sourde (ǯ dans Jean, š dans Michel en français). Mais il y a un contraste s - hs en malécite. Ainsi, nous pouvont nous attendre à un s ou hs en malécite, quand le mot français avait une des consonnes s, š ou ž. Par exemple:

sēsōhs 'Jésus' (il y a un suffixe diminutif - hs au bout de ce nom en malécite);

soliyan 'Julien';
malhsan 'marchand';
sahk 'Jacques'.

In n'y a pas de consonnes labio-dentales (f et v) en malécite. La fricative labio-dentale v des mots français devient une occlusive bilabiale (p) en malécite, par exemple:

lāhkap '(la) cave'.

Il n'y a pas d'affriquées en français moderne. Il n'y a qu'une seule affriquée (c) en malécite, et une autre avec h (hc), mais les consonnes c et hc ne se trouvent jamais dans les mots empruntés du français au malécite.

La latérale (l) et les deux consonnes nasales (m et n) ne changent pas quand les mots qui les contiennent sont empruntés. Par exemple:

piyel 'Pierre';
monīhk 'Monique'.

Il n'y a aucune vibrante en malécite. Elle devient une latérale (l) dans les mots empruntés. Par exemple:

atle 'André';
lāhplison '(la) prison';
malhsan-ikwam 'magasin', 'maison du marchand'.

Le système des voyelles est très simple en malécite. Il n'y a que cinq voyelles brèves: i, e, a, o, ɔ, et quatre de ces voyelles peuvent être longues: ī, ē, ā, ō.

Alors, nous pouvons nous attendre à ce que les i des mots français soient i en malécite aussi. Par exemple:

māli 'Marie';
maliyolhsol 'Marie Ursule';
sīmo 'Simon'.

En malécite, il n'y a pas d'é fermée et è ouverte. Pour toutes les deux, la langue malécite aura un e. Mais cette voyelle peut être brève ou longue. De quoi dépend la longueur de cette voyelle dans les mots empruntés? Il est impossible de trouver assez de mots empruntés pour répondre à cette question.

Les exemples suivants permettent de supposer qu'un é fermé en français deviendra un ē longue en malécite:

kēpek 'Québec':
sēsōhs 'Jésus'.

Mais:

nōwelhsis 'Petit Noël'.

Il n'y a que deux voyelles antérieurs (i et e ou ī et ē) en malécite. Qu'est-ce qui se passe quand il y a une voyelle ö ou ü dans le mot français? Ces deux voyelles doivent être remplacées par quelque chose qui existe dans la langue malécite. Dans l'exemple suivant la voyelle ö du mot français adieu est remplacée par la semivoyelle w en combinaison aven un i (iw):

atiw 'adieu'.

Il n'y a que deux voyelles vélaires en malécite: a et o, et toutes les deux peuvent être brèves (a, o) ou longues (ā, ō). Alors, la voyelle a d'un mot français sera a en malécite aussi, par exemple:

atlēhsis 'le petit André'.

En français il y a une différence entre le ó fermé (de sot) et le ò ouvert (de fort). En malécite il n'y a qu'un seul o et il n'y a aucun contraste phonémique entre o et u (de tout). Tout cela signifie que les trois voyelles vélaires du français (ó, ò et u) deviendront soit une voyelle vélaire brève o, ou une voyelle vélaire longue ō. Par exemple:

pōhtay 'bouteille';
pōhtayāhsis 'une petite bouteille';
pōhtsyawhsəmən 'il le conserve';
sosēhp 'Joseph'.

Dans la seconde syllable du mot pōhtay 'bouteille' (pōhtayāhsis 'petite bouteille'), une voyelle entérieur du mot français est devenue une voyelle vélaire en malécite. Pourquoi? On ne peut pas trouver assez d'exemples pareils, pour expliquer ceci suffisamment.

On peut trouver un "désordre" pareil dans quelques autres mots si une voyelle apparaît en combinaison avec une semi-voyelle. Par exemple:

ãhtwen (ou ãhtwin) 'Antoine'.

Dans le mot moliyan 'Montréal', on pourrait s'attendre à un l aussi au bout du mot, mais le mot malécite se termine avec un n. Je pense que la raison pour cette "irrégularité" est ceci: les Indiens malécites n'ont pas appris le nom de Montréal directement des peuples qui parlaient français, mais le mot est venu du français premièrement à une autre langue algonquienne, et de cette langue à la langue malécite. Il y a des langues algonquiennes ou un n correspond au l du malécite. Dans ces langues il n'y a pas de l. Mais j'ai trouvé ce mot aussi dans la forme moliyal. La forme moliyal est venue directement du français, et la forme moliyan, peut-être, d'une autre langue indienne.

Il n'y a pas de voyelles nasales en malécite. Les voyelles nasales des mots français sont remplacées par des voyelles orales. Nous avons vu des exemples dans les mots pour 'Montréal', 'Antoine', 'Simon', 'Julien', 'marchand', 'André', etc. Voici encore un exemple:

ɔpan 'pain'.

Dans le mot malécite ɔpan, 'pain', nous voyons une voyelle additionelle (ɔ), qui n'est pas là dans le mot français.

Il y a des mots en malécite qui sont originalement des mots français, mais qui sont empruntés par les malécites de l'anglais ou du micmaque. Par exemple, le mot wenõhc signifie 'un homme blanc' (français ou anglais ou autre). Originalement, c'est le même mot que français, mais il était emprunté premièrement par les anglais, ou il est devenu French. Autrement on ne pourrait pas expliquer l'affriquée dans le mot malécite et le manque d'une voyelle après cette affriquée. Le nom pilick 'Kingsclear, French Village' est un autre nom, qui est venu originalement du français (village), mais qui était emprunté directement de l'anglais, du nom anglais village. On peut expliquer l'affriquée malécite c (prononcée č) de l'affriquée sonore anglaise ǰ, qui est devenue sourde en malécite. Le k au bout du mot pilick est un suffixe en malécite.

Il y a un autre mot dans mon corpus, qui est originalement français, mais qui était, probablement, emprunté de l'anglais: šārləht 'Charlotte'. Pourquoi pas directement du français? Parce qu'il y a deux consonnes dans ce mot: š et r, qui n'existent en malécite que dans les mots empruntés les plus récents. Et les mots empruntés les plus récents sont venus de l'anglais.

Un autre mot emprunté du français via l'anglais est pəlecmən 'un français', pəlecmənis wkwēhs 'une française'. La première partie du mot (pəlec-) est venue du mot anglais French, qui était originalement français. C'est étrange que le même mot ait été emprunté une fois comme wenōhc-, une fois comme pəlec-. Peut-être que le mot wenōhc- était emprunté via le micmaque.

Il y a des mots où il est difficile de dire de laquelle des deux langues ils étaient empruntés. Par exemple malhtan 'Martin', (h)pal 'Paul', tepəsən 'David', etc.

Il y a des prénoms en malécite, qui ont deux versions, une empruntée du français, l'autre de l'anglais, par exemple mīhcel et mihsel pour 'Michel', cōsəhp et sosēhp pour 'Joseph'. Les versions mihsel et sosēhp ont été empruntées du français parce qu'elles ont les fricatives, comme les mots français, pas d'affriquées, comme les mots anglais.

Pour finir, je voudrais, presenter deux phrases malécites avec des mots empruntés du français, pour montrer comment ces mots sont devenus vraiment des parties de la langue malécite. Les mots soklə pan 'gâteau' ('sucre pain') et pōhtay 'bouteille' sont des noms animés en malécite, et ils se condiusent comme de vraies mots malécites. Voici les phrases:

nakā tətləsāwal təkē iyil soklə pānəl 'Et maintenant ils ont coupé ce gâteau';

kisīhton tēhsakwtihīkə nəl / élawtat yohōht iyi / pōhtāya 'Il fit des étagères où il mit ces bouteilles'.

FOOTNOTES

[1] Je voudrais remercier Prof. Anthony Lister des corrections qu'il a faites dans le texte français.

[2] László Szabó, "English Loanwords in Malecite," American Speech, 1974: 235-40.