

SYMPOSIUM NO. 4: SOCIAL FACTORS IN SOUND CHANGE

(see vol. II, p. 185-237)

Moderator: Einar Haugen

Panelists: Henrik Birnbaum, Ivan Fónagy, William Labov, Jørn Lund,
Bertil Malmberg, and Fred C.C. Peng

Chairperson: Martin Kloster-Jensen

EINAR HAUGEN'S INTRODUCTION

1. The Contributors and their Papers. Each of the invited speakers in this symposium has done research and thought deeply about the topic of linguistic change. They range from newcomers like Lars Brink and Jørn Lund to elder statesmen like Bertil Malmberg. It is one of the prime purposes of such congresses as this to bring together representatives of different views, different ages, and different countries, so that their ideas may be discussed face to face. Unfortunately, each contributor is limited by the format of the occasion to a short presentation in print of the main results of his research and an even shorter presentation by word of mouth. My function as moderator has been the pleasant one of summing them up and showing how together they constitute an advance toward our understanding of the central problem that is the topic of this symposium. One difficulty is that the authors deal with many situations that I do not know firsthand, and that they take up different aspects of the problem itself. In some cases I have had to go back to other work by the same and other authors to clarify the problem in my own mind.

2. Theorizers and Empiricists. The contributors fall into two categories, which I shall call "theorizers" and "empiricists". The "theorizers" are those who base their discussion largely on informal observation from which they make more or less intuitive generalizations. This is not a pejorative description, for in this field I count also myself. I would count among them Birnbaum, Fónagy, and Malmberg. The others are "empiricists" because they present actual field work, much of which has been statistically treated, so that their conclusions give the refreshing impression that we may be able to treat an old problem in a new way, namely by direct observation. I find this approach most exciting, since it builds on forms of data gathering that have become possible once we had tape recorders, computers, and spectrographs. Phonetic

change used to be considered as something we could observe only over centuries. We are now told that we can catch it on the wing. Instead of observing its results only, we can now see it going on. This development appears especially in the papers of Brink and Lund, Labov, and Peng. It has made possible an empirical sociolinguistics, of which earlier investigators could only dream.

3. The topics. I shall first present a very brief statement of the contents of each paper, beginning with the theorists. Birnbaum is largely concerned with criticizing a linguistic model of decoding advanced by Henning Andersen under the name of "abductive". He does not believe that it can account for the rise of innovations in a homogeneous speech community, a construct which in any case he rejects. Fónagy is here concerned primarily with intonation and its historical development. He rejects all notions that it is a "universal" or that it is a fixed, non-arbitrary and motivated phenomenon. Malmberg sees a "state of language" as "a harmonious achronic system or rather complex of systems" within which the speaker may choose according to situation. His chief example, which he has previously studied in detail, is the Parisian vowel system, or rather its "maximum" and "minimum" systems. He regards the rise of "minimum" systems as the result of a "simplification" that is typical of persons living on the social and spatial periphery of a society. Brink/Lund (as I shall call them jointly) have gathered a vast amount of data on the phonetics of Copenhagen speakers born between 1840 and 1955, fully presented in their massive two-volume *Dansk Rigsmål* (Copenhagen, 1975), unfortunately available only in Danish. Basing themselves primarily on phonograph recordings going as far back as 1913 as well as whatever printed materials are available, they have identified up to sixty regular phonetic changes. They have divided their speakers into two social groups, speakers of "high" and "low" Copenhagen. Labov's work has dealt with a variety of American groups, beginning in the island of Martha's Vineyard in Massachusetts, continuing on New York's lower East Side, and currently in Philadelphia. He has concentrated on Black youth, but has worked with all colors and social classes. Finally, Peng bases himself on extensive data gathering in Tsuruoka, Japan, by his colleague Nomoto. This was a sample first drawn in 1950 and then reexamined in 1971. The novelty in his theory is that one generation is sufficient to

identify the process of sound change. Labov's period is in some sense even shorter, since he studies different age groups synchronically and assumes that young people will carry their innovations on into adulthood. We are fortunate in having a wide variety of data bases, from three continents, as well as considerable variety in theoretical approaches.

4. Stability and Change. Except in immigrant communities, every community studied so far has enough stability of language so that each generation can communicate with every other. At the same time language is known to be changing at a rate such that after some unspecified number of generations it will become unintelligible to its ancestors. These basic facts determine the possibility of two complementary views: that language is stable and can form the object of synchronic study, and that language is constantly changing so that it can form the object of diachronic study. In their extreme form both views become unrealistic, e.g. in assuming complete homogeneity or complete fluidity. Members of the Prague School (e.g. Havránek, see Garvin 1959) described "elastic stability" as desirable in a standard language, but in fact they were only defining the nature of all language, "standard" or not. Labov has invented the latest synonym for this term in his "orderly heterogeneity", which is as much a construct as Chomsky's "ideal homogeneity" to which he opposes it. Both agree that language is "structured", i.e. amenable to description by rules. Chomsky's are categorial, Labov's variable, but there is structure in both. The step from categorial to variable rules is a great step forward in descriptive linguistics, but it was foreseen in historical linguistics, and especially in dialect geography.

Here it is useful to emphasize the concept of "choice" as used in Malmberg's paper. Variable or conflicting rules mean that individuals have the freedom to change language within wider or narrower limits of acceptability. But none of these rules are very helpful so far in predicting the future. Any attempt to predict sound change has to face the problem of showing why people make decisions as they do. But this involves going back into their individual and collective psyches to study their unconscious motivations, an infinite regression that leads us far outside the realm of most linguists' competence, though some have loved to

speculate about it. A careful study of the tiny rule changes in Copenhagen speech pinpointed by Brink/Lund suggests that at any given moment in time there is an enormous amount of unstructured heterogeneity, of vacillation and uncertainty. This may either continue, or be resolved by a later generation, and it may lead either to innovation or to regression.

5. The Problem of Actuation. It is hardly surprising that living language abounds in heterogeneity. It is more surprising that there is no more of it than there is. The basic reason for heterogeneity has been evident ever since men stopped believing in such myths as the Tower of Babel. Recent linguists have re-discovered the fact that language is innate and universal, but the most universal fact about languages in the plural and concrete is that every one of them has to be learned anew by every human being born on this planet. He or she is born to human parents and in a human society, surrounded by the speech output around it. That output becomes the input to the child's own processing of the language for reception and eventually production. The study of the child's language learning (which for some arcane reason has come to be known as "acquisition" -- perhaps it is part of our acquisitive civilization) has become an important field of research. We may look to its results for new light on the extent to which the fully formed child's language differs from that of its environment. We do know that eventually all non-defective children learn to communicate in whatever language variety is spoken around them, in spite of the inevitable differences among individuals in talent, appearance, industry, and success. But human beings are not robots and no given language is imprinted by instinct. Try as they will, people will deviate. Call their deviation a "speech error" or a "creative innovation", as you will; it is the germ of a language change.

6. The Mechanism of Diffusion. Given the fact that more or less random innovations occur, we need to pinpoint the process by which they are spread to other speakers. If they fail to spread, they remain speech errors; if they do spread, they become linguistic changes. On this point our symposium speakers show a clear difference. Brink and Lund appear to believe that the innovations are made in childhood and are then retained for life, unless of course the speaker moves into a new linguistic environ-

ment. Their basis for this claim is the recordings they have studied of the same speakers at various periods of their lives. It must be noted, however, that age 15 was the lowest they studied, which is already after the onset of puberty. Many studies have shown, whatever the cause of it may be, that puberty is a period when language tends to fix itself into an adult pattern that most people find difficult to change. Birnbaum emphasizes the importance of the teens as "the age when growing-up speakers, by imitating their elders, attain the same or nearly same pronunciation as their models." He regards such changes as frequently deliberate, and due to fashion within the generation. At the same time he rejects the simple transfer of one generation to another, since there is a "continuous pattern-setting effect of parents on children, teachers on students, leaders on followers, older on younger playmates and fellow workers, more prestigious on less prestigious..."

Against this view Peng entirely rejects the idea that change takes place across generations. He specifically denies Johnson's (1976) view of an accelerating change over three generations. He has found that Nomoto's speakers showed many changes over a period of 21 years. He suggests that while the rate of change may go down as age goes up and reaches a low point around age 35, it never completely stops. He questions Labov's use of "apparent" time studied in synchronically present generations and advocates the use of "real time". Presumably Labov would agree that this is desirable when the investigator lived long enough, or when his informants do, for he (Labov) refers to Hermann's restudy of Gauchat's famous village of Charmey in Switzerland. Peng suggests as an alternative the use of dialect geographical material, with its mapping of horizontal linguistic change. This, too, is a case of apparent time, however, since the dialects exist synchronically, and we can deduce just how or even approximately when the change took place only by the use of comparative-reconstructive methods.

7. Class Correlations. Our speakers also show certain differences of opinion concerning the role played by social and other classes in the actuation of change. Labov has found that in American cities the upper working or lower middle class, that is, the centrally located classes, lead in linguistic change. The speakers who are most advanced are the ones with the highest aspirations for advancement, who also have the largest number of

local contacts outside the community. Malmberg has fixed his view on the central norm of Parisian French and regards simplification as a major factor, which he then attributes to the lower classes and the provincials, who live on the periphery. In Brink/Lund's detailed account of their three-score changes in Copenhagen, however, the role of social class is rather different. To begin with, they deny that there were what we would call class differences prior to 1750. Before that time the speech of Copenhagen was a local dialect like any other, different from its neighbors, but having much in common with them. In the 18th and 19th centuries a class differentiation took place which reduced contact between different strata of society. A distinct lower-class speech developed, which in general was ahead of upper-class speech. Only since 1900, when everyone is sending their children to publicly supported common schools, are the differences leveling out, or in the view of the élite, the language is being "vulgarized". Unfortunately, it is difficult to compare Brink/Lund's results directly with Labov's, since they operate with only two classes as against Labov's more refined indices of class membership.

On one point everyone seems to be agreed: that women everywhere are more "refined" than men of the same age or class, i.e. have more features classified as "high". Brink/Lund are not willing to grant the existence of a separate "sexolect", but suggest that women are more sensitive (perhaps rather "sensitized") to social status. Fónagy finds that in Hungarian a final rising intonation has lost its marked value as an indicator of "expressiveness". The reason is that it has now become normal among women and young people.

8. Conclusions. Two of our speakers emphasize that it is not language that changes, but people who change language. Peng writes, "People change, and sound change is simply a manifestation (or symptom) of human change." Malmberg reiterates from his Bucharest paper (1969) that "language does not change; man changes languages." These statements are true, but tautological, unless we are speaking of the adoption of new words or the learning of new languages. Phonology tends to fall below the threshold of consciousness for most speakers, and they are rarely aware of making changes in their own speech. It is only with the greatest caution that we can identify any external social reason for such

unconscious change. Nothing in climate, occupation, physiology, character, or history can be causally connected with such large-scale linguistic changes as the Germanic consonant shift, or Umlaut, or the English vowel shift, or even with the decay of inflections in most Romance and Germanic languages.

Brink/Lund even deny that the Copenhagen forms have spread because of the prestige of the capital city. But their claim that they spread "purely by contagion" makes one wonder why they did not spread the other way, during a period when the city was invaded by great numbers of rural immigrants. They believe that new pronunciations spread by virtue of an "inherent plus value", vaguely defined as their being "easier to articulate", and conclude that "sound change is essentially a non-social phenomenon." William Labov, who has done more to correlate social and linguistic variation than anyone else, is equally pessimistic: Bloomfield's assertion of 55 years ago that "the causes of sound change are unknown" is still true.

In spite of the weight of first-hand research and authority which these writers bring to the topic, I cannot let this conclusion stand as the final word of the symposium. I am convinced that the causes are known, but that what is really meant is that the results are unpredictable. Let me briefly sum up my own unsupported and intuitive view of sound change (though it is not unlike that held by Hugo Schuchardt and Otto Jespersen). Sound change is in principle no different from any other change going on in the lives of animate beings everywhere around us. To say that we do not know the causes of change is like saying that we do not know the causes of human fashions, e.g. the length of women's skirts or the shape of men's headgear. We do know that one main cause of human language change is that language is not genetic, but learned, and that no two human beings ever learn anything exactly alike. I do not believe that the parts of any language hang together in Meillet's sense of "tout se tient". If they did, there would neither be sound change nor the development of dialects. I believe instead in what I may call the "amoeba" theory of language, that any aggregation of items we call a "language" or "dialect" is as arbitrary as the movements and splittings of the amoeba. The most important rules of language are simple collocations. Phonetic changes can only have been "actuated" by

individual learners and users, whether as children or adults, who committed errors in hearing or reproduction that were not corrected by themselves or others. Phoneticians can tell us a great deal about the physical and acoustic parameters that favor such errors, but they cannot predict which of them will occur.

To become part of the speech of others, these innovations have to be acceptable to other members of the community. This is the process of diffusion, which has to be both lexical and social. Lexically, the change has to spread from the one item in which it started to other items that in some way are felt to be similar to the first. The neogrammarians' or any other linguistic formulation of such changes or "rules" as they are now called is an ex-post-facto summary of change, not a description of the change itself. As dialect geography clearly shows, a change may stop at any point in its diffusion, before it has spread to the entire lexicon or the entire community. It may even change its domain, be reordered or reorganized, apply to different parts of the system, be lexicalized or grammaticized. "Simplification", which is often resorted to as an explanation, is no real answer, for neighboring dialects fail to simplify in the same way. According to Chen and Wang (1975:267), the final nasal consonant /m/ has been lost in Mandarin, but in Cantonese it is still there. Who could have predicted that? It is vocalized in French, but in English we still have it. A tendency, yes; a universal, no. Besides, in spite of all simplification, every language known seems to be of about equal difficulty, learned at much the same age by children who are exposed to it.

There are too many factors present in every human situation for us to be able to foresee all its possibilities. No sooner has one rule operated for a time than another takes over and messes it up. Such is life, and language is no different.

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COMMENTS FROM THE PANELISTS

Birnbaum did not intend his paper as a major critique of Henning Andersen's abductive model of phonological innovation, for which he has great admiration. He only wished to indicate that it could be improved on some minor points, e.g. the problem of generational sequence. He was concerned with any trend toward excessive schematism. As for being classified as a "theorizer", he wanted to make it clear that he believed in a happy combination of data gathering and theorizing. He agreed that early childhood was the most important period for establishing speech habits, but that puberty also led to readjustments.

Fónagy was stimulated to study French accent after being rebuked for having an 18th century pronunciation on his arrival in France thirty years ago: he made it a habit to place every stress on the last syllable! He has found that French stress is elusive: its placing is a probabilistic function of many variables, including syntax, genre, etc. Today radio and television speakers are increasingly stressing enclitics, which are not stressed in conversational speech.

Labov described his paper as the first report on his Philadelphia study, his largest project so far, using more advanced techniques than his earlier studies. He has adopted the strategy of searching for innovators: where are they in the social spectrum, by sex, class, position etc. How is sound change related to the network of communications and to new ethnic groups that enter society? Can we throw light on change by looking at the people who are doing it? He does not think that the individual is a significant unit: we are dealing with the social pressures which form an individual into a social being as he grows up and assumes

a variety of roles in the social structure. His main motivation in coming to the meeting was to make contact with acoustic phoneticians and the theoreticians who have developed the models we use: Fant, Fujimura, etc. "Ever since 1968 we've made the point that the tools of acoustic phonetics are useful for examining problems of language structure and language change." These tools will require increasing understanding of the mathematical models at the base. A report on the Philadelphia study should be available in three or four months.

Lund, on behalf of himself and Brink, spoke about their findings in the study of Copenhagen pronunciation. They found that "the sound pattern of the single individual will not change significantly after the teenage years unless the linguistic environment is changed rather profoundly." In the book they had taken the position that sound change takes place across generation boundaries, but they did not deny Peng's contention that sound changes in progress can be studied within one generation. But in this case there is often situational variation, with old forms in more formal speech, new forms in more casual speech. Here Malmberg's distinction of maximum and minimum may be applicable, though they found the term "minimum system" problematic. In casual speech there are not only the typical reductions and assimilations, but also subconscious new sound qualities that do not necessarily lead to simplification. Nor can they see anything here in common with aphasic speech or the reduced inventory of phonemes often characteristic of foreigners. They agree with Fónagy that changes in prosody "must be accounted for in the description of linguistic evolution." They question Labov's finding that the most advanced speakers "are those with the highest status in their local community, having found that new pronunciations have low prestige and are often considered vulgar, if noticed at all." They agree with Haugen that most changes are unconscious and that their investigation is difficult to compare with Labov's, since they started from the phonetic variation, and only secondarily examined the social correlation. "No Danish pronunciations are characteristic of the middle classes."

Malmberg noted that his paper "starts from my distinction between a language as a closed, hierarchical system of mutually dependent units, a structure *sui generis*, in our case the phonemic

system, where any change in the number and/or the relations of these units implies the creation of a new language richer or poorer or differently structured." Further, "a state of language..." (a Saussurean term) "is a sociolinguistic concept which for its full definition needs extra-linguistic parameters." "Every system or subsystem ... can function as one of the layers within a state of language." "The degree of mastery and retention of the complexity... is a question of the strength of the social norms which determine the speakers' behavior. The terms 'maximum' and 'minimum' systems must be understood as abstractions." By "simplification" he referred to phenomena occurring in the social and geographical periphery of normative centers and areas in contact with other systems on the linguistic border, including the diffusion of languages to new areas through colonization. He did not have in mind peripheric local dialects, which can be very conservative. "My principal point is the existence of layers of varying complexity and of norms of varying strength and the (socially determined) choice between different possibilities." "My intentionally provocative formulation at the Bucharest Congress in 1967 was made to stress the importance of the choice factor and that of social evaluation in phonetic/phonemic change."

Peng called attention to the two basic assumptions in his paper: (1) That language change is a change in behavior. Only by studying changes in language behavior can we discover changes in the code. Once this step is taken, one can observe changes within a single generation, without waiting for two or more demographic generations. (2) A random sample is more representative of human behavior than one that is previously stratified for class. In his work in Tsuruoka the same questionnaire was administered to 137 informants chosen at random and interviewed 21 years apart. In this way it was possible to make use of real rather than apparent time. In plotting the changes over time, one gets a straight line, showing that all age groups were affected.

Labov agreed that people tend to preserve their vernacular and gave the example of a mother and a daughter who differed widely in the pronunciation of the /aw/ diphthong. But he granted that people change their norms and only now realized that Peng had been studying the formal responses to norms and not the vernacular. He himself was looking for un-

reflecting speech, "the most systematic motor-controlled speech." No one has studied syntactic change, which may indeed be individual (cf. study by René Agneau of the progressive in 19th century English, showing that e.g. George Eliot made increasing use of the progressive in the course of a half century.) He expressed admiration for Peng's use of real time, but in his own work he preferred to begin with people in the context of their local community. He agreed with Lund that whenever changes rise to the level of consciousness, speakers tend to reject them.

Birnbaum commented on the moderator's summary. He gave an example of women's speech as different from men's: women tend to use an implosive /h/ in a word like jaha. He agreed that prediction is dangerous, and gave an example from Polish, the replacement of nasality in final vowels by diphthongization. Also that we can ascertain the causes of change, but that we cannot always explain them. He found the summary to be an important paper, by virtue of the moderator's including views of his own, perhaps unduly pessimistic.

Haugen as moderator responded modestly that he found the non-systematic parts of language more interesting than the systematic ones, whose existence he had never denied. He found that only by assuming an arbitrary disjunction between the parts of a system could one explain that they could change independently. One example is the well-known fact that an adult learner can speak a language fluently and with virtually perfect syntax and lexicon without ever mastering the phonetic system.

Peng noted that he had speculated on the causes of change and found many factors and mechanisms. He did not feel that the generation boundaries were primary, but the fact that speakers pass on a different language from the one they themselves learned. Diffusion of the code and diffusion of the people who accept it are two concurrent dimensions of diffusion. He challenged Lund to explain how he arrived at his conclusion of non-change on the part of individuals.

Fónagy mentioned retrospective studies of linguistic change in the 16th-18th centuries. They show that there are enormous differences between sound change and sound change. Some changes are dependent on sex (one reason given for a difference in women's speech at that time was that it was not good form for them to

open their mouths too wide), others are not. Some changes are socially dependent, some are word class dependent, others are not.

Lund replied that they had made spot checks of the same person recorded in the same speech situation many years later.

DISCUSSION

Simone Elbaz (Paris): "Mon intervention n'est en rien polémique. C'est une mise au point. J'ai le plus grand respect pour tous les grands noms cités, mais je m'étonne de l'absence totale de référence aux travaux d'André Martinet depuis le début de ce Congrès, et même dans l'aperçu de M. Rigault hier, qui cite Jakobson, Saussure, Chomsky en oubliant que la description d'Hauteville (1956) a servi d'exemple à bien des travaux ultérieurs.

Je veux rappeler que Martinet a été l'un des premiers à reconnaître et à étudier les changements linguistiques (cf. Economie des changements phonétiques, 1955); il a toujours dit: "Une langue change parce qu'elle fonctionne".

Récemment, il a cultivé et circonscrit la notion de synchronie dynamique qui, différente de la diachronie conçue comme l'étude et la comparaison de deux états de langue et de la synchronie conçue comme constat d'un état de langue, englobe non seulement l'analyse des variantes dans ce même état de langue, mais encore les prédictions de son évolution.

Cette notion de synchronie dynamique me semble intéressante dans le cadre des discussions de ce matière, c'est pourquoi j'ai voulu la présenter. (cf. Evolution des langues et reconstruction, Paris, PUF, 1975)."

Tore Janson (Stockholm): "Language is not only spoken; it is also heard, and the expectations of the hearer must also be changed. So it is important and possible to study the reactions of the hearers, e.g. in experiments with synthetic stimuli. I have done some experiments and would like to get in touch with people working in this area. The results so far are very interesting."

Lars Brink (Copenhagen): "We have tried to show that the forms of a capital city can be spread purely by contagion, according to what we call 'the Napoleon principle': "The enemy is beaten where he is weakest and is immediately enrolled in the

victor's troops." Of course prestige plays a significant role, but not in spreading new pronunciations. The innovations were never felt to be prestigious. Some innovators may be so, but not their followers, and the innovations would therefore drown in traditional forms.

Henning Andersen (Copenhagen): He called for greater precision in the expression of ideas. He did not think Brink said exactly what he meant when he said that a capital like Copenhagen could spread its forms to the countryside. You do not spread changes. It is the people who change their language to conform to the norms as they perceive them in the capital. He then entered a plea against Haugen's view of language as non-systematic or at least finding the non-systematic parts as more interesting. "We won't understand how more or less stray variation that goes on in speech production at all times may become codified and integrated into a system unless we study it in relation to the systems (or the code) that underlie speech production. Labov's study shows that even minute changes are accessible to some degree of subconscious awareness and confirms that what happens when variations turn into a kind of drift is precisely that what could be stray variation becomes a sort of fashion (and here I subscribe to Haugen's view) and is integrated. If we want to explain how changes can be integrated into one system, but not into another, or how changes can occur in one language but not in another, we need to refer to the systems that the stray variations can be integrated into." He then cited Roman Jakobson's opening statement to the Congress, read by Rischel, to the effect that "there is no gross sound matter in language: everything is formed", etc.

Irmgard Mahnken (Saarbrücken): "The question has been raised of how changes can arise in a homogeneous speech community. There are languages which have not changed for a very long time, and others that have been changing and then have stabilized themselves. At least theoretically we need a model of non-change as well as one of change, especially in the development of literary languages. Very little work is being done on the latter, since the social aspects now being investigated are based on living languages. The question of prestige and of social expression can explain many things now under discussion."

Helmut Lüdtke (Kiel): Sound change is predictable. The question is: how and how far? For example, if we knew Latin but no Romance language and wished to predict in what way a Latin word like clave might change in 2000 years, we could choose from the forms written on the left-hand side of the blackboard. Lüdtke suggested that a limited number of possibilities existed, and one would not choose something like akulavic or que. Sound change moves in an irreversible direction, toward shortening. Lüdtke has a theory which he may explain at the next congress. Sound change is reduction: the allegro forms of today are the lento forms of tomorrow.

Eli Fischer-Jørgensen: "I started changing my language when I was fifty and have continued until now. I spoke a conservative form of standard Danish when I was young, and now I find myself using a pronunciation which is approaching what I consider 'vulgar' Danish. This has happened unconsciously and against my will (but the change appears quite clearly from tape recordings). This is quite contrary to some of the ideas presented here." (J. Lund later commented that this might be due to her having a higher linguistic consciousness than most others.)

Richard Coates (Sussex): One often gets the impression that sound change is either community-internal or due to some catastrophic eruption into the community. Coates wished to point out a third mode which has occurred in the literature recently: a new norm external to the community has been integrated into the linguistic system by the adoption of personas by young children. This is exemplified in the work done by Reed in Edinburgh and recently published in the Trudgill volume of readings. Children who were well grounded in the local dialect were able to adopt pronunciation personas taken from TV personalities, disc jockeys, etc. A well-known boxing commentator's mode of presentation was adopted to describe playground fights by particular children. Here is a new norm, a new vector not due to ordinary situational interaction. It is potentially usable independently of the originally appropriate situation. More than one norm is being sanctioned within the system, highlighting once again the dynamic synchrony which has often been mentioned as a feature of these discussions.

Gilbert Puech (Oullins): [In the absence of a written text, the speaker's French is translated into English.] Puech noted

that changes had here been presented as due to social and geographic stratification across a linguistic community. This view should be complemented by studying the need of a social group for a marker of its identity, a change which concerns the weakest point in its system. Therefore he posed this question to Professor Labov: For Philadelphia modifications have been pointed out as due to the lower middle class. Does this correspond to the emergence of this group as a social category which needs to emphasize its identity more strongly by initiating or accelerating linguistic changes? Is it an active or a passive behavior, a consequence of the existing division?

Pierre Léon (Toronto): "(1) Au sujet de la durée des changements -- question posée par Haugen -- certains changements peuvent être très rapides (cf. Léon: L'accent en tant que métaphore sociolinguistique, French Review, 1974). Les ruraux prolétarisés d'un village du centre de la France ont adopté certains traits de prononciation urbaine (parisienne) et prolétaire (ouvriers de la banlieue parisienne), en moins de 10 ans. (2) Ce changement est ce que Léon appelle le résultat d'une conduite idéologique. La nouvelle articulation des ouvriers du village est ce que Birnbaum nomme ici 'a conceptualized (verbalized) mirror image of mental activity' et Fónagy un processus 'métaphorique'. Faudrait-il dire métonymique? (3) Au sujet de savoir qui est responsable de la variation -- question posée par Haugen, Brink, Lund et Labov, Léon donne des exemples des facteurs de la variation dans son village: jeunes, adultes, hommes, prolétarisés. Dans une enquête sur la standardisation des prononciations dialectales de la France (Léon et Léon, à paraître dans les Actes des Congrès de Miami), les facteurs de la variation se groupent en 2 séries oppositives:

standardisation +	{	jeunes ≠ vieux	}	+ statu quo dialectal
		citadins ≠ ruraux		
		mobiles ≠ sédentaires		
		favorisés ≠ défavorisés		

Tous les facteurs n'ont pas le même poids. (4) Le concept de l'hétérogénéité ordonnée de Labov se retrouve dans les exemples données par Fónagy et se confirment dans les résultats de l'enquête de P. Léon et M. Léon, qui montrent, à côté de la disparition des

systèmes de marques dialectales, une diversification au niveau des types de discours. (5) Le concept de sociolinguistique, tel qu'il est employé actuellement n'est-il pas trop restreint aux phénomènes d'indexation des classes sociales, éventuellement aux catégories sexe et âge? Ne faudrait-il pas tenir compte des marqueurs professionnels (Fónagy) et stylistiques dans une approche phono-stylistique plus large (Léon 1971, Essais de Phonostylistique, Didier) tenant compte des facteurs expressifs des situations de communication?"

Anatoly Liberman (Minnesota): On the predictability of sound change he agreed with Haugen: it can always be explained afterward. There are so many things that can happen that given our framework to-day, the framework of system, which is such a very nebulous thing, we can hardly predict what will happen. Also, some things are more probable than others; but given a proto-language and 100 dialects, it is humanly impossible to predict the future. We can only sometimes predict the past, i.e. explain what has happened, but even that is tremendously difficult.

Birnbaum: "I share fully Professor Elbaz's surprise that in all these papers the name of André Martinet was never mentioned". "In a side comment I referred to Martinet's dictum: 'Language is a balanced system with continuous functional redistribution'". To T. Jansson Birnbaum remarked that we all agree that speech perception is important in sound change. Henning Andersen's whole model is related primarily to perception. To I. Mahnken: "Andersen's model was developed to account for historical changes in a Czech dialect." To H. Lüdtke: "I would not call your procedure 'prediction', but educated guesses about probabilities." Reduction is important, but the factor that counters it is the need of explicitness. These forces are constantly in conflict, and it is very difficult to say which will win.

Labov: (1) On women's speech: we do not all agree that it is more advanced. Where women play a part in national life, they are more sensitive to the national prestige, once a sound change has reached maturity and is stereotyped. They are also normally the leaders in linguistic changes from below or unconscious change, where we are hypothesizing a different kind of prestige. (2) This has not been a panel dealing with restraints on linguistic change. However, following Weinreich's paradigm, many

of the sound changes discussed here do show very powerful unidirectional principles, such as the fact that tense vowels always rise. -- On the question of the upper working class: that is not a final characterization of the group involved because it turns out that the role of these innovators in linguistic change is characterized even better by factors having to do with communications research. They are leaders in certain community networks which are very intense locally, but which reach outside the community, and so we get a relatively homogeneous city dialect. Do they emerge as a new group with a need for identification? "I suspect that Professor Puech's characterization was correct. It is not necessarily a new group. It may be an old group that needs to reinforce its identity. These mysterious factors of prestige which we cannot make explicit may be the result of pressures from new groups entering the community. These are challenging the position of the old group. Just as an adolescent must reassert his position in his parents' community, so the Irish or the Italians or the upper working class may be under pressure from Blacks, Puerto Ricans, and other new groups entering the community. Yes, I suspect that the pressure to reassert identity is the driving force behind this continual renewal of sound change."

Suzanne Romaine (Birmingham): Labov's research is an important attempt to deal with the problem of the transmission of change. But the value of the work being done on social factors in sound change is not (as Labov seems to think) to provide explanations of why language changes, but to give us a taxonomy of how social factors interact with linguistic structure in the implementation of language change.

Haugen: "I think we are still in the midst of a very important and very interesting discussion. I thank you for listening to this segment of a discussion that I am sure will go on at future congresses as well as between congresses."