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10. Some Problems with the Labovian Paradigm

1962 Thomas S. Kuhn published a book called *The Structure of Scientific Revolutions* and since then the world of philosophers of science has not been quite the same. Kuhn is a distinguished historian of science and essentially what he did was to apply the historical method to the theory of science. He showed that disciplines or sciences are like other belief systems in that they make up a complicated structure which almost always excludes at the same time as it includes. Something is placed out of focus in order to focus on something else.

Applying this type of thinking to the humanities in general and linguistics in particular, Dell Hymes has noted that more often than not the humanities manifest both a dominant paradigm and a polemical undercurrent, hardly noticed by the victorious mainstream (Hymes 1974).

The history of linguistics in the 19th century was brilliantly written by Holger Pedersen in 1924. Pedersen views the past as one great preparation for the final stride at the beginning of the century when the three founding fathers Rask, Grimm and Bopp created scientific linguistics. His story is persuasive and elegant; it brings out clearly the continuity in 19th century linguistic thought where the focus is on language change, in particular sound change (cf. Koerner 1989). There is, however, no denying the fact that Pedersen was among the ardent believers in the neo-grammarian paradigm, and thus his story is biased. Recent research has brought to light an undercurrent running through all of the century focussed instead on the general structure of language, universal grammar, *langage* in Saussurean terms (Koerner 1977). I know that this story is vastly more complex, but the simple version is adequate for my purposes here.

My purpose is to make this observation: the dominant paradigm of the previous century was focussed on sound change, whereas the competing paradigm focussed on *langage* and its general grammatical structure. In this century the roles have been reversed: the dominant paradigm is concerned with *langage* and the relationship between specific language structures and a universal capacity for language while the competing paradigm is focussed on language change, and sound change in particular. This competing paradigm is sociolinguistics.

Now the peculiar nature of European universities has seen to it that all paradigms in the history of linguistics still exist. At many universities comparative Indo-

European linguistics is practised as part of an unbroken chain of continuity. Likewise, the neo-grammarian doctrines have been part and parcel of much early dialectological work, at least in Denmark. But the structuralist notions of system and more abstract analyses of phonetic data have been as victorious in dialectology as in general linguistics (cf. for Danish dialectology Andersen 1970).

Mainstream linguistic practice by and large strives to reduce heterogeneity and variation, if not to ignore it. In such a situation a competing paradigm may arise inside the mainstream but focussed on variation and change and this was what happened when in 1966 William Labov published his *The Social Stratification of English in New York City.*

Judged by the sheer output of titles, sociolinguistics was an immediate success (cf. the documentation in Hermann/Gregersen 1978: 37–39), but seen in the rear window, we realize that sociolinguistics has remained a competing paradigm, a paradigm fighting with an enemy that does not see enemies where the sociolinguists are. What I mean is that as far as the transformationalists are concerned, the debate is rather on whether to adopt the latest convention or perhaps to slaughter a few pragmaticists on the way home; the issues that sociolinguistics raise are simply not issues for the general linguist. Sociolinguistics has ended up being a subdiscipline instead of taking over the main role in language research.

The tenets of the Labovian paradigm are thus explicitly polemical and they have remained essentially unaltered for more than two decades now: on the level of ideology or epistemological commitment, Labov and his followers are squarely materialist and empirical. Whatever else one may want to call the Chomskyans, this particular characteristic does not fit. Some of them may be materialist in their philosophy, some may be empirical in their practice, but to my knowledge most of them are neither.

On the level of methodology, Labov uses all the modern techniques necessary for the manipulation of large data files; the sociolinguistic methods are quantitative.

A paradigm has to have its exemplar, and for sociolinguistics the ideal study has to all extents and purposes remained the first one, the Lower East Side survey of the New York speech community. Labov has refined his techniques, as have a great many other sociolinguists, but the basic methodology has not changed. I summarize the exemplar in three points:

1. Labov maintains that the focus is on the speech community, not the idiolect. One person does not make a community, and in particular the lonesome linguist at his desk does not make an informant, however much he talks to himself. We might add here that the use of one single informant is not confined to general linguistics: it has been used by a great many dialectologists as well.
2. Labov is certain that all speech is stylistically stratified and that the vernacular or

the style that is acquired during the formative years is the true and authentic expression of the informant's innermost self. A lot of effort has consequently been directed towards solving the observer's paradox, and that is how to gain access to the speech the informant uses when a monitor is not present.

3. All speech in speech communities manifests variation. This variation may be explained by looking at both the speaker variables and internal linguistic constraints. This means that it is, in principle, possible using quantitative methods to reveal significant differences between speakers if we group the linguistic data according to what social groups their producers belong to. Similarly, we are likely to discover that the manifestation of linguistic variables is to a certain extent governed by the linguistic context. Labov has repeatedly stressed that no such analysis will eliminate variation altogether since there is some inherent variation in speech, but this is seen as a kind of residual leftover by the explanatory analyses. The explanation of linguistic variation attempts to create ordered homogeneity.

In what follows, I shall discuss some of the problems arising from this stance. The discussion will be based upon experiences acquired while Inge Lise Pedersen and I directed a major effort at urban sociolinguistics in Denmark, a project which we call the Copenhagen Study (cf. Albris et al. 1988).

The Copenhagen study grew out of discussions of the literature on variation analysis and was much stimulated by a summer school featuring William Labov, Gillian Sankoff and Jim and Lesley Milroy as teachers. The project is in part *ein Lehrstück,* to speak in Bertold Brecht's terms, it is an attempt to learn the trade, as it were, so that we could inspect the paradigm from within. I shall report briefly on the design of the study.

What is the Copenhagen speech community? Like all cities Copenhagen has witnessed a period of decay as to its centre, with people moving out to the suburbs during the last thirty years. Still, this might only be taken as a sign that the Copenhagen speech community is no longer coterminous with the city of Copenhagen, but rather should be taken to encompass all of greater Copenhagen. Whether one chooses one or the other strategy, one has to come to grips with the influx problem. In the Copenhagen area there is a constant influx of people from all other regions of the country and, though the regional dialects have changed a lot, there are still easily discernible differences between speakers in this respect. So it will not do just to take any directory of Copenhageners and make a random selection of informants if we want to portray Copenhagen speech. In fact, there seems to be an interesting difference here between either portraying Copenhagen as a speech community, including a lot of different languages and regional lects, or portraying Copenhagen speech (cf. Gregersen and Pedersen 1991). In the latter case we select only natives as informants aiming at a socially differentiated sample. The easiest way is, of course, to choose from a randomized list of suitably stratified Copenhageners only those that were

born and bred in Copenhagen, thus filling in the places. But instead of conducting such a survey study we chose to conduct a modified community study.

The advantages of selecting a community as a sort of mini Copenhagen is that in this way we control the immediate environment. If informants are born and bred inside the same small quarter they have been exposed to the same environmental influences in their formative years and any differences between them consequently must be due to their background. We only have to make sure the community is free of any condition that might damage our generalizations different from other Copenhagen quarters.

To cut a very long story short, we chose to work in Nyboder, a small village-like section of central Copenhagen, which has for time immemorial been a workers' quarter for the personnel of the Royal navy. Since the navy personnel include officers, we were, at least in principle, able to find speakers who belonged to two different classes, for convenience referred to as the working class, WC, and the middle class, MC.

Another condition of a more practical nature is the boundedness of the research site. Nobody is in doubt whether or not he or she has grown up in Nyboder since the area is known as a specific community simply because of its houses and the traditions surrounding the naval men.

At the beginning, we opted for a real neighbourhood study, intending to select only informants still living in the area, but since we soon realized that this would have left us with too many compromises, we slackened the conditions so that we selected as informants an equal number of men and women from two social classes and four age groups solely on the condition that they had grown up in Nyboder. It did not matter whether they were born in Nyboder, just so long as they had moved to the area before they began to speak the language. It did not matter whether they still lived there just so long as they had remained until they left home. The ideal design of the study is shown in figure 1. The age groups are as follows: Group I consists of persons from 15 to 25 years, group IIa contains persons ranging from the age of 26 to 39 while the next group, IIb, includes persons from 40 to 60. The last group, group III, is the old boys and girls group of those more than 60. 60 is the age of pension for someone working in the Danish navy.

Age group	I		IIa		IIb		III		
Sex	m	f	m	f	m	f	m	f	
Middle Class	5	5	5	5	5	5	5	5	40
Working Class	5	5	5	5	5	5	5	5	40
	10	10	10	10	10	10	10	10	80

Figure 1: Ideal design of the Copenhagen study

Using these conditions as guidelines for the selection of informants, we were able to gather a number of sociolinguistic interviews lasting from around one hour to three hours. For the average length of interviews, I refer you to the table below. Please note that all of the interviews are rather long.

Cells	Average Age	Average Length
Group I:		
WC ♀	19,4	90 min
WC ♂	21,8	113 min
MC ♀	16,8	86 min
MC ♂	17,8	76 min
Group II:		
WC ♀	31	112 min
WC ♂	32	101 min
MC ♀	31,8	93 min
MC ♂	32	117 min

Table 1: Average distribution of age of interviewees and length of interviews (cf. Albris 1991).

In the Copenhagen project we conducted 95 single person interviews and recorded 15 group sessions. Only a fraction of this material has been transcribed and thus made available for detailed analysis. We concentrated our limited resources on transcribing the single person interviews from the youngest group and from the core group of grown-up persons from 25 to 39 years of age. This last group includes 5 working-class men, 5 working-class women and four of each of the sexes from the middle class. When I report from the study in this paper, I report on the phonetic analysis of these 18 persons undertaken by Henrik Holmberg, Inge Lise Pedersen and myself. The actual design of the phonetic study is shown in figure 2 below.

Age group	I		IIa		
Sex	m	f	m	f	sum:
Middle Class	2	5	4	4	15
Working Class	2	4	5	5	16
sum:	4	9	9	9	31

Figure 2: Actual design of the analysis of phonetic variables in the Copenhagen Study

Before we could analyze the tapes, however, we had to overcome a major problem, namely how to define style (cf. Gregersen 1988).

In the standard methodology it is quite easy to define all other styles than the vernacular. Labov's notion of style is based on a continuum of attention or monitoring. When attention to speech as such is greatest, we have the most formal style; when attention is minimal, we have the vernacular. In a 1985 paper Labov characterizes the vernacular as "the unreflecting spontaneous speech of native speakers when they are engaged in interaction with their family and peers". He further maintains that "the vernacular is that form of language that is learned first, controlled best, and is most free from individual variation."

So, how do we gain access to the vernacular? Or rather, we thought we had solved this problem by having our field workers discuss topics of an emotional character, by having field workers and informants act, in short, as what we have come to term intimate strangers (Albris 1991, Gregersen 1988). The obstacle was how to isolate these passages of the interview from the more formal passages. There is, of course, no problem in isolating word-list style or minimal-pairs style if one has included a word list and a list of minimal pairs in one's interviews but most interviews by far are really conversations which have their peaks of formality and their valleys of informality. So the question concerning the vernacular is not whether it is there once it is there – the question is: how do we find it? Obviously, what we are trying to isolate is not the speech used with family or peers. If we had recorded this type of speech, we would not need the interviews at all since the interview is the poor man's sociological way of circumventing the anthropological problem of drawing close to people, close enough, that is, for them to use the same kind of speech as the one they would use in the family or with their peers. The interview is, so to speak, a sociological surrogate for participant observation (Gregersen 1990).

As you will remember, we define style as intraspeaker variation. But this intraspeaker process is of course social, i.e., aimed at signalling something to the interlocutor(s). Thus, our response to the style detection problem must be based on an analysis of how we detect that a social relationship changes from a formal, distant one into an intimate and informal one in everyday interaction. We suggest the solution which follows.

We distinguish three foci for style detection. Please bear in mind that segmental phonetics cannot be one of them, since that would entail a vicious circle for the phonetic style analysis.

The first focus is *the linguistic structure of interaction*. A free conversation is supposedly characterized by being a give and take on both sides, whereas the typical interview structure is overwhelmingly question/answer structured, the interviewer asking the questions, the interviewee supplying the answers.

The second focus is *the topic*. Topic is a typical rubber concept which may be stretched and tightened to suit present interests. The notion of topic that we are after is one that is higher than the sentence topic and yet lower than the all-embracing topic of an entire conversation. "What did you talk about?" I might ask, and the field worker would answer: "Well, sex, drugs and immigrants; stuff like that". The notion of topic is clearly too abstract. So what we need is a topic in between, covering a passage of speech. The topic should be separated from the related notion of speech event or genre. We take this to refer to the most abstract categorization of what types of linguistic acts may be performed. Thus we may distinguish banter, gossip, narration, negotiation and a host of other categories. Obviously these categories are culture specific and the relationship between, e.g., narration and the sociolinguistic interview is in itself a thorny question which I do not have the space to go into here (but cf. Møller 1991).

The third focus is what Labov has termed *the channel cues*. Among the channel cues most important for the perception of phonetic performance are pitch, tempo and intonation, but paralinguistic features such as nervous laughter, coughing and pausation, though much less frequent, are certainly significant when they are present.

The definition of style that we concluded was operational resulted from those passages of the interviews where the following criteria apply:

1. the field worker is unquestionably in control of the conversation and
2. topics of a non-emotional character are being treated in a non-emotional way, and
3. the tempo of speech is standard and attention is apparently on the monitoring of speech.

These stretches of speech we classify as non-casual speech. We next proceed to delimit stretches of speech where

1. no single person seems to be totally in control of the conversation and
2. topics with a highly emotional charge are treated in an emotional way and
3. the informant deviates from his or her standard tempo of deliverance and/or

his or her speech is accompanied by other signs of attention to content rather than to expression.

These stretches we classify as casual speech.

Finally, those stretches of speech where the sentence list is read out or discussed are singled out as manifestations of sentence list style. Armed with these definitions and definitions of a number of phonological variables, we set out to eliminate variation or reduce it considerably in our analyses. The most frequent variable of all was the variable AN. This acronym stands for the occurrence of a short <a' vowel followed by an alveolar consonant or nil. Examples are *kande* 'pot', *kat* 'cat' and *ja* 'yes'.

The results for the AN variable are shown in Table 2 below. In order to understand this table you should be aware of the peculiar nature of this variable. The short **<a'** phoneme in the advanced standard speech of Copenhagen has four different manifestations according to the linguistic context. Instead of analyzing the phonological <a' variable only to discover that the different linguistic contexts affect the manifestation, we subdivided the phoneme into four different phonological variables: AN, AM, AJ and ANR. Space does not permit going into details here, but suffice it to say that the AN variable is in itself a subdivided phonological unit, an allophone in the structuralist terminology. The AN variable has for a long time been known to the entire Danish speech community as a stereotypical marker of social identity. In IPA terms this means that the more the <a' phone is an epsilon, the more one sounds like a Copenhagen working-class man and the lower one's AN value will be.

As you can see for yourself, this holds for some persons, but by no means for all in the core group. On the contrary, the means for the four cells we are interested in are remarkably close. However we test them, they are not significantly different.

Now look at the differences among styles. You have the differences between means at the far right and you can see that in all cases except one the mean values for casual style are lower than the corresponding values for non-casual style. The groups responsible for this overall significant style difference are the grown-up women, particularly the middle class women.

Before I scrutinize this result, I ask you to consider one aspect of this table of index values more closely. Why ever should we obtain such diverse figures in a table such as this one? Remember: we have controlled the variable of sex, we have controlled the variables of class, age and upbringing and, furthermore, we have defined the variable so that linguistically conditioned variation should be minimal. So why are the figures in the cells so diverse?

And again: why are not all of the figures whole numbers? If this sample were completely determined by the well-known speaker variables and by style criteria, we would have had whole numbers in all cases. This is, incidentally, what lies behind the

							mean	style diff.
WC	women	**2.01**	**1.75**	**1.59**	**1.54**	**1.57**	**1.69**	
Group I		1.91	–	1.69	–	1.50	1.70	1
	men	**1.67**	**1.83**	**1.68**	**1.63**		**1.70**	
		1.59	1.67	1.72	1.79		1.69	-1
WC	women	**1.31**	**1.62**	**1.76**	**1.92**	**1.82**	**1.69**	
Group IIa		1.57	1.79	1.80	1.90	1.92	1.80	11
	men	**1.82**	**1.86**	**1.67**	**1.75**	–	**1.78**	
		1.96	1.64	1.71	–	1.93	1.81	3
MC	women	**2.13**	**1.84**				**1.98**	
Group I		2.10	1.88				1.99	1
	men	**2.00**	**1.76**				**1.88**	
		2.05	1.93				1.99	11
MC	women	**1.44**	**1.88**	**1.42**	**1.89**		**1.66**	
Group IIa		1.67	1.98	1.67	1.94		1.82	16
	men	**1.72**	**1.77**	**1.62**	**1.62**		**1.68**	
		1.81	1.76	1.78	1.52		1.72	4

Table 2: Results for the AN variable: casual style: **bold type;** non-casual style: ordinary type. (cf. Holmberg 1991: 121).

way of speaking so often met with in the literature that a particular variant is typical of this or that sex, class, style or age group. This way of speaking, to my mind, significantly underestimates the variation we find when we investigate long passages of genuine speech.

There are two ways of progressing now if we want to explain the variation in this table. In both cases what we wish for is linguistic homogeneity, and we still take it for granted that our task has come to an end when we have created cells, which manifest maximal homogeneity. One strategy is to begin a search for the complex speaker variable which is the "real" determinant behind all of these messy figures. In the Copenhagen study we took a short cut here, inspired by the work of Barbara Horvath on the Sydney speech community (Horvath 1985). Horvath has performed a so-called principal-components analysis directly on her linguistic index values in order to create linguistically homogeneous groups. This done, she tries to track down what speaker variables correlate with the linguistic group membership.

We did that in the way which follows. The most homogeneous group figures in this table are those for the Middle-class men's casual speech. As you can see, the possible means for individuals range from 1.62 to 1.77. If this is taken as a valid measure of the linguistic homogeneity of a cell, we may look at the other groups from this perspective. In this case it is rather easy to see that a considerably more homogeneous group of women might be created by moving the first and the third informants from the middle class to the working class and the last three informants from the working class to the middle class. I shall not go further into the details here, but the results are that we can indeed create linguistically more homogeneous groups concerning casual speech by regrouping women. Since this is one topic of the paper by Inge Lise Pedersen (1995), I shall stop here and confine myself to a few remarks about this strategy.

This is a tiresome task since it must be performed for all variables separately. Obviously, a regrouping of informants on the basis of their index scores must be justified by finding in some speaker variable a common denominator for the new groups, and there is no guarantee that the speaker variable capable of explaining the AN distribution will explain the other <a' variables as well.

The second strategy takes as its starting point a discussion of the concept of style. If styles are defined by the criteria I mentioned, the significance of the stylistic differences in the phonetic index values – and they are substantial in our data since only one of four **<a'** variables did not show some stylistic difference – must be that a change in the values means a change in style. The normal interpretation would be that as soon as the values connoting relaxation or working class – note that the direction is normally the same – are introduced, we make a mental note of this, saying "Aha, a style shift is taking place".

Now, if this were true, we would expect that the difference among styles would show up as clean figures. Style 1, the casual speech would be characterized by the exclusive use of variant 1; style 2, the non-casual speech would only have variant 2. This is not the case.

This is awkward, but not damaging. A lot of words occur only once in a lifetime, and certainly most words in a conversation occur only once. We might speculate that the manifestation of such words is free to range over more AN values simply because the words are semantically salient anyway. This would lead to the next probable solution, which consists of finding certain keywords which we might listen to so that they would be the touchstones for a style shift. Naturally, for this strategy to work, the keywords would have to be frequent. I have selected two very frequent words and subjected them to an analysis. The words chosen were *ja* meaning 'yes', and *han* meaning 'he'. Both of these words are frequent enough to occur several times in most of the interview passages labelled as belonging to casual or non-casual speech. There are exceptions to this, as you will see from the figures below, but on the whole,

reporting the results for *hem* only, no casual speech passage was entirely without an instance.

What Table 3 below shows is the result of a sorting procedure. All the passages analyzed for phonetic values were searched through by a computer. The computer stopped at any instance of the word *han,* made a note of its place in the transcript and read what phonetic realization the word was given at that particular time. What we receive, then, is a list of phonetic values for the same words in the same style produced by the same person.

I see this as an ultimate test of the explanation by homogeneity strategy. If we used the phonetic differences to signal style, it would be my prediction that we would find clean differences between the styles: AN1 being style 1, and AN2 being style 2.

working class:

women	casual	1.31	1.62	1.76	1.92	1.82
		<1.13>	<1.92>	<1.89>	<2.00>	<2.00>
	non-casual	1.57	1.79	1.80	1.90	1.92
		<1.50>	<1.91>	<2.07>	<2.00>	<1.73>
men	casual	1.82	1.86	1.67	1.75	
		<2.09>	<2.00>	<1.90>	<1.92>	
	non-casual	1.96	1.64	1.71	–	1.93
		<….>	<1.50>	<2.00>		<….>

middle class:

women	casual	1.44	1.88	1.42	1.89
		<1.37>	<2.05>	<1.50>	<1.87>
	non-casual	1.67	1.98	1.67	1.94
		<1.85>	<….>	<1.48>	<….>
men	casual	1.72	1.77	1.62	1.62
		<2.00>	<1.87>	<1.56>	<1.73>
	non-casual	1.81	1.76	1.78	1.52
		<2.00>	<….>	<1.68>	<1.00>

Table 3: Results of the study of the single word *han* 'he', in two different styles for the 18 speakers of group IIa (between 25 and 39 years of age). The figures for *han*

are in brackets, please notice that the other figures include the figures for *han.* In all cases, however, the AN variable has a frequency above 100, most frequencies lie between 150 and 200.

One of the persons who does style shift significantly is informant no. 40, the first person in the upper row. Her mean for casual speech is 1.31, and her mean for the word *han* is significantly lower, viz., 1.13. In fact, she produces 7 instances of a *han* pronounced with the very high or closed variant of <a' making this word a perfect imitation of the standard pronunciation of the word *hen* which means 'to' or 'towards'. In structuralist terms informant no. 40 has a merger of epsilon and <a' in a specific style and a specific word. The only reason her word for 'he' is not perfectly equivalent to her word for 'to' or 'towards' is that she produces one instance of 'he' with a lower vowel, the more neutral AN2 variant. Still, that might be explained away.

As we glance through the table, we obtain more positive results: informants nos. 43 and 44, the last two in the upper row, both have a clean 2, but, alas, the reason is that they only produced one instance of the word. For informant no. 43, there is an interesting addition, though, since she produced a lot of instances in non-casual speech, in fact 16 A2s, 1 AN1 and 1 AN3. This means that her clean figure is the result of slight variations as well.

For the first person among the middle-class men, the clean figure is due to his production of only 3 instances in casual style and 1 in non-casual style. The same explanation applies to the upper right-hand corner, the last among the working-class women.

For the second person among the working-class men, there is a beautiful explanation. As you can see, his figures are decidedly wrong, his overall AN value is higher in casual speech than in non-casual speech. I submit that we can find the explanation for this in his life history. He was born as a middle-class person since he is the son of a very high-ranking naval officer. His parents divorced and he dropped out of school at the time of the interview, and then worked as a cook. He married into a distinctly working class family, and our interpretation of his style shift is that he tends towards the middle-class values when he is relaxed and talking with a male interviewer interested in his childhood days in Nyboder. Since his family vernacular would have been middle-class, he has the standard variant AN2. Note that this explanation depends on the prior identification of the AN2 value as middle-class, an identification which is not evident from the Copenhagen study itself.

But apart from this wonderful corroboration of the keyword hypothesis, the table shows clearly that variation obscures the signal values. Our sorting procedure makes it possible to evaluate a final explanation of this. Since we made a note of the appearance in the text patterns of AN1s occurring together after a row of AN2s, this could have been an indication of minute style shifting patterns. Having looked through all

the files, I have not found any indication that this was the case. There seems to be very minute variation within the same line.

This must mean that if perception of style is governed by phonetic signalling, it operates just like the statistical analyst, computing means and significant differences as the conversation rolls along. I for one am not too comfortable with the psychological plausibility of this result. I note, however, that Ronald Butters in his plenary lecture to the Congress explicitly stated that he believed that this is precisely what occurs (Butters 1993).

This does not mean, however, that it is impossible to style shift without producing any phonetic differences. If that were the case, a style shift detected by the three criteria mentioned above should for all informants be accompanied by phonetic shifts in mean values – and that clearly is not the case, witness the examples of the fourth working-class woman and second middle-class man in Table 2 above, just to take the most obvious cases.

We are obliged to conclude that it is possible to communicate a style shift by other means than phonetic ones, prominent among these means are, I am sure, the changes we used to detect style shifts in the first place, that is, changes in conversational interaction, changes of topic and changes in the use of channel cues.

The last question and the most important one would then be: why are the phonetic differences between styles present if they are not used to signal style shifts? The phonetic differences are there because speakers cannot help producing them. They are a sign of their social identity, and the reason no one uses only AN1 or AN2 exclusively is simply that all of us have chequered social histories. For the theory of change, the importance of these minute variations in proportions of phonetic values is that such variation is the precondition of change. Change starts out as such subconscious acts of identity, once their direction becomes the same, a change will take place sooner or later.

Sometimes it is tempting to write the history of linguistics as going in circles. Periods focussing on change and variation are followed by periods of intense research on homogeneous, abstract and universal grammatical structures. But this is a nightmare implying that we can never learn to integrate the systematic grammatical view within the empirical variationist one; that we can never learn to see the individual as a member of several groups, which is precisely why he or she is unique. I think I have shown that it is necessary to work on such an integration if we want to explain style differences rather than just state them. But I might be wrong.

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