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SYNCHRONY VS. DIACHRONY, OR SAUSSURE 75 YEARS AFTER

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The present paper basically represents a written version, with only a few minor changes and some additions¹, of the one delivered orally at the Naples meeting. It represents a view of the «state of the art» rather than a new hypothesis about how languages change, a subject with which linguists have recently been increasingly concerned. No one can reasonably deny that all natural languages are subject to change, that they are continually changing, a fact which considerably upset many 18th century commentators on the state of the language. But it is one thing to realize that change exists, irrespective of whether we welcome or deplore it, and quite another to try to account for the way that change operates and spreads, and for the mechanisms which trigger it off, even if we accept, and I personally would be inclined to do so, as Lass (Lass, 1980) has claimed, that true, logically water-tight explanations are beyond us. But this is anticipating something we shall have to return to towards the end of this paper.

It is obviously not my intention to examine whether, or to what extent Saussure is still «relevant» today in the light of

¹ A good deal of what follows formed the object of discussion of the annual meeting of the *Società Italiana di Glottologia* held at Pavia 15-17 September 1988. The title of the conference was *Modelli esplicativi della diacronia linguistica*, which therefore covered some of the same ground as the present paper. I gratefully acknowledge the stimulus provided by many of the papers presented on that occasion. I should also like to thank my colleague Rosanna Sornicola with whom I have had the opportunity to discuss certain matters raised in this paper, discussions which have helped me to clarify my ideas on a number of points.

«advances» in linguistics over the last 75 years or so. This time span is, of course, not quite accurate, but it will do as a sort of signpost and no more. Much more pertinent is the question of Saussure's «relevance». Whatever the position may be in the physical sciences (although Kuhn's theory of scientific revolutions provides a considerably more sophisticated model than one of simple linear progress), the history of linguistics can in no way be seen in terms of successive theories giving a more accurate and reliable account of the data and therefore replacing the theory previous to it, consigning it, as it were, to the dustbin of history, which is not to deny that Kuhn's theory of «scientific paradigms» may not be illuminating also as far as our discipline is concerned. This sort of arrogance in dealing with our predecessors is no longer tolerable. The early structuralists, for example Bloomfield, who have in their turn been «superceded», treated «pre-scientific» linguistics with condescension, if not outright contempt, but surely all great thinkers are always «relevant» and never out of date. There is no progress from Aristotle to Kant or from Kant to Wittgenstein or from Wittgenstein to Austin and Searle, though we may want to look at metaphysics, or logic and its relation to language in somewhat different ways, or we may want to ask rather different questions as a consequence of what recent authors have had to say on a particular subject. In a certain sense, of course, we «know» more than Saussure, because new data has come to light which may allow us to make different inferences and therefore construct new theories. But that surely does not mean that our theories are, in a meaningful way, necessarily «truer», for theories in language have more or less explanatory power, they are not true in the sense in which it is true to say that $3 \times 3 = 9$. To sum up then: it is certainly not my intention here to use hindsight to «correct» Saussure's alleged mistakes or shortcomings, but rather to have a fresh look at the synchrony-diachrony dichotomy, in order to assess what light recent studies in linguistics, and in particular sociolinguistics, can throw on the process of language change.

In spite of the very strong tradition that has associated the

concept of diachrony with the history of the remoter phases of certain languages, and in the past particularly with extinct languages, the synchrony-diachrony opposition has, in itself, nothing to do with a recent-remote dichotomy. Saussure's two terms refer to processes, not to the objects they study. In spite of the fact that the vast majority of the work done in synchronic linguistics has been concerned with the present state of a given language, there is no theoretical justification for equating synchrony with something that concerns the living language of the present and diachrony with the dead languages of the past. It would be naive in the extreme to think of change as something that happened in the past, and therefore is of no concern to the modernist. Neither can we equate diachronic linguistics with the altogether much larger category of the History of the Language (cfr. Varvaro 1972-73), since this comprises both the internal and the external history of the language, with an emphasis on one or other of these two elements that varies from one author, or school of thought, to another, whereas diachrony, in so far as it represents a historical approach to the study of language at all, is only concerned with the former of the two terms in question. Linguistic change in a remote period of the past very often seems so much more radical than anything we actually observe as going on under our noses, precisely because of our historical perspective. We observe the input and the output, the state of the language before and after the change, and see that a radical restructuring has taken place, precisely because the intermediate stages escape observation, both because the documentation for remoter phases of the past is usually woefully inadequate and also because it is difficult to interpret. Let us take as an example what is usually known as «Grimm's Law», a radical restructuring of the consonant system of a language, if ever there was one. What we know about the consonant system of pre-*proto-Germanic* has to be inferred, since we have no direct evidence of it; indeed, we don't even know whether such a unitary language ever existed and we only have rather vague hypotheses about where and when it might have been spoken. What we can in fact observe, and even that only par-

tially, is the output, but not the input. Even where we have a continuously documented history like that of the development of the Romance languages out of (a form of) Latin, it is the surviving documents that dictate our view when Italian or French, shall we say, were born; the fact that what one may with some approximation call French and Italian were first written down in the Strasbourg Oath and in the Cassino document are mere accidents of history, since clearly we must suppose a continuum stretching back into the remotest past, along which we arbitrarily choose a number of cut-off points to suit our convenience as historians. Whereas few Romance Philologists would consider Latin (even vulgar Latin) the «same» language as modern Italian (or French, or Spanish), it remains a matter of debate whether language spoken in England at the time of King Alfred is a form of English or not, although the fact that today it is generally referred to as «Old English» indicates that most scholars accept the hypothesis that in some sense we are dealing with the same language. On the other hand, no one in his senses would maintain that the language of the time of Queen Elizabeth was anything but English, in spite of the changes, some of them very considerable, that have taken place during the last 400 years. We may of course think of change as concerning two linguistic forms so different that scholars prefer to classify them as different languages (though whether two variant systems count as the «same» or as «different» languages often has more to do with historical rather than purely linguistic criteria), but this merely means that we are looking at the end product of change, change as it manifests itself over a period of centuries rather than of years, and if we want to have a fuller understanding of how change takes place, where it originates and how it spreads, such a long perspective can throw very little light on the problems we are interested in. To put things somewhat bluntly, historical linguists may be concerned with the remote stages of the language, but this is by no means a necessary condition of its status as an intellectual discipline, and in any case historical linguistics and diachrony, though epistemologically related concepts, are not synonyms. On the other hand, although

synchronic analysis has usually taken as its object the present state of some language, often with particular emphasis on its more informal, spoken registers, there is no reason why any documented stage of a language may not be considered along its horizontal, synchronic axis, subject to rules, whether within a generative or any other theoretical framework, to account for its structure, including its underlying structure, that make no reference to any previous known states of the language. These rules have a generative *history*, which, to put it somewhat paradoxically, are seen entirely in *synchronic* terms. To what extent such a strict, categorial separation between the two levels of analysis is in fact useful for the linguist, historical or otherwise, is basically what I am concerned with in this paper.

There can be no doubt that Saussure was to a great extent interested in vindicating the central importance of synchrony after a century of strictly historical linguistics. It is probably true to say that 19th century scholars considered historical linguistics to be the only legitimate branch of the science of language, although it is worth recording that during the 17th and 18th century, the period curtly dismissed by Bloomfield and his fellow structuralists as the pre-scientific phase of linguistic studies, philosophers (linguistics was not yet a separate «discipline») enquiring into the nature and origins of language essentially adopted what today we would call a synchronic approach. This reaction against the predominance of the historical tradition, in which Saussure himself was brought up, perhaps explains the tone of some of his statements — provided of course we accept that the form in which the *Cours* has come down to us accurately reflects Saussure's attitude, as illustrated in the following brief excerpts in Wade Baskin's translation:

The first thing that strikes us when we study the facts of language is that their succession in time does not exist insofar as the speaker is concerned. He is confronted with a state. That is why the linguist who wishes to understand a state must discard all knowledge of everything that produced it and ignore diachrony. He can enter the mind of the speakers only by completely

suppressing the past. The intervention of history can only falsify his judgment.

(Saussure 1960: 81)

The opposition between the two viewpoints, the synchronic and the diachronic, is absolute and allows no compromise.

(Saussure 1960: 83)

Since changes never affect the system as a whole but rather one or another of its elements, they can only be studied outside the system. Each alteration doubtless has its countereffect on the system, but the initial fact only affected only one point; there is no inner bond between the initial fact and the effect that it may subsequently produce on the whole system. The basic difference between successive terms and coexisting terms, between partial facts and facts that affect the system, precludes making both classes of fact the subject matter of a single science.

(Saussure 1960: 87)

Clearly Saussure was not so naive as to suppose that present-day forms, especially morphological irregularities, cannot be «explained» in historical terms, in other words that present-day forms can be seen as the result of some regularly operating «sound law», especially as the neogrammarians had maintained that there are no exceptions to these, and such apparent exceptions as there are merely reflect the linguist's inadequate formulation of the «law». He himself cites OE *fof fet* and OHG *gast - gasti - gesti* > NHG *Gast - Gäste*, the kind of thing that is taught in historical linguistics courses all over the world. What Saussure denies is that the laws of i-mutation are relevant to an understanding of the category «plural» in English or German, that its historical genesis tells us anything about the way it functions or, since psychological considerations are very much present in Saussure's treatment, that past forms of the language are in some way present in the speaker's mind. But even that is debatable: there is no question that the grammar of present-day speakers of English contains some version of the do-support rule, but does that mean that a sentence like *I know not* is au-

tomatically rejected as ungrammatical, or is it not rather present in the speaker's mind as some kind of linguistic fossil? It could of course be said that this is special pleading, since we are talking of a sophisticated speaker with some awareness of the historical facts of language, but I would like to argue that such an awareness is no less part of the speaker's competence, or rather constellation of competences of his native language, than his sociolinguistic competence which allows him to place his interlocutors at a fairly precise point along a social scale.

But let me return to a more central part of my argument: let us accept that a historical understanding of grammatical categories is not relevant to our competence as speakers of the language: my point is rather less obvious. In the second passage quoted above Saussure seems to exclude the possibility of the joint operation of synchronic and diachronic facts, which must receive entirely separate treatments in the linguist's analysis. I believe recent studies have shown that synchronic and diachronic rules cannot be so easily separated, that we are more and more coming round to seeing synchrony in dynamic, not merely in static terms. It is worth noting here that before Saussure settled on the terms «synchronic» and «diachronic», he had talked of «static» and «evolutionary linguistics». Just what I mean by the presence of a dynamic dimension in synchrony should become clear later on in the course of this paper.

Let me now briefly look at two other points. Like the generativists, Saussure sees language as a series of discrete states, for binary oppositions, which are at the centre of his view of how language functions («in language there are nothing but differences»), necessarily entail discreteness. Just how many horizontal or successive synchronic states this implies is not clear — in theory one could of course argue that the number is necessarily infinite, since it is always possible to find a smaller number within any series, but this is obviously not a practical, common-sense solution. The generativists have an answer to this, though it is an answer in terms of speakers rather than in terms of states of the language. The other point is that Saussure sees language, *langue*, as a homogeneous entity, variety being an aspect of

parole, which is not really the concern of the linguist, whose primary interest must be in *langue*. If for the sake of argument we may roughly equate Saussure's *langue* with Chomsky's «competence», then the latter's celebrated dictum that

Linguistic theory is concerned with an ideal speaker-listener, in a completely homogenous speech-community, who knows its language perfectly and is unaffected by such grammatically irrelevant conditions as memory limitations, distractions, shifts of attention and interest, and errors (random or characteristic) in applying his knowledge of the language in actual performance.

(Chomsky 1965: 3-4)

is a corollary or extension of a view implicit in Saussure's theory of language. Both of these assumptions have been questioned by linguists within the last twenty years.

This view of language change as consisting of the replacement of one discrete state by another is practically identical with the way the generativists treat the problem of linguistic change, in a theory enunciated in the first place by Chomsky & Halle in a book of enormous importance and profound insights which only the most foolhardy would wish to disparage, and subsequently by authors like Elizabeth Traugott and above all Lightfoot. Change is seen as taking place between one generation and another of speakers, with the parents having Grammar₁, and children Grammar₂, which represent different competences not necessarily reflected in different outputs, though to what extent it is possible to assume that different inputs have identical outputs I do not intend to discuss at present. Successive grammars are characterized above all by restructuring, triggered off by rule additions or rule insertions, which modify the preceding grammar. This is how Lightfoot puts it:

a sudden cataclysmic, wholesale restructuring of the grammar, whereby the exceptionality is, in a sense, institutionalized and the derivational complexity eliminated at a stroke.

(Lightfoot 1979: 122)

It is interesting to note that this «catastrophic» view of language change seems to pass from Saussure via the American structuralists right down to the generativists. This is how Hockett had put it some twenty years earlier:

Sound change itself is constant and slow. A phonemic restructuring, on the other hand, must in a sense be absolutely sudden. No matter how gradual was the approach of early ME /ae/ and /a/ towards each other, we cannot imagine the actual coalescence of the two other than as a sudden event: on such-and-such a day, for such-and-such a speaker or tiny group of speakers, the two fell together as /a/, and the whole system of stressed nuclei, for that particular idiolect or idiolects, was restructured.

(Hockett 1958: 456-57)

Hockett's view of how language functions, and therefore also of how it changes, is clearly not identical with that of Lightfoot, for whom the restructuring process does not take place within the idiolect, a concept rejected by a later generation of linguists, of the individual speaker, but between different generations of speakers. What seems to me significant is the way both scholars talk of a sudden, brusque change, the locus of which for Lightfoot is the speaker's competence, implicitly opposed to the gradualness in his performance, whereas for Hockett phonetic change may be gradual, but once we talk in terms of a system, in other words of *phonemic* change, we must conceive of this as being sudden and abrupt.

What one could roughly describe as the «sociolinguistic» view of language change goes back some twenty years to the 1968 Lehmann and Malkiel volume, and above all to the seminal contribution by Weinreich, Labov and Herzog contained in it. This represents a major reassessment of some of the most important issues in historical linguistics and above all of the theoretical status of the discipline itself. Weinreich *et al* initiated an ongoing process, for more recent studies continue to deal with the theoretical issues involved, to which have been added certain new ones, like the problem of explanation and predictability,

which I intend to return to briefly towards the end of this paper. Weinreich *et al* sum up the state of the art at the time of writing in these words:

It would not be unfair to say that the bulk of theoretical writing in historical linguistics of the past few decades has been an effort to span the Saussurian dilemma, to elaborate a discipline which would be structural and historical at the same time.

(Weinreich, Labov & Herzog 1968: 98)

One fundamental insight is certainly their concept of «orderly heterogeneity», a direct result of Labov's own studies in New York City, Martha's Vineyard and later on of Black English Vernacular. Hopefully the thesis espoused by the authors is accepted wisdom today, but their paper is still of profound interest. No less stimulating in a way is Labov's 1974 paper, with its discussion of the now famous *meat/mate* merger, that is to say *apparent* merger, which has been taken up by later authors like Milroy & Harris. Just as no modern dialectology is possible without the insights provided by sociolinguistics (a label Labov himself rejects, since for him all linguistics is necessarily social), so historical linguistics today owes a great deal to the work of the sociolinguists. Romaine, herself an outstanding sociolinguist, applies these insights in a study of the relative clause in Middle Scots, or, it would probably be more accurate to say, uses this to exemplify her thesis. John Harris observes:

If a name had to be given to the relatively youthful tradition..., then I suppose *socio-historical linguistics* (Romaine 1982) would be as concise as any. Briefly put, the main aim of this tradition has been to integrate the fields of historical linguistics and sociolinguistics. The latter term can be taken here to refer loosely to «variationist» approaches to linguistic analysis; that is, to studies which focus on the variable and dynamic aspects of language, in contrast to the categorical and essentially static orientation of modern theoretical linguistics... Although solid real-time evidence of how change proceeds is still lacking, we now have, thanks to variation studies, considerable evidence from

«apparent time»; that is from socially and geographically differentiated variation which can be taken to reflect temporal variation. Much of this evidence confirms the view that sound change involves subphonemic shifting by small increments or across phonetic continua.

(Harris 1985: 1-2)

Lyons in his *Foreword* to the British edition of Labov's 1972 volume refers to the validity of the synchronic-diachronic opposition as well as to the assumption, shared, as we have seen, by Saussure and the generativists, of a homogeneous stable form of the language. Both these concepts are being undermined by the type of research associated with Labov and those working in his wake.

As long as we are working on the macroscopic scale in historical linguistics, establishing systematic correspondences between successive states of the «same» language that are separated by intervals of several generations, the distinction between synchronic and diachronic linguistics is theoretically justifiable. So too, it may be argued, are the methodological postulates of synchronic homogeneity and diachronic regularity. But the global differences between language-systems with which we operate on the macroscopic scale cannot be satisfactorily explained in terms of successions of instantaneous transitions from one homogeneous and stable, or metastable, state to the next in real time. The Saussurean dichotomy of synchronic and diachronic description breaks down completely on the microscopic scale; and the suggestion, which has sometimes been made by structuralists, that by taking smaller and smaller time-intervals between synchronic states of the «same» language we can approximate to a progressively more accurate description of the heraclitean flux of language change, is nonsensical. We very soon come to the point where the dialectal and stylistic differences within what we normally think of as a single speech-community at a particular point in time are as great as the differences between two diachronically distinct states of the «same» language.

(Lyons in Labov 1978: xiii)

Thanks to studies in Britain by Trudgill, Trudgill & Goxcroft, James and Leslie Milroy, Harris and Macaulay, to name just a few, we can see the relevance of «variationist» linguistics to diachronic analysis, and via the technique of observing change in «apparent time», used for example by Trudgill, we can almost see language change in progress. On the reasonable assumption that innovation is more likely to originate among the younger generation than among the older, scholars, by observing the same feature at the same point of time in successive generations, have allowed us to gain glimpses on where change begins, how it spreads and what kind of social constraint it is subject to. At this microlevel, as Lyons points out, the synchronic-diachronic dichotomy is no longer helpful, since synchronic variation cannot do without a diachronic or dynamic dimension. It is not enough merely to say, to take what is perhaps the best-known example, that post-vocalic (r) among New Yorkers has a distribution that is clearly correlated to the speaker's social status, sex, style of speech and age, since the last parameter has a dynamic, not a merely static «state-of-the-language» significance, since we can observe the way in which a certain feature is tending to develop, especially if this is taken together with Trudgill's concept of «dialect accommodation» (Trudgill 1986). What is evidently happening is that traditional non-rhotic New York speech, as attested by earlier American dialectologists, is becoming rhotic like what used to be called «General American», but has more recently been referred to as «network» (Dillard 1980: 13). It should be pointed out that we are not concerned here with «free variation», an idea that has been around for a long time, though it is now largely discredited, but with what we have seen Weinreich *et al* call «ordered heterogeneity», variation which, if not rule-bound in the strict sense of the term, is amenable to statistical analysis and therefore to a probabilistic predictability, sensitive on the one hand to purely linguistic, i.e. contextual constraints, and on the other to the social parameters we have already mentioned. This being so, it is difficult today to accept Chomsky's ideal speaker-listener as anything but an abstract heuristic device, with no relation to anything that actual-

ly exists in the real world and how language functions in it.

Let me take two examples from the history of English to see what historical change might look like when seen in a synchronic perspective, and in what way the two dimensions, synchrony and diachrony, interlock and can be considered a single process seen from different standpoints. My first example concerns the third person plural personal pronoun in OE which in late ME was replaced by the Old Norse loan form *they* (*their*, *them*). The OE paradigm was

Nom./Acc.	<i>hie</i>
Gen.	<i>hira</i> , <i>hiora</i>
Dat.	<i>him</i>

Simplifying the situation somewhat, we have the following early ME reflexes *hi*, *her*, *hem* of this paradigm. During the 14th century *they* begins to replace the older form *hi*, first in the northern dialects and later gradually also in the south. The point to note is that the different forms are adopted in different dialects at different times: in the English of Chaucer — London English of the last half of the 14th century — we find *th-* forms for the nominative, but *h-* forms for the possessive and the oblique case, but whereas at the same period further north the substitution is already complete in all its forms, (*they* - *their* - *them*), further south the Old Norse forms made much slower progress, so that at any one time we might find anything from a complete *th-* paradigm in the north to a complete *h-* paradigm in the south, with an intermediate situation like that in the English of Chaucer, who generally writes *they* for the subject case, but *her* and *hem* for the possessive and the oblique case. Incidentally, it is interesting to note that modern English has redistributed the OE case forms so that whereas previously we had a common form for the nom. and the acc. and a separate form for the dative, we now have one form for the nom. and a common form for the acc. and dative, i.e. the form I have called the oblique case. One way of explaining this change is of course by saying that ME sound changes affecting unstressed

syllables had drastically reduced the number of distinctive forms and so made a change imperative in order to keep the singular and plural forms apart, but this is true also to a large extent of OE, four centuries and more before the *th*-substitution. We shall briefly return to the question of the functional explanation of language change a little later in this paper, for the present the point I want to make is this: a speaker at the time of Chaucer would have observed a certain dialectal distribution of the forms in question, but this distribution can also be looked at from a diachronic point of view, in the sense that in certain areas the change had already taken place, whereas in others it was either partial (the nom., but not the other cases) or still in its early stages, with a preponderance of *h*-forms. In other words, what synchronically looks like a typical case of dialect variants of the same functional unit, can also be seen diachronically as a change in progress.

My second example comes from the history of syntax and concerns the much studied development of the forms with *do*-support. (For the history of affirmative *do*, see Stein in this volume). At any one point of time we get forms with and without *do*-support in the same text, though as time passes the proportion of one to the other varies. In some cases the choice undoubtedly depends on the main verbs: certain verbs seem to resist *do*-support much longer than others: whereas we get forms like *I know not* until very late, almost up to the present day, with a verb like *see* *do*-support becomes obligatory much earlier. What is certain is that the two types of structures existed side by side for well over a century, but whereas in the case of the third person plural pronoun the synchronic dimension is represented by the distribution of the variant forms between the different dialects, in the case of *do*-support the parameters would seem to be on the one hand lexical and on the other perhaps contextual even dependent on different discourse functions. In any case, to talk of a Grammar₁ (without *do*-support) suddenly and abruptly being replaced by a Grammar₂ with *do*-support seems to be forcing the evidence considerably.

In a rather different way one might say that the synchrony-

diachrony dichotomy breaks down, perhaps «is neutralized» would a more accurate way of putting it, in the Chomsky-Halle analysis of the English sound system. The authors have reversed the traditional model whereby a diachronic process «explains», i.e. accounts for a synchronic rule, for example of the way proto-Germanic *i*-umlaut *mus* - \rightarrow *musiz* \rightarrow *mys* accounts for modern English *mouse* - *mice*. In *The Sound Pattern of English* the authors use synchronic transformational rules to account for successive states of the language, as exemplified by the successive synchronic cuts represented by Hart, Wallis Cooper and Batchelor in their description of the sound system of English. When they talk of «vowel shift», a term in current use in all diachronic accounts of the language, they mean something very different from what the historians of English understand by it. To put it in a perhaps excessively simplified form, the underlying system postulated by Chomsky & Halle presupposes a «continental» system for the the English vowels, from which the «English» system is *derivationally* derived, in other words by synchronic, not by diachronic rules. That this is what in fact happened *historically* in no way determines or conditions the theory, which has a completely different epistemological status to the historian's account of «what actually happened», but it does at the same time also purports to explicate certain historical facts. As Lass puts it:

At this point it doesn't matter particularly whether we look at (6) [a schematization of the Great Vowel Shift] historically, as a mapping in real time from one vowel system to another, or synchronically, as a derivation of Hart's surface vowels from an underlying system like that of ME. In any case we need the same rules to get from one inventory to another.

(Lass 1976: 64)

The generativists' account of change depending on a theory of discrete states is, as I have maintained above, a direct corollary of the Saussurean view of language and its (theoretically) infinite number of synchronic states into which it is possible to

cut up the diachronic continuum. The generativist view is well exemplified by Lightfoot's account of the modals, in which a radical re-analysis of the «pre-modals» transforms these into the new category of modals. The model is one of G_2 , which has the category «modals» replacing G_1 which has «pre-modals», though this still leaves to be explained why in the same writer (to talk of «speakers» in this context makes no sense) we sometimes find the same item being used like an ordinary verb with an object and at others in its purely modal function. One presumes that the answer would be that in this case we are in the presence of a pre-modal, which may have a dual output, and that G_2 really only takes over when these verbs can no longer be used autonomously and therefore followed by an object. If we were to argue that so long as we have a potential dual output, even though this is not actualized, i.e. found in a corpus, we are dealing with G_1 , we might find ourselves in serious difficulties and conclude that many speakers continued to have G_1 long after others had abandoned it, because sporadic non-modal uses of the verbs occur long after the date set by Lightfoot for the re-analysis. I am not, of course, here concerned with establishing just when this took place, but with the model of syntactic change postulated by Lightfoot, a model which, if rigorously applied, I believe fails to give us a convincing and easily visualized picture of how language changes in fact actually occur.

According to a more recent view (Romaine 1982b echoed by Harris 1985) instead of postulating a wholesale re-analysis (words belonging to category X are recategorized as belonging to class Y), we must see lexical items gradually, almost one at a time, transferring from one category to another. This model of gradual lexical diffusion accounts for the fact that recategorization may be, or may at any one point of time be only partial: not all lexical items may have, or may have yet transferred to the new category and in principle there is no reason why the process may not stop at any given stage. Examples of this model of change can perhaps be found most easily in the field of phonology, which perhaps accounts for the variations between long

and short vowels in words like *food* and *good*, which would otherwise require extremely complex contextual (not to say highly improbable) rules in which the vowel is sensitive at the same time both to the consonant preceding and the one following it².

In the field of word formation it is, in the words of Kastovsky 1982, «even more difficult than elsewhere in the grammar, to keep apart competence and performance, synchrony and diachrony», the two sides of the coin being represented on the one hand by lexical expansion by means of borrowing, a diachronic process in which certain lexical items are added to the language, whose use is then with the passage of time codified so that these items are felt to be a stable part of the system, and on the other by such typically syntactic, rule-bound devices as suffixation, compounding and conversion, which clearly belong to the field of synchrony. In another passage the author speaks of the

predictiveness which word-formation rules share with syntactic rules: both are productive in so far as they define an output *in potentia*, and not something which has already been produced by some speaker, i. e. a corpus.

For the purposes of convenience it may be useful to treat the diachronic and the syntactic processes separately, but lexicalization is an arbitrary process not predictable by rule and therefore, like many other diachronic, or more specifically historical facts of language not explicable in terms of the linguistic system itself,

² For example, even a rule which takes into account the parameter $C^v - C^u$ (voiced vs. unvoiced consonants) is inadequate. All four possible combinations $C^v - C^v$ (*boom*), $C^v - C^u$ (*boot*), $C^u - C^v$ (*food*), $C^u - C^u$ (*shoot*) can have a long vowel, but for example *soot* ($C^u - C^u$), *good* ($C^v - C^v$) and *book* ($C^v - C^u$) have short vowels. *Boot* and *book* not only have the same structure, but also the same initial consonant, so that any hope of establishing some sort of rule of vowel length based on contextual constraints seems destined to failure. Another way of saying this is to affirm that the /u:/ - /u./ opposition in English is inherently unstable: the variations observed in a number of British dialects seem to bear out this hypothesis.

but in relation to certain social, economic, cultural factors that operate outside the language, but contribute to modify it at certain points. As Saussure (1960: 85) had put it «changes are wholly unintentional while the synchronic fact is always significant».

The older view that language operates above all by means of binary oppositions in which one discrete and exclusive category contrasts with another, of which the «catastrophic» model of language change of the generativists is the natural development, is giving way to theories in which categories are seen to be much more indeterminate and open-ended. Linguists (e.g. Huddleston 1984, also Matthews 1974 and in certain sections also Quirk *et al* 1985) talk of prototypical vs. peripheral category members, a concept which implies a certain amount of indeterminacy and is therefore only one step removed from what Coates 1983 on modals calls «fuzzy» sets and Romaine 1982b «squishy» categories. In dialectology Chambers & Trudgill 1980 (cf. also Trudgill 1983 and 1986) talk of «fudged» lects as well as mixed lects: the former represent a realization of a particular phoneme (usually a vowel) which lies half way between // and //, whereas the latter refer to alternative realizations of one and the other, in which one member is generally predominant, but where the output in any particular case is never predictable. This is of course entirely in line with the findings that Labov reported in his study of New York English (Labov 1966). As Trudgill points out, such concepts are incompatible with the traditional isogloss representation of dialectal differences, since the very idea of a line which neatly separates a form X from another form Y implies discrete and complementary categories, which we do not find in the real world. Dialectologists have of course known this for a long time, but the use of isoglosses as convenient shorthand representations to illustrate dialect differences has tended to emphasize the clear-cut distinctions rather than intermediate grey areas. If gradualness, indeterminacy, fuzziness, call it what you like, has crept back into linguistic theory via sociolinguistics, there is surely no reason to deny the validity of the gradualist model also along the diachronic axis. The gradualist hypothesis of change is also espoused by Baron 1977 in a book

which seeks to relate historical change to language acquisition. Indeterminacy is seen in a somewhat different perspective in Hankammer 1977 on «multiple analyses», which deals not with the well-known type of structural ambiguity, in which different deep structures generate the same surface structure — Chomsky's «flying planes can be dangerous» has become a classic of this type — but puts forward the hypothesis that the speaker may assign a given utterance to two competing deep structures at the same time: not to one *or* the other, but to one *and* the other. Obviously an analysis like this raises formidable theoretical problems, which would merit further attention, but it is mentioned here to illustrate a tendency and a way of looking at change that seems to cut across some of the basic assumptions that I see as deriving, directly or indirectly, from the Saussurean view of how language functions.

The question therefore arises: are the generativist and the sociolinguistic («variationist») views incompatible, and if so, what is this incompatibility due to? In the generativist model the locus of change is the individual mind, the speaker's competence, a typical mental, psychologically based construct, which gives rise to the theory of a series of separate and discrete grammars—clearly linked to one another by a great number of common features, the novelty of G_2 with respect to G_1 being, to put it in its simplest form, that the former has added one or more rules to the latter; but as a system each is discrete and to a certain extent incommensurable with the other. As a model of change this seems to me to be very similar to Saussure's famous chessmen image. On the other hand, in the sociolinguistic model the locus of change is outside the speaker, in the social structures that serve as a vehicle for the transmission of changes, even though they cannot be said to give rise to them. The problem of the diffusion of linguistic change, what is frequently called the «actuation» problem, is admirably discussed in the Milroys' 1985 paper, in which they demonstrate the pertinence of the concept of social networks in relation to linguistic innovation. In so far as variation in language reflects variations in social structures, language loyalty being a very potent factor in social co-

hesion and identification, the sociolinguistic model functions admirably, but it clearly cannot tell us *why* change should operate in a certain direction, shall we say why certain (social or regional) dialects should have vowel closure at a certain point, or why consonant lenition should take place, or any other type of change one can think of. In a sense then the adherents of the generativist and of the variationist model of change seem to be talking about different things. For Lass 1980, and perhaps more particularly 1987, the locus of change is not the speaker, though he is of course the vehicle of it: speakers seem, he observes, «rather like Tolstoy's 'little men', caught up in great historical currents whose import they're unaware of, but who nevertheless play their 'ordained' parts in the larger design» (Lass 1987: 162). To him language, and in particular language history, is an autonomous object which exists independently of the speaker: «In neither the Saussurean nor Chomskyan vision can history be an independent object that transcends the speaker — since the enterprise is focused totally on him, not his language» (Lass 1987: 156) and «to look for explanatory models in individual (or even social) psychology when the phenomena are patently not individual, social or psychological is counterproductive» (Lass 1987: 172). This leads on to two final connected points, which I can deal with only very briefly here. What is the status of explanation in historical linguistics (indeed, in linguistics *tout court*) and is change in any way predictable? The former problem is dealt with at some length in Lass 1980, in which his answer to the question of whether any explanation in diachronic linguistics is possible is a most decided no, and though many of his arguments have been accepted as valid by his critics, the consensus seems to be rather (e.g. Romaine 1983) that Lass's requirement of deductive nomological explanations is much too strong a hypothesis and inappropriate to an object like language. The explanations usually offered by historical linguists may therefore not pass muster with Lass's very rigorous requirements, may not be «explanations» in his sense at all, but they are useful *ad hoc* devices that help us to understand what in effect happened and how and in what contexts, linguistic

or social, a certain change caught on or failed to catch on. The functionalist model of language change has an old and honoured history, and in our own times perhaps the most forceful statement of its basic premises is found in Martinet 1955, but cf. also Samuels 1972 and his controversy with Lass in the 1987 volume. The extreme teleological view has recently been put forward by Vennemann for whom, to put it rather crudely, all change is change for the better³. At the other extreme we have Lass's absolute skepticism as regards the value of the functional approach: it seems to me that he has a pretty strong case when he argues with his usual verve that the functionalist hypothesis sometimes works, in which case we get avoidance of homophony and similar phenomena, but at other times it patently fails, so what we get is mergers, collapsing of morphological markers and syntactic ambiguity. It is of course true that in the long run languages sort themselves out, as it were, in one way or another, but a theory which has so little predictive power, in which we never know exactly when it will apply or when it will fail, does not seem to me to add very much to our understanding of the mechanism of linguistic change. Let us take a simple, rather obvious example: the wholesale collapse of the OE morphological system, at least as far as the noun and the adjective are concerned, can hardly be seen as being functional, though it clearly had profound repercussions on the whole of English syntax. It would be difficult to deny that the decay of OE unstressed syllables led to, or at any rate contributed to the decay of morphological markers, which in its turn affected the syntax of the language, making English, among other things, a rigorously SVO language. This may well be part of a very long-term drift (cf. Vennemann 1975, but the Sapirean concept of «drift» is of course also part of Lass's hypothesis), but it is difficult to see how this can be described as being «functional» in any

³ This theory was expounded by Vennemann in a paper entitled «Language Change as Language Improvement» read at the Pavia Conference mentioned in Note 1. I do not know whether he has published an earlier version of this theory anywhere, if so I apologize for my ignorance.

meaningful sense of the word. The weak functionalist hypothesis, as expounded for example by Samuels 1987, claims no more than that the language takes remedial action, but even that is difficult to maintain given the large number of homophones in English, which can of course be disambiguated in context, but this is hardly an argument for the language re-establishing an equilibrium to remedy the damage done by change which has obscured vital distinctions.

A final word about predictability: this is a subject that has attracted the attention of linguists in recent years, so that reference to Lass 1980 and 1987 and to Aitchison 1987 and this volume might be considered sufficient. I certainly do not intend to discuss this extremely controversial subject, which comprises not only the question *if* predictability is a viable hypothesis in linguistics, but also exactly what meaning one would wish to attach to the term, in case of a positive answer. As Aitchison points out, the negative arguments on this point are much more powerful than the positive ones, particularly if they are toned down from «never» to «hardly ever», i.e. certain changes in language are highly unlikely, a plain empirical fact which we all incorporate into our descriptions: most linguists would probably be surprised and disconcerted to encounter an innovation like [x] > [m] or a rule which says «reategorize all transitive verbs as prepositions». This sort of approach certainly at best makes for a very weak predictability hypothesis, but then one might object, whoever pretended that linguistics is in fact a science, except in the sense in which we talk of economics as being one of the *social sciences*, and it might be observed in passing that economists have probably been considerably less successful in their predictions than linguists. But that is of course entirely by the way. The point it seems to me is that probabilistic predictions are useful explanatory aids in a diachronic analysis: *post hoc, propter hoc* in our case may not be so much a sign of a faulty argument or an acknowledgement of failure, but simply a recognition that what we are trying to do as historical linguists is to relate the past to the present (or at any rate a successive stage of the language), trying to associate observed regularities with what we know about the general principles

of how languages function. *Post hoc* certainly, because of course «we know the end of the story», but what we want to know is how we got there and above all to see whether the direction taken by the story is merely the result of a casual concatenation of circumstances, explicable *a posteriori*, or part of a larger pattern which helps us to understand how, if not why, languages in a certain sense change and at the same time remain essentially the same.

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