

ON EXPLAINING LANGUAGE CHANGE TELEOLOGICALLY*

ARLETA ADAMSKA-SALACIAK

Adam Mickiewicz University, Poznań

1.0. Teleological explanation in historical linguistics has often been viewed with reserve and suspicion, even by those practitioners of the discipline who seem to implicitly rely on it in their attempts to account for language change.

In his book *On explaining language change* (1980), Roger Lass dismissed this kind of explanation as not only inapplicable to linguistics, but also methodologically faulty. The discussion which followed Lass' book has shown, however, that there are linguists (e.g. Itkonen 1981 and 1983) prepared to defend the status of teleological explanation, and that this task is not unimportant, given that, as is commonly conceded, language change — and perhaps linguistic phenomena in general — are not amenable to explanation of the deductive—nomological (hence: D-N) type.

At least two aspects of Lass' position seem objectionable to me: his evaluation of teleology in general as methodologically unsound and his choice of allegedly representative examples of this type of explaining in historical linguistics. Since the specific arguments Lass uses to question the respectability of teleological explanation have convincingly been shown by Itkonen to be irrelevant¹, I shall not deal with them here, concentrating, instead, on trying to show some assets of this explanatory strategy.

1.1. Teleological explanation in the broadest sense is claimed to explain phenomena by reference not to their (efficient) causes, but rather to their effects, goals and functions. Consequently, this type of explaining stands in sharp contrast to most people's idea of what counts as scientific explanation. In particular, it is clearly incompatible with the dominant trend in the so-

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¹ See especially Itkonen 1981.

called Anglo-Saxon metascience (cf. Radnitzky 1968), which is related to the neopositivist ideal of the unity of science and advocates reducing all explanation types to the D-N model. According to this view, explanations which cannot be so reduced — and teleological explanation is a case in point — do not qualify as scientific.

Clearly, then, if we want to give teleological explanation a chance, we must divorce ourselves from this type of philosophy of science and look for some other framework which could accommodate it; such a framework seems to be provided by the so-called continental schools of metascience. These schools appear to recognize the special character of the human and the social sciences, from which follows the need for special explanation types.²

1.2. Teleological explanation was first advocated by Aristotle, whose philosophy ultimately gave rise to what von Wright (1971) calls the Aristotelian (teleological, finalistic), as opposed to the Galilean (causal) view of science. It is the latter, undoubtedly the more popular of the two traditions, that lies at the foundation of contemporary Anglo-Saxon philosophy of science. The tradition was particularly strong after the scientific revolution of the 17th century and reached its climax in 19th century positivism. As is widely known, it was only towards the end of the 19th century that various antipositivistic trends (such as phenomenology and hermeneutics) appeared in European philosophy. These, in turn, contributed to the growth of holistic psychology as well as several brands of structuralism and functionalism in the social sciences, all decidedly non-positivistic in spirit.

Characteristic of the trends mentioned — and perhaps most explicitly formulated in hermeneutic terms — is the claim that the investigation of human phenomena is qualitatively different from the investigation of physical reality. These differences presuppose a mode of explaining in the human sciences which (of necessity) operates *ex post facto*, does not involve prediction and somehow reflects the fact that in those disciplines facts, norms, values, etc., are typically investigated “from within”, being more easily accessible to the researcher, *qua* human being, than phenomena of the external world.

A number of distinctions have been introduced to capture the differences between explanation in the physical and the human sciences, some of the proposals going as far as to suggest that what one is dealing with in the humanities is understanding (*Verstehen*) rather than explanation (*Erklären*). What is especially significant in the present context is that this (e.g. hermeneutic) understanding depends on the notion of intentionality: the scientist under-

takes to understand the *purpose(s)* of an agent, the *meaning* of a sign or symbol, the *significance* of a social institution or custom. In other words, as in teleological explanation, it is goals and/or functions that are sought, rather than (efficient) causes.

Another relevant requirement of both hermeneutics and phenomenology is the postulate of methodological holism, i.e. the demand that not only the parts should be viewed as contributing to the understanding of the whole, but also the whole as indispensable for the proper understanding of each of its parts. This particular idea must presumably be held responsible for the appearance of functionalism and the characteristic explanatory strategies associated with it.

1.3. A few explanation types, which developed in response to the postulates just discussed, bear close similarities to what is traditionally known as teleological explanation. Two of these are: the so-called “rational explanation”, proposed by Dray (1957, 1964) for history, and the so-called “pattern explanation”, first discussed by Kaplan (1964) as an explanation type which dominates the social sciences.

To mention just the salient features of both, the former is founded on the belief that historical explanation does not rely on general laws (which are missing from the domain) and therefore cannot be reduced to the D-N model. Dray proposes to explain historical events by assuming them to be purposive (either deliberative or non-deliberative) actions, undertaken by rational agents, and by trying to display the rationale behind each such action. The historian is expected to reconstruct the agent's motives and beliefs and to show how, granted these motives and beliefs, the action in question was rational, i.e. “the thing to have done (...) rather than merely the thing that is done on such occasions” (Dray 1957: 124). Naturally, more than one course of rational action may be available to the agent in a particular situation, which makes rational explanations *ex post* ones and prevents them from offering predictions. Finally, rational explanation is most appropriate for history, because the historian does not normally ask *why* something happened (what made it happen), but rather *how* it could have happened:

the demand of explanation is (...) met if what happened is merely shown to have been *possible*; there is no need to go on to show that it was necessary as well (...) to explain a thing is sometimes merely to show that it need not have caused surprise.
(Dray 1957: 157; his emphasis)

The second type mentioned, i.e. “pattern explanation”, is a typically holistic model, consisting — roughly — in explaining phenomena by identifying them as parts of larger wholes. The explanatory strategy is to set up a pattern into which the explanandum might fit: the pattern that emerges may in turn

² I am simply assuming that linguistics is precisely this type of discipline, since to argue for it would require — at least — another paper. In any case, historical linguists, irrespective of their views on teleology, tend to agree with this claim (cf. e.g. Lass 1980, Romaine 1981).

be part of a still larger whole, etc. As in rational explanation, prediction is impossible here: none of the relations in the proposed pattern is necessary or exclusive, each can have functional alternatives. A pattern is seldom finished completely, which is due both to the method itself and, perhaps more importantly, to the subject matter: human systems are characteristically dynamic and not free from inconsistencies and ambiguities.

Closely related to the "rational" and the "pattern" models is the functionalist approach in the social sciences, which employs many explanatory strategies — with teleological explanation as the leading type — that were first used in biology, especially physiology. The analogies which the functionally-minded social scientists choose to draw from biology are typically based on the physiological notion of homeostasis and lead to attempts at explaining social phenomena by invoking the notions of system, structure, part-and-whole relations and, above all, function. As in rational explanation and in the more general "pattern" model, here too the emphasis is often on the *how* and not the *why*: to say that certain activities have certain functions is not always the same as to give the reasons for their having been undertaken. What a functional explanation offers can be viewed as

a description of how the results of certain activities help maintain a system. The teleological element in such an explanation is not the idea of a particular result to be secured, but the acceptance of the system to be maintained as an ongoing concern" (Emmet 1958 : 103).

It seems apposite in this context to mention a distinction which has been introduced by Achinstein (1977: 349) in order to capture the differences between various kinds of functions referred to in functional explanations, namely, between *design*, *use* and *service* functions, which specify, respectively, what something was designed to do, what it is used to do, and what it actually does. The three may, but need not, be identical in a particular case. Thus, what was said above about functional explanations sometimes providing the answer to a *how*- and not to a *why*- question applies to functions of the third, and perhaps also of the second, type, while an explanation invoking functions of the first type could probably be thought of as closest to the classical causal model of explaining (telling us *why* something occurred).

It is worth noting that in the study of social phenomena we are often unable to say whether the function something serves is also the function it was designed to serve; frequently, it is clearly safest to stop at identifying something as a service function with respect to some system, without ascribing it to the (conscious or unconscious) action(s) of some agent(s).

So far, I have drawn no distinction between teleological and functional explanation, which is more or less in accordance with what has commonly been practised in both philosophy and linguistics. There are contexts, however, in which distinguishing between the two seems in order: to me, it seemed indis-

pensable for discussing and comparing different varieties of explaining language change teleologically.³

The criterion for drawing the distinction required is the presence vs. absence in the phenomenon to be explained of a purpose consciously entertained by an agent. In 1906, the psychologist William Stern distinguished between what he called *Absichtsteleologie* and *Anlageteleologie*, where the former was meant to refer to situations which involved a conscious striving (*Absicht*) to achieve a certain goal, and the latter — to situations in which an inherent, unconscious disposition or goal-directedness (*Anlage*) played a part. A similar suggestion was subsequently made by Dorothy Emmet (1958), who proposed to distinguish between the "teleology of purpose" and the "teleology of function" claiming, that it was the latter that seemed closer to the Aristotelian notion of teleology:

there is good traditional precedent for thinking of teleology in terms of what nowadays are called "goal-directed activities" in which there is persistence towards a terminal state in spite of deviations and set-backs, but not necessarily conscious envisagement of an objective. (...) Aristotle's interest in processes developing towards states of maturity was (...) a teleology of this kind rather than a teleology of purpose and design. (Emmet 1958 : 51)

2.0. The point I have been trying to make so far is that the interest in teleological explanation is not a symptom of methodological obscurantism, and that the real issue is not whether teleological explanations are admissible, but rather how best to analyze them. Given that there is no one accepted standard of explaining in diachronic linguistics and, especially, that the D-N model is not applicable to the discipline, it seems reasonable to abandon the prejudice about the allegedly "unscientific" character of teleological explanation. This should be facilitated by the awareness that such explanation types as those just discussed — all closely related to teleological explanation — are not only accepted within a certain metascientific tradition, but also commonly practised in numerous disciplines.

I would now like to argue that also in historical linguistics teleological explanations can be, and in fact have been, successfully employed.

2.1. It seems that teleological explanation has found its way into the study of linguistic change in two distinct, though not totally unrelated, ways. On the one hand, many linguists seem to have arrived at it "empirically". Certain changes simply appear to "make sense" when viewed teleologically: given their effects on the language system (or, more commonly, on one of its subsystems), invoking a goal or function to explain them almost forces itself

³ See Adamska 1986, chapters 3 and 4, as well as Adamska 1988.

upon the researcher. The most common types of such changes are those involving the operation of the principle of isomorphism of meaning and form, manifested, for instance, in the elimination of homonymy, preservation of contrast or minimization of paradigmatic allomorphy. Another type of change which is believed to require a teleological explanation of some sort is what Sapir called "drift", i.e. a set of changes distributed over long periods of time and apparently cumulative in some direction.

On the other hand, for many linguists the teleological character of (all) linguistic change is a direct consequence of their views on the nature of language. More specifically, according to this line of thought, language should be regarded as a primarily social institution and language use — as a form of human (purposive, though not always conscious) behaviour. Thus, language change is merely an aspect of language use and, consequently, its effects are claimed to reflect the changing needs and goals of the speakers. This second approach interests me more, since it seems to offer a principled justification for introducing teleology into the explanation of linguistic change. Another reason for my devoting more attention to this particular approach is the fact that the controversy surrounding teleological explanation in diachronic linguistics has so far centred on instances of the first type, i.e. on teleological explanations arrived at "empirically".⁴

2.2. Let me first take a look at the views of a group of German linguists of the 1920s and 1930s, including Wilhelm Havers, Eduard Hermann, Wilhelm Horn and a few others. For them, the teleological character of language change was as obvious as the teleological character of language use: the strength of this conviction may be inferred from the fact that they called themselves *Teleologen*.

Their views were, on the one hand, a reaction to some tenets of the neo-grammarians position and, on the other, a continuation of the ideas of such linguists as Bopp, von Humboldt, Schleicher and Whitney. They also found some general support in the methodological postulates of contemporary holistic philosophy and psychology.

The *Teleologen* were, for instance, familiar with Stern's distinction between *Anlage-* and *Absichtsteleologie* and maintained that of the two only the former was applicable to the study of language. In other words, conscious purpose on the part of the speakers was deemed irrelevant to linguistic change, whereas some sort of immanent goal-directedness was considered crucial for its proper understanding.⁵

⁴ Lass' criticisms of teleological explanation seem to refer exclusively to examples of the first type.

⁵ Incidentally, both *Absichtsteleologie* and *Anlageteleologie* should probably be viewed as subtypes of Emmet's teleology of purpose, the former pertaining to conscious,

The actual application of *Anlageteleologie* to the study of language history consisted in the demand that each change should be analyzed in terms of the purely etiological factors, i.e. the so-called *Bedingungen* (conditions) of change, as well as in terms of the essentially teleological *Triebkräfte* (moving forces), residing in the unconscious and common to all members of a speech community. The only limits to explaining a particular change in this way were, allegedly, those resulting from the paucity of data at the analyst's disposal in a given case.

While the *Teleologen's* *Triebkräfte* constituted the teleological aspect of language development, their *Bedingungen*, i.e. the static conditions, were equivalent to the traditional *Ursachen* (causes) of change. In the writings of this school, *Bedingungen* were always defined as external with respect to language and largely idiosyncratic. They made change possible, providing the environment for it, but did not cause it. Accordingly, they could probably be thought of as the standing (as opposed to the triggering) conditions of change.

The *Teleologen* divided the conditions into those residing in:

- (i) the external form of language (sound material, accent, rhythm, sentence structure, etc.),
- (ii) the psycho-physical nature of man (reflecting the *Volkesseele*, with its logic, thinking, emotions, etc.),
- (iii) the external world (natural, cultural and social environment).

(Havers 1931: 11–122)

The *Triebkräfte*, on the other hand, were defined as universal and active, though not obligatory, i.e. as something closer to the necessary than to the sufficient conditions of change. Their universality was believed to spring from the "Gleichgesetzigkeit der menschlichen Seele" (a postulate of contemporary "understanding" psychology), in which they resided. This was at the same time the reason for the unpredictability of their operation: as the spiritual forces of man, they could not be subject to any "natural necessity".

The notion of the *Triebkraft* (or, alternatively, *Tendenz*, *Streben*, *Disposition* or *Potenz*) goes back at least to Bopp, who distinguished between the tendency towards clarity of expression (*Deutlichkeit*) and the tendency towards economy or ease (*Bequemlichkeit*) (Slotty 1935). Later, Otto (1919) differentiated between the tendency towards beauty (*Schönheit*), clarity (*Klarheit*) and ease (*Bequemlichkeit*). The *Teleologen* elaborated on these distinctions. Thus, Havers' *Triebkräfte* fall into the following six categories (Havers 1931: 144–84):

- (1) the tendency towards clarity or concreteness (*das Streben nach Anschaulichkeit*), e.g.:

the latter to unconscious purposes: the difference between the two does not consist in the presence versus absence of *purpose and agent* (as in Emmet's dichotomy), but rather in the *degree* of the agent's *consciousness* of the purpose.

- (1) origination of grammatical categories
 - (ii) preservation of forms
 - (iii) avoidance of homonymy;
- (2) the tendency towards emotional discharge (*das Streben nach emotionaler Entladung*), e.g. divergences from the norm in the area of syntax and vocabulary;
- (3) the tendency towards economy of effort (*das Streben nach Kraftersparnis*), manifested in two antagonistic tendencies: towards the (syntagmatic) economy of production and the (paradigmatic) economy of perception;
- (4) the tendencies of order (*die Ordnungstendenzen*), e.g. analogical regularization;
- (5) the tendency towards beauty of expression (*das Streben nach Schönheit des Ausdrucks*), e.g. striving for rhythm, avoidance of cacophony, use and spread of neologisms and archaisms;
- (6) social impulses (*der soziale Triebkreis*), e.g. rules of politeness and manners in speaking.

All these tendencies are present in language at the same time. Due to the already mentioned unpredictability of the activation of a particular tendency, as well as to the fact that many of the tendencies are antagonistic, no exceptionless changes are conceivable to the *Teleologen*.

This conceptualization of language change seems rather close to what, some forty years later, was greeted by many as a novel claim, namely, to King's (1975) arguments in favour of the "multiple causation" of change. The difference between these and the *Teleologen's* postulate that change is typically a result of the operation of several conflicting (and explicitly named!) tendencies is that the latter seems to be a more systematic proposal, following from the general view of language adhered to by its authors, while the former springs from an essentially "negative" empirical observation (that we are unable to discover *the* cause of language change).

Regrettably, the *Teleologen's* work seems to have fallen into almost total oblivion, due, presumably to the advent of structuralism and to the rather insignificant number of practical applications of the model by the *Teleologen* themselves. A violent and, in my view, totally unjustified attack on Havers (1931) by Bloomfield (1934) may have played a part as well.

Thus, although quite a few ideas of the *Teleologen* have subsequently reappeared in the literature, they have seldom been accompanied by a realization of where and when they were first developed⁶.

2.3. Both the character and the later history of the approach to teleology advocated by Martinet are rather different. For one thing, the model was formulated with the explicit purpose of eliminating teleology from diachronic

⁶ A notable exception is Anttila 1978.

linguistics; it has also fared much better than the previously discussed one, securing some followers and provoking discussion.

Given that Martinet rejects teleological explanation and opts for reducing it to the causal model, it may seem paradoxical that some of those currently interested in the subject (notably, Vincent 1978, Lass 1980 and Eliasson 1983) have taken his ideas as their point of departure for analyzing the place of teleological explanation in the study of sound change. I would like to argue, however, that, despite Martinet's own claims, they are not mistaken when they refer to his model of explaining change as a typically teleological one.

Martinet believes historical linguistics to be an explanatory, not a descriptive, discipline. He distinguishes (after Hermann, as he says) between the *Bedingungen* (*conditionements*) and the *Triebkräfte* (*forces motrices*) of change, understanding the terms, however, in a crucially different way: the former are defined as the internal, and the latter as the external factors of change (Martinet 1964 : 19). Both kinds of factors are treated as causes or conditions of change, with the provision that no single factor of either type is on its own sufficient to bring a change about: each time, it is a combination of factors that triggers the process. Martinet argues, moreover, that the internal factors are not "passive", or even "less active" than the external ones, claiming, in effect, that the former are indeed of more immediate relevance to his explanatory strategy.

As far as the question about the (non-)teleological character of change is concerned, Martinet believes (1964 : 18) that it can only be resolved empirically and that the ultimate answer must be negative. He explicitly condemns the pro-teleological views of Sweet, Passy, Jespersen and the Prague School linguists (Martinet 1957 : 272f.). At the same time, however, he declares, in the manner of all teleologists, that language never ceases to change while it is used and that it is the changing needs of its users that cause it to change (1962 : 135, 1984 : 542), since it is primarily a tool in their possession. Nevertheless, instead of making use of this general motivation for change while explaining particular instances of it, Martinet puts forward the allegedly universally explanatory "principle of economy" (1964 : 94). The principle is manifested in phonological change, for example, on at least three levels (Cherubim 1975 : 32):

(i) functionally — as the tendency towards preserving the distinctiveness of phonemes (the principle of maximum differentiation);

(ii) structurally — as the tendency towards maximum integration of phonemes within systems ("pattern attraction");

(iii) physiologically — as the tendency to reduce articulatory effort in accordance with the asymmetry of human speech organs. In connection with (ii), Martinet introduces the notion of "phonological integration" (equivalent to the Prague School notion of "system harmony"), whose interaction with (i) and, especially, (iii) is said to govern the route of change (Martinet 1957 : 270f.).

It is not difficult to notice that these proposals are quite similar to the ideas underlying teleological models of change, which attempt to explain the phenomenon by resorting to some "principle", "tendency" or "trend". To account for the lack of uniformity in the outcomes of change, one must postulate either a number of conflicting tendencies or the operation of the same tendency in different domains of language. Martinet does both: like Bopp and the *Teleologen*, he sees a major conflict between "the expressive needs of man and his tendency to reduce his mental and physical exertions to a minimum" (Martinet 1978 [1952]: 150), as well as many further antinomies, resulting from the different effects of the operation of the principle of economy in the various spheres discussed above.

Martinet's principle of economy can be resolved into two major components as far as phonemic systems are concerned: that of preservation of contrast (i.e. of "useful" phonemic oppositions) and that of pattern attraction (i.e. filling the "holes" in the phonemic system). The two have led to the formulation of two much discussed, but widely used, concepts associated with Martinet: that of "functional load" and that of chain-shifts. Both these concepts, contrary to Martinet's intention, have often been regarded as paradigmatically teleological.

The idea behind the notion of functional load as an explanatory device is simple and intuitively plausible: the greater the functional load of a phonemic opposition, the more resistant the opposition will be to change, and vice versa. In other words, all other things being equal, a phonological opposition which is useful for mutual comprehension will be more stable than one which is less useful (Martinet 1964: 42). The problem with this sort of definition is, of course, that we never know exactly whether "all other things" are (or, indeed, can be) equal; neither do we have any established, non-circular criteria for measuring the functional load of oppositions. What seems certain is that, if the functional load hypothesis is treated as a causal principle enabling us to explain changes deterministically (i.e., among other things, predict their occurrence and outcome), it clearly does not work. King (1967) has shown that examples of changes violating the principle are at least as easy to find as examples of changes conforming to it⁷.

⁷ In order to do that, King resolved the "strong" version of the functional load hypothesis into three empirically testable claims:

(i) "the weak point hypothesis": sound change is more likely to start with oppositions bearing low functional loads (or: a phoneme of low frequency of occurrence is more likely to be affected by sound change);

(ii) "the least resistance hypothesis": if there is a tendency for x to merge with either y or z , the merger will occur for which the functional load of the merged opposition is smaller;

(iii) "the frequency hypothesis": if an opposition $x \neq y$ is destroyed by merger, that phoneme will disappear in the merger for which the relative frequency of occurrence is smaller (King 1967: 194f.).

Significantly, however, King's invalidation of the "strong" version of the principle has not led to the rejection of the notion of functional load. Many linguists still seem to find it intuitively correct, which only makes sense on condition that they interpret it not in causal, but in teleological terms. If the functional load hypothesis is taken to refer not to a (deterministic and empirically falsifiable) law of change, but rather to one of the tendencies manifested in it and constantly interacting with other tendencies, it is, I think, unfair to stigmatize it as "based on a simple and rather sloppy metaphor, with no empirical support" (Lass 1980: 93), unless one is prepared to reject (on the same grounds, i.e. due to their not being exceptionless laws) all the tendencies and regularities that have been observed in language change.

Anyway, it is not the notion of functional load, but that of chain-shifts which has provided the starting point for some recent discussions on teleology in language change among linguists who regard the problem as an empirical one.

The introduction of the notion by Martinet seems to have been intended as part of the attempt to eliminate teleology from the explanation of apparently purposeful changes by representing such changes as sequences of events of the causal type. A *drag-chain* is said to occur when sound B changes into C and this causes sound A to change into B; a *push-chain* is involved when a sound A changes into B, causing the original B to change into C. The respective teleological versions of these definitions, which Martinet seeks to avoid, can be formulated as "A moves into the place in the phonological system vacated by B *in order to fill it*" (pattern attraction), and "B changes into C *in order to avoid clashing with the incoming A*" (preservation of contrast).

One must ask at this point whether Martinet's reduction is successful: does one really get rid of teleology by claiming that, in the first case, the shift $B \rightarrow C$ "drags" A into B's position and, in the second one, the shift $A \rightarrow B$ "pushes" B into the position of C? What seems clear from the start is that changes of the type discussed cannot be explained causally, if by a cause one understands either a sufficient or a necessary condition. To take sufficient conditions first, it is simply not true that $B \rightarrow C$ always brings about $A \rightarrow B$ (i.e. some slots in the system do remain empty), or that $A \rightarrow B$ always brings about $B \rightarrow C$ (if this were the case, mergers would not occur). Similarly, when we consider necessary conditions, it is not true that A will *not* change into B unless B has first changed into C or that B will not change into C unless A has first changed into B.

Thus, if the mechanisms proposed by Martinet under the labels of drag- and push-chains are causal ones, they are causal in some special sense of the word, since the preceding change in the shift is neither sufficient nor necessary for bringing the following one about. Either the first change is merely *one* of the factors which contribute to the occurrence of the second one (an interpretation

Martinet would probably have endorsed) or the relation between the two is one of pure contingency. Needless to say, the former interpretation is distinctly related to the strategy behind teleological explanations: the linguist assumes the operation of certain tendencies in change (in this case: the tendency towards phonological integration and the tendency towards preservation of contrast) and evokes them to account for attested changes; if a given change does not manifest a certain tendency, or if its result is the reverse of the expected one, the hypothesis about the existence of the tendency is not falsified, since the tendency was never claimed to be either unique or exceptionless (otherwise, it would not be a tendency but a law).

One is forced to conclude that Martinet's attempt to eliminate teleological overtones from the explanation of apparently purposeful sequences of changes proves unsuccessful; any explanation referring to drag- or push-chains remains teleological, despite the use of "causal" terminology.

In 1978 Vincent made an attempt to approach the problem of chain-shifts from the perspective of the ontology of change. He wanted to find out whether sound change exhibited teleology of purpose, believing that the answer to this question consisted in establishing whether there was (conceptually) a type of change that must be considered purposeful and, secondly, in finding reliable examples of this type of change among the phonological histories of natural languages.

Drag- and push-chains could be potential candidates for the paradigmatically teleological change type needed. However, according to Vincent's assumptions, the former do not qualify as teleological, because, under Martinet's gradualist view of change, the realizations of phonemes "scatter around local frequency maxima, with their range of variation being conditioned by other phonemes in the vicinity" and, consequently, if /B/ for some reasons shifts to /C/, the realizations of /A/ "will be able to stray unmolested" into the area of the original /B/, resulting in a gradual shift along the chain (Vincent 1978 : 411).

This interpretation agrees with the one suggested above, namely, that one should treat the first change in the shift as merely increasing the chances of the following one(s) taking place. Naturally, from the point of view of the effects of a completed drag-chain shift on the phonological system, we can still talk about the teleological aspect of the shift in the sense of the teleology of function (a possibility Vincent acknowledges on p. 413).

As for push-chains, Vincent comes to the conclusion that, were such changes to be found, they would indeed qualify as purposeful, since "the only reason B shifts to C is to avoid clashing with the incoming A" (1978 : 411). However, in Vincent's opinion, there are no clear-cut examples of push-chains, which leads him to maintain that, until a well-documented case of a push-chain is found, he has the right to deny that there can be teleological changes.

As a reaction to this challenge, Eliasson (1983) presents an apparently undisputable instance of a push-chain from the history of the Central Scandinavian vowel system. He thereby gives an affirmative answer to the question: "Is sound change teleological?" by adducing what both he himself, as well as Vincent, would probably consider to be "empirical evidence".

It is my contention that Eliasson's example cannot decide the question of the presence or absence of teleology in phonological change, not because it is not convincing, but rather due to Vincent's misplacing of the problem. Examples such as this one can at best be treated as supportive, never as decisive, evidence. It is the linguist's theory of language and of language change that must provide an answer to the question we are concerned with; if the answer is positive, the theory must further specify what sort of teleology is involved in language change, i.e. what the purpose(s) or function(s) of change can be and where to look for them. For those who believe in the teleological character of all linguistic change, Eliasson's example may serve as a nice illustration of the fact that some changes reflect the operation of certain tendencies (here: the tendency towards preservation of contrast) fairly clearly, but it is not a "proof" that sound change is teleological. It would probably be still less of a proof when examined by those who *a priori* deny the teleological character of change: any such example can in principle be dismissed as a result of pure chance, the more so that, under Eliasson's view (and he adopts that exposed in Campbell and Ringen 1981), the purpose — whatever it is — of the shift discussed must, rather naively, be taken to reside "in the language", with no further qualification (i.e. no mention is made of speakers' preferences, the sociolinguistic context, etc.).

The above presentation of Martinet's proposals and of the debate they stimulated shows, I believe, that this particular attempt to reduce teleology has failed: the explanatory strategies offered remain teleological, despite the superficial elimination of teleological language (*in order to*, etc). Accounts of change which resort to the notions of functional load and chain-shifts, whether regarded as reflecting the "reality" behind the change or as purely heuristic devices, are inevitably teleological.⁸

2.4. Having discussed one teleological approach to change which has practically been forgotten, and one which is fairly well known, but not recognized as teleological by its author, I would like to proceed to the discussion

⁸ This seems related to the fact that Martinet's views on language change are not very different from those of the *Teleologen* or, particularly, from those of Prague School teleologists. Thus, he recognizes virtually the same tendencies of change as the Praguians did, even though the terminology is occasionally changed (their "system harmony", for instance, corresponding to his "phonological integration"); he also emphasizes the role of the internal structure of the phonological system and its optimum states in the occur-

of a model which is both explicitly teleological and, as I want to argue, worthy of being adopted. Unfortunately, it has not yet found many practitioners, despite being highly praised for its theoretical merits. The model I have in mind is that of "abductive" and "deductive" change, proposed by Henning Andersen in 1973.

According to Andersen, historical phonology ought to "explain fully and explicitly how sound change takes place and why" (1973 : 783), the question "why" being equivalent for him to the question "to what end?" (1978 : 20). Since it is the phonological system (rather than, e.g., gradual and imperceptible phonetic alterations) that is believed to be the single most important factor giving rise to change, accounting for change presupposes a detailed study of the structure of this system.

The phonological system is a semiotic one; it organizes signs into paradigms and syntagms. The "signs" in this case are phonemic feature values (plusses and minuses), each of them forming with its opposite a minimal (binary) paradigm. The feature values or, traditionally, terms of phonological oppositions, are obligatorily combined into syntagms, with the phoneme being the smallest such syntagm. More precisely, the phoneme is a simultaneous, hypotactic syntagm, in which any higher-ranking immediate constituent is of more central importance than the corresponding lower-ranking immediate constituent (e.g., depending on the language, the feature of gravity may rank above that of flatting, or vice versa).

All this has consequences for the understanding of phonological change. The terms of phonological oppositions, although themselves relational and asynthetic (having "otherness" as their only meaning), are manifested by simultaneous, concrete acoustic properties, which often gives rise to ambiguities similar to those presented by synthetic morphemes. Different resolutions of these ambiguities may ultimately lead to changes going in different directions (the so-called "bifurcating" changes).

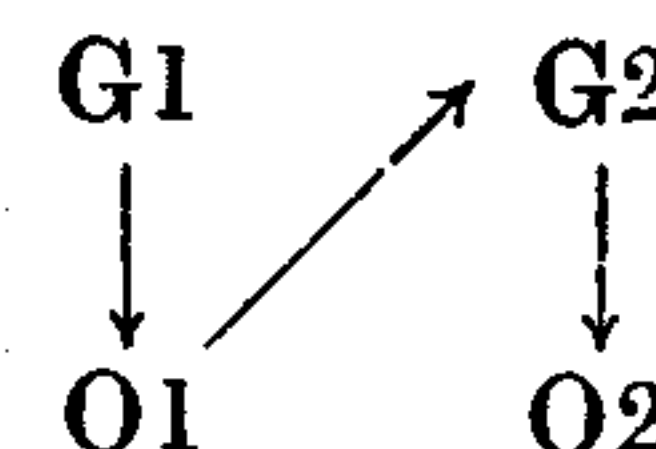
In Andersen's model of phonological change (and of linguistic change in general), attention is focussed on the different sources and types of innovation made by language learners. Change is regarded as an essentially social fact in that it amounts to the codification of a particular innovation through contact among speakers.

The phenomenon of linguistic innovation in the phonological structure, symbolized by different underlying representations, can only be explained on the basis of ambiguities in the corpus of utterances from which the new grammar is inferred. Observed phonetic innovations, in turn, can only be explained as manifestations of the phonological structure of the language.

rence and direction of change, with speakers being at best peripheral (cf. Lass 1980 : 121) and, finally, he elevates the inherently teleological principle of minimum effort to the position of an omnipresent factor which gives change direction.

Innovations of the former type are covert, of the latter overt. The two types of innovation are termed abductive and deductive respectively.

In the model, language change is neither direct change between the linguistic outputs of consecutive generations of speakers nor "grammar change" in the generative sense; rather, "the verbal output of any speaker is determined by the grammar he has internalized and (...) any speaker's internalized grammar is determined by the verbal output from which it has been inferred" (1973 : 767), as in the following:



The unobservable abductive innovations, which are inferrable only from their deductive consequences, result in correspondences between the learner's grammar (G2) and homologous elements of the grammar of his models (G1), which is not directly accessible to the learner, but from whose output (O1) G2 was inferred. Deductive innovations manifest themselves as correspondences between the output O2 of the learner's grammar G2 and the output O1 of his models' grammar G1.

Thus, abductive innovations arise mainly in the process of language acquisition and are motivated by the difficulties of analysis of the acoustic signal, while deductive innovations find their motivation in the newly formed grammar G2, whose constitutive relations they make manifest. Consequently, each deductive innovation must be preceded by an abductive one.

As the names of the two innovation types suggest, language acquisition, in which most change originates, is claimed to be made up of processes which are basic to all activities of the human mind, namely, induction, deduction and abduction.

The language learner's data consist of the verbal outputs of his models ("result" in terms of logical inference), which, combined with the knowledge he already has about this language (set of general "laws"), serve him as premisses for making a hypothesis about what the grammar of his language may be (inferring the "case"). This is an instance of the type of reasoning which Peirce called abduction. The other two types of reasoning serve to test the validity of the learner's hypothesis, both by checking new utterances of the models against the relevant parts of the newly formed G2 (induction) and by producing new utterances in conformity with G2 and observing other people's reactions to them (deduction).

The major type of innovation that can arise in the course of these processes results from abductive inferences: since the learner's goal is simply the formation of a grammar that conforms to his data (rather than a grammar identical

to that of his models), nothing prevents him from making hypotheses that lead to underlying representations which differ from those of his models, provided that both sets of underlying representations produce identical outputs. It is important to realize that both innovating and non-innovating abductions are equally possible.

Andersen distinguishes two major types of change on the basis of the different configurations of abductive and deductive innovations which make them up. The simpler of the two is *evolutive change*, made up of a single abductive innovation followed by deductive ones. Evolutive change is fully explainable in terms of the phonological system which gives rise to it. The other type is *adaptive change*, which, unlike evolutive change, can only be understood with reference to factors external to the linguistic system proper: motivation for it comes from the communicative system, i.e. either from changes in the material or spiritual culture of a community or from language contact. In this type of change, the speaker adjusts his pronunciation to the new norms in those respects where the norms cannot be derived from the phonological structure he has set up. Any adaptive change involves two abductive innovations: the first one introduces an ad-hoc adaptive rule, which ensures the desired conformity between the speaker's output and the corpus of utterances embodying the new norms, the second one occurs when a new generation of language learners takes the observable output at face value and formulates a new grammar on its basis. As in evolutive change, the abductions are followed by deductive innovations, one type of which involves the gradual loss of the adaptive rule(s) and the other — the inclusion in the phonological structure of rules which are consistent with its defining relations, rules that bring about neutralization and variation changes.

After this very sketchy presentation of Andersen's model, let me concentrate on its teleological aspects. Like for the *Teleologen*, for Andersen too change is conceived of as a necessary correlate of the functioning of language; thus, the teleological character of change reflects the teleological aspects of language acquisition and use.

The goal of learners in the process of language acquisition, during which evolutive change occurs, can be stated most generally as the formation of a grammar conforming to the observed data. Within the model, language acquisition is an entirely goal-directed process, based on the principle of feedback; cf. the following:

(the) cyclical application of induction and abduction, by which a grammar is built up, is a goal-directed process, analogous in all essentials to the teleological models described by Miller, Galanter and Pribram (1960 : 29 ff.), and termed TOTE units (for TEST-OPERATE-TEST-EXIT). Induction corresponds to the Test phase, abduction to the Operate phase; the process is repeated until induction provides no further cause for abduction and the Exit phase is reached. (...) If his analysis is

deficient, the learner's utterances may cause misunderstandings or elicit corrections, which may prompt him to revise his analysis. This part of grammar formation, too, is a goal-directed process describable in terms of the TOTE model. Deduction corresponds to the TEST phase. But the Operate phase which follows when deduction produces negative feedback, is itself a TOTE unit, consisting of induction-abduction-induction (...) In other words, the process of deductive testing includes induction, and can be described as a TOTE unit with an embedded TOTE unit (...) (Andersen 1973 : 776 f.).

The teleological character of adaptive change seems equally obvious, its goal being the achievement of some sort of conformity of one's linguistic output with the output of those one identifies with.

Andersen is wholly aware of this teleological orientation of his model and explicitly describes it as such, availing himself of the distinction between the teleology of purpose and the teleology of function, both of which he believes to be relevant to the explanation of linguistic change.

Of the three major types of innovation, namely, abductive innovation in evolutive change, primary abductive innovation in adaptive change, and deductive innovation, the first one has neither purpose nor function:

The learner who formulates a grammar on the basis of the verbal output of his models has as his goal a grammar that will produce that output. (...) The source of abductive innovations is to be found in distributional ambiguities in the verbal output from which the new grammar is inferred. These ambiguities are causes of change, however, only to the extent that they do not prevent the abductive innovations any more than they occasion them. They are necessary but not sufficient conditions for the innovations to which they give rise (1973 : 789).

It is different with the other two types of innovation:

The primary abduction by which a speaker adapts his pronunciation to new norms is an innovation which is not only goal-directed (...), but purposeful. Whether or not a speaker is conscious of his attempt to adjust his pronunciation to that of others, he can be said to modify his phonology with a definite intent. Here, then, one may speak of teleology of purpose, and define the final cause of the innovation as the intended new pronunciation.

Deductive innovation, the process by which learners of a language produce acceptable deviations from the received norms, involves a teleology of function in two distinct ways. Since it is produced deductively, any acceptable deviation fulfils the important function in the process of grammar formation of validating the learner's analysis of the stylistic values (...) or phonemic relations (...) with which the deviation is consistent. Besides this validating function, (...) this addition of a new (rule) serves to make more explicit the relations that constitute the phonological structure (1973 : 789 f.).

It should probably be added at this point that, although Andersen's model

has not found many followers, the author himself had demonstrated convincingly (notably in his 1973 analysis of a consonantal change in Czech) that the application of the model can lead to interesting insights.

3.0. The three teleological approaches to diachronic linguistic explanation which I have concentrated on are by no means isolated. One needs only to think of the writings of Whitney, Jespersen, Sapir, the Prague School and, more recently, Itkonen, to realize that teleological accounts of linguistic change have a long and rich tradition in the study of language history and that they have been proposed by some of the most distinguished practitioners of the discipline.

Moreover, although we may not immediately perceive them as such, most taxonomies of change are also implicitly teleological: in classifying a change as a split or a merger, a phonologization or a dephonologization, not to mention such widely used terms as assimilation or dissimilation, what we take as our criterion are the effects or functions of the change in question, and not its causes.

Given its unquestionable popularity, what are the merits of teleological explanation in our discipline? Is it merely a last resort, a poor substitute for what we cannot achieve in the study of language change, i.e. for "proper" D-N explanation? I believe not, though with some reservations.

Let us first look at the clear advantages. Conceding, as I think one must, that language change can have no goals apart from those of language users, what is it that teleological explanation of change has to offer?

One obvious answer is that it captures very well the essence of adopting an innovation in that it specifies the speakers' sociolinguistic goals which are realized through the act of adoption. It must be kept in mind, though, that it is only the mere *fact* of adoption of a particular variant, and not the *content* of what is adopted, that is thereby explained.

Another process which is undoubtedly amenable to a kind of teleological explanation is language acquisition. Language acquisition is a teleological process, which allows innovations to arise in its course, though the innovations themselves are neither purposeful nor functional. As Andersen has shown, teleological explanation in this case helps us understand *how* innovations can occur, but it does not — and cannot — explain *why* a particular innovation should have arisen exactly where and when it did.

This is about all that is wholly unproblematic about teleological explanation in historical linguistics, and it agrees with the reasonable view that language change itself is not a purposeful phenomenon, but occurs as a correlate of the purposeful processes of language acquisition and language use, and is, consequently, susceptible to being explained in a teleological way.

It is clear, none the less, that teleological explanations have been offered for

many more varieties and aspects of change than those just mentioned. One area in which they have gone unchallenged comprises sets of long-term changes or drifts. Identifying a sequence of changes as making up a drift (i.e. as sharing a common goal), as well as accounting for an individual change by declaring it to be part of a drift, are clear instances of what I discussed at the beginning under the label of pattern explanation. The process of postulating a drift (which, of course, is identifiable only retrospectively) resembles the practice of the social scientist who looks for regularity and structure in his data, trying, as it were, to put together the scattered pieces of a jigsaw puzzle. Moreover, as in the case of the social scientist's procedure, the more extensive the emergent pattern, i.e. the more elements seem to fall into place, the more "real" the pattern (here: drift).

Needless to say, what applies to the postulation of drifts holds true also of the introduction by linguists of such terms as the First Germanic Consonant Shift, the Great English Vowel Shift, etc. These are more than just labels attached to sets of changes: without them, the changes would simply not be perceived as having something in common. In addition to being instances of description, such labels are therefore instances of interpretation, this particular type of interpretation being based on the same methodological operation as the one underlying the stipulation that certain longer periods in the history of languages manifest various language-specific drifts.

If we stick to the claim that the goals of linguistic change are no other than those of language users, various problems seem to arise. The only way to connect the goals postulated by the linguist for a given long-term change with the goals of the speakers of the language is to maintain, together with the *Teleologen*, Sapir and Itkonen, that what we are dealing with is some sort of "unconscious purposiveness" or "unconscious rationality". In the case of drift, this "unconscious teleology" is invoked to account for the speakers' decisions whether or not to adopt particular innovations, where the decisions are believed to be governed by the general direction of the drift of the language: only those variations which conform to the drift will gain the community's approval and become adopted. According to Itkonen, each such adoption is a collective rational action and linguistic changes in general can also be viewed as such actions:

The linguistic community could be said to act as a 'rationality filter' on innovations and abductions. The end result is that the community as a whole seems to change its language. On closer inspection, however, this result turns out to be an aggregate of innumerable individual actions performed under social control (Itkonen 1983 : 211).

However reasonable this may sound (and however short we may be of an alternative explanation), I am not sure we have the right to resort to "unconscious teleology" as long as there is not much independent evidence suggesting

that something like this really is operative in language change. What is unquestionable with reference to the act of adoption of a given innovation — irrespective of whether the change in question is part of a larger drift or a single short-term change — is the psychological reality of such (usually unconsciously entertained) goals as the speakers' desire to identify with a prestige group, to stress their local or group loyalties, etc. Thus, "unconscious teleology" may be indispensable for accounting for some social aspects of change, which, as is commonly agreed, form the essence of the act of adoption. This, however, is the only case in which the usefulness of the concept cannot be questioned: the speakers's goals in adopting a given innovation are typically extra-linguistic and are therefore on a different level of description than many of the so-called "goals of change" (such as eliminating empty slots from phonological systems or reducing paradigmatic allomorphy) adduced by linguists in order to explain it. If speakers do also entertain goals of the latter type, no-one has, to my knowledge, argued for it convincingly.

Apparently, all we can do when explaining linguistic changes by reference to such goals and functions is to treat these as "service functions", without claiming that they are also "design functions", i.e. that they were (consciously or not) aimed at by language users. It is, of course, intuitively plausible that the psychological tendency towards order and regularity, for the operation of which we have ample evidence from different areas of human activity (including the preoccupation of linguists with systematizing the effects of their findings!), may also be ultimately responsible for the results (i.e. "service functions") of certain linguistic changes. This, however, still remains to be shown.

According to my "minimalist" view, then, the design function of an adoption is always identical with some sociolinguistic goal(s) which the speakers want — consciously or unconsciously — to achieve; its service function, i.e. its function with respect to the language system, may, on the other hand, consist in increasing the isomorphism of meaning and form, restoring the symmetry of the system and the like. As far as I can see, *a given change may have no service function at all.*

Given the nature of teleological and functional explanations in other disciplines, it is, I believe, perfectly legitimate for a historical linguist to say that such and such a change served such and such a function in a given language subsystem and/or made such and such later developments possible, irrespective of how the change in question got there in the first place, i.e. what the speakers' motivation for adopting it was.

It is easy to see that it would be much more prudent to treat service-function statements as functional *descriptions* of linguistic changes, rather than functional *explanations* thereof. Accordingly, it seems to me that the real controversy surrounding teleological/functional explanation concerns not so much what stands behind the adjectives "teleological" and "functional" as

what we mean by "explanation". As Peirce and Popper, among others, recognized long ago, the distinction between explanation and description is in reality less sharp than is commonly assumed: historical linguistics is certainly not the only discipline where the boundaries between the two are not clear enough. Besides, it is hard to imagine the scholarly community of historical linguists suddenly ceasing to refer to what they do as "explaining language change" and redefining their task as "describing the effects of change", a pursuit almost universally believed to be both less ambitious and less advanced methodologically.

Despite Lass' hostility to teleology, which I certainly do not share, I wholly sympathize with his view (1980 : 100) that one way of approaching the phenomenon of language change might consist in "developing some kind of structure other than explanation as the desired output of historical linguistic pursuits". Since putting this proposal into practice seems a fairly remote possibility at the moment, it is, I think, the other way out Lass noticed, namely, "developing an epistemology making non-deductive explanation respectable", that makes a more realistic goal for diachronic linguists to pursue.

Given the general methodological considerations presented here, as well as the frequency and, arguably, a reasonable degree of success with which teleological explanations have been employed by historical linguists of all persuasions⁹, it is probably fair to conclude that, as far as the second of Lass' proposals is concerned, teleological explanation remains the most likely candidate for the explanatory mode we seek.

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⁹ I would like to think of Hock's 1986 book as indicative of the most recent climate of opinion. The author explicitly admits teleological explanation into the study of language change and, although he does not appear to be concerned about the different varieties of this mode of explaining or, indeed, very specific about what exactly he means by teleology, he certainly seems unaffected by Lass' sweeping criticisms of the notion. I take this to be a welcome sign of the granting of full legitimacy to teleological explanation by practising historical linguistics.

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