CHANGE OF LANGUAGE AS A PROTOTYPE FOR CHANGE OF LINGUISTICS

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1. Historiography of science

For a long time, historiography of science has existed apart from general historiography. The two have been divided by their differential attitudes vis-à-vis the concept of *progress*. It has been—and still is—axiomatic that general history, i.e. the economic and political history of societies, has no goal and, accordingly, exhibits no progress in the sense of approximation to some goal; Hegel and Marx are continually derided for having thought otherwise. By contrast, the history of science seems to have a definite, even if unattainable, goal, namely *truth*, and the change, or indeed the progress, of science is generally understood as continuous, cumulative approximation to this goal.

Popper and his followers seemed to call the cumulative view of scientific change into question, by claiming that older theories are *falsified*, and then replaced by new and better theories. In reality, however, Popper remains fully committed to the cumulative view, as can be seen from the following equations that he accepts: scientific change = scientific progress; scientific progress = increasing 'truthlikeness' (i.e. greater approximation to truth); increasing truthlikeness = greater empirical content as measured in a *theory-neutral* language. According to Popper, then, history of science and general history remain two qualitatively different things that have to be described and explained by qualitatively different methods.

The situation changes with Kuhn, who rejects the view of science approaching truth in a rectilinear, cumulative fashion. A new theory or theoretical tradition may embody different *aims* and *values* than its predecessor did; in such a case there is no theory-neutral language in which the two could sensibly be compared. It is a self-evident fact that the aims and values of any community are subject to historical change. Kuhn is merely saying that science, or the community of scientists, is no exception. It follows that the methods of general historiography become applicable to the history of science.

From among Kuhn's followers I wish to single out Feyerabend and Laudan who, at the present level of abstraction, may be taken to represent the same approach to the history of science. They consider each scientist or group of scientists in his or its own, more or less unique historical situation; and they explain a given scientific change by showing that in the light of the contemporary *norms of rationality*, it was a rational thing to do. (Notice: *a* rational thing to do, not *the* rational thing to do. Scientific changes are never necessary or predictable.)

2. General historiography

In the late fifties and the sixties there was a lively discussion about the nature of historical explanation. On the one hand, positivistic philosophers of science like Hempel and Nagel criticized existing historical explanations for making no reference to deterministic or statistical laws and concluded that historiography is as yet a non-scientific discipline. On the other hand, historians and philosophers of historiography defended the sui generis character of historical explanations and criticized positivists for their aprioristic predilection for physics, expecially classical mechanics.

In this context Dray (1957) coined the term 'rational explanation', which is simply meant to capture the idea that actions by a historical agent are explained by showing that he had *good reasons* for performing them. Showing that there were good reasons for acting in a certain way requires taking into account the agent's goals and beliefs as well as his larger social context. To be rationally explained, actions must fit into their surroundings. Dray insists that rational explanations are not *nomic* explanations, i.e. explanations by *laws*. To understand why an action was done, it is enough to know that it was a rational thing to do. Knowing that there is a regularity of rational actions to 'support' this action does not strengthen the explanation in the least. The situation is rather the inverse one: If we know that in a situation X an action Y is a rational thing to do, then we automatically know that the same is true of *all* situations similar to X and of *all* actions similar to Y.

Explaining an action is presumably a descriptive undertaking. Therefore Dray has been criticized even by non-positivists for using a normative term like 'rational' to characterize his concept of ex-

planation, instead of simply characterizing it as 'teleological' or 'purposive'. However, there is a good methodological reason for Dray's choice of terminology. Rationality is a minimal assumption in the sense that we do not have to explain why people act rationally; it is irrational actions which need a special explanation. In this respect *rationality* plays the same role in the explanation of human behaviour as *inertia* does in classical mechanics. This comparison has been made by Ch. Taylor and Laudan among others.

The concept of rational explanation may be summarized by quoting Davidson (1975:11): 'The cogency of a teleological explanation rests on its ability to discover a coherent pattern in the behaviour of an agent. Coherence here includes the idea of rationality both in the sense that the action to be explained must be reasonable in the light of assigned desires and beliefs, but also in the sense that the assigned desires and beliefs must fit with one another. The methodological assumption of rationality does not make it impossible to attribute irrational thoughts and actions to an agent, but it does impose a burden on such attributions.'

The preceding analysis primarily applies to actions done by individual agents, but it may be extended to actions done by collective agents as well. Here Smelser's (1962) theory of collective behaviour deserves to be mentioned. Collective behavior (which is distinguished from 'conventional', unchanging behavior) is regarded by Smelser as a *solution to a problem* created by what he calls 'structural strain' (or 'dysfunction'). This kind of problem-solving activity may seem awkard or superficial, but it can be shown to possess a rationality of its own. Three things are of special importance here. First, it makes sense to speak of *collective* rationality. Second, it makes sense to speak of *unconscious* rationality. Third, it makes sense to speak of *deviant* rationality, i.e. 'rationality' which makes prima facie non-rational or even irrational behavior understandable.

3. Historiography of language (= diachronic linguistics)

In the fifties and sixties linguists did not participate in the general discussion about the nature of historical explanation. Since then, however, there has been a growing interest in the methodology of linguistics. The concept of synchronic grammatical explanation, especially as practiced in transformational grammar, was much debated in the seventies (cf. e.g. Itkonen 1978a:9.2). The concept of

diachronic explanation is also becoming the object of intense discussion. Here I wish to single out Lass (1980).

Most practitioners of diachronic linguistics seem to entertain the positivistic overall conception of science, according to which nomic explanation is the only acceptable type of explanation: explaining an event or action means subsuming it under a law. To this more or less implicit *theory* of diachronic linguistics Lass opposes three facts about the actual *practice* of diachronic linguistics. First, there are no universal or deterministic laws of linguistic change. Second, such statistical laws of linguistic change as exist merely sum up the changes observed so far; because of this, and more generally because of their very statisticalness, they are too weak to explain the occurrence of any particular change. (For instance, I have tried to describe and explain the change of the declension system from Classical Latin to Merovingian Latin and from Merovingian Latin to Old French (cf. Itkonen 1978b); at no point was it very meaningful or relevant to ask by how strong or weak regularities the changes might be supported). Third, teleological or functional explanations are rather common, but they are post hoc, which means that they are not supported by any laws. In brief, there is a *contradiction* between the nomic theory and the non-nomic practice of diachronic linguistics. I agree with Lass that such a contradiction exists, but I disagree with him as to how it ought to be eliminated. From the non-nomic nature of diachronic linguistics Lass infers that linguistic changes cannot be explained at all. From the same fact I infer that linguistic changes need a non-nomic type of explanation: and *rational explanation is there just to fulfill this need.*

It may be good to add that my interest in rational explanation stems from my work on the methodology of pragmatics. Psycholinguistics, though a *descriptive* discipline intent on providing causal explanation of speaking and understanding, cannot help invoking such *normative* entities as the rationality principles postulated by contemporary pragmatics (cf. Itkonen *forthcoming*: chap. 3). Thus rational explanation is needed in causal linguistics anyway, and it seems eminently reasonable to find out how far its application can or should be extended.

When I accept rational explanation as the right way of explaining linguistic change, I am at the same time accepting 'methodological spectacles' that force me to view linguistic change in a definite way. The methodological spectacles provided by classical mechanics cer-

tainly distort the nature of linguistic change in unacceptable ways and must therefore be rejected. But I cannot of course be certain that the spectacles of *rationality* that I am now wearing are conclusively the best ones. Alternative ways of conceptualizing linguistic change and doing justice to its non-nomic nature may be provided by the systems theory on the one hand and the evolutionary theory on the other. I am aware of these alternatives, but I just find them less convincing than the position I am now defending.

This last remark may need some justification. It may seem simply preposterous to claim that linguistic change is in any sense 'rational'. However, I do not ask you to change your accustomed idea of what linguistic change is; but I do ask you to change your accustomed idea of what rationality is. In connection with Smelser (1962) it already became evident that it makes sense to speak of *unconscious rationality*. Now, this concept is being increasingly employed in different human sciences. For instance, cognitive psychology, which has by now supplanted behaviorism and associationism, considers mental processes as rational activities. In the study of sentence production and perception it is being realized that even the minutest subprocesses are governed by unconscious rationality. To quote Fodor (1975:173): 'For all we now know, cognition is saturated with rationality through and through.' Accordingly, we now face a simple choice: Either linguistic change is not a cognitive —or psychological—phenomenon, in which case I am wrong; or it is a cognitive—and therefore 'rationality-saturated'—phenomenon, in which case I am right.

It was pointed out above that linguistic change in any case exhibits statistical regularities, which fact must now be reconciled with the non-nomic character of rationality. This can be done easily enough. It is important to realize that rational behavior does not mean entirely unpredictable behavior (and it certainly does not mean *chaotic* behavior). Every particular situation admits only a certain—even if indefinite—number of actions as rational and excludes an incomparably greater number of actions as irrational. Therefore the concept of 'possible linguistic change' remains viable also in the context of rational explanation. The smaller the number of actions admitted as rational by the situation, the more predictable they are and the more obviously the actions performed exhibit statistical regularities. What is important, is to notice that such regularities do not *explain* the actions. On the contrary, the regularities turn out to be aggregates of actions each of which has its own rational explanation.

A rational action is an action which is an adequate means for attaining or maintaining a goal. If linguistic change is rational, what is its goal? In morphological and syntactic change it is, I think, to maintain or restore the isomorphism between meaning (or function) and form. I single out the case where there are differences of form with no corresponding differences of meaning. Then, supposing that a change will occur, two *rational* courses of action are possible: either to eliminate the formal differences; or to abduce new meanings and thus to create semantico-functional differences to match the formal differences. The traditional *analogy* is a special case of the former alternative.

It may be good to point out that discussing analogy in terms of 'depth' and 'surface' is justified only if the overall linguistic theory makes such a terminology necessary. Recent non-transformational theories of grammar make it clear that this unduly dichotomous terminology can be dispensed with (cf. e.g. Kac 1978). It seems much more plausible to regard analogy as holding between more or less concrete or abstract classes of entities, this distinction being such as to admit of *gradation*. This conception is, incidentally, in agreement with Paul's (1886/1975) account of analogy.

Changes amenable to rational explanation exemplify what I have called 'short-term teleology' (cf. Itkonen 1978c). Here I must leave open the question whether long-term teleology not reducible to short-term teleology exists.

4. Historiography of linguistics

For expository reasons I started here from history of science and arrived at history of language. From the ontological point of view the inverse way of proceeding would have been the right one, because natural language obviously precedes science. Therefore, if both change of language and change of science are impelled by the same force, namely rationality, change of language could justifiably be called the 'prototype' for change of science.

What was just said holds true of *any* science. Is there any reason to single out *linguistics* in particular? I think there is, at least to some extent. If we consider the time span of the last more than two thousand years, it is clear that the history of physics shows much more progress or, to put it in more neutral terms, much more quali-

titative changes than does the history of linguistics (more precisely, of grammatical theory). Therefore it is the history of linguistics, and not of physics, which resembles the history of language in its lack of progress. In this respect the history of linguistics is not unique, however. The history of philosophy, too, moves at a slow pace and forms, if not a downright circle, then at most a spiral. Aristotle's position in philosophy is still today very strong, but not stronger than Panini's in linguistics. More generally, this *factual* brevity of history applies to all genuinely humanistic sciences; just compare Thucydides to any modern historian. Therefore it is only logical that my ideas concerning linguistic change were fully anticipated at least more than one hundred years ago. I conclude by quoting Whitney (1875/1979):

'There is always one element in linguistic change which refuses scientific treatment: namely, the action of the human will. The work is all done by human beings, adapting means to ends,... The real effective reason of a given phonetic change is that a community, which might have chosen otherwise, willed it to be thus;...' (p. 73).

'Once more, there is nothing in the whole complicated process of name-making which calls for the admission of any other efficient force than the *reasonable action*, the action for a definable purpose, of the speakers of language: their purpose being, as abundantly shown above, the adaptation of their means of expression to their constantly changing needs and shifting preferences' (p. 144, emphasis added; cf. also the entire passage 143-52).