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A Note on Explaining Language Change

The principal type of social change could be characterized as being the unintended collective result of an indefinite number of individual, often antagonistic actions. To illustrate, suppose there is a number of persons A, B, C, etc. who all want to have some desired thing X. What is the end result? Quite typically, it is that no one will have X. On the other hand, the overall situation of A, B, C, etc. will be changed in a way intended by no one. In economic life, for instance, the goals and actions of those who sell are opposite to the goals and actions of those who buy. The end result is a sort of 'balance' which has been intended by no one, but which may, nevertheless, be considered 'reasonable' for the society as a whole (cf. Itkonen 1983: 301-302). It is for this type of phenomenon that Adam Smith (1776/1979) and Hegel (1807/1970) coined the expressions *invisible hand* and *List der Vernunft*, respectively. These metaphors suggest that people are led unknowingly and even against their will towards some predetermined goal by some 'higher being'.

In Itkonen (1982) I suggested that linguistic changes that exhibit a sort of 'long-term teleology' might be viewed as belonging to the same category as the 'invisible hand'-type phenomena.[1] Of course, the reality behind the 'invisible hand' (=IH) metaphor has to be uncovered. Schelling (1978), for instance, takes up this task, establishing a distinction between

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[1] I was impressed by Lass' (1974) bold hypothesis about the existence of *sui generis* 'orthogenetic' processes, and I found it natural to support him. In his 1980 book Lass rejected this hypothesis, and adopted it again in the mid-80's. When I questioned him on this, he gave a characteristically trenchant answer: "Only an idiot does not change his mind." I have to agree. For a discussion of Lass (1980), see Itkonen (1981).

'micromotives' and 'macrobehavior', and showing how the latter grows out of the former. The reading of Schelling (1978) confirmed me in my earlier view that typical or 'short-term' linguistic changes are qualitatively different from IH phenomena, and made me sceptical about the feasibility of any IH account of linguistic change. The reason is simply that the central characteristic of IH phenomena is lacking here. Linguistic change is *not* a sort of 'balance' between various (attempts at) changes made by antagonistic groups of speakers. If there is (permanent) antagonism, it simply produces a dialect split.

Following Keller (1990), Nyman (1994) argues that linguistic changes should be viewed as IH phenomena. He realizes (p. 174) that this is meaningful only on condition that IH phenomena are taken to result from "largely uniform" or "more or less homogeneous" (rather than antagonistic) individual actions. But he is wrong to assume that this is the "traditional" conception of IH phenomena (cf. the reference to Smith and Hegel above).

I want to argue here that if linguistic changes are viewed as social aggregates of several *uniform* or *homogeneous* individual actions in the way suggested by Keller and Nyman, then the IH model as explicated e.g. by Schelling (1978) is the wrong model for linguistic change. If we want to find in the field of general sociology a model that might apply to linguistic change, it is rather the functionalist model of Smelser (1962), which makes linguistic changes appear, in Nyman's (p. 169) correct characterization, as "collective unconsciously rational actions carried out by consensual individuals".[2] I am merely repeating here the argument I have presented in Itkonen (1983: 201-211) and (1984).

It is time to illustrate the issue with a couple of examples. Consider these two cases (reproduced from Itkonen 1982). First, Proto-Germanic had the stress alternation *wárþ* vs. *wurdúm*,

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[2] Notice that 'consensual' is virtually synonymous with 'uniform' or 'homogeneous'.

which produced in Old High German the consonantal alternation *ward* vs. *wurtum*. This instance of 'one meaning - two forms' was felt to be disturbing (as we can infer *post hoc*), and the 'one meaning - one form' situation was (analogically) restored in Middle High German forms *ward* vs. *wurden*. (And later there was a further levelling, producing *wurde* vs. *wurden*.) Second, the Latin forms *es* ('you [sing.] are') and *est* ('he/she/it is') became fused in Proto-Romance as *es*. This instance of 'two meanings - one form' was felt to be disturbing (as we can again infer *post hoc*), and in Spanish it was eliminated by borrowing the form for 'you are' from the future tense, producing the alternation *eres* vs. *es*.[3]

The bipartite nature of these changes is self-evident: first we have a sound change obeying the 'material aspect' of language, and then we have a morphological change obeying the 'spiritual aspect' of language. Let us now ask the following question: How can the IH account enhance our understanding of these changes? In order to have an answer, let us quote Nyman's characterization of a prototypical IH explanation:

> [Consider] how a footpath running from a bus stop across a lawn to a supermarket might have come about: many people - not necessarily all - who get out of the bus intend to reach the supermarket as easily as possible. If the well-paved way to the market place [sic] requires extra steps, people may tend to take a shortcut across the grass. Such a footpath is an IH-phenomenon: while every shortcutter intends to spare steps, nobody intends to make a pathway. It just comes about as a consequence of the grass being trampled on. (Nyman 1994: 173-174.)

Let us now try to apply this account to explaining linguistic changes, e.g. the changes described above. What do we get? We get the rather trivial truth that everybody did the same thing, and for the same reason. (And if there were people who did not do this

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[3] These examples are quite simple in themselves, but I maintain that what is true of them is true of all more complex cases as well.

thing, i.e. people who 'did not trample on the grass', they just constituted a dialect group of their own.) Nobody consciously intended to change e.g. Old High German into Middle High German, or Proto-Romance into Spanish; it just happened.

It seems clear to me that there is no point constructing an elaborate IH machinery to teach us these elementary truths. Every sane practitioner of diachronic linguistics has always known them. Now it also turns out that it may not have been entirely correct to characterize·the IH model as the 'wrong' model. It is, rather, a superfluous model. More precisely still, it is the right model in those cases, and only in those cases, where it happens (as if led by an invisible hand?) to coincide with the functional-teleological view of linguistic change.

To this Nyman can, and does (pp. 175-176), reply that there are changes with no functional-teleological explanations. I agree. Purely random changes indeed seem to exist. But my point is that the IH account in no way enhances our understanding of such changes. Saying that they just come about as aggregates of several individual actions amounts to repeating, with extra machinery, the obvious truth that they are random changes (and that language is a social phenomenon 'carried' by individual persons).

I have argued all along that the explanations that linguists *in fact* offer to linguistic changes turn out, on closer inspection, to rest on the notion of (unconscious) rationality.[4] Nyman disagrees. He wishes to abandon the model of rational explanation as well as the more general notions of teology and functionalism, and to endorse the IH model. In view of this, it is interesting to note that the only linguistic example that he offers of 'IH explanation' is taken *tel quel* from Itkonen (1983: 194-195), even if he does not care to mention this minor detail.

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[4] It is no counter-argument to say that innovation and adoption may serve different goals as long as they can both be given (different) rational explanations.

What is at issue is the vowel centralization on Martha's Vineyard as described by Labov (1963). Nyman proceeds to give an impeccable rational explanation for this change, in terms of rationality principles and practical syllogisms (for these notions, see Itkonen 1983: 65-66, 69-70, 73-78, 98-100, 177, on the one hand, and 49-53, 95-102, on the other). I agree on every point (except that I do not see why rational explanations have to be called 'IH explanations'). I also cannot but agree with Nyman's conclusion that explaining linguistic changes requires taking non-linguistic goals into account. I could hardly do otherwise since this is my conclusion too, and one that I have illustrated in Itkonen (1983: 194-195) by giving a rational explanation to the vowel centralization on Martha's Vineyard as described by Labov (1963).

There is one further point on which I agree with Nyman. Particularly in economic literature, it is customary to view the workings of the 'invisible hand' as beneficial. Nyman is perfectly right to question this conception. Wars are a prime example of how the invisible hand operates. Since antiquity it has been known perfectly well that when country A and country B go to war, more often than not both A and B will be davastated, a result certainly intended neither by A nor by B (cf. the first paragraph of this note). Yet this knowledge has never been able to prevent wars, which goes to show the power of the 'invisible hand'. In economic life the 'invisible hand' may have a somewhat more benevolent appearance, but it is just appearance. This is shown rather graphically by the imminent destruction of the world that will be produced by unlimited economic growth spurred (you guessed it) by the 'invisible hand'. Given these facts, I suggest that it is time to replace this term by a more adequate one, namely 'invisible paw'.

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