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CAUSAL MODELS IN LINGUISTICS+)

1 Non-causal linguistic descriptions

Descriptive or autonomous linguistics, which analyzes the notion "correct sentence of a language L", is, as such, free from causal thinking. In a grammatical derivation of the type

$$\begin{array}{c} S \\ aSb \\ aabb \end{array}$$

it is senseless to speak of causes and effects. Such a derivation merely says that, given the rules "S→aSb" and "S→ab", one can go from "S" to "aabb", just as in the logical deduction $\frac{p \;\&\; q}{p}$ one can go from "p & q" to "p", given the rule "p & q⇒p". No one would say that in deductive logic we are dealing with causes and effects.

On the other hand, it is often natural to *add* a causal interpretation to a grammatical description. Consider the standard type of phonological rule A→B/__C. It seems natural, within a production grammar, to think of C as a cause which, acting upon A, produces the effect B. Such an interpretation is coherent only if C is taken to be the *psychological* representation of the linguistic environment in question; A must also be interpreted psychologically, whereas B may be the actual surface form. Description combined with causal (psychological) interpretation obviously represents a more complex hypothesis than mere description: The latter may be trivially true while the truth or falsity of the former remains entirely in doubt.

Causal interpretations are implicit in much current linguistic work as well as in linguistic terminology (cf. e.g. "Auslautsverhärtung", which clearly implies that "Verhärtung"

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occurs because the environment is "Auslaut"). In this paper I try to indicate what should be done in order to make the role of causality in linguistics more explicit. However, I wish to emphasize that in my opinion it would be a logical impossilibility to think that causal descriptions could literally replace the autonomous-type descriptions (cf. Itkonen forthcoming a). Knowledge of "correct sentence" is (primarily) a case of pre-experimental knowledge. To think that the notion of "correct sentence" could emerge as a result of experimentation seems to me a fallacy analogous to thinking that the notion of "centimetre" is a result, and not a precondition, of measuring operations. Hence, causal descriptions must build upon one or another notion of autonomous grammar; their function is to enrich and modify the latter.

There may also be other types of non-causal linguistic descriptions besides these carried out in the spirit of autonomous grammar. Cicourel (1973) demonstrates the possibility of ethnomethodological description of language. Now, ethnomethodologists explicitly deny any interest in causality (cf. Zimmerman & Wieder 1970). This is understandable, given that they consider the "methods" used by ordinary people in interpreting their everyday life on a par with methods used by scientists in their work; and causal descriptions of the latter do not appear very fruitful. (This shows, incidentally, why Dray's (1957) "rational explanation" cannot, pace Hempel and Stegmüller, be a case of empirical, causal explanation.)

Finally, I wish to point out that the term "causality" is being used in a sense wide enough to cover both teleological and mechanistic causation (as it appears in behavioral regularities).

2
Causality in physics and in sociology

The philosophy of the natural sciences was dominated, until recently, by the hypothetico-deductive conception of science,

as expounded e.g. in Nagel (1961) and Hempel (1965). These authors regarded the construction of axiomatic theories as the ultimate goal of science. Significantly, they accorded at most a heuristic role to the concept of causality. For instance, Hempel (1965: 352...353) emphasized that DN-explanations include also functional, non-causal explanations.

This conception of science has been increasingly criticized for its lack of concern with the actual practice of empirical scientists. One of the most forceful criticisms has been presented in Harré (1970). He notes that physicists, for instance, are not primarily interested in setting up systems of sentences, consisting of axioms, inference rules, correspondence rules, and operational definitions. Rather, they are interested in discovering the actual constitution of the universe, i.e. those causal mechanisms which make things happen in the way they do happen. Consequently, the notion of causality and the models for (hypothetical) causal mechanisms acquire a central importance. From a rather different perspective, von Wright too has been arguing for the theoretical, not-just-heuristic importance of causality. In von Wright (1974: 70), for instance, he notes that "the basis of functional laws [like Ohm's law] are causal relations between (variations in) determinate states". Functional laws, formulated as laws of cooccurrence, seem in fact to conceal the real state of affairs.

It is very interesting to note that Hempel has come to accept the correctness of such and similar criticisms:

> "And as for the claim that formalization makes explicit the foundational assumptions of the scientific discipline concerned, it should be borne in mind that axiomatization is basically an expository device, determining a set of sentences and exhibiting their logical relationships, but not their epistemic grounds and connections. A scientific theory admits of many different axiomatizations, and the postulates chosen in a particular one need not, therefore, correspond to what in some more substantial sense might count as the basic assumptions of the theory; ..." (Hempel 1970: 152).

> "Thus, a model in the sense here considered is not only of didactic and heuristic value. The statements specifying the model seem to me to form part of the internal principles of a theory and as such to play a systematic role in its formation" (op. cit.: 158).

I accept also the notion of probabilistic, i.e. non-deterministic, causality. Some philosophers consider this as an incoherent notion (e.g. Stegmüller 1970: Ch. VII, 5). I disagree with them, for two reasons. First, the notion of causality is inseparable from the notion of making happen or preventing from happening (von Wright 1974). Now, statistical regularities of the type "$p(A \mid B) = r$" are often nomological or law-like, which means that by producing B we increase the probability of A and by suppressing B we decrease the probability of A. The analogy to "genuine", deterministic causation should be clear enough.

Second, in current sociology it is an axiomatic truth that the discovery of statistically significant correlations or cooccurrences is only the first, and purely preliminary, step in a full-fledged sociological analysis. The crucial part of such an analysis consists in revealing the (hypothetical) causal mechanism which produces the observed correlations. But since social behavior is notoriously variable, the required notion of causality can only be of probabilistic nature. Models for social(-psychological), and hence necessarily probabilistic, causation are developed e.g. in Blalock (1964) and Boudon (1974).

3 Causality in linguistics

Empirical linguistics is supposed to be concerned with regularities exhibited by the actual linguistic behavior of real (i.e. non-ideal) speakers. As a result of the discussion in the previous section, and with special reference

to the Hempel-quotation, I state it as a desideratum of any adequate methodology of empirical linguistics that it should provide causal models for linguistic behavior. Attempts at axiomatization are commendable, but nevertheless of secondary importance only.

Given the social nature of speaking, it also follows that the required causal models must be of probabilistic, non-deterministic nature. In this context it is interesting to note that transformational grammar (= TG) operates only with the notion of deterministic causality. As far as I can see, there are two reasons why TG has adopted this mistaken view.

First, TG consistently refuses to tackle actual linguistic data and prefers to stay at a level of abstraction or "idealization" where the regularities to be accounted for by the linguistic theory are of the type "Every normal English child acquires the English language". It may indeed seem plausible to consider such a regularity as deterministic. (But notice the qualifying attribute "normal", which already relativizes the regularity in question.) A sociological regularity of equal abstractness would be, for instance: "Each normal Japanese child gets accustomed to the Japanese culture." Now, every sociologist would agree that such a regularity is completely useless because of its very abstractness. When one actually gets down to investigating real children, individual differences and variations immediately appear. - Blalock (1964: 18) points out that deterministic regularities are acceptable as "idealizations" only if the idealization can be closely approximated under experimental conditions or if the disturbing factors bringing about the less-than-ideal situation can be precisely identified and measured. Neither of these conditions is fulfilled in sociology or in linguistics. I regard TG's deterministic assumptions as a sign of its "empirical immaturity".

Second, at the level of autonomous linguistics (the "standard" version of) TG generates strings identifiable as grammatically "clear cases". TG has apparently mistaken this non-statistical, all-or-none type of description for the (necessarily non-statistical) description of deterministic <u>spatio-temporal</u> systems. So much is evident from Dougherty's (private communication) remark that algorithmic generation and the laws of astronomy are equally cases of "mechanical explanation".

Now, if it is agreed that what we need are probabilistic causal models, from where do we get them? Blalock's (1964) models are perhaps the best known in sociology. However, they are inapplicable in linguistics, because they are based on Pearson's product-moment correlations, which require the variables to be of the <u>quantitative</u> type. Now, apart from some phonological variables, the normal linguistic variables are of the <u>qualitative</u> or nominal type, i.e. their values cannot be ordered on a quantitative scale. (For instance, it does not make sense to ask whether e.g. "genitive", as a value of the variable "case", is smaller or greater, and by how much, than "accusative".) In other words, the only quantitative aspect of the normal linguistic variables consists in the frequencies with which units exemplifying their values occur.

Consequently, what we need are probabilistic causal models for qualitative variables. Such models are developed in Boudon (1974). They are insofar restricted that they deal, primarily, only with dichotomous variables like $x_a = A/\bar{A}$. However, this restriction can be overcome, to some extent at least, by noting that e.g. the trichotomy A/B/C can be transformed into the two dichotomies A/BC and B/AC.

Boudon defines two basic types of model, namely <u>disjunctive</u> and <u>conjunctive</u>. It is the characteristic property of disjunctive models that the causal forces of their independent variables remain constant in all contexts. By contrast, conjunctive models allow for the fact that the causal force

of an independent variable may depend on its context, i.e. there may be (positive or negative) <u>interaction effects</u> between independent variables. Conjunctive models are much more realistic than disjunctive models. At the same time they are mathematically more complex. Although the algebra required for computing first-level interaction effects is understandable even to a non-mathematician, the actual calculations must be carried out by a computer. Computing higher-level interactions is even more difficult.

The nearest thing to a linguistic causal model I know of is the notion of "variable rule" developed by Labov, Cedergren, and the Sankoffs. I wish to criticize this notion on two accounts. First, Labov's "variable constraints" are said to "favor" the occurrence of a given linguistic feature. It is obvious that this terminology is implicitly causalist. However, no definition of causality is given. In particular, no distinction between probability and causality is made, although these are clearly different notions. (It may be possible to predict the probable occurrence of A on the basis of B even if B is not the cause of A.) Second, "variable rule" is identifiable as a <u>disjunctive</u> model: Labov and the others explicitly assume that variable constraints remain uninfluenced by their context. However, it is quite easy to show, using Labov's own data, that this assumption is false. Hence, linguistic data require <u>conjunctive</u> models.

Consider the much-discussed case of the elimination of the word-final -d/-t in some dialects of English (Labov 1972: 222). We can identify the following variables:

x_a	(= dependent variable) = -t/-d disappears (= A) or not (= $\bar{A}$)
x_b	= The following word begins with a consonant (= B) or not (= $\bar{B}$)
x_c	= A morpheme boundary does not precede (= C) or does precede (= $\bar{C}$)
x_d	= Lower working class (= D), upper middle class (= $\bar{D}$)

Labov assumes that we have the structure of figure 1; but the structure of figure 2 is what we have in fact.

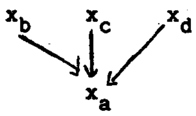

fig. 1

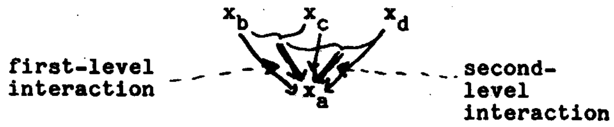

fig. 2

The technical details as well as a more thorough discussion of disjunctive and conjunctive models can be found in Itkonen (forthcoming b).

The notion of "explanation" (or, more neutrally, of "analysis") implicit in Boudon's models is identical with Salmon's process-like notion of "statistical explanation", which identifies the explanatory import as the <u>increase</u> of the probability occasioned by the consideration of a new independent variable:

$$\underset{\text{1. stage}}{p(A \mid B) = r} < \underset{\text{2. stage}}{p(A \mid B \,\&\, C) = s}$$

4 Implications for the linguistic theory

Variable rules, even when reinterpreted as (conjunctive) causal models, are of a rather concrete type. That is, (the values of) the variables are such easily accessible or observable entities as surface linguistic features, age, sex, or social class as economically defined. Even if the direction of causation can be determined (which is not always easy in non-experimental research), an adequate notion of the causal mechanisms involved can be achieved only by postulating intervening variables of a more theoretical type between the initial, (semi-)observable variables.

What kind of theory is going to provide the required intervening variables? To this I have no definite answer.

I merely point out that because of its very abstractness TG is in no obvious way responsive to so-called external evidence relating to various types of actual linguistic behavior. Causal models discussed above constitute one type of external evidence (or a type of mini-theory based on such evidence). Criticism of TG's use of "immunization strategies" or "protective devices" is by now common-place. Insofar as causal considerations have, or are taken to have, no relevance as far as the testability of TG is concerned, TG's position is indeed methodologically indefensible.

A striking example of TG's attitude vis-à-vis disconfirmation is Kiparsky's (1975) claim that the hypothesis of the psychological reality of TG descriptions can only be verified, but not falsified. This extraordinary view is based on the alledged fact that we have here an existential statement of the type "$(\exists x)Fx$", i.e. "There is a psychologically real mechanism of such and such a type"; and it is well known that existential statements can only be verified, not falsified. Now, there is of course a misunderstanding here. The statement that Kiparsky has in mind must certainly be of the type "$(y)(\exists x)Gyx$", i.e. "For all speakers, there is...". Empirical science deals with (universal or statistical) regularities, not with disconnected, non-generalizable facts. Now a statement with mixed quantifiers can be, strictly speaking, <u>neither</u> falsified <u>nor</u> verified. But this does not mean, of course, that we can adopt no rationally justifiable attitude vis-à-vis this kind of statement. If all available evidence is against the statement - as experimental evidence is in fact against the psychological reality of TG - then we are permitted to regard its falsity as highly probable. It is this unpleasent truth that Kiparsky tries to explain away.

What is important here is not Kiparsky's mistake, but the fact that he was led to commit it in the first place. That is, he could not have argued for the infalsifiability of TG (<u>qua</u> psychologically real) if he had not been aware of the tremendous wealth of protective devices that TG makes available to its practitioners. It would seem that we cannot have an empirical linguistic theory, i.e. a theory disconfirmable by facts of language use, unless we make the theory more concrete. To this extent, then, my argument supports the general positions of the various types of "concrete phonology" and "concrete syntax".

5
Concluding remark

Diesing (1972) points out that in the investigation of complex social and psychological processes the computer simulation offers a more realistic and more flexible tool than the multivariate analysis, some forms of which were discussed in sect. 3. Presumably the same is true of the investigation of language production and perception.

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