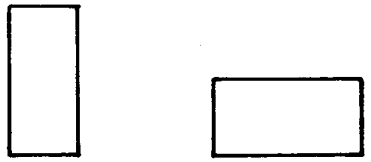
1 NON-CAUSAL AND CAUSAL: A PRELIMINARY DISTINCTION

Causal linguistics inquires into those *mechanisms*, whether biological, psychological or social, which contribute, in one way or another, to the occurrence of linguistic behaviour. This type of behavior includes, on the one hand, momentary happenings like speech production and perception and, on the other, protracted processes like language acquisition and language change. Indeed, the mere existence of language is based upon an ongoing social process. Non-causal linguistics must then be defined as a type of linguistics which does not investigate the above-mentioned mechanisms, or which investigates 'language in itself', abstracting from the mechanisms that sustain it. It seems natural to identify non-causal linguistics as here defined with 'autonomous linguistics' (henceforth to be abbreviated as 'AL'), as defined in *GTM*. It will be seen as we progress that the notion of non-causal or autonomous linguistics is not as straightforward as one perhaps might think. In the present context, however, I wish to clarify and to justify the basic distinction between non-causal and causal. The examples I shall use are very simple. They should establish my point because of their very simplicity.

Suppose I have to describe some figures, for instance these two:



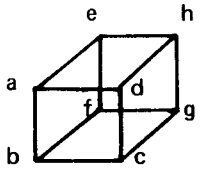
It is obvious that *A* and *B* are partly identical with, and partly different from, each other. More precisely, they are identical in form and size, i.e. they are rectangles with one side of 1 cm and the other of 2 cm, whereas their positions are different: *A*'s is vertical while *B*'s is horizontal.

I said that I have to describe the figures *A* and *B*. But I could also have said that I have to describe my perceptions, or my perceptual

knowledge, of *A* and *B*: if I did not perceive them, it presumably would not occur to me to describe them. This interpretation of describing *A* and *B* rests on the fact that when we perceive (relations of similarity and of difference between) objects, we are, or can become, conscious of what we perceive. And we can at wish describe what we are conscious of.

I just identified (description of) *A* and *B* with (description of) my perceptual knowledge of *A* and *B*, but in a more careful analysis the two must be kept separate. First, *A* and *B* may exist even if I never perceive them. Second, even if I do perceive them, my perceptions may in fact be misperceptions, which means that the description of my perceptual knowledge of *A* and *B* differs from the ('true') description of *A* and *B*. Notice that the distinction between perception and misperception (and, therefore, between true description and false description) presupposes the existence of objective criteria, and that such criteria exist, if at all, only in a community of people having perceptions and communicating them to each other.

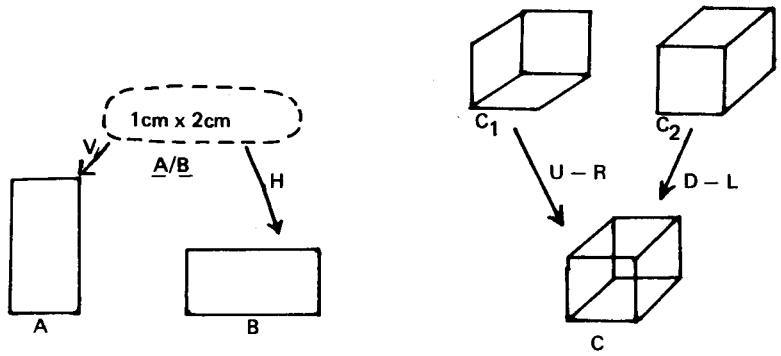
To take another example, consider the following figure, known as a 'Necker cube':



C

Taken three-dimensionally, *C* can be perceived in two different ways, though not simultaneously: either the face *abcd* or the face *efgh* seems to be nearer to the onlooker.

In the case of *A* and *B* we have to do with two figures which, as given, are different from each other, i.e. different in position, but from which two aspects may be abstracted under which they are identical with each other, namely form and size. In the case of *C*, in turn, we have to do with a single figure, i.e. a figure numerically identical with itself; but from it two figures differing from each other in their respective positions may be abstracted, which means that *C* is ambiguous. How could we describe these facts in a more perspicuous way? One way would be the following:



In the first case the dotted circle *A/B*, which we might call 'deep structure', presents what is common to *A* and *B*. By applying to *A/B* two operations, which might be called 'verticality transformation' and 'horizontality transformation', or 'V' and 'H' we get *A* and *B*, which we now recognize as 'surface structures'. In the second case the deep structures C_1 and C_2 represent the disambiguated interpretations of the ambiguous surface structure *C*. The operations applied to C_1 and C_2 are instances of the 'transparentification transformation'. In the case of C_1 the face made transparent is up and right (hence the label 'U-R'), whereas in the case of C_2 it is down and left.

I have presented here one way of systematizing similarities and differences between certain figures. The resulting descriptions might be thought of as exemplifying a nonstandard sort of geometry. The important thing about them is that just like geometrical descriptions of the standard sort, they cannot be regarded as causal or psychological descriptions. The situation changes completely, however, if, instead of just systematizing identities and differences implicit in the figures *A*, *B*, and *C* (as I perceive them), I ask how, i.e. by what mechanisms, I actually perceive them. Once I ask this question, I have moved into the obviously *causal* domain of the psychology of perception.

The difference between non-causal, (quasi-)geometrical description of figures and causal, psychological description of the perception of figures is quite unequivocal and need not be expressly justified. It is clear that non-causal description is logically primary *vis-à-vis* causal description: before answering the question *how* for example the figures *A*, *B* and *C* are perceived, we must know, and be able to describe, *what* these figures are; this means that a *how*-question is in fact always a combined *what-and-how*-question. It is possible that an answer to the

non-causal *what*-question somehow coincides with the true answer to the causal *how*-question. But whether this is or is not so, can only be decided by means of those *experimental* methods which are in normal circumstances employed to answer the *how*-question, and go beyond the *non-experimental* methods employed to answer the *what*-question.

Interestingly enough, it so happens that our non-causal descriptions of *A*, *B* and *C* can, as such, be reinterpreted as partial descriptions of how perceptual mechanisms operate upon inputs identical with these three figures. In particular, this reinterpretation would seem to be in line with the hypothesis of 'figural synthesis', for which Neisser (1967:143-5) finds support in his analysis of a Necker cube in apparent motion. All we have to do is to take the arrows as representing processes of *construction* that are based on preattentive stimulatory cues and produce the perceptions proper (op. cit.: 94–7). The important thing, however, is that mere conceptual analysis of the figures in question would never have justified Neisser's hypothesis of figural synthesis; experimental evidence was also needed. In other words, the two types of description clearly differ as to their methodological status. The causal description is an empirical hypothesis; but it seems slightly odd, at least in the present case, to say that the non-causal description, *qua* conceptual analysis, is 'hypothetical', in spite of the fact that conceptual analysis can in rather obvious ways be (nonempirically) falsified (cf. *GTM*:9.3, 10.2, 11.1).

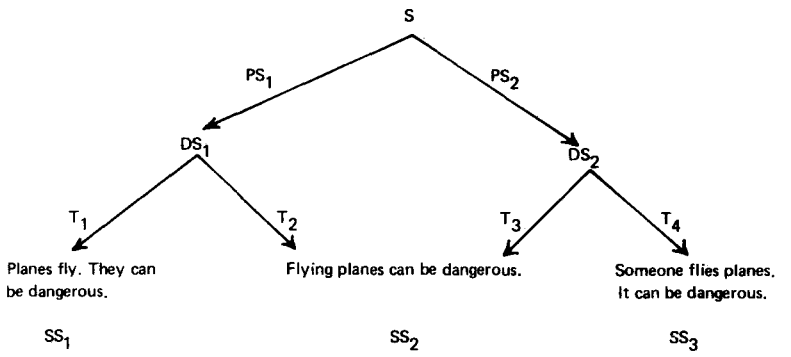
Next, let us consider the causal/non-causal distinction in linguistic descriptions. The rewriting rules introduced by transformational grammar (henceforth to be abbreviated as 'TG') are, at a certain level of abstraction, similar to inference rules of formal logic. The greater or lesser complexity of rewriting rules is in this context irrelevant. For instance, the following description is as good an example of a *generative* grammar as any:

$G = r_1 \,\&\, r_2$	For example:	S
$r_1 = S \rightarrow Sa$		Sa r_1
$r_2 = S \rightarrow a$		aa r_2

The string, or 'sentence', *aa* is generated in two steps, i.e. by one application of each of the rules r_1 and r_2 (which suffice to generate all strings of *a*'s). It is impossible to construe r_1 as a cause which, acting upon *S*, produces *Sa* as its effect. In other words, grammatical descriptions are just as non-causal as logical inferences. For instance, no one would say that Modus Ponens is a cause which, acting upon the formulae

'$p \supset q$' and 'p', produces the effect 'q'. The conception of linguistics presented in Chomsky (1957) amounts to nothing more than generating strings identifiable as inscriptions standing for correct sentences of a given natural language.

Consider the following example, which is meant to bring out the analogy between descriptions of figures and descriptions of sentences:



The relation between the surface structures SS_1 and SS_2, or between SS_2 and SS_3, is the same as between the two rectangles in so far as the two entities in question are partly identical with, and partly different from, each other. These facts are again described in a systematic way by deriving the two entities from a common deep structure, i.e. DS_1 or DS_2, by means of different transformations. SS_2 in turn corresponds to the Necker cube;[1] its ambiguity is again described by deriving it from two different deep structures (obviously by means of different transformations). If to the structure-changing transformations we add the structure-expanding phrase-structure rules, which derive deep structures from the axiomatic S-symbol, we have here in a nutshell both the rationale and the general structure of TG's 'classical' version.

Chomsky (1965) introduced TG's 'psychologistic' turn: he continued to formalize small bits of his own intuitive knowledge of English, but he now assumed that the resulting transformational-generative descriptions automatically possessed some kind of psychological reality. In my terminology this would mean that from non-causal descriptions consonant with the idea of AL they were changed into causal ones.

Two things have to be kept in mind here. On the one hand, it is possible, in principle, to interpret the originally non-causal description

of the three (surface) sentences SS_1, SS_2 and SS_3 in a causal manner, for instance by taking the transformations to represent components of the processes of sentence perception. (To be sure, Chomsky's current position is not as easy to pin down.) On the other hand, experimental evidence has shown that any such interpretation is almost certainly false (cf. Fodor *et al.* 1974:241). In generative phonology, however, it is sometimes quite natural to reinterpret originally non-causal descriptions in causal terms: in a rule like A→B/_C, for instance, *C* may be taken as (part of) the cause which acts upon *A* and produces *B*.

The upshot of our discussion of non-causal descriptions of figures/sentences as contrasted with causal descriptions of perceiving/producing figures/sentences is as follows. The non-causal (or autonomous) vs. causal (or non-autonomous) character of linguistic descriptions[2] must be evaluated from two viewpoints at the same time: first, how they are meant by those who are proposing them; second, whether the way they are meant is or is not justifiable. One and the same description may be interpreted either non-causally or causally, and it may be that one interpretation is plausible while the other is implausible (or simply false). Notice, in particular, that a true causal description must also be (or contain) a true non-causal description, whereas the converse is not the case. In this sense, then, causal descriptions answering the *what-and-how*-question logically presuppose non-causal descriptions answering the *what*-question, but not vice versa.

Some of the implications of the preceding paragraph need to be spelled out. My conception of non-causal vs. causal (linguistic) descriptions requires a pragmatic and historical conception of science. Just looking at a scientific description, we cannot tell whether it is non-causal or causal. Rather, we have to take into account the *intention* 'behind' it, or the *goal* which it is meant to serve, and hence to embed it into the scientific *activity*. Once we do this, we realize that the goal (or the intention) of making true non-causal descriptions is less complex than, and logically primary with respect to, the goal of making true causal descriptions. Accordingly, the criteria used to decide whether or to what extent the goal has been achieved, are more complex in the causal than in the non-causal case; in the former, but not in the latter case they ought, in particular, to include the requirement of experimental evidence. If the (potential) criteria are identical in the two cases, there is no justification for claiming that a causal description distinguishable from a non-causal one has been proposed. Moreover, both the goals of scientific activity and the criteria used to evaluate its results are *historically* given; a theory which was judged plausible (and thus *was* plausible) one hundred years

ago, may today be (judged) implausible. The inverse assumption of literally time-independent and yet human goals and/or criteria seems to me a contradiction in terms.

The non-causal/causal distinction as here defined seems to vindicate the distinction between AL and psycholinguistics, as it has been formulated and defended in Itkonen (1974) and *GTM*, or in Kac (1974, 1980). I emphasize in particular that the necessarily *human* character of language must not be thought to imply that all linguistic descriptions are necessarily of *psychological*, and therefore causal, character. To see this, one only needs to think of any description of conventions or norms. For instance, in Itkonen (1974:203–8) I present a simplified 'grammar' of the legal processes in Finland. It is quite clear that this kind of grammar is not a psychological description, but just a way of systematizing the conventions or norms governing legal processes. Yet is it also clear that the criminal law is a human phenomenon with no less necessity than language. It follows that it is wrong to regard AL descriptions as descriptions of *competence*, except in the quite general sense that the norms described are by definition *known* (cf. (iii) below).

In the remainder of this chapter I shall consider two recent, quite dissimilar attacks against the concept of AL, one from the camp of 'experimental linguistics', presented by Derwing and Prideaux, and the other from the transformational-generative camp, presented by Chomsky and Lightfoot.

Derwing (1980) argues that, whatever the programmatic statements of TG, it remains just as committed to the notion of AL as the earlier schools of structural linguistics, and he recommends rejecting this notion in its entirety. I accept Derwing's position on the first point, but contest it on the second. The following seem to me the most important of his objections against AL:

(i) The rise of AL, initiated by Saussure, coincided with, and was at least indirectly influenced by, the rise of behaviorism in psychology.
(ii) AL rests on what Labov calls the 'Saussurean paradox': although language is presumably *social*, knowledge of it is acquired in AL by means of *individual* intuition.
(iii) If AL investigates linguistic knowledge, then it must be part of psychology, because 'knowledge' is a psychological concept.
(iv) AL purports to investigate 'linguistic structure' independent of any speakers, but this kind of structure is ontologically nonsensical.

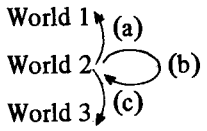
(v) AL, being restricted to a formal conception of language, is unable to account for the use of language.
(vi) AL descriptions do not possess psychological reality.

Ad (i): AL goes back to Pāṇini and Dionysios Thrax, who described the noun and verb paradigms of Sanskrit or Greek with no regard to psychological processes of any kind. More generally, any school-grammar belongs to AL.

Ad (ii): The 'Saussurean paradox' is not a paradox, but a conceptual necessity. There are compelling reasons to postulate the existence of (social) rules, including rules of language. Because rules are not concrete things, they cannot be observed, but only intuited. Besides, acts of observation are necessarily no less subjective than acts of intuition (cf. Itkonen 1977, 1981a).

Ad (iii): Knowledge, whether of electricity or of English, is always psychological. Therefore it is necessary to distinguish knowledge from its object. Knowing the meaning of the sentence 'One can't go on living in a state of rebellion' is different from knowing why someone uttered it. In the first instance we have knowledge of rules or conventions of language, whereas in the second we have knowledge of intentions, i.e. of psychological phenomena. A description of conventions (which is analytically based on knowledge of conventions) is not a matter of psychology, unlike a description of intentions (based on knowledge of, or hypothesis about, intentions). This point is the same as the one made in connection with describing the criminal law (p. 7).[3]

This point is important enough to deserve some additional clarification. Consider Popper's (1972) tripartite ontology, i.e. ontology divided into the 'worlds' of physical events, of psychological events and of concepts and/or norms; and let the arrows stand for the three basic types of *act* of knowledge, their heads pointing to the respective *objects* of knowledge:



If we confine our discussion to scientific acts of knowledge, the arrows (a), (b) and (c) are characteristic of, respectively, physics, psychology and AL (plus any other discipline relying on the method of conceptual analysis).

Ad (iv): Rules of language are conceptually dependent on speakers, because a rule exists only to the extent that it is followed and (at least potentially) known to exist. AL gives a systematic description of rules of language and sometimes identifies it (or its postulated referent) with 'linguistic structure'. The use of this term may be ill-understood, but it is not obviously wrong. It is not obviously wrong to say that Pāṇini investigates and describes the *structure* of Sanskrit verb and noun paradigms.

Ad (v): Viewed as analysis of linguistic rules, AL also investigates the *pragmatic* rules of speaking. In so far as it makes at all sense to view AL as analysis of linguistic competence (cf. p. 7), AL investigates the *competence to speak*, given that the only logically coherent concept of competence is the *competence to act* (cf. Itkonen 1976:218–19). In fact Searle (1969:17), who is generally credited with launching the speech act theory, thinks of himself as engaging in the study of Saussure's *langue*. Derwing has been here misled by his unwarranted identification of AL with TG.

Ad (vi): 'Psychologically real' description is a type of causal description, and therefore it goes without saying that AL descriptions *qua* non-causal descriptions do not possess psychological reality.

Prideaux (1980a, b) repeats most of Derwing's (1980) objections, but he adds a couple of new ones which deserve to be mentioned here:

(i) Alternative AL descriptions are comparable to, because as arbitrary as, alternative geometries. It does not make sense to say the Euclidean geometry is better than the non-Euclidean one, or vice versa, until an 'empirical domain' has been adduced.
(ii) AL makes the use of 'external', i.e. non-intuitive, evidence impossible.

Ad (i): The analogy between AL and geometry, as drawn by Prideaux, is fallacious because alternative AL descriptions of English, for instance, obviously share the same subject matter. A more accurate analogy holds between AL descriptions and alternative versions of Euclidean geometry. Surely Euclid's systematization of the three-dimensional common-sense space was *objectively* better than that of any of his (known) predecessors. Similarly Euclid's systematization has afterwards been (slightly) corrected on undisputably objective grounds. Few would deny the usefulness of Euclid's *non-causal* analysis.

Ad (ii): It is tautologically true that AL, being restricted to analyzing (the intuitive knowledge of) rules of language, does not make use of

external evidence. But this is not what Prideaux means. He wants to say that AL denies the possibility of any other kind of linguistics. This is definitely a mistake. AL, being the conceptual precondition of linguistic investigations dealing with external evidence (cf. Itkonen 1980:340-5), is situated at a *different level* than the latter. Therefore defending AL is fully compatible with espousing the use of external evidence. (This is my own methodological position.) All that matters is that conceptual distinctions are preserved. Before answering the question 'How is the sentence *X* produced or perceived?' one must, *as a matter of logical necessity*, have answered, in one way or another, the question 'What is the sentence *X*?' (cf. p.3). It is important to notice, however, that one need not have a *complete theoretical* answer to the latter question, i.e. a complete theoretical AL description, before embarking upon answering the former one.

Although Prideaux (1980b) takes lingustics to be a 'cognitive' science, he hesitates to reduce it to psychology. This is an untenable position because non-psychological cognition just does not make sense. Nevertheless, Prideaux's gut feeling is clearly on the right track in so far as he seems to be dimly aware of the above-mentioned distinction between (knowledge of) conventions and (knowledge of) psychological mechanisms.

While Derwing and Prideaux want to reject the concept of AL, Chomsky (1980:189-202) denies the existence of any such concept. His main objections against anything like AL are the following:

(i) AL is defined by demarcating it against non-autonomous or causal linguistics striving after psychological reality; but the concept of 'psychological reality' is nonsensical. Linguistics, which is part of psychology, is in every respect similar to physics; but it would be nonsensical to distinguish between theories of physics which are and those which are not striving after 'physical reality'.

(ii) There is no point in distinguishing between different types of evidence in terms of their (presumed) differential capacity to promote plausible hypotheses about unobservable psychological mechanisms. The best linguistic theory reveals the most about the psychology of language, and therefore whatever evidence it uses is automatically the most relevant from the psychological point of view. In particular, it is wrong to suggest that 'external evidence' has a somehow privileged status *vis-à-vis* 'internal', or intuitive, evidence.

Ad (i): In current discussion the concept of psychological reality has been defined in a perfectly clear and unobjectionable way (cf. Linell 1979:8-17), but it may nevertheless be well to add here a clarificatory example. It is often assumed that the 'if-then' construction of English is simply identical with the material implication of classical logic, which implies among other things that the equivalence '$(p \supset q) \equiv (\sim pVq)$' is part of an adequate description of English. This analysis possesses some plausibility considered from within the AL tradition, but it is not valid as a description of people's actual thought processes. Experimental studies have shown the natural-language implication to be not a *truth-functional*, but a *causal* notion; in particular, the equivalence '$(p \supset q) \equiv (\sim pVq)$' is not *psychologically real* (cf. Wason & Johnson-Laird 1972). This example merely confirms that answers to the *what*-question and to the (*what-and-*)*how*-question must satisfy different sets of criteria.

The preceding reply, combined with the replies to Derwing (1980) and Prideaux (1980), should be enough to dispel any doubts concerning the viability of the interconnected concepts of AL and of psychological reality. Yet I also want to pursue Chomsky's (1980) analogy between linguistics and physics a little farther. It is simply wrong to say that all theories of physics are to the same extent interested in the 'physical reality'. According to the current understanding, physics, as this term is generally used, is interested above all in discovering those *causal mechanisms* that make (physical) events happen in the way they do happen (cf. 2.1). However, there is also *another* kind of analysis of physical reality, which does *not* raise the causal question, namely Lorenzen's so-called protophysics, which analyzes those ideal rules of measuring length, time and mass that make empirical physics possible and thus constitute its logical precondition (cf. Böhme 1976). It can be argued that the relation of AL to psycho- and/or sociolinguistics exactly parallels that of protophysics to empirical or 'genuine' physics (cf. Itkonen 1974:289, *GTM*:44-6, 1980:344-5).

Chomsky's (1980) recurrent insistence on the perfect similarity between physics and (psycho-)linguistics should finally put the myth of TG's 'mentalistic' character to rest. Repeating what I said in *GTM* (pp. 317-18), there is no methodological reason why, in the TG opinion, theoretical physics could not be called 'mentalistic physics'.

Ad (ii): Chomsky assumes without question that his linguistic theory is the best one; and because he employs nothing but intuitive evidence (of English), he further assumes that intuitive evidence (of English) is the best kind of evidence.[4] These assumptions are entirely unwarranted, however. Chomsky's exclusive use of intuitive evidence *de facto* confines

his theory to the AL tradition. Today there is an increasing number of AL theories, of which I wish to mention here only the non-transformational approaches of Hudson (1976) and Kac (1978). It is as yet an open question which of all these theories, judged on AL criteria, should be preferred to the others.

If, however, we consider various types of external evidence in addition to intuitive evidence, then Chomsky's linguistic theory seems to be disconfirmed in relatively unequivocal ways (cf. Fodor *et al.* 1974, Dressler 1977, Linell 1979). In his search for a background science for linguistics Chomsky (1980) seems to have abandoned psychology in favor of biology, but this is a rather transparent immunization strategy: evidence from psycholinguistic experiments speaks against his linguistic theory, but there is no conceivable biological evidence that could do likewise; therefore he retreats from psychology to biology.

Finally, Chomsky is absolutely right to argue that external evidence has no privileged status; on the contrary, it is intuitive evidence which is privileged, because logically primary, *vis-à-vis* all other types of evidence. However, contrary to what Chomsky is saying or implying, there are today several more or less equal AL theories, i.e. theories based solely on intuitive evidence, and therefore consideration of external evidence cannot be postponed or should at least be actively encouraged, *if* psychological reality is taken as the goal of linguistic descriptions.

As a student of linguistic change, Lightfoot (1979) is one of the few representatives of TG who have actually gone beyond the practice of AL and may therefore, with some prima-facie plausibility, question its viability. In fact he adduces the same arguments as Chomsky (1980) *against* AL, and *for* the best, or the 'most restrictive', linguistic theory that need not distinguish between different types of evidence (pp.15-21). On closer inspection, however, Lightfoot's account turns out to be inconsistent, because he cannot help espousing the notion of AL as defined here. He notes (p.12) that 'grammatical descriptions' capture the 'substance', but not the 'form', of language, or of 'linguistic competence'; in order to capture the 'form', 'extra-grammatical', or 'external' evidence is needed (cf. also p.77). It is obvious that Lightfoot's 'substance/form' distinction is identical with my distinction between the *what*-aspect and the (*what-and-*)*how*-aspect of language. It may be added that Kac (forthcoming) draws an identical distinction between the *de re* and the *de dicto* aspects of linguistic knowledge, correctly characterizing the former as the legitimate subject matter of AL and entrusting the latter to empirical or causal linguistics.

Two closing remarks. First, when we come to discuss linguistic *pragmatics*, a subject that lies outside of the traditional type of AL, we will see that the distinction between non-causal and causal descriptions may not always be as clear-cut as it is at the 'lower' linguistic levels that were (implicitly) taken as the topic in what precedes. Second, the preceding discussion tended to equate human causation with *psychological* causation; but in many cases the *social* determinants of behavior are much more interesting, and the question of how, precisely, they are internalized in the individual psyche may safely be ignored.

Notes

1. Hockett (1954) too uses the Necker cube to illustrate syntactic ambiguities. It is important to realize that both in the geometrical and in the linguistic example the same facts could be presented e.g. in the form of a matrix.
2. Paul (1920:24), for instance, recognized the non-causal nature of AL (i.e. 'deskriptive Grammatik').
3. As far as the refutation of psychologism is concerned, the classical work is Husserl (1913: esp.p.111); cf. also Katz (1981) and Itkonen (1983a).
4. I am interested in what Chomsky does, not in what he says he does.