Qualitative vs. Quantitative Analysis in Linguistics*

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The nature of the data that a science is meant to account for determines the nature of the science. So far there is no unanimity as to the true nature of the data of grammar.[1] This state of affairs has several consequences. On the one hand, the methodological status of grammar remains in doubt; on the other, the nature of socio- and psycholinguistic data, which must be delimited against grammatical data, remains equally in doubt. From this latter fact it follows, in turn, that the methodological status of socio- and psycholinguistics is not quite clear either.

In this paper I intend to give, first of all, an explicit and consistent account of grammatical data and of its relation to socio- and psycholinguistic data. As a consequence, I shall be able to show, at a relatively abstract level, what precisely is the relation of grammar to socio- and psycholinguistics. Secondly, I shall consider questions of linguistic methodology in more detail. Grammar or autonomous linguistics will be shown to be methodologically similar to philosophy and logic. The distinction between non-autonomous and autonomous linguistics is that between causality and the lack of it; moreover, the adequate notion of causality must be here, as in social science in general, the probabilistic one. Causal models for socio- and psycholinguistics will be outlined in the remaining part of this paper. The view of the relationship between qualitative and quantitative analysis, as it emerges here, is directly generalizable to human sciences in general.

I Data

A. Grammar

There are two principal views concerning the nature of grammatical data: Either the grammarian investigates sentences invented by himself

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[1] By "grammar" I mean Saussurean autonomous linguistics. It follows, somewhat awkwardly, that e.g., "performance grammar" is not a type of "grammar." I use the term "science" in the sense of the German "Wissenschaft." Consequently, e.g., logic and philosophy are (nonempirical) "sciences."

or he investigates a corpus of actual utterances. It is also often thought that even if the former alternative is the case, it is the latter which *ought* to be the case. This conflict of opinions can be summed up as a conflict between intuition and observation. I shall review here first some proposed solutions.

Bloomfield and Harris are usually identified with the view that all one has to do is to describe a given corpus. This is false in a twofold sense. First, a cursory reading of Bloomfield (1935) and Harris (1961) shows that, whatever their methodological statements, in their descriptive practice these authors use *only* self-invented examples, i.e., reject observation and espouse intuition. Second, even their methodological statements contradict the view that corpus-description is all that counts. Bloomfield clearly recognized the creative and open-ended nature of language; for him this fact was "obvious" and in no need of special emphasis:

> ". . . it is obvious that most speech forms are regular, in the sense that the speaker who knows the constituents and the grammatical pattern, can utter them without ever having heard them; moreover, the observer cannot hope to list them, since the possibilities of combination are practically infinite" (Bloomfield 1935: 275).

Thus, transformational grammar (= TG) did not invent the creativity of language. What it did invent, were recursive rules to describe creativity. However, this is a mistake in two ways. First, sentences which natural speakers can and do utter, i.e., which constitute (the output of) their competence, contain a minimal amount of recursivity. Second, the theoretical considerations which seem to allow an infinite application of recursivity in the "ideal speaker's" competence are based on the assumption of an analogy between logical induction and what might be called "grammatical induction." However, there is no such analogy (cf. Itkonen 1976 a).

Bloomfield also recognized that because of the conventional or normative nature of language, quantitative analysis is not needed in grammar:

> "However, there is another and simpler way of studying human action in the mass: the study of conventional actions. . . . Here the linguist is in a fortunate position: in no other respect are the activities of a group as rigidly standardized as in the forms of language. Large groups of people make up all their utterances out of the same stock of lexical forms and grammatical constructions. A linguistic observer therefore can describe the speech-habits of a community without resorting to statistics" (Bloomfield 1935: 37).

The same argument to show the irrelevance of statistical considerations to grammar recurs in Chomsky (1957: 16). What Bloomfield and Chomsky are trying to do here is to justify the actual descriptive practice of most modern grammarians. In my opinion their position is entirely correct with respect to that particular type of data with which they are concerned. When one has to do with a set of reasonably well-established rules, one can perfectly well describe *possible* correct (results of) actions without paying attention to which actions are in fact performed in space and time, and how often. However, one can be interested in more than one type of data, and different types of data require different methods of description. Performance grammar is a perfectly sound concept, and it requires taking statistical and probabilistic aspects of language into account (cf. below).

Harris notes explicitly that although, in his opinion, the grammarian must start from a corpus, his description necessarily goes beyond it:

"The interest in our analysis of the corpus derives primarily from the fact that it can serve as a *predictive* sample of the language" (Harris 1961: 244; emphasis added).

"When a linguist offers his results as a system representing the language as a whole, he is *predicting* that the elements set up for his corpus will satisfy all other bits of talking in that language" (op. cit.: 17; emphasis added).

Harris carries out his morphemic analysis with the aid of such theoretical constructs as "frames of substitution;" these are selected on account of their contribution to the over-all simplicity of the description:

"The criterion which decides for *-ing*, and against *un-*, as the relevant environment in determining substitution classes [for verbs] is therefore a criterion of usefulness throughout the grammar, a configurational consideration" (Harris 1957: 143, n. 6; also 150).

The same argument recurs in Chomsky (1957: 55): "the only ultimate criterion in evaluation is the simplicity of the whole system."

Neither Bloomfield nor Harris presents a coherent and acceptable account of the nature of grammatical data. Yet, at least in my opinion, their positions were better than they are today made to appear. In particular, TG has seriously misrepresented Harris's grammar-conception, characterizing it as non-predictive and non-theoretical description interested merely in matters of "observational adequacy."

Chomsky's view of the nature of grammatical data has been explicitly self-contradictory from the start: In Chomsky (1957: 13) he claims that grammar explicates an intuitive concept whereas elsewhere (op. cit.: 49) he claims that, just like physics, grammar explains and predicts observable events. In Chomsky (1965) he emphasized the intuitive aspect, but recently he has returned to the observational, physicalist conception:

"... our scientist S ... studies language exactly as he studies physics, taking humans to be 'natural objects'" (Chomsky 1976: 315).

In this context linguists' capacity for self-contradiction seems almost unlimited. Chomsky has never studied speakers as natural objects. All he has ever done is to study his own intuitive knowledge of English with the aid of self-invented sample sentences; and this is something that natural objects cannot do. Similarly e.g., Lieb (1976: 198) and Wunderlich (1976: 81) claim that grammatical descriptions must absolutely be based on a corpus; and yet they themselves not once make use of a corpus in

their own published work. Sampson (1975) has tried to give a consistently observational account of grammatical data. Significantly, even he manages to present his case without the help of any actual corpus (for criticism, cf. Itkonen 1976 c and d).

Unlike the linguists mentioned so far, Labov has *actually* investigated real corpora, and therefore he can recommend the use of observation at least without being guilty of any obvious self-contradiction. Labov's position here is not quite easy to pin down. He generally seems to identify use of intuition with investigation of idiolects, which he considers methodologically unsound (e.g., Labov 1972: 191—202). Elsewhere, however, he admits that use of intuition is indispensable (op. cit.: 227 and 234). More recently he has come to explicitly entertain the view that in Chomskyan "clear cases," where results of intuition, observation, and experimentation coincide, or can safely be assumed to coincide, intuition suffices all alone, and insistence on the use of observation/experimentation results from misunderstanding (Labov 1975: 7—14). This view is, as it were, extensionally identical with mine; yet I find it unsatisfactory because it fails to indicate the precise relation between intuition and observation, or what it means for the two to "coincide."

Thus Labov no longer advocates a wholesale rejection of intuition in favor of observation/experimentation, or the replacement of grammar by socio- and psycholinguistics. However, such a position seems to be gaining ground, in spite of the fact that it stands in stark conflict with the descriptive practice of the majority of today's linguists, notably generativists and Montague-grammarians. Therefore I shall explicitly refute it in Sect. B and C.

So far we have been dealing with a straightforward dichotomy between self-invented sample sentences and a set of actual utterances, i.e., a corpus. The study of the latter type of data is quantitative in the sense that it must take relative frequencies of (different variants exemplifying) grammatical categories into account; moreover, in experimental-psycholinguistic research the data may be quantifiable also in the sense of containing several degrees of correctness or acceptability. The former type of data is that investigated by Saussure, Bloomfield, Harris, Chomsky, and Montague, among others. Its study is non-quantitative, i.e., qualitative, in a twofold sense. First, it has nothing to do with relative frequencies. Second, the data is discrete (= categorical, two-valued): these are the "clear cases," which are (known to be) definitely correct, and are contrasted with all other, less than clear cases. Consequently, observation is connected to the quantitative analysis of actual utterances whereas intuition is connected to the qualitative analysis of conceptual possibilities, i.e., either correct or less than correct sentences which may or may not be exemplified by actual utterances. In my opinion this dichotomy, as here characterized, is perfectly justified.

This simple dichotomy has been challenged by Ross (1973), who, with his notion of "squish," wishes to introduce a grammar-conception which is at the same time intuitive and quantitative. Notice, first of all, that Ross is primarily interested in establishing the non-discrete, quantifiable nature of *theoretical* concepts like "noun phrase." This leaves open the possibility that the questions or criteria which are used to rank particular words as to their "nounphrasiness" admit only of a discrete, yes-or-no answer, as in the genuine Guttman scale (cf. e.g., Mayntz et al. 1976: 57—63). However, since Ross wishes to fill as many slots of his implicational scale as possible, he subjects his noun phrase candidates to a set of "NP tests," i.e., lets them undergo definite transformations or inserts them into definite contexts, and comes out with a huge number of sentences, prefixed with one or two stars and/or question marks, which no one would ever have reason to utter. It is only natural that, as Ross's battery of stars and question marks so eloquently demonstrates, there can be no clear and reliable intuitions here. Yet it is possible that the data, in spite of its general unreliability, exhibits some kind of hierarchy of correctness, and is hence quantifiable. Now it seems reasonable enough that in experimental psychology, where we are interested in discovering the "profile" of the subjects' capacities, we must move them to do what they would not normally do, e.g., utter incorrect or outlandish sentences, and to try to do what they are not quite able to do, e.g., evaluate in a precise way the (in)correctness of outlandish sentences. But all this must be carried out in conformity with a general experimental methodology. It is certainly a mistake to rely here on one's intuition alone, as Ross is doing. As a consequence, pairing intuition with qualitative analysis and observation/experimentation with quantitative analysis remains a valid principle.

In the remaining part of this section I shall present, somewhat schematically, my own conception of the nature of grammatical data. A more detailed account is to be found in Itkonen (1974) and (1978).

Grammar traditionally concentrates upon the concept "correct sentence (or speech act) of L." A corpus consists of exemplifications of the concept "factually uttered sentence of L." These are two different concepts, as can be seen from the fact that, on the one hand, there are indefinitely many *correct* sentences of L which have never actually been uttered (but must, by definition, be described by grammar) and, on the other, there are indefinitely many actual utterances of *incorrect* sentences of L (i.e., sentences which, by definition, must not be described by grammar). Observation pertains only to space and time; and a corpus consists of spatiotemporal occurrences. Since grammar is not, and could not be, based on a corpus (cf. above), observation is incompatible with grammar. It is convenient to call the act of knowledge pertaining to "correct sentence" by the name of "(linguistic) intuition."

The distinction between "correct sentence" and "factually uttered sentence" is a special case of the general distinction between normativity and factuality, or between what one ought to do and what one does in fact. It is a well-known philosophical truth that this distinction cannot be eliminated by reducing one of its terms to the other. In particular, trying to derive "ought" from "is" means committing the "naturalistic fallacy."

Analyzing the concept "correct sentence" means analyzing those *rules* (or norms) which make sentences correct. Each correct sentence can be analyzed into a set of quite trivial rules — ultimately rules connected with particular words (i.e., word *-types)* and constructions — which make it correct. Such rules are potential objects of *conscious* knowledge. Just as an action contains a physical "substratum" without being identical with it, a rule could not exist without its own substratum, i.e., a regularity of actions, but is not reducible to it. Attempts at such a reduction, which amount to attempts at eliminating (linguistic) normativity altogether, are criticized in Itkonen (1976 b, c, and d).

Rules are not spatiotemporal entities and therefore cannot be observed but only intuited. The notion of correctness is inseparable from the notion of rule. Consequently, when one is observing a correct utterance, one's observation (of space and time) is in fact *subordinated* to one's intuition (of rule). This is the general relation, in linguistics, between intuition and observation (cf. also Friedman 1975). It parallels the general relationship between a rule and the actions conforming or failing to conform to it: the former is a *conceptual precondition* of, or a priori vis-à-vis, each of the latter. Analogously, Durkheim (1938: 57) insists that "social facts" must be analyzed in themselves, isolated from their individual manifestations. On the other hand, it is clear that, taken as a whole, the domain of non-normative actions is presupposed by the domain of rules and institutions.

Due to the normative, conceptual nature of the data, data-gathering in grammar does not consist in looking for new occurrences in new spatial and/or temporal regions, as in empirical science, but in *reminding oneself* of what one knows already (i.e., after one has learned the language in question). More precisely, the grammarian does not remind himself of what he or someone else has said as a matter of fact, but what one ought to say. In other words, the situation is here the same as in philosophical analysis, as characterized by Wittgensteinian philosophers of language (cf. e.g., Hare 1971 and Specht 1969: Sect. 9).

In each type of scientific data-gathering a subjective act of knowledge takes hold of something objective. In physics, the act is observation, and it pertains to measurable events. In logic, the act is logical intuition, and it pertains to inference rules and/or to the concept "valid formula." In grammar, as we have seen already, the act is linguistic intuition, and it pertains to rules of language. Rules of language exist *qua* objects of (three-level) *common knowledge* (cf. Lewis 1969: Ch. II). One's (subjective) intuition is identical with one's (three-level) contribution to common knowledge: one's intuition both pertains to a rule and, in part, creates it (for details, cf. Itkonen 1977).

There is only one possible way in which incorrect observations and intuitions, whatever their cause, can be corrected, namely through the intervention of others. If this possibility did not obtain, we would have a solipsistic, private-language universe. However, such a universe is logically impossible (cf. Itkonen 1974: Ch. II). Because observation and intuition are subjective by nature, they are necessarily fallible. Thus I emphatically reject the view that intuitions are somehow "incorrigible." However, I do claim that linguistic intuition is (objectively) *certain* to the same extent as logical or philosophical intuition.

Ringen (1975) recognizes that the role of intuition is, in principle, the same in grammar, logic, and philosophy. In addition, he requires, in particular in Ringen (1977), that in all these cases the use of intuition must be somehow philosophically justified. A similar claim is also forwarded in Cohen (1976). Now, it could be pointed out that Wittgenstein's refutation of private languages guarantees at least a modicum of intersubjective validity for those rules (of language or of logic) which our intuition pertains to. Moreover, our world happens to such that, as even Labov is willing to admit, there *are* "clear cases" about which we do possess a rather secure knowledge. The world might have been otherwise; but it is not. I do not think that there is anything more one can say. As Wittgenstein (1958: 136 and 180; 1969: 18 and 23) points out, a doubt or a need for justification which does not come to an end sooner or later is not even doubt. I think one must let the chain of justifications stop here, but Ringen and Cohen would apparently like to go one step farther.

B. Sociolinguistics

In Sect. A I made a precise claim concerning the relation between grammatical data on one hand and socio- and psycholinguistic data on the other. In Sect. B and C I intend to show that my claim is in fact true.

Grammar could be replaced by sociolinguistics only if it could be shown that the sociolinguist makes no reference, either explicitly or implicitly, to those normative phenomena which constitute the data of grammar or, equivalently, if it could be shown that the sociolinguist can dispense entirely with intuition and rely solely on observation (as well as on his capacity for theory-construction). If, as I claimed in Sect. A, grammatical data constitute an indispensable precondition of sociolinguistic data, then of course the sociolinguist could not possibly give up the use of intuition.

Social behavior exhibits no strictly deterministic or nomological regularities and can therefore be described and explained only statistical-probabilistically. It is often claimed that linguistic behavior is practically unpredictable, but this claim cannot be taken seriously. It is clear that probabilities for predicting actual utterances are extremely low. However, if one refers only to the general semantic content of potential utterances, it is not at all difficult to predict that people will say such and such under such and such circumstances. Furthermore, and more importantly, if we resort to *conditional* probabilities, i.e., if we ask what is probably the case, given that an utterance — either any utterance or more specifically an utterance containing such and such material — has been made, we can establish perfectly reliable probabilities with which different grammatical categories or constructions will occur (cf. Sect. II B). This characterization holds true independently of whether grammatical categories are exemplified by one invariant or by two or more variants. In the latter case we have the standard data for the Labov-type analysis of variation.

The question is now, whether it is possible for the sociolinguist to state the relative frequencies and to infer the corresponding probabilities without any reference, explicit or implicit, to the normativity of language. To show that this is not the case, it is enough to point out that there are in fact *two* types of probability of occurrence here, and that the sociolinguist's descriptive practice is adjusted to this fact. First, there is the occurrence of either one correct invariant exemplifying the grammatical category X or of two or more correct (or at least plausible) variants exemplifying the category Y. Second, there is the occurrence of variants, whether correct *or* incorrect, exemplifying the categories X and Y. It is with the *first* type of (probability of) occurrence that the sociolinguist is dealing. This can be seen from the fact that he operates with the notion of invariant or categorial rule, and may even regard such rules as representing the *normal* state of language (cf. Labov 1969: 738 and 1972: 223; Bailey 1973: 33 and 84); but in connection with the second type of (probability of) occurrence there could be *no* invariant or categorial rules, because there always can, and do, occur incorrect variants contradicting them, i.e., making them less than invariant.

What I just said, can be illustrated by considering Labov's descriptive practice. He states explicitly that sociolinguistic data is not described as such but is, rather, processed in accordance with "certain universal editing rules;" after the editing, "the proportion of truly ungrammatical and ill-formed sentences falls to less than two percent" (Labov 1972: 203). Now, it is clear that Labov edits his original data and evaluates the edited data as either correct or as "truly ungrammatical and ill-formed" on the basis of his *intuitive* knowledge about the *rules* of language. He cannot be relying just on observation, because what he is doing is precisely to evaluate his observations as either correct or incorrect. Furthermore, it would not help if Labov, instead of just relying on his own intuition, would try to experiment with the intuitions of his fellow sociolinguists, i.e., their reactions to the data at hand. Not only utterances, but also reactions to utterances are either correct or incorrect, and it is only on the basis of intuition about linguistic rules that an incorrect reaction can be recognized as incorrect. On the other hand, it is reasonable to say that Labov can *consult* (rather than experiment with) the intuitions of his fellow sociolinguists. This kind of consulting (which is always possible, although normally unnecessary) is located at the level of intersubjective, *pre-experimental* understanding.

The preceding argument involves an apparent difficulty. The learning of rules necessarily starts from observation, but in the course of this learning process there occurs a "leap" from observing actual occurrences to (intuitively) grasping the rule, which subsequently serves as a *criterion* for evaluating what is observed. Because the gap between factuality and normativity can be neither bridged not eliminated, as we know from philosophy, one has to leap over it. That one is in fact able to do so, is, if you wish, a proof of the creative nature of learning (cf. also Itkonen 1976 b).[2]

C. Psycholinguistics

The inner logic of TG has led its practitioners to base their descriptions on more and more outlandish sentences (cf. the discussion of Ross in Sect. A). It is only natural to assume that in such cases there are no reliable and intersubjectively valid intuitions, and this assumption has in fact been experimentally verified, e.g., in Spencer (1973). This state of affairs has, perhaps understandably, provoked a backlash: It has been claimed, e.g., in Derwing (1973), that the use of intuition is always unscientific and should be replaced by experimental methods. For instance, whether something is or is not a correct sentence, can presumably be established only as a result of experimentation. I have already explained, on general grounds, why I consider such a view as inadmissible. In this section I

[2] This kind of "leap" is closely similar, although perhaps not identical with what Rescher calls "imputation." For instance, causal or nomic necessity cannot be inferred from mere regularities of observable events. Rather, it is something that the human mind "imputes" upon observable events, and cannot help doing so (Rescher 1973: Ch. II). This explication of causality can, in turn, be shown to be practically identical with von Wright's (1974) "manipulative" or "interventionist" notion of causality.

shall show in detail that, rather than a result of experimentation, the concept of "correct sentence" is built-in into the experimental design and therefore constitutes one of its conceptual preconditions. I shall base my discussion on Greenbaum & Quirk (1970), which is one of the most thorough investigations in the field.

The central concept in Greenbaum & Quirk's study is "relevant noncompliance" (= RNC). In the present context we can ignore why they consider this concept so important. The test person is given a sentence, i.e., a "test sentence," on which he is instructed to perform an operation to reach a new sentence, i.e., a "target sentence." If he fails to carry out the instruction, i.e., if the sentence he produces is not the target sentence but an "evasion" of it, then his behavior is a case of RNC (op. cit.: Ch. II). To illustrate:

Test sentence		Target sentence
	negation	
He can certainly drive a car	→	He cannot certainly drive a car
non-deviant	RNC? — big	deviant
	question	
She has mentioned it at all	→	Has she mentioned it at all?
deviant	RNC? — small	non-deviant

Now, it is immediately clear that the deviant or non-deviant, i.e., incorrect or correct, nature of test or target sentences is known on the basis of intuitive, *pre-experimental* knowledge: it is a fact which precedes the experiment and determines its character.

Notice also that the pre-experimental concept of correctness is a built-in component of the very concept of RNC. A target sentence may have several variants, or as the authors put it, "we may *decide in advance* that the target sentence may have more than one form" (op. cit.: 20; emphasis added). Whether a sentence is merely a variant of the target sentence or an evasion of it, and hence a token of RNC, depends on whether it is, again pre-experimentally, known to be (in)correct like or unlike the target sentence (op. cit.: Ch. III). To illustrate:

Test sentence		Variants of the target sentence
	question →	{ Will he probably stay late?
He will probably stay late		Will he stay late probably? }
non-deviant		deviant

	question	Evasion of the target sentence
He will probably stay late non-deviant	→	Is it probable that he will stay late? non-deviant

A change which in one context produces a variant may in another context produce an evasion. In the above example, changing the place of "probably" did not affect the status of the new sentence. However, when the test sentence is "He deeply admires X," moving "deeply" to the end of the sentence upon questioning it produces an evasion of the (deviant) target sentence "Does he deeply admire X?"

Actually the way in which the intuitive notion of correctness determines the experimental design can be demonstrated even more directly. Greenbaum and Quirk note that there is simply no point in setting up experiments when there is "no reason to believe that we would have much less than 100 per cent acceptance" (op. cit.: 18). In such a case, experimentation is "a slightly absurd exercise, with the results a foregone conclusion" (Wason & Johnson-Laird 1972: 78). To me it seems clear enough that people who wish either to eliminate or to "justify" intuition even in the clear cases are engaged precisely in such "absurd exercises."

To think that the concept of "correct sentence" emerges as a result of experimentation is to commit a fallacy analogous to thinking that the concept of "centimetre" *results* from measuring the height of a person and from noting that he is, e.g., 185 centimeters tall.

D. Conclusion

Clear cases, i.e., well-established rules, can and must be investigated by means of intuition. Unclear cases, i.e., less than well-established rules, as well as actual linguistic behavior in general, must be investigated by means of observation. There must obviously be an area which *mediates* between intuition and observation, or between rule and action. Not surprisingly, this area is diachronic/geographical/social *variation.*

Grammatical data is a conceptual precondition of socio- and psycholinguistic data. Therefore grammar is a "transcendental" science in relation to socio- and psycholinguistics in precisely the same way as Husserl's "phenomenological psychology" and Winch's "aprioristic sociology" are transcendental sciences in relation to experimental psychology and empirical sociology, respectively. The following characterization of phenomenological psychology applies, *mutatis mutandis,* to grammar as well:

"Although it is true that empirical psychology is able to bring to light valuable psychophysical facts . . . , it nevertheless remains deprived of . . . a definite scientific evaluation of these facts so long as it is not founded on a systematic science of conscious life which investigates the psychical as such with the help of 'immanent' intuitive reflection. . . . The experimental method is indispensable . . . But this does not alter the fact that it *presupposes* what no experiment can accomplish, namely, the analysis of conscious life itself" (Kockelmans 1967: 425; emphasis added).

Thus phenomenological psychology is interested primarily in the necessary a priori of every possible empirical psychology" (op. cit.: 447).

Notice also the following justification of the Winch-type sociology:

„Thus when we elucidate concepts we are elucidating the possibilities of social life, and conversely when we explain social life we elucidate the concepts available to members of that society. . . . We now see that the social sciences are permeated by conceptual considerations" (Ryan 1970: 145).

A science need not be transcendental in relation to a human or social science only. Lorenzen's (1969) "protophysics," which investigates the hierarchically-orded norms of measuring length, time and mass, is a transcendental science in relation to physics (cf. also Böhme 1976). That is, it investigates the concept "possible physical event" just as grammar investigates the concept "possible correct utterance." Interestingly, in his "Amsterdam Lectures" Husserl clearly anticipated protophysics as a "general aprioric science of nature."

II Methods

A. Grammar

Each scientific grammar-conception aims at describing the similarities and differences between correct sentences of L in a maximally systematic or general way. TG tries to achieve this by generating all and only correct sentences of L with their "correct" structural descriptions by means of as few grammatical rules as possible. Instead of representing rules of language by a *list* of corresponding rule-sentences, a scientific grammar represents them by a *system* of grammatical rules. This amounts to replacing piecemeal atheoretical certainty (about the existence of rules and the truth-value of rule-sentences) by systematic theoretical uncertainty (about the truth-value of scientific hypotheses expressible in terms of grammatical rules).[3]

[3] In other words, the following entities must be carefully distinguished: a) rules of language, which may or may not exist; b) rule-sentences, which purport to describe single rules of language and are true or false in a self-evident, "necessary" way (cf. below); c) grammatical rules, which are used to make theoretical hypotheses (e.g., the "complex NP constraint") about large sets of rules of language.

In Sect. I it already became evident that, due to the non-spatiotemporal nature of its data, grammar must be, at least in some general sense, similar to logic and philosophy. This methodological similarity can be demonstrated in considerable detail. Generative grammars and systems of logic are interested in correctness and validity of sentences, respectively, not in their empirical truth. Correctness and validity are properties whereas empirical truth is a relation. That is, unlike the criteria for empirical truth, the criteria for correctness and validity do not lie outside of the sentence, but in the sentence itself. As an "extended axiomatic system" (Wall 1972: 197—212), a generative grammar is neither true nor false, and cannot of course be taken as an axiomatic theory. However, it has its own metagrammar which does make either true or false claims about the generative capacity of the grammar, in precisely the same way as the metalogic of a logical system makes true or false claims about the latter's generative capacity.

The requirements to generate *all* and *only* correct sentences of L have their counterparts, within logic, in the requirements of completeness and soundness, i.e., the requirements to generate all and only valid formulae expressible in the relevant formal language. Even if a system has been proved as *formally* sound and complete, it can still be (nonempirically) falsified by showing that it either generates an *intuitively* invalid formula or fails to generate an *intuitively* valid formula. For instance, although von Wright's original "monadic" system of deontic logic with the two axioms "$\sim (Op \,\&\, O \sim p)$" and "$O(p \,\&\, q) \equiv (Op \,\&\, Op)$" was formally sound and complete, it was nevertheless falsified, because it generated the intuitively invalid formula "$Op \supset O(\sim p \supset q)$," which says that doing something forbidden, i.e., $\sim p$, commits one to doing anything whatever, i. e., q. This system was also open to the criticism that it did not generate the intuitively valid formula "$P(p \vee q) \supset (Pp \,\&\, Pq)$," which says that if one is permitted to do p or q, then one is permitted to do p and one is permitted to do q (for details, cf. Itkonen 1975 b). On the other hand, there is an important difference between the *research interests* of grammar and logic, and this has clear (although little-noticed) repercussions on how the formal structure of grammars should be conceived of (cf. Itkonen 1976 a).

Furthermore, philosophical analysis too is structurally similar to grammatical analysis. For instance, the analysis of the concept "knowing that one knows," as presented in Hintikka (1962), consists in postulating a "basic" or deep meaning from which several "residual" or surface meanings are derived. Generality is achieved, once again, by replacing a list of (apparently) disconnected items through a system which shows in a perspicuous way their similarities and differences (cf. Itkonen 1975 b). To give another example, the analysis of the concept "virtue," as presented in Plato's *Meno,* consists in looking for a definition which

would capture the "essence" of virtue or the *basic* virtue, of which all cases of virtuous behavior are mere (surface) exemplifications. Although the analysis remains uncompleted, it nevertheless offers many clear instances of falsification in philosophy (cf. Itkonen 1978: Ch. XI).

The above-mentioned types of analysis can be identified as cases of *explication,* as defined in Pap (1958). According to Pap, explication consists in replacing intuitive necessity by formal necessity. Now, it can be shown that rule-sentences, i.e., sentences describing single rules[4], can be falsified neither by incorrect actions nor by correct actions. Briefly, they are unfalsifiable, or necessarily true or false (cf. e.g., Itkonen 1975 b: Sect. 2). It is clear that what we have here is *intuitive* necessity. On the other hand, it is natural to interpret the TG-type analysis as attempting a systematic translation of sentences about (e.g.) "correct in L" into sentences about "generable by G_L." Insofar as such a translation cannot be made, in either of the two directions, the grammar G_L has been falsified. It is a *formally* necessary truth (or falsity) that a grammar generates or does not generate a given sentence. Consequently, grammatical analysis does translate intuitive necessity into formal necessity. Analogously, von Wright transforms the intuitive necessity of, e.g., the sentence "If one ought to do p and is not forbidden to do q, then one is permitted to do both p and q" into the formal necessity of "$(Op \,\&\, \sim O \sim q) \supset P(p \,\&\, q)$" by constructing his monadic system of deontic logic. The same holds true of philosophical analysis as well, as Pap himself has shown. Hence, explication functions as the common denominator of grammar, logic, and philosophy.[5]

The data of grammar, logic, and philosophy is not spatiotemporal (even if, to repeat, it has its spatiotemporal "substratum"). *A fortiori,* these sciences are not concerned with matters of causality, i.e., with discovering cause-effect relationships in their data. Indeed, the word "autonomous" in the expression "autonomous linguistics" could be understood as meaning "autonomous vis-à-vis causal influences." On the other hand, it is well known that the causalist terminology makes

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[4] My rule-sentences are what Leech (1974: Ch. 5), for instance, calls "basic statements." The important thing here is that unlike the basic statements of natural science, the basic statements of grammar are not about particular spatiotemporal occurrences. One can easily convince oneself of this by looking at Leech's examples. My claim is that, here as elsewhere, the basic statements of grammar are about (maximally simple) *rules of language.*

[5] Of course, this is not to deny that there are important differences between logic and philosophy on the one hand and grammar on the other. In particualr, logic and philosophy have a *prescriptive* research interest: they intend to *improve* our rules of inferring and thinking. By contrast, grammar (as it exists today) is content to describe rules of speaking as they are.

frequent intrusions into grammar. For example, it might be said that in German [g] becomes [k] *because* it occurs in the word-final position. The causal mechanism which is vaguely implied here must be of psychological nature. The investigation of such mechanisms is a useful accompaniment of grammatical analysis (cf. Sect. II B). However, it must be clearly understood that, as an empirical-experimental undertaking, investigation of causality is qualitatively different from grammatical analysis, which, as we have seen, is a case of explication, or conceptual analysis. It is also impossible that the former might replace the latter (cf. Sect. I C).

Chomsky has never really grasped the relationship between grammar and psychology. He makes an elementary mistake when he tries to explain grammatical data extragrammatically, i.e., psychologically, but refuses to consider any extragrammatical evidence, i.e., facts of linguistic "performance," preferably elicited under experimental conditions. This procedure has been rightly accused of circularity, e.g., in Botha (1971) and Derwing (1973). Surprisingly, Chomsky fails to see the point:

> "Some have argued that the approach outlined here is 'circular,' failing to see that if this were true, the various empirical principles postulated within theories of generative grammar would be irrefutable, whereas they are, in fact, all too refutable and have been repeatedly modified in the light of new discoveries and observations" (Chomsky 1975: 11).

Chomsky is here guilty of two confusions. First, he confuses "circular," in the sense intended by his critics, with "tautological" or "unfalsifiable." Secondly, he confuses "falsifiable" with "empirical." Grammatical and logical descriptions are equally "circular:" In both cases descriptions of data of the type X are tested against (new) data of the type X, and in both cases descriptions are falsifiable (cf. above). There is nothing wrong with this kind of (non-tautological) circularity. However, Chomsky's "psychologism" exhibits a wrong kind of circularity: He makes the hypothesis that descriptions of *grammatical* data have *psychological* relevance, but he "tests" this hypothesis only against more grammatical data. The fact that grammatical (or logical) descriptions are falsifiable proves absolutely nothing about their psychological relevance, or about their allegedly empirical status.

To be sure, Chomsky occasionally pays lip-service to the investigation of performance data, e.g., in Chomsky (1976: 306), but he never follows this advice in practice. In *op. cit.* he still maintains the myth of the psychological reality of transformations and simply ignores all *experimental-psycholinguistic* evidence, reviewed, e.g., in Fodor et al. (1974), which clearly points in the other direction. The confusion between grammar and psychology (of which the pre-Chomskyan autonomous linguistics was entirely free) has been made complete by Chomsky's (1976: 304—305) proposal to eliminate altogether the distinction between conscious knowledge and unconscious psychological mechanisms and to refer to both uniformly by the term "cognizance."

Since grammatical data is not spatiotemporal, it goes without saying that Hempel's (1965) deductive-nomological model of (empirical) explanation is inapplicable in grammar. However, it is significant that it is precisely the D—N model, and not any of the statistical models of explanation, which has been proposed as an adequate model of grammatical explanation, e.g., in Wunderlich (1974). This choice recognizes, at least, the discrete nature of grammatical data (cf. Sect. I A), but then mistakes it for the discreteness of a fully deterministic *spatiotemporal* mechanism. It should be self-evident, however, that (macro-)explanation of actual social, including verbal, behavior can be of *statistical* character only (cf. Sect. B).

This section sums up the general argument of Itkonen (1974) (cf. also Itkonen 1975 a and 1976 c). Its more philosophical implications will be mentioned in Sect. C. Congenial grammar-conceptions have been presented in Ringen (1975) and Lass (1976).

B. Socio- and psycholinguistics

Sociolinguistics, as this term is currently understood, is an empirical science, and therefore its methodology cannot be discussed without explicit reference to the philosophy of empirical sciences. The latter was dominated, until recently, by the hypothetico-deductive conception of science, as expounded, e.g., in Popper (1965), 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 D—N 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." What is more, if it is said, e.g., that a definite change happens as a function of time, such a formulation actually *conceals* the real state of affairs, because time as such (just as little as place as such) could not bring about any changes.

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 state as a desideratum of any adequate methodology of empirical linguistics that it should provide causal models of linguistic behavior. One type of model will be described in what follows. Attempts at axiomatization are commendable, but nevertheless of secondary importance only.

Speaking is a form of social behavior. It is an undisputed fact that each type of social behavior, including rule-governed or normative behavior, exhibits a larger or smaller amount of unpredictable variation. Therefore description and explanation of actual linguistic behavior must be of *statistical* nature. It can even be stated as a geral principle that if a linguistic description is not statistical, then it cannot be empirical, i.e., it cannot deal with regularities in space and time. (However, statisticalness does not entail empiricalness.)

For some time, Hempel's (1965) inductive-statistical (= I—S) model, which for convenience is presented here together with his D—N model, provided the standard model of statistical explanation:

$$\begin{array}{ll} \text{D—N} & \text{I—S} \\ (x)\,(Fx \supset Gx) & P(Gx\,|\,Fx) = r \\ Fa & Fa \\ \hline Ga & Ga \quad r \end{array}$$

In the D—N model the explanandum "Ga" follows logically from the explanans. By contrast, in the I—S model the explanans only confers the probability *r* upon "Ga." Moreover, according to Hempel, the event *Ga* can be said to be explained only when *r* is relatively high.

Salmon (1971) presents a different, process-like notion of statistical

explanation. According to it, we start with a probability statement "P(G|F) = r," which states that if anything is a member of the reference class F, it is a member of the attribute class G with the probability *r*. F is said to be homogeneous with respect to G if no attempts to further specify F affect the probability *r*. Next, we choose a category H and form, with it, a new reference class F & H, which is hopefully more homogeneous with respect to G than F alone was. If this is the case, the probability of G will increase, relative to F & H. On the other hand, relative to the other new reference class F & $\bar{H}$, the probability of G will decrease. For this reason (and some others) I agree with Stegmüller (1973: 330—350) that in the present context the term "statistical explanation" should be replaced by a more neutral term like "statistical analysis." This kind of analysis consists, then, in changing the prior weight *r* into the posterior weights *s* and *t*, irrespective of whether *r* is high or low:

$$\begin{array}{ccccc} P(G|F \,\&\, H) = s & > & P(G|F) = r & > & P(G|F \,\&\, \bar{H}) = t \\ \text{2. stage} & & \text{1. stage} & & \text{2. stage} \end{array}$$

In a situation like this, H is said to be *statistically relevant* to G. Statistical relevance may in turn be an indicator of *causal* relevance.

Reference classes may be homogeneous as far as we know, i.e., epistemically homogeneous, but still ontologically inhomogeneous. The two types of homogeneity undisputably coincide in (well-confirmed) D—N explanations, where "P(Gx|Fx) = 1," i.e., "(x) (Fx ⊃ Gx)," holds true: here F is literally included in G. For dogmatic determinists, the use of statistical analysis is always, e.g., in microphysics, a sign of ontological inhomogeneity.

It will be seen that Salmon's notion of statistical analysis corresponds rather well to the descriptive practice of social scientists in general, and sociolinguists in particular.

Actual linguistic behavior is described by performance grammars. Suppes (1972) develops a context-free performance grammar in which rules are given constant probabilistic weights in the following way:

$$r:\ X \rightarrow \left\{\begin{array}{c} Y \\ Z \\ W \end{array}\right\} = \begin{array}{l} r_1: X \rightarrow Y \\ r_2: X \rightarrow Z \\ r_3: X \rightarrow W \end{array} \quad \text{and } P(r_1) + P(r_2) + P(r_3) = 1$$

In other words, the rules in any block must have probabilities the sum of which equals 1. What, precisely, each $P(r_i)$ is, must be determined empirically. The probability of a definite utterance or, more realistically, of an utterance exemplifying definite (high-level) grammatical categories, can be computed, relative to other utterances, simply by multiplying the probabilities of the rules figuring in its derivational history.

Suppes-type grammars may be useful first approximations. However, giving fixed probabilities to rules is quite unrealistic because rules of natural language grammars obviously apply with a higher or lower probability depending upon their (linguistic and/or extralinguistic) contexts (cf. also Klein 1974: Ch. 4). What is needed, is obviously a method of ascribing *conditional* probabilities to rules. Salomaa (1969) develops a grammar in which the probability of a rule is made dependent on the immediately preceding rule. This is an improvement vis-à-vis the Suppes-type grammar, but even from the intralinguistic point of view there is no reason why we should restrict our attention to the influence of the immediately preceding rule. Moreover, concepts like "preceding" and "following" cease to be meaningful when we take, as we must, the influence of extralinguistic factors into account.

A performance grammar operating with conditional probabilities makes statements of the type "$P(A|B) = r$." It is natural to interpret B as an independent, causally effective variable vis-à-vis A and to start looking, in the way suggested by Salmon, for additional independent variables C, D, etc. If B, C, D, etc. are linguistic variables, a causal interpretation can be achieved only by postulating a psychological (or perhaps physiological) mechanism which connects them with A. Thus, for instance, if A is the application of the devoicing rule and B is the word-final position, then in order to say that B *causes* A (with a certain probability), we obviously need to assume a (stronger or weaker) disposition to do A which is, under some circumstances, triggered by definite factors, including B.

Performance grammars deal indifferently with outputs of obligatory and optional rules. As is well known, Labov's notion of "variable rule" concentrates solely upon the latter. Labov's merits in arousing the general interest in sociolinguistics are beyond dispute. In what follows, however, I shall argue that the notion of "variable rule" is open to two objections, at least one of which is decisive. First, it is hardly satisfactory from the viewpoint of the philosophy of science to merely refer to a computer program when it should be explained how the probabilistic weights connected with different variable constraints are arrived at (cf. e.g., Cedergren & Sankoff 1974). It also follows that the corresponding notion of *causality* remains opaque, at the very least. Secondly, and more seriously, Labov and his associates explicitly base their notion of variable rule on the assumption that there are no *interaction effects* between variable contraints or independent variables. In other words, they assume that the force with which B influences A in "$P(A|B)$" remains the same in all contexts, e.g., in "$P(A|B \& C)$" or "$P(A|B \& D)$." Now, it is quite easy to show, with Labov's own data, that this is not the case (cf. below). Consequently, Labov-type variable rules must be rejected. — This very simple argument has been presented already in Klein (1974).

Here I shall outline what I take to be a more adequate causal model for linguistic behavior. In particular, it is able to handle cases of interaction. It is taken directly from Boudon (1974), which I recommend to linguists' attention. Blalock (1968) has been used as background information.

The significant linguistic variables are of nominal or qualitative nature, i.e., their values cannot be ordered on a quantitative scale. (The only quantitative aspect of values of such variables consists in their frequencies of occurrence.) Usually there is no upper limit to the number of the values of a (linguistic) qualitative variable. Unfortunately, Boudon's models 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.

We are developing *causal* models here. Causality is normally an asymmetric notion, in the sense that the effect of a sufficient cause is not a sufficient, but a necessary condition of that cause; and the same holds, *mutatis mutandis*, of the effect of a necessary cause. Only effects of sufficient *and* necessary causes are sufficient and necessary conditions vis-à-vis those causes, but such cases of *equivalence* are seldom met in practice. Now, our models are restricted to equivalence between causes and effects. Boudon points out (op. cit.: 35) that some work by Guttman, for instance, bears indirectly upon the possibility of constructing (more realistic) models of *implication.*

As has been noted before, no strictly deterministic regularities exist in social science. Therefore, once the formulae *p* and *q* are replaced by the classes A and B, we notice that instead of the truth-functional notion of equivalence, we have here the following type of *weak* equivalence:

Not:

	q	~ q
p	true	false
~ p	false	true

but:

	A	$\bar{A}$
B	often	seldom
$\bar{B}$	seldom	often

In other words, it is never the case that A and B on the one hand and $\bar{A}$ and $\bar{B}$ on the other would totally coincide. Instead, they overlap to a larger or smaller extent:

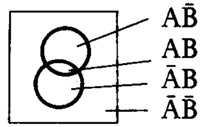


fig. 1

We are dealing, so far, with two dichotomous variables x_a and x_b with, respectively, two values $A/\bar{A}$ and $B/\bar{B}$. Our data consists of cross-tabulated frequencies, e.g.:

	A	Ā	
B	35	20	55
B̄	15	30	45
	50	50	100

fig.2

We make the following notational convention:

	A	Ā	
B	n_{ab}	$n_{\bar{a}b}$	n_b
B̄	$n_{a\bar{b}}$	$n_{\bar{a}\bar{b}}$	$n_{\bar{b}}$
	n_a	$n_{\bar{a}}$	N

fig. 3

Now we can define: $\frac{n_{ab}}{n_a} = \frac{n_{ab}/N}{n_a/N} = \frac{p_{ab}}{p_a} = p_{b,a}$

"$p_{b,a}$" is identical with "P(B|A)," except that the former deals with frequencies while the latter deals with probabilities.

Next we define a suitable coefficient of correlation (= phi-coefficient):

$$f_{ab} = p_{b,a} - p_{b,\bar{a}}\ ; \quad f_{ba} = p_{a,b} - p_{a,\bar{b}}\ ; \quad \varphi_{ab} = \sqrt{f_{ab}\, f_{ba}}$$

f_{ab} can be transformed into $\frac{p_{ab} - p_a\, p_b}{p_a\, p_{\bar{a}}}$. Given that the numerator is identical, in the probability notation, with "P(A & B) — P(A) × P(B)," it is clear that if the difference does not equal 0, then there must be a (positive or negative) correlation.

For instance, with the data of fig. 2 we get:

$$f_{ab} = \frac{0.35}{0.50} - \frac{0.20}{0.50} = 0.7 - 0.4 = 0.3\ ;$$

$$f_{ba} = \frac{0.35}{0.55} - \frac{0.15}{0.45} = 0.64 - 0.33 = 0.31\ ; \quad \varphi_{ab} = 0.3$$

The φ-coefficient measures the strength of the correlation between x_a and x_b. As such, it says nothing about the degree of the certainty or the probability that such a correlation does in fact obtain. As usually, this question is decided by a significance test. A chi-squared test would be applicable here. However, I can dispense with discussing significance tests, because, as e.g., Labov (1972: 49) points out when referring to the "extreme generality of linguistic behavior," any moderately large sample can be safely taken as representative of its speech community.

Next we have to define a *causal* coefficient (= d-coefficient). It must be mentioned that in connection with dichotomous variables the talk of causality is the most natural when Ā is not just the complement of A (= "anything but A"), but has a well-defined status of its own (e.g., "female" as the opposite of "male"). It is a well-known truth that numerical, non-experimental analysis cannot as such tell us the *direction* of causality. This can be decided or assumed only on the basis of additional considerations. — Let us take the same data as in fig. 2, and let us assume that there is reason to believe that the causal influence goes from x_a to x_b. Now the group of those 35 units which are both A and B among those 50 units which are A, is divisible into two subgroups: either they are B *because* they are A (= a_{ab}), or they are B for some other reason (= e_b). It is natural to think that e_b can be measured by the proportion of the 20 ĀB-units among the 50 Ā-units, since the ĀB-units are B although there is no causal influence a_{ab}. Presumably the same proportion among AB-units would be B, even if they were not A, i.e., they would be B for some other reason (= e_b).

We get the following equations:

$$p_{b,a} = a_{ab} + e_b$$
$$e_b = p_{b,\bar{a}}$$

Therefore:

$$a_{ab} = p_{b,a} - e_b$$
$$= p_{b,a} - p_{b,\bar{a}}$$
$$= 0.7 - 0.4 = 0.3$$

So we see that in this maximally simple case f-coefficients and a-coefficients coincide.

We can equally calculate the a_{ba}-coefficient (which, to be sure, conflicts with the assumed direction of causality) and then the symmetric d-coefficient:

$$d_{ab} = d_{ba} = a_{ab}\sqrt{\frac{p_a\, p_{\bar{a}}}{p_b\, p_{\bar{b}}}} = a_{ba}\sqrt{\frac{p_b\, p_{\bar{b}}}{p_a\, p_{\bar{a}}}}$$

In practice it is often convenient simply to use f- and a-coefficients instead of φ- and d-coefficients.

The equation "$p_{b,a} = a_{ab} + e_b$" can be transformed into "$p_b = a_{ab}\, p_a + e_b$," i.e., "0.55 = 0.3 · 0.5 + 0.4." When the decimal numbers are converted into integers, the equation can be read as saying that the 55 B-units consist of three groups: first, those 15 AB-units which are B *because* they are A; second, those 20 AB-units which are B for some other reason; third, the 20 ĀB-units which, of necessity, are B for some other reason.

If we assume that both A and Ā are causally effective, the following picture emerges:

Something is B *because* it is A ($= a_{ab}$), or for other reasons ($= e_b$)

Something is B *although* it is $\bar{A}$; hence it is B for other reasons ($= e_b$)

	A	$\bar{A}$
B	35	20
$\bar{B}$	15	30

Something is $\bar{B}$ *although* it is A; hence it is $\bar{B}$ for other reasons ($= e_{\bar b}$)

Something is $\bar{B}$ *because* it is $\bar{A}$ ($= a_{\bar a \bar b}$), or for other reasons ($= e_{\bar b}$)

fig. 4

Here I have defined the meaning of (probabilistic) causality of our models. As far as I know, Labov-type sociolinguistics has given no corresponding definition.

Next, let us consider cases with three variables. Let the initial numerical data be as follows, with x_c as the dependent variable:

	B	$\bar{B}$	
C	374	120	494
$\bar{C}$	206	300	506
	580	420	1000

fig. 5

We get:

$$f_{bc} = a_{bc} = p_{c,b} - p_{c,\bar b} = \frac{0.374}{0.580} - \frac{0.120}{0.420} = 0.36$$

To put it differently, B is statistically relevant to C:
$P(C) = 0.49 < P(C|B) = 0.64$

That is, the probability of the joint occurrence of B and C is more than random: $P(B \& C) = 0.37 > P(B) \times P(C) = 0.26$

Now we take into account a third variable x_a, which we assume also causally influences x_c, and cross-tabulate the data accordingly:

	A			$\bar{A}$			
	B	$\bar{B}$		B	$\bar{B}$		
C	294	72	366	80	48	128	
$\bar{C}$	126	108	234	80	192	272	
	420	180	600	160	240	400	1000

$f_{ac} = 0.29$
$f_{ab} = 0.30$
$f_{bc} = 0.36$

fig. 6

Next we compute the (assumedly) causal a-coefficients. In a case with more than two variables this presupposes that we have in mind a definite model depicting the causal relationships that we assume to obtain. In the present case the most simple model would be this:

$$\begin{array}{ccc} x_a & & x_b \\ & \searrow \; \swarrow & \\ & x_c & \end{array}$$

fig. 7

The a-coefficients are computed as before, except that this time they are identifiable as *conditional* correlation coefficients. For instance, if we consider the influence a_{bc}, there are this time *two* situations where it can obtain, namely either with the positive or with the negative value of x_a. (Of course, the same is true, *mutatis mutandis*, of the influence a_{ac}.) Consequently, we get two values "$p_{c,ab} - p_{c,a\bar b}$" and "$p_{c,\bar a b} - p_{c,\bar a \bar b}$." If the values are identical, we have proof that the influence of x_b upon x_c is constant, or independent of any other factors. Models with constant a-values are called *disjunctive* models. If, on the other hand, we have "$(p_{c,ab} - p_{c,a\bar b}) \neq (p_{c,\bar a b} - p_{c,\bar a \bar b})$," then we have proof that a_{bc}, i.e., the influence of x_b upon x_c, is itself influenced by, or dependent upon the variable x_a. If this is the case, we have to do with a *conjunctive* or *interaction* model.

My argument here can be summed up as follows. Labov assumes that disjunctive models are sufficient in sociolinguistics. I am going to show that Labov's own data require the use of conjunctive models. To repeat, the notions of disjunctive and conjunctive model are directly taken from Boudon (1974).

We get the following a-values:

$$a_{ac} = p_{c,ab} - p_{c,\bar a b} = \frac{0.294}{0.420} - \frac{0.080}{0.160} = 0.2$$
$$= p_{c,a\bar b} - p_{c,\bar a \bar b} = \frac{0.072}{0.180} - \frac{0.048}{0.240} = 0.2$$
$$a_{bc} = p_{c,ab} - p_{c,a\bar b} = \frac{0.294}{0.420} - \frac{0.072}{0.180} = 0.3$$
$$= p_{c,\bar a b} - p_{c,\bar a \bar b} = \frac{0.080}{0.160} - \frac{0.048}{0.240} = 0.3$$

The first noticeable thing here is that the f-values and the a-values are seen to differ:

$f_{ac} = 0.29$; $a_{ac} = 0.2$
$f_{bc} = 0.36$; $a_{bc} = 0.3$

We are primarily interested here in the causal influences of x_a and x_b upon x_c. But we also notice that there is a correlation between x_a and x_b:

$$p_{b,a} - p_{b,\bar a} = \frac{0.420}{0.600} - \frac{0.160}{0.400} = 0.3$$

Hence, assuming that it is x_a which influences x_b, we must replace our original model with the following one:

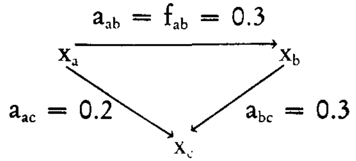


fig. 8

With these values we are able to distinguish between the *direct* and the *indirect* influence of x_a upon x_c. It can also be seen that the correlation coefficient f_{ac} consists of these two influences in the following way:

$$\begin{aligned} f_{ac} &= a_{ac} + f_{ab} \cdot a_{bc} \\ &= a_{ac} + a_{ab} \cdot a_{bc} \\ 0.29 &= 0.2 + 0.3 \cdot 0.3 \end{aligned}$$

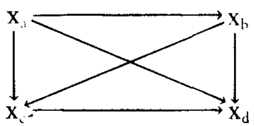
The value f_{bc} can be computed in a similar way, but it is intuitively less natural.

To sum up, we have here the following three disjunctive models, which are interrelated through simple algebraic transformations:

$$\begin{aligned} p_{c,ab} &= a_{ac} + a_{bc} + e_c \\ p_c &= a_{ac}\, p_a + a_{bc}\, p_b + e_c \\ f_{ac} &= a_{ac} + f_{ab} \cdot a_{bc} \end{aligned}$$

These models could be called "proportion model," "multiple regression model" (in a figurative sense), and "correlation model," respectively.

Our present case is the prototype of a disjunctive model. All more complex disjunctive structures can be generated from it by a simple recursive rule. That is, the f-values of, e.g., a disjunctive structure with four variables can be analyzed in the same way into simpler f-values and ultimately into a-values:



For instance:

$$\begin{aligned} f_{ad} &= a_{ad} + a_{bd} \cdot f_{ab} + a_{cd} \cdot f_{ac} \\ &= a_{ad} + a_{bd} \cdot a_{ab} + a_{cd} \cdot (a_{ac} + a_{bc} \cdot f_{ab}) \\ &= a_{ad} + a_{bd} \cdot a_{ab} + a_{cd} \cdot a_{ac} + a_{ac} \cdot a_{bc} \cdot a_{ab} \end{aligned}$$

fig. 9

Notice that the above equation exhausts all possibilities of going from x_a to x_d. It is of course a different question whether such pure disjunctive structures are ever found in reality.

Our simple model with three variables represents a clear case of Salmon-type statistical explanation: $P(C|A \,\&\, B) = 0.7 > P(C|B) = 0.64 > P(C|\bar{A} \,\&\, B) = 0.5$. Moreover, it already suffices to illustrate

the sense in which hypothetical models can be said to be constructed and tested in non-experimental social and psychological research. If we have assumed, for instance, that there is no (causal) relationship between x_a and x_b, then this assumption is — temporally at least — refuted by the existence of the correlation $f_{ab} = 0.3$. Remember, however, that statistical relevance may, but does not have to, be an indicator of causal relevance. On the other hand, (genuine) lack of statistical relevance entails lack of causal relevance.

Our model can also be used to illustrate the distinction between frequency and probability. If our data exhibits a near-perfect agreement with the assumption of a disjunctive structure, it may be legitimate to infer that the values ascribed to our model represent objective probabilities, i.e., "real" properties of our subject matter, whereas our observational values are partly due to chance.

Let us take one more example of a disjunctive structure. Let the data be as follows:

	B	$\bar{B}$	
C	32	24	56
$\bar{C}$	18	46	64
	50	70	120

$$\begin{aligned} f_{bc} &= 0.3 \\ P(C) &= 0.45 < P(C|B) = 0.64 \end{aligned}$$

fig. 10

Thus x_b is statistically relevant to x_c, and we assume that this is a sign of causal relevance as well. Suppose, however, that we take a third variable x_a into consideration and obtain the following data:

	A			$\bar{A}$			
	B	$\bar{B}$		B	$\bar{B}$		
C	30	15	45	2	9	11	
$\bar{C}$	10	5	15	8	41	49	
	40	20	60	10	50	60	120

$$\begin{aligned} f_{ab} &= 0.5 \\ f_{ac} &= 0.57 \\ f_{bc} &= 0.3 \end{aligned}$$

fig. 11

We have been assuming that we have the same type of model as in fig. 8. But when we compute the a-values, we get the following:

$$\begin{aligned} a_{ab} &= 0.48 \sim 0.5 \\ a_{ac} &= 0.53 \sim 0.57 \\ a_{bc} &= 0 \sim 0.02 \end{aligned}$$

In other words, in spite of the statistical correlation $f_{bc} = 0.3$, there is no causal influence between x_b and x_c. The same thing can be expressed in the probability notation as follows:

$$\left[P(C|A \,\&\, B) = \frac{30}{40}\right] = \left[P(C|A) = \frac{45}{60}\right]$$

In a case like this, the statistical relevance of x_b to x_c disappears, or "x_a screens off x_b from x_c," as Salmon (1971) would say.

The present data is compatible with two entirely different interpretations, namely these:

$$\begin{tikzcd} & x_a \arrow[dl] \arrow[dr] & \\ x_b & & x_c \end{tikzcd}$$

fig. 12

$$\begin{tikzcd} x_b \arrow[d] \\ x_a \arrow[d] \\ x_c \end{tikzcd}$$

fig. 13

Figures 12 and 13 correspond, respectively, to Lazarsfeld's notions of "explanation" and "interpretation." In both cases the original connection between x_b and x_c is made to disappear, but in differing ways. "Explanation" means the discovery of a common cause. "Interpretation" means, in social science, the discovery of an intervening variable which makes the original connection psychologically understandable. A good example of sociolinguistic "interpretation" is Labov's discovery in his study of the centralization of vowels at Martha's Vineyard, that the correlation between the centralization and the social group "middle-aged fishermen" was mediated by the variable "positive attitude towards staying in the island."

Now we can finally take up a sociolinguistic example. Our data is taken from Labov (1972: 222). We can identify here the following four 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 (= $\bar{D}$)

The data can be presented in the following way:

		C		$\bar{C}$	
		B	$\bar{B}$	B	$\bar{B}$
D	A	97	72	76	34
	$\bar{A}$	3	28	24	66
$\bar{D}$	A	79	23	49	7
	$\bar{A}$	21	77	51	93

fig. 14

The above numerical data has been achieved by converting percentages into frequencies. This is of course an illegitimate procedure, but I have no other choice, since the data has not been properly cross-tabulated. What is important, however, is that the presence or absence of interaction effects is evident even from data that has been manipulated in this way. As a further consequence of this manipulation, no dependencies between the independent variables x_b, x_c, and x_d can become apparent.

When we compute the coefficients a_{ba} and a_{ca} separately with the two values of x_d, we get the following results:

$$\left.\begin{array}{ll} & \text{with C} \quad \text{with } \bar{C} \\ D: & a_{ba} = 0.27 \sim 0.42 \\ & \text{with B} \quad \text{with } \bar{B} \\ & a_{ca} = 0.21 \sim 0.38 \end{array}\right\} \text{negative interaction}$$

$$\left.\begin{array}{ll} & \text{with C} \quad \text{with } \bar{C} \\ \bar{D}: & a_{ba} = 0.56 \sim 0.42 \\ & \text{with B} \quad \text{with } \bar{B} \\ & a_{ca} = 0.3 \;\sim 0.16 \end{array}\right\} \text{positive interaction}$$

In other words, we have here *two* levels of interaction: First, the influences a_{ba} and a_{ca} depend upon variables x_c and x_b, respectively. Second, the interaction a_{bca} is dependent upon the variable x_d. That is, Labov assumes that we have the structure of fig. 15, whereas the structure of fig. 16 is what we have in fact:

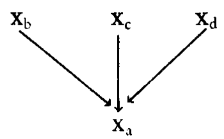
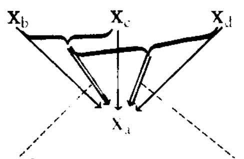


fig. 15



first-level interaction

second-level interaction

fig. 16

The computation of interaction values is rather laborious (cf. Boudon 1974: Ch. 9). It requires, among other things, redefining a_{ba} and a_{ca} as $a'_{ba} = a_{ba} + a_{bca} \cdot p_c$ and $a'_{ca} = a_{ca} + a_{bca} \cdot p_b$, respectively. Finally we get:

D:	$a'_{ba} = 0.34$	$\bar{D}$:	$a'_{ba} = 0.45$
	$a'_{ca} = 0.3$		$a'_{ca} = 0.23$
	$a_{bca} = -0.17$		$a_{bca} = 0.14$

Notice in particular that the positive value of x_d brings about the negative interaction $a_{bca} = -0.17$ whereas the negative value of x_d brings about the positive interaction $a_{bca} = +0.14$.

The interaction value a_{bca} can be defined as follows:

$$p_{a,bc} = a_{ba} + a_{ca} + a_{bca} + e_a, \quad \text{hence}$$
$$a_{bca} = p_{a,bc} - (a_{ba} + a_{ca} + e_a)$$

This last equation can be transformed into the following form:

$$a_{bca} = \underbrace{\underbrace{(p_{a,bc} - p_{a,\bar{b}c})}_{\text{the influence of } x_b \text{ upon } x_a \text{ in the case of C}} - \underbrace{(p_{a,b\bar{c}} - p_{a,\bar{b}\bar{c}})}_{\text{the influence of } x_b \text{ upon } x_a \text{ in the case of } \bar{C}}}_{\text{the influence of } x_c \text{ upon the influence of } x_b \text{ upon } x_a \text{, or:}}$$

$$x_c \rightarrow (x_b \rightarrow x_a)$$

We get the identical value for the second-level influence $x_b \rightarrow (x_c \rightarrow x_a)$. We can check our definition (cf. fig. 14):

$$\bar{D}: \quad a_{bca} = (0.79 - 0.23) - (0.49 - 0.07) = 0.14$$

The interaction value as here defined has the disadvantage of ranging from 2 to -2. When converted into a d-value, however, it will remain within the limits of 1 and -1. For simplicity, this conversion is not carried out here.

The causal efficacy of x_b and x_c is based on psychological mechanisms while that of x_d is based on social mechanisms (which, to be sure, must

always be psychologically mediated in each individual case). Consequently, our model is applicable in non-experimental psycholinguistics as well as in sociolinguistics. Furthermore, our model obviously conforms to Salmon's model of statistical analysis because consideration of new variables gives us more specific knowledge of the probability of the dependent variable x_a. As Salmon himself emphasizes, his model is that of empirical, not of theoretical, explanation (or rather analysis). In the present state of sociolinguistics, however, more theoretical models would hardly be realistic.

C. Conclusion

The variables of natural science, i.e., length, time, and mass, are inherently quantitative or measurable. By contrast, linguistic variables are typically non-measurable or qualitative. Their only quantitative aspect consists in the *frequencies* with which units exemplifying their values occur. Since such units occur with a fair amount of unpredictability, any models purporting to represent the causal mechanisms which make them occur must be of statistical-probabilistic nature. Unlike in natural science, in social science the (relative) unpredictability of the data is due to the historicality, the culture-dependence, and the (limited) free will of the research objects. Apart from this, however, the construction of causal models is largely similar in social science and in natural science.

Linguistic variables are subordinated to the notion of normativity: units exemplifying or failing to exemplify their values are (aspects of) correct or incorrect actions. Rules of language determine (the values of) the linguistic variables as conceptual possibilities; they constitute conceptual preconditions of actual bits of (correct or incorrect) linguistic behavior. Analysis of linguistic rules is a qualitative, conceptual undertaking, technically speaking a case of "explication." As such, it is similar to logical or philosophical analysis, and fundamentally different from the construction of (probabilistic) causal models. Explication, characteristic of grammar, and causal analysis, characteristic of socio- and psycholinguistics, stand in a relation of complementarity to each other; the one cannot be replaced by the other. The study of linguistic variation mediates between the two.

The philosophical tradition which claims that methods of natural science, ultimately methods of causal analysis, must be applicable in all sciences, is identifiable as *(neo-)positivism.* Logic and philosophy are prime exceptions to the positivistic program, and, as we have seen, grammar too is an exception to it. The basic similarity of grammar, logic, and philosophy consists in the fact that in all these cases people investigate, or reflect upon, the rules of their *own* behavior, that is, rules of speaking, inferring, or thinking. More precisely, people reflect upon what they know that they ought to do. This type of knowledge, "agent's knowledge," is to be distinguished from the "observer's knowledge" characteristic of natural science and, to some extent, of empirical sociology and psychology as well. Intuition and (self-)reflection, which are characteristic methods of explication, are qualitatively different from observation and experimentation, which are characteristic methods of natural science. To this extent the results achieved here vindicate the standpoint of *hermeneutics* which is a philosophical tradition claiming that it makes a difference whether one investigates physical phenomena or human phenomena.

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