The Relation Between Grammar and Sociolinguistics

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1. The Problem. During the last ten years we have been witnessing the emergence of a 'variationist' school of linguistics. Within it, furthermore, it is possible to distinguish between the Labov-type 'quantitative' approach and the Bailey-type 'dynamic' one, although there are signs of an increasing convergence of these two approaches (cf. Fasold and Shuy 1975). In this paper I identify variationism simply with 'sociolinguistics'. It is characterized by the fact that it investigates relative frequencies of linguistic features exhibited by *actual utterances.* (As for the 'dynamic' approach, this holds, for example, of Bickerton 1971 more than of Bailey 1973.) Sociolinguistic descriptions are to be contrasted here with 'grammatical' descriptions which are characteristically about (correct) sentences or speech acts *invented by the grammarian,* or about his linguistic intuition pertaining to such sentences or speech acts.[1] If the data under investigation are illustrated, or supplied, by means of examples whose origin is not indicated, it is legitimate to assume that the examples have simply been invented by the grammarian on the basis of his knowledge of the language to be described. That is, if the data do not consist of actual utterances (made by other people), we have to do with 'grammar'.[2] (However, the converse implication does not hold; cf. below.)

What is at issue here is the well-known (but little-understood) distinction between linguistic corpus and linguistic intuition. Even such champions of the corpus-thinking as Bloomfield and Harris seldom make use of a corpus *as a matter of fact.* For instance, practically all examples in Bloomfield 1933 and Harris 1951 are invented ones; cf. 'Bill will misbehave just as John

[1] Actually, these two formulations are not at all equivalent; cf. the end of this paper.

[2] 'Grammar' is used here both in the sense of a (synchronic-)grammatical description of a particular language and in the sense of a science which provides such descriptions. The term 'science', in turn, is used in the sense of the German 'Wissenschaft', i.e. as referring, for example, both to physics and logic.

did' and 'Bill will be bad just as John was' (Bloomfield 1933: 251); 'the family from whose beautiful daughters I learned German' and 'the family who bought the house lives here' (Harris 1951: 293). Consequently, Bloomfield and Harris are just investigating their own linguistic intuitions, which means that, according to my criterion, they are 'grammarians'. It is clear enough that, according to the same criterion, traditional school grammar, transformational grammar (=TG), stratificational grammar, and Montague grammar, for example, are equally subsumable under 'grammar'.

By contrast, sociolinguistics investigates (relative frequencies in) a corpus. For instance, when Labov notes that in a group of 17 adult Negro speakers the English copula is deleted in 16 per cent of the cases after a pronoun, he is doing sociolinguistics (Labov 1969: 730).

Consequently, we have reached the following two definitions: if, and only if, you study relative frequencies in a corpus (whether or not in relation to extralinguistic variables), you are doing sociolinguistics. If you study sentences or speech acts without studying any corpus, you are doing grammar. Notice that having no corpus is a sufficient, but not a necessary condition for doing grammar. That is, we must also reckon with the *inessential use of a corpus:* the grammarian knows that a given construction, e.g. 'x differs from y', is correct, but he refuses to illustrate it by means of an invented example and scans written texts or recorded speech until he comes upon a suitable example, e.g. 'it differs from standard English'. (Such a fragment of a sentence can be found in Labov 1969: 715.) This kind of procedure is based on the mistaken idea that the methodology of natural science must at any cost be imported into grammar.

The relation between linguistic intuition and linguistic corpus is certainly central to the theory of linguistics. It is only the more surprising that this relation has never been represented in an explicit and self-consistent way. First, Bloomfield and Harris claim that linguists must describe corpora of actually uttered sentences; but through their own descriptive practice they show that linguists need not do so (cf. above). Nowadays Lieb and Wunderlich, for example, are guilty of the same contradiction (cf. Itkonen 1976d: 7-10). Second, Labov claims that grammars not based on a corpus merely describe idiolects and succumb to the 'Saussurean paradox' (Labov 1972: 185-201); but he himself presents his variationist analyses entirely within the framework of such grammars (Labov 1969: 761; also 1972: 227, 234). Third, TG claims that it describes at the same time linguistic intuition and observable linguistic events (i.e. a corpus), or even that 'intuitions are observable events' (Dougherty 1974: 133).

In this paper I intend to present an explicit and self-consistent account of what is at issue here. I intend to show what, precisely, is the relation of sociolinguistics to grammar, or of the quantitative linguistic analysis to the qualitative one. The results are directly generalizable to other human or social sciences as well. It will be seen that grammars in the primary case are not descriptions of idiolects; nor is there any 'Saussurean paradox'.

What I have to say here about sociolinguistics fits quite naturally into, and hence confirms, my general conception of linguistics, as presented, for example, in Itkonen 1974 or 1975a.

*2. **The Grammar-Conception.*** I characterize my grammar-conception here as briefly as possible since I have presented it at greater length elsewhere (cf. the bibliography). I have answered objections against my grammar-conception in Itkonen 1976d.

Grammar traditionally describes the concept 'correct sentence (or speech act) of a language L'. A corpus provides exemplifications of the concept 'factually uttered sentence of L'. These are clearly 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 which are utterances of *incorrect* sentences of L (and therefore must, by definition, not be described by grammar). It follows that insofar as a grammar of L is an analysis of the concept 'correct sentence of L', it not only need not, but also *cannot* be an analysis of a corpus of actual utterances.

The distinction between 'correct sentence' and 'factually uttered sentence' is a special case of the general distinction between normativity and factuality, or between what ought to be and what is. It is a well-known philosophical truth that this distinction cannot be eliminated by reducing one of its terms to the other. The fact that a grammar of L (as here defined) cannot be based on a corpus is merely one consequence of the above-mentioned truth.

What a grammar of L is analyzing when it analyzes the concept 'correct sentence of L' are those *rules* of L which are relevant to it, i.e., rules on the basis of which actual or possible utterances (or reactions to utterances) are identified as correct, incorrect, or doubtful. The following are examples of rules, or rather of sentences (truthfully) referring to rules: 'It is correct to pronounce the words *tin* and *pin* differently, and incorrect not to do so', *'x is easy to please* is correct while *x is easy from please* is incorrect', 'It is correct to refer to dogs by *dog* and incorrect to do so by *cat'* (for more discussion, cf. Itkonen 1976d).

English contains thousands of such and similar rules. They are utterly trivial and have no scientific importance whatever.[3] However, they have a considerable *metascientific* importance, for the following reasons. In spite of their extreme concreteness, rules are nevertheless *universal* entities: they determine what one ought to do (i.e. say) on *any* occasion of a given type, or what is true of *all* correct utterances, actual or possible, of a given type. Their utter triviality means that they are self-evident or known without a possibility of doubt; and this means in turn that the truth or falsity of sentences referring to them is known in a similar way. Consequently we have here something that we cannot have in empirical science, namely universal sentences which are not falsifiable by single spatio-temporal occurrences and hence do not qualify as empirical, according to Popper's criterion. If someone makes the utterance *John is easy from please,* he does not falsify the (rule-) sentence 'It is correct to say *x is easy to please'*, because he is speaking incorrectly. And if someone makes the utterance *John is easy to please,* he does not of course falsify the sentence in question, since he is speaking correctly. Consequently this sentence is literally infalsifiable: nothing that a given person utters on a given occasion can falsify it.[4]

[3] I do *not* say that there are nontrivial rules too, because if they are nontrivial, they are not rules (in my sense).

The rule-conception which I am using here is generally adopted in the theory of social science; cf., e.g., Ryan 1970: 141 (where, to be sure, no distinction is made between rules and sentences referring to them):

> A causal generalization has one task to fulfil, namely telling us what will and will not happen under particular conditions; irregularities are thus falsifying counter-instances to the causal law. But rules are not falsifiable in any simple way – except of course that it may be false to say that there is a rule – and breaches of a rule are errors on the part of those whose behavior is governed by it.

I only wish to show here that language contains rules similar to those encountered in areas standardly investigated by social science. This should surprise no one, considering that language is a social phenomenon *par excellence.* Yet many linguists have found my rule-conception very hard to swallow (cf. Itkonen 1976d).

A given state of language consists of rules. More precisely, at the unscientific or atheoretical level language is merely a *set* of rules. At the scientific or theoretical level language reveals itself as a *system* of rules. In this sense it can be said that it is only grammar which shows what language *really* is. The systematic character of language is brought out by making theoretical generalizations about large sets of rules. One way of making such generalizations is to try to generate the sentences of L with as few grammatical rules as possible. (Notice that 'grammatical rule' has practically nothing to do with 'rule' as here defined.) Writing a scientific grammar means replacing piecemeal atheoretical certainty, expressible in rule-sentences, by generalizing theoretical uncertainty, expressible in grammatical hypotheses.

As a description of rules of L, a grammar of L is a qualitative, non-statistical analysis. However, at any given moment the language L contains rules which are changing. Such rules are no longer known in the same self-evident way as the rules mentioned above. It is known that instead of one previous invariant there are now two or possibly more variants. But if one

[4] It is, of course, possible, in principle, that English might change in such a way that the construction 'x is easy from please' becomes correct. However, this has no effect on my argument, as I have shown in detail elsewhere. Briefly, what a language has been or will be does not invalidate claims about what it is.

wants to know to which point, precisely, the change has advanced, then one must resort to an analysis of the relative frequencies of the different variants. Consequently, the *change of language* is the exact point where the qualitative analysis and the quantitative one overlap, or where the quantitative analysis forces its way into grammar. More generally, conceptual change provides the crucial link between normativity and spatio-temporality, or between conceptual analysis and empirical science.[5]

3. The Relation of Sociolinguistics to Grammar. It is a fact that actual human or social behavior, which is located in space and time,[6] exhibits no strictly deterministic or nomological regularities. The philosophical explanation of this fact must be sought in the complementary notions of free will and human fallibility. One can possibly dispute the offered philosophical explanation, but one cannot dispute the indeterministic or *variable* character of actual human behavior. This fact is expressed, for example, in Boudon's claim that in social matters the truth-functional implication 'if p, then q' must be replaced by the 'weak' implication 'if p, then generally q' (Boudon 1974: 18-20).

Of all linguistic phenomena, only actual linguistic behavior is located in space and time (cf. note 6). An empirical science investigates only what is located in space and time. Like actual human behavior in general, actual linguistic behavior is variable. Variable behavior can only be described statistically. Consequently if a linguistic description is empirical, it is statistical. By contraposition, if a linguistic description is nonstatistical, it is nonempirical. Because (standard variants of) TG and Montague grammar are nonstatistical, they are nonempirical as well.[7] (Notice that statisticalness does not as such entail empiricalness.)

Because, to use Hempel's (1965) terms, actual linguistic behavior does not admit of deductive-nomological, but only of inductive-statistical explanation, it might seem at first glance that empirical linguistics, or more generally empirical social science, is methodologically similar to microphysics, given that the latter too deals with phenomena which can be explained only statistically. The obvious difference is, however, that any tentative regularities discovered so far by empirical social science are historical and culture-dependent, whereas this is, of course, not true of the regularities of microphysics.

Now, if grammar is description of rules and sociolinguistics is description of actual behavior, are these two models of linguistic description genuine

[5] In grammar, statistics is needed when we do not know what our language is. Linguistic change is the most important such case, but not the only one (cf. below).

[6] 'Located in space and time' means here something which could, in principle, be specified, though perhaps not exhaustively, in terms of centimeters and seconds. Hence rules, unlike actions either conforming to or violating rules, are not located in space and time, although they are of course *historical*, as their liability to *change* clearly demonstrates.

[7] More arguments to the same effect are presented in my previous writings.

alternatives to each other? In other words, is it possible – as variationists are inclined to claim – that grammar could be replaced by sociolinguistics?

This question would be answered affirmatively, if both of the following two conditions were met. First, assuming that sociolinguistics investigates the linguistic behavior of human beings just like the kinetic theory of gases investigates the behavior of gas molecules, sociolinguistics must take each and every item of linguistic behavior into account. Second, this must be done without any reference, explicit or implicit, to those normative phenomena which constitute the subject matter of grammar.

It is not difficult to see that in sociolinguistic research neither of these conditions is usually fulfilled, and that even if the former condition can, in principle, be fulfilled, the latter cannot. Labov states explicitly that sociolinguistic data are not described as such but are, 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 per cent' (Labov 1972: 203). Here we have exact information about the relation between normativity and factuality: after the editing, the portion of correct utterances, or utterances which at least are not 'truly ungrammatical', out of all actual utterances is as high as 98 per cent. Labov correctly notes that this refutes Chomsky's a priori claims about the 'degenerate quality' of actual speech (cf. Chomsky 1965: 58 and 1966: 32, n. 8). On the other hand we see that the concepts 'correct sentence' and 'factually uttered sentence' remain clearly separate.

In a similar vein Bailey (1973: 126) notes that there are several types of 'raw linguistic data' which must necessarily be discounted in linguistic analysis. Lockwood (1972: 8-9) makes the same observation from the viewpoint of stratificational grammar.

Now, I claim that Labov does the editing and recognizes 'truly ungrammatical and ill-formed sentences' in the edited material only on the basis of his knowledge of the *rules of language*, as I use this term. In this sense, then, rules of language, or knowledge thereof, constitute a *precondition* of sociolinguistic research. If it is true, as I claim it is, that grammar investigates a precondition of sociolinguistics, then the relation between the two is that of, not alternativeness, but of *complementarity*. In a perfectly similar way, an analysis of the conceptual systems of common-sense knowledge, as presented in Winch 1957, or, more concretely, in Berger and Luckman 1966, constitutes a precondition of empirical sociology.[8]

Notice that there are *two* different senses in which a conceptual or normative system can be a precondition of a given science. First, each and every science has its own procedural or methodological rules (cf. Itkonen 1974: Chapter 5, Section 1; for a detailed exposition, cf., for example, Mittelstrass 1974). Second, in human or social sciences, but *not* in natural

[8] This is why it is absolutely false to understand Winch as claiming that empirical sociology should be *replaced* by his 'aprioristic sociology'.

sciences, it is also the case that research objects have their own conceptual or normative systems, and it is a precondition of meaningful human science that the researcher should understand the conceptual or normative systems of his research objects and, in this precise sense, be able to 'identify himself' with the latter. This difference between human science and natural science could be expressed by saying that the data of the former (e.g. utterances) are *inherently* normative,[9] whereas those of the latter (e.g. minerals) are not (cf. Itkonen 1976d). It is, of course, in this latter sense that the rules of a language L constitute a precondition of sociolinguistic descriptions of L.

It is important to see that the complementarity between grammar and sociolinguistics is in no way mechanical or absolute. *Linguistic change* mediates between the two, since it belongs to both (cf. above).

Even if sociolinguistics should, implausibly, describe truly unedited data about a language L, i.e., each and every item of linguistic behavior of a group of speakers of L (cf. Cedergren and Sankoff 1976: 352), it cannot help viewing these data through the spectacles of normativity provided, so to say, by the rules of L; that is, it cannot help classifying the data as correct, incorrect, or doubtful. The same is obviously true of any kind of *error* analysis.

From the above, it follows that we must make a clear distinction between *two* types of variation. First, there is the variation between correct variants V_1 and V_2; this type of variation must be contrasted with the existence of invariant or categorical rules governing the occurrence or non-occurrence of invariants I. Second, there is the variation either between correct and incorrect variants or between invariants and, so to speak, their incorrect variants:

a) $1 > V_1 \sim V_2 > 0 \neq I = \left\{ \begin{matrix} 1 \\ 0 \end{matrix} \right\}$

b) $1 > V_1 \sim V_2 \sim *V > 0 = 1 > I \sim *I > 0$

Perhaps surprisingly for variationists, both Labov (1969: 738, also 1972: 223) and Bailey (1973: 33, 84) state that invariant or categorical rules represent the *normal* state of language, that is, the state which constitutes both the starting point and the end point of linguistic change, or the change from 0 to 1 (and vice versa). This means that they are referring here to the *first* types of variation. Because of the pervasive variability of human behavior, there could obviously be *no* invariant rules, if one were to account for each and every item of linguistic behavior (cf. Boudon's notion of 'weak' implication). At most, some invariant rules could uphold the 0 - 1 dichotomy as long as the linguistic evidence would remain relatively small. However,

[9]This claim can be justified even in cases where we are not dealing with (actions conforming to) social rules proper, namely by pointing out that the concepts of common-sense thinking – as well as of scientific thinking – are normative in the sense that they can be used either correctly or incorrectly; cf. Itkonen 1976c: 63-65.

even in that case their status would have fundamentally changed. If we are speaking of the first type of variation, sentences referring to invariant rules are not falsified by single incorrect occurrences; hence they are in this sense unfalsifiable. However, if we are speaking of the second type of variation, a sentence referring to what in the previous context was an invariant rule is no longer unfalsifiable, even if it is as yet *de facto* unfalsified. That is, it no longer refers to a rule (in fact, to a *set* of rules, in the sense intended by Ryan and myself), but to a regularity.

With the aid of the concepts that I have developed so far, I can now proceed to expose the basic contradiction in Labov's and Bailey's position. They claim that all valid judgments about language must be based on observation of linguistic behavior; these judgments are of the type 'such and such occurs with such and such a relative frequency in such and such a context'. Other types of judgments are purely subjective, i.e. based on intuition, and are therefore at best suspect and at worst worthless. Now, we have seen that Labov and Bailey make normative judgments about their data, judgments which lead to the editing of the data and to the setting up of invariant rules (*not* regularities). My question here is: are these judgments of the type which, according to Labov and Bailey, are solely acceptable (i.e. judgments to the effect that something occurs with some frequency)? The answer is no – they are judgments of precisely the type which, according to Labov and Bailey, ought to be unacceptable, i.e. *intuitive* judgments, or judgments as to whether that which occurs (e.g., an utterance like *John is easy from please)* is or is not correct. Because the variationist approach rests on intuitive judgments, it is suspect to the same extent as the latter are.

Notice also that although Labov and Bailey maintain that linguistic intuition pertains only to idiolects, they are certainly willing to make an exception when it comes to that intuition on the basis of which they themselves evaluate and edit their data. Consequently there is apparently at least one type of intuition which is not suspect or limited to an idiolect, but rather has the capacity to achieve socially valid or *objective* results.

What I have said here is in fact a direct consequence of my previous claim that the knowledge on the basis of which Labov edits his data and, when necessary, evaluates it as 'truly ungrammatical and ill-formed', is knowledge about *rules of language.* It is a very simple truth that rules do not exist in space and time, and hence cannot be observed like utterances either conforming to or violating rules. Since the act of knowledge pertaining to rules cannot be called observation, it is convenient to call it *intuition.*[10] The notions of rule and intuition constitute a precondition of sociolinguistics. Now it becomes comprehensible why Labov is stuck with a grammatical framework based on intuition (cf. above): he could get rid of it only at the cost of getting rid of a precondition of his own science, which would of

[10]Moreover, it is advisable to distinguish intuition from *introspection* as used, for example, in psychophysical experiments; cf. Ringen 1976.

course wipe this science out of existence.

There is an apparent way out: Labov might say that instead of just relying on his own intuition, he also consults the intuitions of his fellow sociolinguists. However, this strategy only moves the problem one step higher – consulting others does not reduce normativity to observable data, because intuitions about, or reactions to, utterances are just as normative as utterances themselves. In the case of rules (as here defined) consulting others would be an idle ceremony, strictly comparable to mathematicians consulting each other as to whether or not '2 + 2 = 4' is true. The mere fact that this can be done, as it obviously can, turns neither mathematics nor grammar into an (empirical) science dealing with observable data. This can be seen from the fact that *if*, improbably, a mathematician and a grammarian would deny, respectively, the truth of '2 + 2 = 4' and *'John is easy from please* is incorrect', such reactions would be brushed aside as *incorrect* behavior (due, no doubt, to some pathological reasons).

Consequently, sociolinguistics is empirical only within the limits of normativity, and this in a *twofold* sense: the normativity in question stems either from the sociolinguistic practice of the researchers or from the linguistic practice of the research objects. Grammar investigates the latter type of normativity. It is a mistake characteristic of empirical sciences, or sciences posing as empirical, that they are unaware of their own preconditions. And even if they can be made aware of their preconditions, they assume that these can be analyzed by means of the same hypothetico-deductive methods as the empirical data proper. Anyone with any knowledge of Kant should know that this is impossible. Hermeneutics has characteristically drawn attention to the precondition or *possibility* of science, and has shown that this cannot be analyzed through observation and experimentation, but only through reflection and intuition (cf. Apel 1973; also Bubner 1976). Positivists or empiricists have consistently misunderstood claims about the possibility of science as (empirical) claims about observable data. The reason is quite simply that the notions of reflection and intuition transcend the conceptual universe of positivism. As Habermas (1968: 7) succinctly puts it, 'the lack of reflection *is* positivism'.[11]

4. The Solution: A Change of Ontology. So far I have claimed that intuition *qua* knowledge of rules is not only legitimate but also necessary in linguistics. Before I go on to justify this claim, I must answer an obvious objection. Several attempts have been made to show that rules can be reduced in one way or another to spatio-temporal data; this would mean, at the same time, that intuition can be reduced to observation. I have presented detailed refutations of such attempts in Itkonen 1976b and c, and need not repeat my arguments here.

[11]For a more detailed discussion of positivism and hermeneutics, cf. Itkonen 1974, 1975a, and 1976d. The general philosophical background is discussed exhaustively in Radnitzky 1970 and Habermas 1970.

In Section 1 we noticed that Bloomfield's, Chomsky's, and Labov's positions vis-à-vis the distinction between corpus and intuition, or observation and intuition, are all inherently inconsistent, although in different ways. These different positions have a common denominator, namely the wish to get rid of intuition (and, at least implicitly, of normativity). The differences depend on the extent to which this wish has been realized in practice. Labov investigates real corpora and claims that this is all one has to do.[12] Bloomfield investigates his own intuition (at least in Bloomfield 1933), but claims that one has to investigate real corpora. Chomsky investigates his own intuition and also tries to justify this practice (cf. Chomsky 1957: 13, and 1965: 21); however, in his more explicitly methodological statements he claims that he is investigating observable events (Chomsky 1957: 49, and 1972: 13-14). Bloomfield's and Chomsky's positions are explicitly inconsistent and need not be expressly refuted. Labov's position is not explicitly inconsistent, and therefore in Section 3 I undertook to show that it is nevertheless implicitly inconsistent.

In a situation like this, I think the following general advice offers the only rational course of action. If there is a fact which conflicts with your conceptual apparatus, and if in spite of repeated attempts, you cannot explain this fact away, then you must change your conceptual apparatus. For some fifty years, at least, linguists have been relying on a space-time ontology, or an ontology of observable events, which conflicts with the existence of normativity and the use of intuition. During this whole period of time, linguists have made absolutely no progress in their attempts to resolve the contradiction between their space-time ontology and the use of intuition (which, to repeat, is inseparable from the existence of normativity). Notice, in particular, that although Chomsky has in part endorsed the use of intuition, he has never had any systematic theory or methodology of it. The only methodology which he has ever known is the Popper/Nagel/Hempel-type hypothetico-deductive conception of science which deals with observable data, not intuition (cf. my works listed in the bibliography; also Ringen 1975). Given this situation, it is surely advisable to give up at last the attempt at eliminating intuition and normativity and to accept an ontology in which rules *qua* objects of intuition are accorded an objective, irreducible existence. There was no cogent reason to accept the space-time ontology in the first place, and in some philosophical traditions it was in fact never accepted. Moreover, even from the empiricist point of view my proposal is not as shocking as it might seem at first glance, because within the empiricist ontology the data of logic and philosophy are generally accorded, at least implicitly, precisely that kind of status which I claim the data of grammar, i.e. rules of language, should also have. It is no coincidence that I have been arguing all along for the basic similarity of grammar, logic, and philosophy.

[12]Occasionally, however, Labov admits that we cannot help relying on our intuitions, cf. Labov 1972: 227, 234.

I shall present, first, the general form and, second, the precise content of my ontology. Its form is quite simple, and self-consistent in a way in which the form of Bloomfield's, Chomsky's, or Labov's ontology is not. Its content is more complex and perhaps less easy to grasp.

Any act of knowledge is necessarily subjective. For instance, observation of physical reality is necessarily subjective. The term 'intersubjective observation' is a misnomer; what it means to say is that subjective observations of physical reality, when intersubjectively compared, produce remarkably stable results. Observation is subjective, but it pertains to and gets hold of something objective, namely physical reality. Now I claim that in a precisely similar way linguistic intuition, which is subjective, pertains to something objective, namely rules of language. In a similar way, too, logical intuition and philosophical intuition are subjective acts pertaining to something objective, namely rules of inferring and conceptual systems, respectively.

How do rules exist, precisely? My answer is that they exist as objects of *common knowledge*. This notion has a long history (cf. Itkonen forthcoming: Chapter 5, Section 1; also below). I use here the explication of it given in Lewis 1969. Common knowledge is three-level knowledge of the following type:

1) A knows$_1$ X.
2) A knows$_2$ that B knows$_1$ X.
3) A knows$_3$ that B knows$_2$ that A knows$_1$ X.

When this definition holds of a thing X and of practically any two members A and B of a given community, we say that X is an object of common knowledge. Physical things or states of affairs can also be objects of common knowledge, but their existence is independent of whether or not this is the case. By contrast, rules, for instance, cease to exist, if they cease to be objects of common knowledge.

If only the first level would hold of (practically) every member of a community, we would have a set of solipsistic consciousnesses or, supposing, in particular, X to be a rule of language, a set of private languages. For conceptual reasons, this is impossible, as Wittgenstein has conclusively demonstrated. If only the first two levels would hold of any two members of a community, we would have a set of cases which resemble Apel's (1973) notion of 'methodological solipsism'. Every member of the community would only have unidirectional access to every other member – although he could (try to) control others, he could not conceive of the possibility that he might in turn be controlled by them. Hence it is only at the third level that genuine *social interaction* (which is a logical precondition for having consciousness and language at all) becomes possible. Still higher levels of common knowledge (or belief) exist, as everyone who has tried to bluff in poker knows, but they do not essentially change the basic pattern of social interaction which is established already at the third level.

Common knowledge is a complex configuration which cannot be reduced to simpler forms, e.g., to a set of solipsistic consciousnesses, without being destroyed. If we say that X exists as an object of common knowledge, then, in my opinion, we have explained what it means for X to exist *objectively*. (Of course, here X is not a physical thing or state of affairs.) Now, supposing X to be a rule of language, how can X, as an objective entity, be an object of subjective linguistic intuition? My answer is that A's three-level knowledge about X is simply identical with his (subjective) intuition about X. That is, his intuition about X is his contribution to common knowledge about X and, *eo ipso*, constitutes the latter in part. A's intuition both pertains to X and, in part, creates it.[13]

Remember that *every* act of knowledge is subjective. Let us call observation of physical reality a case of (subjective) 'physical knowledge'. By analogy, A's intuition about the rule X can be called a case of (subjective) 'social knowledge', because it necessarily contains a reference to social interaction, i.e., to what *others* know, and what *they* know that A knows. A's (three-level) knowledge about X can, in principle at least, be false, but the same is also true of his knowledge about physical states of affairs or about rules of inferring or rules of arithmetic.[14] If his knowledge happens to be false although he thinks it is true, then it can be corrected in the only way in which false subjective knowledge can in general be corrected, namely through the intervention of others.

A's intuition about X is *de facto* about an object of common knowledge, but in general A does not, of course, understand his intuition about X in such terms, even if he has been made conscious of it. However, in a theoretical context like the present one, this is possible. For instance, my intuition, which is part of common knowledge, can pertain to common knowledge as a whole, thanks to the *reflexive* character of human consciousness.

It must be emphasized, once again, that rules could not exist without (potentially conscious) behavior trying to conform to them. For instance, rules of language determining the concept 'correct sentence of L' could not exist without there being people who speak correctly over 90 per cent of the time (cf. above). Similarly, rules can only be learned by observing actions conforming or trying to conform to rules. And yet rules are *different* from behavior, or 'correct sentence' is *different* from 'factually uttered sentence'. On this point I disagree with Lewis. He tries to get rid of normativity by redefining it in terms of memories and expectations about actions in recurrent situations. If this is taken literally, then it gives a *statistical* and hence inadequate explication of rules. An adequate explication, which renders the 'either-or' character of genuine or invariant rules, is possible only at

[13] Actually, this formulation is more suggestive than exact, because common knowledge is independent from the contribution of any *particular* person; cf. below.

[14] A native speaker of English is just as likely to deny the truth of the sentence 'Two and two makes four' as of the sentence *'John is easy to please* is correct English'.

the cost of an implicit reference to *correct* memories and expectations, thus invalidating Lewis' original purpose (cf. Itkonen 1976b: 43-44, and 1976c: 69). The same argument also applies to the term 'recurrent situation', since Lewis is clearly speaking of situations which are *correctly* identified as 'recurrent'. Notice that, once again, we are dealing primarily with normativity stemming from the practice of the research objects, i.e. speakers of L. Therefore my argument cannot be countered by claiming that the same is true of natural science as well.

I have characterized here what I take to be the mode of existence of the data, not only of grammatical analysis, but of logical and philosophical analysis as well. It is generally agreed, both that logic and philosophy deal with objective data, and that they do not deal with observable data. However, there is much less agreement on what it is, precisely, that logic and philosophy do deal with, and how it exists and can be known.

Although rules are 'based on' behavior, they are not reducible to it. They are *sui generis* entities, with an ontology of their own. Consequently, they are 'social facts' in Durkheim's sense. Indeed, in my opinion common knowledge provides an adequate explication of Durkheim's notion of 'collective consciousness', which is defined as being constituted by an interpenetration of individual consciousnesses:

> Sans doute, il ne peut rien se produire de collectif si des consciences particulières ne sont données; mais cette condition nécessaire n'est pas suffisante. Il faut encore que ces consciences soient associées, combinées, et combinées d'une certaine manière; c'est de cette combinaison que résulte la vie sociale et, par suite, c'est cette combinaison qui l'explique. En s'agrégeant, en se pénétrant, en se fusionnant, les âmes individuelles donnent naissance à un être psychique si l'on veut, mais qui constitue une individualité psychique d'un genre nouveau. Voilà dans quel sens et pour quelles raisons on peut et on doit parler d'une conscience collective distincte des consciences individuelles. (Durkheim 1938: 127)

Similarly, the notion of common knowledge can be used to make Hegel's and Dilthey's notions of 'objective spirit' intelligible.

5. There Is No 'Saussurean Paradox'. Rules of language constitute the institutional framework within which actual speaking takes place. The one could not exist without the other, but it is clear that rules are primary with respect to any particular acts of speaking. In this sense, then, an institution is the precondition of institutional behavior. Since sociolinguistics investigates actual speaking (and the influence of extralinguistic variables on it), grammar *qua* investigation of linguistic rules investigates a precondition of (the data of) sociolinguistics. At the atheoretical level, knowledge of linguistic rules is a precondition of actual speech; at the theoretical level, knowledge of linguistic rules is a precondition for describing actual speech.

An institution is objective; one's knowledge of it is subjective. As

Durkheim pointed out long ago, the existence of an institution is independent of the knowledge of any particular person (just as the existence of a rule is independent of any particular action). But if no one any longer knows that an institution exists, then it has ceased to exist.

I submit that so far I have presented a consistent account of linguistic intuition, linguistic rules, and linguistic behavior (or corpus). However, there remains one important objection. The rationale behind Labov's variationist approach is to a large extent his distrust of intuition. If his claim that intuition can only pertain to idiolects is true, this would mean that there simply are no objective or socially valid rules of language.

The first point to note here is that, as a result of Wittgenstein's refutation of private languages, it is quite clear that there can be no genuine idiolects and that all rules of language are more or less social (cf. Itkonen 1974: Chapter 2). Nevertheless, many contemporary linguists seem to think that Labov's claim is true, or at least that it is not obviously false. In my opinion, it owes its initial plausibility to an historical accident, namely the descriptive practice of TG. TG started by describing sentences of any two speakers, A and B, of which it was true that A knew they were correct, A knew that B knew they were correct, and A knew that B knew that A knew they were correct. However, driven by the inner logic of a framework which emphasizes the importance of recursivity and of the capacity to say one and the same thing in many different ways, TG started to investigate more and more complex or otherwise outlandish sentences, i.e., sentences that no one ever utters in fact. Since there is no actual practice to support such sentences, there can be no socially valid or objective rules which would unequivocally determine their correctness or incorrectness. It follows that when representatives of TG claim, nevertheless, that such sentences are or are not correct, they are in reality only describing their own idiolects. In a situation like this, 'language goes on a holiday', as Wittgenstein would say.

It is an understandable reaction against such abuses to claim that linguists must describe *only* what actually gets uttered, but nevertheless it is an *over*-reaction. There is an indefinite number of sentences which are commonly *known* to be correct, regardless of whether or not they have ever been uttered. (This is directly confirmed by, for example, Bloomfield's and Harris' descriptive practice.) On the other hand, actual speech contains elements which are commonly *known* to be accidental and incorrect. Consequently we see that there *are* socially valid or objective rules of language, and that we just cannot help operating with them.

Labov's great merit was to open up an area of inquiry, namely linguistic variation, which had been ignored by the dominant schools of linguistics. When rules are less than certain, as in linguistic change or in complex or unfamiliar cases of language use, statistical description of actual occurrences (though not of *all* actual occurrences) is mandatory. This is where my thesis of complementarity comes into play. However, to keep the proper balance between conceptual analysis and empirical science, it is good to bear in mind

that additional criteria for knowledge, to be provided by statistical considerations, are needed only when knowledge (of language) is less than certain. As the example of Bloomfield and Harris already showed, there is a very large number of cases where no such criteria are needed.

The 'Saussurean paradox', as formulated by Labov, amounts to the claim that there is a fundamental mistake involved in the attempt to investigate social aspects of language by investigating one's own intuition. In the present context this 'paradox' can be solved easily enough. Linguistic rules do exist, but in a different way than, for example, linguistic actions. As a consequence, rules cannot be observed like actions. The act of knowledge pertaining to rules is called intuition. Both observation and intuition are necessarily subjective. Rules are necessarily social entities. Consequently, there can be nothing wrong with using intuition in an investigation of such social entities as rules. Quite on the contrary, this is the *only logically possible* way of investigating rules (as here defined).[15] What looks like a paradox to Labov is thus seen to be a logical necessity. By distinguishing between act of knowledge and object of knowledge we are able to account for the apparently puzzling fact that one can investigate something objective by 'merely' investigating one's own knowledge or intuition. The point is that one does *not* investigate one's own knowledge or intuition, but what it is about.

[15] I repeat that the *learning* of rules is only possible through a combination of observation and intuition. But once rules are learned and known, they are used as a criterion to evaluate what is observed. Moreover, intuition about rules is not comparable to knowledge about theoretical concepts of natural science which are inductively inferred from observable data and/or employed to deductively explain observable data. First, the former is certain while the latter is hypothetical. Second, due to the unpredictability of human behavior, there are no 'correspondence rules' which would connect rules to behavior in the same way as theoretical concepts are connected to observable data; cf. Itkonen 1976b.

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