Esa Itkonen

Concerning the Synthesis between Intuition-based Study of Norms and Observation-based Study of Corpora

Motto 1: "The central issue here concerns the role of **normativity** in linguistic data. I do not think that the importance of this concept has yet been grasped in current theoretical linguistics. As long as this continues to be the case, no adequate understanding of the metascientific status of linguistics can, in my opinion, be reached" (Itkonen 1978: vi).

Motto 2: "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" (Itkonen 1980a: 350).

Motto 3: "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. (...) 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" (Itkonen 1977a: 239).

Motto 4: "La vérité, l'âpre vérité" (Danton, quoted as the motto of Stendhal's *Le rouge et le noir*).

1. General background

Ferdinand de Saussure conceptualized any given language as consisting of two distinct aspects, namely *langue* and *parole*. *Langue* was interpreted as a social institution which, like any other institution, is a system of norms (or rules or conventions). *Parole* was taken to be the actual behavior which conforms to, and occasionally deviates from, the norms of the institution. By definition, norms cannot be observed but only intuited. Actual linguistic behavior can of

course be observed, but since it is an instance of **normative** behavior, this type of observation is not just a simple matter of sense-impression, but must always contain an element of intuition.

De Saussure's overall conception has been widely accepted, with occasional terminological variation. For instance, Trubetzkoy's (1958 [1939]) respective terms for *langue* and *parole* were *Sprachgebilde* and *Sprechakt* (cf. Itkonen 2003a: 149–150), while Hjelmslev (1963 [1943]) used the terms 'system' and 'process' (or 'text') (cf. Itkonen 1968: 456). It goes without saying that a language qua system of norms is something of an **idealization**:

> Any realistic conception of grammar allows for linguistic variability, and is therefore susceptible of further development in the direction of sociology. Of course, such a conception forces one to give up some of the formal elegance which had been acquired by adapting linguistic descriptions to the model of formal languages. But this is as it should be because formal languages admit neither of variability nor of (spontaneous) change, whereas natural languages do. To be sure, it may be a practical necessity to resort to the use of artificially homogeneous forms of language, as long as modes of description which would be theoretically more justified are not available. In particular, it seems that an exact specification of several logical properties of natural languages requires, for the present, that variability be artificially eliminated. (Itkonen 1974: 318)

The actual descriptive practice of ordinary working grammarians shows beyond any doubt that the variation-eliminating idealization of a more or less homogeneous *langue* is indeed a **practical necessity**. This was clearly realized e.g. by Bloomfield (1933: 37):

> In no 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**" (emphasis added; quoted in Itkonen 2003a: 37).

The correctness of Bloomfield's claim can be — and has been — demonstrated quite concretely. In recent years, based on corresponding grammars, I have been both investigating and teaching the following ten non-European languages: Diyari, Hindi, Hua, Rapanui, Swahili, Tamil, Wari', West Greenlandic, Yagua, Yoruba. This is how I summarize the lesson that I have drawn from my efforts:

> With increasing urgency, it is being claimed nowadays that "everything in language is dynamic, emergent, and variable". When I see or hear people making this claim, I

> ask myself whether they have ever read the grammar of any language and, if so, how much they have understood of what they have read. If everything in language is variable, then the only valid type of linguistic description must be statistical in character. The fact is, however, that while grammars provide the primary way of describing languages, they in general use no statistics at all. This elementary truth is well confirmed also by the grammars utilized in this book. (To be sure, Payne & Payne 1990 counts as a partial exception.)
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> The variation in actual (linguistic) behavior can only be described by statistics. If grammarians do not use statistics and, by implication, do not describe actual (linguistic) behavior, what is, then, the subject matter of their (non-statistical, categorical) description? It is the structure, or more generally, the system, of the language in question. It follows that structure is primarily existent, and only secondarily 'emergent'. This point has also been forcefully argued by Givón (1995: 175–176), who emphasizes the importance of "taking structure seriously". Structure/system is in turn identical with Saussure's langue (with the qualification that some parts of the langue may be non-categorical, not in the sense of 'statistical', but in the sense of 'gradual' or 'continuum-like'). Thus, in my opinion, the fashionable criticism of the langue – parole distinction is based on a misunderstanding. This misunderstanding has been made possible by a related one, namely the view that this distinction was a conceptual innovation. But, in fact, Saussure merely gave a systematic expression to a practice that had always existed (and will always exist). Every grammarian describes langue (and not parole, or actual linguistic behavior). This is true of Pānini, Tolkaappiyanaar, Sībawaihi, Apollonius Dyscolus, Varro, Thomas of Erfurt, Arnold & Lancelot, and so on (for extensive documentation and discussion, see Itkonen 1991 and 2000). It is also true of all the grammars utilized in this book.
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> In a **complete** description of any language, there is room **both** for categorical description **and** for statistical description. For some 30 years, I have been exploring the precise relation between these two types of description. However, they are **asymmetric** in the sense that, as shown by the history of linguistics, there can be categorical description without statistical description, but not vice versa. (Itkonen 2005a: 3–4)

What **is**, then, the relation between intuition (about norms) and observation (of a corpus of utterances)? How is the **synthesis** between the two to be achieved? In my published work, Itkonen (1978) and (1983) are devoted to *langue* and *parole*, respectively. From among hundreds and hundreds of pages, it is not easy to make a succinct and illuminating selection. Perhaps the following excerpts from Itkonen (1980a), which was written already in 1977, will be helpful here:

> In this context linguists' capacity for self-contradiction seems almost unlimited (...) E.g. Lieb (1976: 198) and Wunderlich (1976b: 81) claim that a grammatical description must absolutely be based on a corpus; and yet they themselves not once

> make use of a corpus in their own published work. (...) 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. (...) More recently he has come to explicitly entertain the view that in Chomskyan 'clear cases', where the 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'. (...) 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. It 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. (...) (p. 336–337; emphasis added).

[There follows on p. 338 a discussion of Ross-type 'squishes', i.e. gradient or continuum-like phenomena.]

> Rules [or norms] 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. (p. 339)

(…)

> Moreover, our world happens to be 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. (p. 340)

(…)

> [Labov] 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. (...) The preceding argument involves an apparent difficulty. The learning of rules 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 nor eliminated, as we know from philosophy, one has to leap over it. (p. 341–342)

(…)

> To think that the concept of 'correct sentence' emerges as a result of experimentation is to commit the 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" (p. 344).

Up to now, we have reached the conclusion that the (normative) subject matter of grammatical analysis is the conceptual precondition of the (spatiotemporal) subject matter of those subdisciplines that investigate actual linguistic behavior, namely psycholinguistics, sociolinguistics, and diachronic linguistics. Absolute black-or-white dichotomies are impossible in language (cf. Itkonen 1978: 108–109).Therefore there must be some sort of **mediation** between (linguistic) normativity and (linguistic) spatiotemporality. This is how I envision it:

> Linguistic change, or more generally **linguistic variation**, and extraordinary use of language are, then, the two cases where atheoretical linguistic knowledge is less than certain, or where the social control of such knowledge is less than absolute. The possibility of spontaneous change is a necessary precondition for the continuous functioning of language, and distinguishes natural language from such artificial normative systems as formal logic or the game of chess. Moreover, linguistic change represents the exact point at which linguistic normativity and linguistic spatiotemporality contact each other, or merge into each other. For my general conception of science, such a point is of absolutely crucial importance since it provides the natural link between the empirical sciences and conceptual analyses in the widest sense. (Itkonen 1978: 153; emphasis added).

Grammatical analysis of 'clear cases' is a non-empirical undertaking comparable to philosophical or logical analysis, and it needs to be complemented by empirical considerations insofar as linguistic variation and extraordinary or non-standard uses of language (both of which are subsumable under 'less-than-clear cases') are also taken into account. The other linguistic subdisciplines are straightforwardly empirical undertakings (with an ineliminable normative component, to be sure). What is the methodological status of empirical subdisciplines like sociolinguistics or psycholinguistics? As indicated in my second motto, their overriding goal is to construct **causal models** for linguistic behavior.

Correlational models, based on Boudon (1974), have been proposed for sociolinguistics in Itkonen (1977b), (1980a: 349–363), (1983: 260–278), (2003b: Chap. XVI). At the same time, I have defined such crucial notions as **statistical causation** and **statistical explanation**. I have also explored the relation between statistical behavior and **free will** (see, in particular, Itkonen 1983: 92–95 and 2003b: 192–194). It is a sad fact that the mainstream sociolinguistics, in Finland and elsewhere, has never shown any interest for these theoretical questions.

Both **postulational** (or 'analytic') models and **synthetic** models have been proposed for psycholinguistics in Itkonen (1983: 278–313). The notion of synthetic model has been borrowed, with modifications, from Diesing (1972). It is the greatest shortcoming of the philosophy of the socio-psychological sciences that it has been unable to grasp the existence of synthetic models with sufficient clarity.

As far as diachronic linguistics is concerned, it is legitimate to speak of a 'causal model' only in the general sense of referring to the bipartite ('causal') mechanism consisting of (psychological) innovation and (social) acceptance. It has been described, e.g., in Itkonen (1982), (1983: 201–211), (1984), (2002a). Regardless of the more specific features that characterize causal description in sociolinguistics, psycholinguistics, and diachronic linguistics, it is the basic tenet of Itkonen (1983) that a common denominator can be discerned in all these empirical subdisciplines, as summarized in the notion of **rational explanation**.

My overall conception of the methodology of linguistics has not changed since 1983. Nevertheless, three subsequent developments deserve to be mentioned. First, additional support has been given by the **history of linguistics**: the logically prior subdiscipline of grammatical analysis (or 'autonomous linguistics') has everywhere been the first to develop, as

predicted; and, as predicted, the history of grammatical analysis has been everywhere similar to the history of philosophy and logic, and dissimilar from the history of the natural sciences (cf. Itkonen 1991, 2000, 2001a). Second, although the methodology of **typological linguistics** was already discussed in Itkonen (1983: 211–219), it has become the object of sustained analysis only in Itkonen (2003a: Appendix 5, pp. 172–199) and (2005a: Chap. XI). Third, the role of **analogy** in the causation of linguistic behavior is focused upon in Itkonen (2005b). — My overall conception was summarized in my plenary talk at the section 'Philosophy of Linguistics' of the 11th International Congress of Logic, Methodology, and Philosophy of Science, published as Itkonen (2002b).

A couple of years ago, urged by my friends Chris Sinha and Jordan Zlatev, I decided to write a book-length compendium of my methodological views, a plan that resulted in Itkonen (2003a). This is how I characterize the motivation for, and the nature of, this book:

> In my earlier publications I have tried to give a rather comprehensive answer to the question 'What is language?' In the present book I give a more condensed answer to the same question. To put it bluntly, this book offers a rapid, intensive course designed to enable beginners to master the essentials of the philosophy of linguistics. If you understand what follows, then you can move on to read the hard stuff, like Itkonen (1978) and (1983), which, even if sold out, can be unearthed at any decent library. (2003a: 10)

2. Intuition vs. observation in grammar-writing

Karlsson (2005) — henceforth to be abbreviated as 'K-2005' — criticizes what he considers to be the defects of Itkonen (2003a). He starts by deploring the fact that I give no account of the 'ten non-European languages', not realizing that these constitute the subject matter of an entirely different book, i.e. Itkonen (2005a). Itkonen (2003a) cannot be criticized for not being Itkonen (2005a).

According to K-2005, the "most erroneous claim" of Itkonen (2003a) is made in a passage from p. 23, where I claim that the "twofold irrelevance of spatio-temporal evidence, mentioned above, demonstrates the **non-empirical nature of grammatical description**. The irrelevance of space and time entails the irrelevance of the act of knowledge that applies to spatiotemporal

occurrences, i.e. **observation**, and indicates the need for some other act of knowledge, namely **intuition**."

This claim is qualified on p. 34, as the answer to 'standard objection xii-b)':

> **Data** is always uncertain in connection with **variation**: there is no reliable intuition about frequencies of occurrence, investigated in the context of geographical or social dialects or of linguistic change. The same applies to extraordinary use of language. (...) The data known with [intuitive] certainty coincides with so-called **clear cases**, and the data not known with certainty covers the domain of less-than-clear cases. The boundary between the two is necessarily vague; "but to deny a distinction because of its vagueness is, of course, a semantic naiveté of the first order." (Pap 1958: 401, n. 18).

When we compare these quotations from Itkonen (2003a) with some of the quotations given earlier, we see that, over the years, my position on this issue has remained exactly the same: in grammatical description, intuition is enough always, and only, in the clear cases; elsewhere observation is needed. Hence, K-2005 is wrong to say that, in my opinion, observation is not needed at all.

What, exactly, is the nature of the "twofold irrelevance of space and time" that is characteristic of the clear cases? It can be explicated as follows:

> A (sentence) grammar has to account for (or 'generate') all and only correct sentences of a language; but on the one hand, there are an indefinite number of **correct** sentences which never have been or will be uttered (i.e. exemplifications of which have never occurred or will never occur in space and time) and which **must** nevertheless be accounted for by the grammar; and, on the other hand, there is an indefinite number of **incorrect** sentences which have been or will be uttered, i.e. which **must not** be accounted for by the grammar (in spite of the fact that corresponding utterances have occurred or will occur in space and time). Therefore space and time are irrelevant to the grammar, namely in the twofold sense that we have here the combinations 'correct & not spatio-temporal' and 'incorrect & spatio-temporal'. (Notice what is **not** claimed here: it is **not** denied that language-acquisition is based on observation of speech, i.e. utterances; nor is it denied that other types of linguistic research may or must concentrate on spatio-temporal utterances.) (Itkonen 2003a: 22)

The distinctness of the concepts 'correct sentence' and 'spatio-temporal utterance' follows from the general truth that 'ought' can be neither reduced to nor derived from 'is' (op. cit., Chap. 24). This truth also entails that there has to be a 'leap' from observing utterances to (intuitively) understanding the

norms they exemplify (p. 39, 65–66). It is not my fault if K-2005 does not understand these basic truths.

It has become clear up to now that, as far as the data of grammar-writing is concerned, my position can be summed up by the following two statements: "In the **clear** cases it is fully satisfactory from the methodological point of view to use sentences the correctness of which has been established on the basis of intuition" and "Attested examples are needed when the expressions get more and more unusual and complicated: it is in such cases that intuition is the most unreliable". Interestingly, these statements are to be found in Hakulinen & Karlsson (1979: 25, 63). To repeat an expression from Itkonen (1980a: 337), their position is "extensionally identical" with mine. But the 'intension' (i.e. interpretation) is all wrong, for the following four reasons.

First, the role of observation is hugely exaggerated at the cost of intuition, to the point of nearly complete self-misunderstanding. For instance, in Chapter 2 of Hakulinen & Karlsson (1979) there are 83 example sentences (including those that occur in the running text), **all** of which have been invented by the authors on the basis of their linguistic intuition. In Chapter 12, there are 369 example sentences, only **one** of which (given on p. 269) has perhaps been observed to occur. Hence, in light of Hakulinen & Karlsson (1979), observation is not needed in grammar-writing at all. For my part, I reject this conclusion.

Second, because of the de facto over-reliance on intuition, categorical statements are made that corpus data could easily have shown to be false (cf. Pajunen 2001: 375-382).

Third, and inversely to the previous point, when attested examples are used in fact, there is often no genuine reason for doing so. For instance, in the footnote on p. 77 the authors proudly proclaim that the following sentence has actually been attested: "Tämä kesäloma oli ensimmäiseni moneen vuoteen." But any fluent speaker of Finnish knows perfectly well the correctness of this sentence without any need of resorting to some corpus. So what is going on here? We clearly are dealing with a phenomenon that I have called "**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" (Itkonen 1977a: 239).

Let it be added that there is also an experimental counterpart to the inessential use of a corpus:

> Greenbaum & Quirk (1970: 18) note that there is simply no point in setting up experiments where there is "no reason to believe that we would have much less than 100 per cent acceptance". 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' (Itkonen 1980a: 344).

Fourth, when over-emphasis on observation is combined with an inessential use of corpora, the end result is that intuition comes to be seen as more or less irrelevant and negligible. From the philosophical point of view, this is a disaster. The normative-conceptual core of linguistics — of **any** type of linguistics — is forgotten, and linguistics is taken to be just one natural science among others.

This conclusion is indeed drawn by K-2005. As much is evident from his claim that, from the methodological point of view, non-prototypical animals (e.g. albino ravens) are exactly on a par with incorrect sentences. Notice that there is no reason why we could not speak e.g. of non-prototypical galaxies, with the consequence that any methodological difference between linguistics and astronomy disappears. But this is wrong. As argued in Itkonen (1974: 172–183), (1978: 198–219), and (2003a: 49–64), linguistics is a typical human science insofar as it contains **two levels** of knowledge, norm, and action: there are norms at the level of research objects, i.e. norms of language, and there are norms at the level of research, i.e. norms of linguistics; moreover, the linguist must come to **understand** the first-mentioned norms and hence to **identify** him-/herself with the research objects in the sense of learning to **act** in the same way. In these crucial respects, any natural science is dissimilar.

Those who argue that grammar-writing (not to speak of other linguistic subdisciplines) is just one type of natural science, must of course deny the two-level nature of linguistics. I call this position 'positivism'. Itkonen (1974) was criticized by Dahl (1975 [1980]) from the positivist point of view. This criticism was answered in Itkonen (1976b: 41–46) and (1980b). Today K-2005 follows in Dahl's (1975 [1980]) footsteps.

One cannot help wondering what makes people endorse positivism, given that it is so obviously false. The following answer has been suggested: "The impulse to use Darwin [as a model to be imitated] is just as misguided as was the impulse to use Newton. What is the nature of this impulse? It is, first, to

feel hopelessly inferior to the representatives of the 'hard sciences' and, second, to think that one can get rid of this harrowing feeling by blindly imitating one's betters (or those whom one considers such), come what may" (Itkonen 2003a: 198).

Taken in by his own 'observationalist' rhetoric, K-2005 is convinced that the value of a grammar depends on how many examples from reputable authors it contains. For K-2005, the apotheosis is reached when a grammarian brings it about to have on a single page — **on a single page**! — "quotations from Milton, Winthrop, Tarkington, Lowell, Lee, Holmes, Marlowe, Hobbes, Bradford, Churchill, and Mather". Grammars that strive after this ideal are called "ambitious" by K-2005. This is an unfortunate choice of words. A moment's reflection is enough to show that grammars of this type do not qualify as **ambitious**, but as **tedious**. It is only too common that when people run out of ideas, they start filling their pages with more and more examples, in the hope that mind-numbing accumulation and cataloguing of data would somehow be accepted as a surrogate for theoretical insight. As an advocate of tedious grammars, K-2005 is bound to endorse the current vogue in linguistics which can perhaps be summed up in the slogan "Data is everything, theory is nothing!" It is my duty to firmly state that the attitude manifested in this stultifying slogan is inimical to any even halfway adequate notion of **general linguistics**. We have here a dramatic reversal of Paavo Ravila's slogan, enunciated in 1944, "No theory, no facts" (cf. Itkonen 2004: 320).

At this point, it is good to repeat that there are many important theoretical questions that can be answered only by statistical analysis of large corpora. For instance, Pajunen (2001) contains a judicious combination of intuition-based qualitative analysis and corpus-based quantitative analysis, fully in the spirit of Itkonen (1977a) and (1980a), even if — as is generally the case in corpus linguistics — no explicit causal models are given.

Just like any other grammars, grammars of 'exotic' languages are primarily, and often exclusively, based on intuition, but this time it is — at least in the early stage — the intuition of one or more informants, rather than the intuition of the linguist, i.e. of the person who is to become the author of the grammar in question. In other words, grammars of 'exotic' languages tend to be based on **elicitation**. This process is characterized by Haiman (1980: xi) as follows:

> Kamani Kutane, my coeval and neighbour in Sara Village, has been my major informant both at Lufa and, in correspondence, in Canberra and Winnipeg over seven years. (...) I will always remember Kamani for his thought experiments: given a

> minimally contrasting pair of sentences, he would construct elaborate background stories which would be appropriate for only one of these sentences. Eventually I would understand one of these, and we could move on. It was by means of such continued thought experiments that he was able to make clear to me that most mysterious of all Hua forms, the gerund in *-gasi'* [discussed in Itkonen 2005a: 92–93].

The fact that grammars of 'exotic' languages are based on intuition-cum-elicitation is of course fully compatible with **texts** being used for illustration: "Running texts are given, at the end of the chapter, only from five basically unwritten languages, i.e. Diyari, Hua, Rapanui, Wari', and Yagua, to show that — contrary to a wide-spread misconception — **structural complexity** is not a characteristic feature of written language only" (Itkonen 2005a: 9). It is clear that elicitation by means of thought experiments, as described by Haiman, goes much beyond K-2005's ideal of grammar-writing, i.e. passively observing what has once been uttered.

As for my own notion of grammar-writing, it is not adopted from the generativist tradition (as K-2005 suggests), but from the typological-functional tradition, as applied in Itkonen (2001a),with more than 900 examples from some 40 languages, and more recently in Itkonen (2005a), with ten case studies of non-European languages. The rise and fall of generativism in Finland has been described in Itkonen (1999).

3. The 'atheoretical vs. theoretical' distinction

K-2005 finds fault with my notion of 'atheoretical rule-sentence' (as opposed to 'theoretical rule of grammar'). The objections are often expressed in a rather cumbersome way, and therefore the reader may wish to consult Dahl's (1975 [1980]) more transparent formulations, answered — to repeat — in Itkonen (1976b) and (1980b).

What is at issue here is the distinction, adumbrated already by the 'two-level' character of linguistics, between basic statements (about primary data) and theoretical generalizations. These two types of linguistic entities are strongly dissimilar, in the following sense. From the **scientific** point of view, the 'rules of grammar' (as constituents of theoretical-linguistic descriptions) are interesting because they are supposed to reveal the non-obvious structure-cum-function of language. By contrast, rule-sentences possess absolutely no interest because of their utterly obvious or trivial nature. From the **metascientific** or **philosophical** point of view, rule-sentences possess an

enormous interest, because they reveal the ultimately **normative** nature of linguistic data and thus show that, contrary to an obstinate misconception, linguistics is not a natural science. By contrast, rules of grammar — being hypothetical in character, just like theoretical descriptions in each and every academic discipline, including philosophy and formal logic (cf. Itkonen 1978: Chaps. X–XI) — possess no special interest.

Over the years, I have given many examples of rule-sentences, but in general I have preferred this one: 'In English the definite article (i.e. *the*) precedes the noun (e.g. *man*)'. This preference of mine is so well known that, for instance, in Edinburgh, April 1987, Geoffrey Sampson humorously suggested that I may have founded a whole new discipline called '*the*-ology'. Now, in what way can rule-sentences reveal the normative nature of linguistic data? — by showing that, unlike universal statements of the natural-science variety, they cannot be falsified by single spatiotemporal occurrences. For instance, our rule-sentence cannot be falsified by an utterance like 'Man the came in', because *man the* is incorrect; and it cannot be falsified by an utterance like 'The man came in', because *the man* is correct.

This is the basic argument for normativity. Upon hearing it, people feel a nearly irresistible urge to disagree. Over the last 30 years or so, I have heard many objections, in fact, exactly 12 in number, and, labelled as the 'standard objections', they are first formulated and then answered in Itkonen (2003a: 18–21, 32–35). For reasons of space, they cannot be discussed here. But before you believe to have found the perfect objection against the basic argument for normativity, I advise you to check whether your objection is among the 12 'standard objections'. — Let us now at least have some quotations from Itkonen (1978) to support and further elucidate what was said above:

> The hypothesis 'All pieces of metal expand when heated' is falsified if we find a piece of metal that does not expand when heated. On the other hand, a sentence referring to a rule is not falsified simply because there occur (what looks like) counter-instances to it. Consider the sentence 'In poker a full house beats a flush'. This sentence it not falsified by the fact that in one particular game a player with a flush takes in the pot even though someone else is holding a full house. Such a performance is **incorrect**, whereas the rule-sentence is about **correct** performances only. It is a remarkable fact that behavior violating a rule does not falsify the corresponding rule-sentence. The reason is that what one **does** has no direct relation (although it certainly has **some** relation) to what one **ought** to do; and a rule-sentence is precisely about what one **ought** to do. In other words, a rule-sentence is about possible (correct) actions which ought to be done, and not about factual actions,

whether correct or incorrect, which are done. (Of course, a rule would cease to exist — in any strong sense of 'existence' — if correct actions conforming to it were no longer done as a matter of fact; but this is a different question.) Now since counter-instances are simply irrelevant, we cannot even specify the circumstances in which our rule-sentence could be taken to be falsified. But this means that rule-sentences do not satisfy the most basic requirement imposed on empirical hypotheses and theories, according to which "**criteria of refutation** have to be laid down beforehand: it must be agreed which observable situations, if actually observed, mean that the theory is refuted" (Popper 1963: 38) (p. 157).

[There follows a demonstration that sentences describing rules of a game and those describing rules of a language behave methodologically in the same way.]

As an example of a rule of language, the rule about the English definite article is unnecessarily abstract or general. For my purposes, it is sufficient to divide this rule up into a set of lower-level rules which determine for each particular noun in English that, when the noun is correlated with the definite article, the article precedes, and does not follow, it. The corresponding rule-sentences are of the type 'In English, the definite article precedes the word *man*', 'In English the definite article precedes the word *woman*', etc. It is clear that such sentences refer to genuine rules, and not to particular spatiotemporal events, because they refer to word-**types** (or word-concepts), and not to any particular one from among those potentially infinitely many spatiotemporal word-**tokens** which may exemplify any given word-type. (p. 160–161)

(…)

I am, however, not concerned here with establishing the boundary between atheoretical and theoretical (...) Accordingly I am concerned with establishing the existence of those absolutely clear cases that may be taken as the two extremes of the continuum leading from atheoretical to theoretical: At one end, we have rule-sentences like 'In English the definite article precedes the word *man*' or 'In English the plural of *boy* is *boys*'; at the other end, we have grammatical hypotheses like the 'subject raising transformation' or the 'A-over-A principle' (cf. 9.5 below). There can be, and are, falsificatory counter-instances to such hypotheses, for the obvious reason that neither their scope nor their truth is known with certainty (cf. 5.3–4 above). (p. 161–162)

(…)

It is an essential requirement for rule-sentences that they be absolutely trivial: sentences about language whose truth or falsity is not known immediately and

> beyond the possibility of doubt are *ex definitione* not rule-sentences. Therefore there can be no scientific interest in stating rule-sentences of a given language. However, there is a considerable metascientific interest in merely realizing that there **are** such sentences, in view of the fact that, because of their non-empirical nature, they flatly contradict the claim that linguistics (in the sense of 'grammar') is an empirical science. (...) When I direct attention to rules, I am not reintroducing any new entities. All that I offer is **reinterpretation** of some well-known facts. This can be made more precise as follows. Each science must have its own set of basic statements, i.e. statements dealing with the simplest aspect of that region of reality with which the science in question is concerned. The basic statements of natural science are about particular spatiotemporal occurrences (cf. above). What do the basic statements of grammar look like? Bach (1974: 61–63) and Leech (1974: 84–90) give examples of, respectively, morpho-syntactic and semantic basic statements. For instance:
>
> 'The past tense of *play* is *played*. The past tense of *sing* is not *singed* but *sang*.' (...) 'I am an orphan' is synonymous with 'I am a child and have no father or mother'. (...)
>
> Notice that those 'basic statements of grammar, i.e. rule-sentences, which contain theoretical terms like ['past tense'] or 'synonymous' are not immediately understandable to a layman. To this extent, then, they do not just express prescientific or atheoretical everyday knowledge. However, it is obvious that they could be reformulated in purely atheoretical terms. Besides, the meaning of those theoretical terms that occur in the rule-sentences here considered can be easily taught to everybody. (p. 166–168)

All these claims and arguments have been made many times in my various publications, and they are repeated also in Itkonen (2003a), sometimes verbatim. K-2005 finds (atheoretical) rule-sentences **superfluous** because they are allegedly **identical** with (theoretical) rules of grammar. As shown by the quotations just given, this is nonsense. In a rare moment of lucidity, to be sure, K-2005 admits that rule-sentences may serve the purpose of clarifying the notion of correctness (i.e. normativity). This is, exactly, their **sole** purpose.

But then K-2005 takes this admission back. There is no correctness, there is nothing to clarify. Why? — because there is, after all, no difference between empirical hypotheses like 'All ravens are black' and rule-sentences. Why is there no such difference? — because, just like a rule-sentence cannot be falsified by **incorrect** utterances, this empirical hypothesis cannot be falsified by albino ravens, i.e. ravens that are **non-prototypical**. This is a monumental confusion. Of course the empirical hypothesis is falsified by albino (= non-black) ravens. Just **look at** the empirical hypothesis as it is formulated here before your very eyes. It is **not** about **protypical** ravens, it is

about **all** ravens. It claims that **all** ravens, without exception, are black. Therefore if, and when, we find an entity which is a raven and yet is not black (for instance, an albino raven), then the hypothesis is falsified. If the hypothesis had been formulated **differently**, i.e. as 'All prototypical ravens are black', and if we have reason to consider albino ravens as non-prototypical, then — and only then — the hypothesis would not be falsified by albino ravens. — If you do not understand this argument on the first reading, I advise you to read it again.

4. Dealing with the 'standard objections'

Among my 12 standard objections, there are an objection — i.e. standard objection v) — against the use of a theoretical term like 'definite article' and an objection — i.e. standard objection iii) — pointing out that the definite article may occasionally also follow the noun. This is how I first formulate and then answer these standard objections in Itkonen (2003a):

> v) "Since the rule-sentence A [= 'In English, the definite article (i.e. *the*) precedes the noun (e.g. *man*)'] contains theoretical terms/concepts like 'definite article' and 'noun', it is not analogous to a simple generalization like B [= 'All ravens are black'], which is expressed in ordinary language." This is true, as such, and this is why we illustrated the terms in question with the words *the* and *man*. These illustrations do make A analogous to B" (p. 19).
>
> (…)
>
> iii) "It is just false to say that the definite article (always) precedes the noun; just think of an expression like *Ivan the Terrible*." This objection is disingenuous. The one who makes it understands perfectly well the meaning of A, but pretends that he does not. ("If I were a robot which reads the sentence A, I would not know how the sentence A is meant to be taken; maybe I am a robot; therefore the meaning of A is not clear.") This person might want to falsify B by **painting** a raven white. ("The meaning of B is not clear, i.e. it was not specified in what sense 'black' should be taken, and this thing here is a non-black raven, is it not?") (p. 18–19).

K-2005 repeats standard objections v) and iii), but without acknowledging that they are taken from me and that I have already answered them. This is bizarre behavior.

To round off the picture, and to show that I have left no stone unturned, I next give the **complete answer** to standard objection iii):

"Having established the existence of clear cases *qua* counter-examples to any universalist pretensions of variationism, (...) I must add that the behavioral basis for the clear cases is not as unequivocal or 'clear' as that which it is the basis for. That is, clear cases are idealizations. Lyons (1977: 568–569) enumerates the three most important types of idealization at hand, namely 'regularization', 'standardization', and 'decontextualization'. These may be clarified, respectively, as follows. First, obvious mistakes, due say to drunkenness, must be discarded. Second, idiosyncratic elements like inside jokes must be discarded. Third, sample sentences must be imagined to be uttered in a standard or neutral context. It must be emphasized that all these idealizations are so natural that they might almost be said to be made **instinctively**. They already underlay the grammatical theories of Ancient Greece and India. Perhaps more importantly, they have always underlain the practice of language teaching, and have thus proved their worth (cf. Itkonen 1978: 149). Therefore it would in my opinion be nonsensical to try to **undo** these idealizations, which does not of course mean that they should or could not be **analyzed**. The existence of the above-mentioned quasi-instinctive idealizations has certain interesting implications concerning how my atheoretical norm-sentences [= rule-sentences] (cf. 3.2) ought to be formulated. I have claimed that just as (the clear cases of) norms are known with certainty, so is the truth of the corresponding norm-sentences. To this it has been objected that a norm-sentence like 'In English the definite article precedes the noun' has several prima facie counter-examples, e.g. '**Ivan the** Terrible' or 'Look at that **man**, **the** fat one over there'. Until such cases are explicitly accounted for, the norm-sentence can presumably neither be nor be known to be true. But accounting for them amounts to nothing less than giving an exhaustive description of the English determiners as they occur in all possible contexts. Such a task is in turn inseparable from writing a theoretical description of the entire English language. But no theoretical grammar can be known to be true. Therefore our norm-sentence cannot be known to be true either. — This is an interesting argument, but it must contain a fallacy somewhere, because its conclusion is self-evidently false. The fallacy consists in ignoring the workings of our quasi-instinctive idealizations. When I utter my norm-sentence to someone who knows English (as well as the rudiments of the linguistic terminology), he immediately understands which norm I am referring to, and he knows the sentence to be true. A computer could not perform the same feat, because it could not grasp the norm until it had been told all the prima facie exceptions to it. A normal speaker of English, by contrast, grasps the norm on the strength of his capacity to idealize, part of which is the capacity to disregard the prima facie exceptions. It is this aspect of the clear cases which Lyons captures with his previously quoted, apparently circular statement that he intends to concentrate on utterances whose **pretheoretical** status with respect to correctness can **reasonably** be assumed to be determinable. When formulating our norm-sentences, we must have some minimal trust in the intellectual capacities of our audience. In general our trust is justified. Therefore I could just as well have formulated my norm-sentence as '*the man* is right, and *man the* is wrong', and could have let the audience infer, or rather recognize, the intended norm (cf. Itkonen 1978: 325–326, n. 90, and 167–168). The only members of my audience who would betray my trust are professional linguists.

> Urged by their professional interest, and therefore acting in a computer-like fashion, they would pretend not to understand the norm which they do understand in fact, and would ask to be explicitly told all the prima facie exceptions. Although I intend to give an **atheoretical** (or pretheoretical) description, and they think they are asking me to give what I intend to give, they would in fact be asking me to give a **theoretical** description. This is the fallacy in their argument. (Itkonen 1983: 262–264)

At the outset of this paper I said that *langue*, qua idealization, is a **practical** necessity. But to the extent that it turns out to be one of our 'instinctive idealizations' of the type discussed in the preceding quotation, it is not just a practical, but also a **conceptual** necessity:

> Consider an analogy: Is the painting of Mona Lisa 'nothing but' patches of paint (= variation, *parole*), or is it the picture of a woman (= discreteness, *langue*)? Of course it is the latter. Certainly there is variation, but to some extent we as speakers do not experience it; we might be said to be so constrained as to ignore it. It would be a big mistake to disregard this fact, or the way that speakers **in reality** conceptualize language. The distinction between a conglomerate of patches of paint and a painting is analogous to the distinction between phonetics and phonology. Is everything just phonetics? Is there no phonology? Or should phonology be discovered again? (Itkonen 2003a: 35)

K-2005 finds it **inconsistent** that I pay an equal amount of attention to intuition-based non-causal analysis, as evidenced by Itkonen (1978), and to observation-based causal analysis, as evidenced by Itkonen (1983). The fallacy that K-2005 is here guilty of may be clarified with the aid of the following analogy. A building is a whole which has such parts as the walls, the doors, the windows, and the roof. It is not inconsistent to speak both about the walls and about the roof. Anybody should understand this, because the example is familiar and simple. Now, linguistics is a whole which has such parts as grammatical theory, sociolinguistics, psycholinguistics, and diachronic linguistics. Why does K-2005 not understand that it is not inconsistent to speak both about grammatical theory and about sociolinguistics, for instance? The reason must be that, for K-2005, this realm of phenomena is complex and unfamiliar. Data-collecting leaves one ill-prepared for the complexities of theoretical thinking.

Is it possible to **reconstruct** the inference that has led K-2005 to this fallacious conclusion? I think so. Let A and B stand for grammatical theory and sociolinguistics, respectively. Now, the K-2005 type inference goes as follows: "Itkonen says that there is A and B; but B is not A, which means that

B is not-A; but to claim that there is A and not-A is the same thing as to assert the contradiction 'p & ~p'; therefore Itkonen's overall view of linguistics is inconsistent." If I am not mistaken, this is K-2005's lasting — and only — contribution to the methodology of linguistics: to show, with unique clarity, how **not** to think.

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Contact information:

Esa Itkonen
Dept. of General Linguistics
Henrikinkatu 4a
FI-20014 University of Turku
Finland
e-mail: esa.itkonen@utu.fi

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