Esa Itkonen

CONCERNING DAHL'S (2004) NOTION OF 'LINGUISTIC COMPLEXITY'

> Mottos: "Es lässt sich über eine Sprache kein Urteil fällen, mit andern Worten, ihr Wesen lässt sich nicht erkennen, wenn man sie nicht mit andern [Sprachen] zusammenhält ... Ohne einen Überblick über das ganze Gebiet der Sprache überhaupt [kann] eine einzelne nicht erkannt werden." "Wenn wir nicht wissen, wie etwas geworden ist, so kennen wir es nicht" (August Schleicher in 1863).

1) Historical Background

According to the received view, the Neogrammarian doctrine had two, and only two, pillars, namely sound change and analogy. This view certainly has some foundation in facts. It is supported e.g. by the following passage:

> "Der Symmetrie des Formensystems is also im Lautwandel ein unaufhaltsam arbeitender Feind und Zerstörer gegenüber gestellt. Man kann sich schwer eine Vorstellung davon machen, bis zu welchem Grade der Zusammenlosigkeit, Verworrenheit und Unverständlichkeit die Sprache allmählich gelangen würde, wenn sie alle Verheerungen des Lautwandels geduldig ertragen müsste, wenn keine **Reaktion** dagegen möglich wäre. Ein Mittel zu solcher Reaktion ist nun aber in der **Analogiebildung** gegeben. Mit Hilfe derselben arbeitet sich die Sprache allmählich immer wieder zu angemesseneren Verhältnissen, zu festerem Zusammenhalt und zweckmässiger Gruppierung in Flexion und Wortbildung. So sehen wir in der Sprachgeschichte ein ewiges Hin- und Herwogen zweier entgegengesetzter Strömungen. Auf jede **Desorganisation** folgt eine **Reorganisation**. Je stärker die Gruppen durch den Lautwandel angegriffen werden, um so lebendiger ist die Tätigkeit der Neuschöpfung" (Paul 1975 [1880]: 198; emphasis in the original).

The history of every language is pictured as an eternal battle between two antagonistic forces, which were also called (speaker-oriented) *Bequemlichkeit* and (hearer-oriented) *Deutlichkeit* in the tradition that preceded Hermann Paul. The morphosyntactic system of every language is constantly 'attacked' by its 'enemy', namely sound change, and without the therapeutic interference of analogy, these attacks would sooner or later lead to mutual incomprehension and total destruction. Thus, analogy is seen as a **means** (*Mittel*) to an **end**, namely maintaining any language as a functioning system of communication. Hence the history and the very existence of every language is imbued with **rationality**.

As obvious as this conclusion is, it is seldom spelled out explicitly. William Dwight Whitney is here a laudable exception. With his natural-science background, it was easy for him to detect the basic difference between the method of natural sciences and the method he used in his own discipline:

> "[T]here is nothing in the whole complicated process of [linguistic change] which calls for the admission of any other efficient force than the **reasonable action**, the action for a definable purpose, of the speakers of language: the purpose being, as abundantly shown above, the adaptation of their means of expression to their constantly changing needs and shifting preferences" (Whitney 1979 [1875]: 144; emphasis added). "[What is] involved in the process [of linguistic change] ... is simply the exercise ... of ... the faculty of adapting means to ends, of apprehending a desirable purpose and attaining it. It is different only in its accidents ... and not in its essential structure, from that other process, not less characteristic of **human reason**, the making and using of **instruments**" (p. 145; emphasis added).

The same overall view of linguistic change is represented e.g. by Heine et al. (1991), with their emphasis on (creative) **problem-solving**. There is no other way to make linguistic change comprehensible than by using one or another type of **rational explanation** (cf. Itkonen 1984), and this is what happens in fact. Yet, when spelled out, the idea of rationality in general meets with resistance. Why is this? Whitney (1979 [1875]) knew the reason:

> "One great reason why men are led to deny the agency of the human will in the changes of speech is that they see so clearly that it does not work consciously toward that purpose. ... [E]very one is welcome to hold that alterations of speech are not made by the human will; there is no will to alter speech; there is only will to use speech in a way which is new; and the alteration comes of itself as a result"(pp. 146-147; discussed in Itkonen 2005: 53-55).

Let it be added that, according to Itkonen (1983a), the property of being amenable to rational explanation is the common denominator of such linguistic subdisciplines as pragmatics, (experimental) psycholinguistics, sociolinguistics, diachronic linguistics, and universals research. The roots of rational explanation are indicated by the title of a subsection in Itkonen (2008d): "La empatía deviene explicación racional". Large questions are looming here.

Now let us go back to Hermann Paul. His 'tug-of-war' scenario of linguistic change that was given in the quotation above is supported in his book by a huge amount of data from the history of various Indo-European languages. Whether one likes Paul's colorful metaphors or not, one just cannot ignore the evidence that he adduces. (Well, obviously, one can do that, and one has done exactly that; but doing so is wrong.)

The ideal state of all linguistic systems as well as the chances of ever reaching it are characterized by Paul as follows:

> "Jede Sprache ist unaufhörlich damit beschäftigt, alle unnützen Ungleichmässigkeiten zu beseitigen, **für das funktionell gleiche auch den gleichen lautlichen Ausdruck zu schaffen**. Nicht allen gelingt es damit gleich gut. Wir finden die einzelnen Sprachen und die einzelnen Entwickelungsstufen dieser Sprachen in sehr verschiedenem Abstande von diesem Ziele. Aber auch diejenige darunter, die sich ihm am meisten nähert, bleibt noch

> weit genug davon. Trotz allen Umgestaltungen, die auf dieses **Ziel** losarbeiten, bleibt es ewig unerreichbar" (p. 227; part of the original emphasis deleted).

Contrary to the received view, however, what precedes is not the whole picture of the Neogrammarian doctrine, but only one half of it. The other half is given in Chapter 19 (= 'Genesis of Word-Formation and Inflection'), which contains the following statements on its first page:

> "Wir haben uns vielfach mit der analogischen Neuschöpfung auf dem Gebiete der Wortbildung und Flexion beschäftigt. Wir müssen jetzt die urspüngliche, **nichtanalogische** Schöpfung auf diesem Gebiete ins Auge fassen. ... Die eigentliche **normale** Entstehungsweise alles Formellen in einer Sprache bleibt daher ... die **Komposition**" (p. 325; the last emphasis in the original).

Here Paul states explicitly that the **normal** way that anything formal emerges in a language is not analogy but something else, namely *Komposition*. What does *Komposition* mean? And why has this crucial passage been overlooked as far as the interpretation of the Neogrammarian doctrine is concerned?

Let us answer the second question first. Chapter 19 is very badly organized (and, to a lesser extent, the same is true of the book as a whole). It has three parts: compounding (pp. 326-347), derivation (pp. 347-349), and inflection (pp. 349-350). As the page numbers indicate, an inordinate amount of attention is paid to compounding, while the two other structures receive only a perfunctory treatment. This renders the argument hard to follow. Perhaps it can be grasped only by the most dedicated among the conoscenti of the Neogrammarian doctrine.

But what does *Komposition* mean? The best English translation seems to be **condensation**. It stands for a gradual process which begins with a syntactic group of independent words. In the second phase these are put together so as to produce a compound word. Next, one (here: the second) part of the compound word is weakened so as to form a derivational affix (here: suffix). Finally, by means of additional weakening, a derivational suffix becomes an inflectional one. To sum up:

syntaktische Wortgruppe > Kompositum > Ableitungssuffix > Flexionssuffix

syntactic pattern > compounding > derivation > inflection

Figure 1

Let us not be distracted by the fact that while the entire process is called *Komposition*, its second phase is called *Kompositum*. — Exactly 100 years later T. Givón captured the spirit of Figure 1 with his slogan "Today's morphology is

yesterday's syntax". Today, the continuum of Figure 1 is in general called 'grammaticalization'.

Next, the different phases of *Komposition* will be described and exemplified.

> a) "[Komposita entstehen] aus der syntaktischen Aneinanderreihung ursprünglich selbstständiger Elemente. Es sind dazu Verbindungen jeglicher Art tauglich" (p. 326). Der Übergang vom syntaktischem Gefüge zum Kompositum ist ein so allmählicher, dass es gar keine scharfe Grenzlinie zwischen beiden gibt" (p. 328). "Eine Vorbedingung für die Entstehung eines Kompositums ... besteht darin, dass die zugrundeliegende syntaktische Verbindung als Ausdruck eines einheitlichen Begriffes gefasst werden kann [= **reanalysis**], ..." (p. 329). [Examples from German, Latin, and French: *Morgenrot, respublica, bien-heureux, sur-le-champ, heute [< hiu tagu], videlicet, peut-être, auffahren*]
>
> b) "Ableitungssuffixe ... entstehen anfänglich stets so, dass ein Kompositionsglied die Fühlung mit dem urspünglich identischen einfachen Worte verliert [= **reanalysis**]" (p 347). "Am bekanntesten sind aus dem Deutschen [die Ableitungssuffixe] *-heit, -schaft, -tum, -bar, -lich, -sam, -haft*. Der Typus eines Wortes wie *weiblich* z.B. geht zurück auf ein altes Bahuvrihi-Kompositum, urgermanisch **wīðo-līkis*, eigentlich 'Weibergestalt', dann durch Metapher 'Weibergestalt habend'. ... Bei einem Worte wie *Schönheit* hat sich erst innehalb des Westgermanischen **aus der syntaktischen Gruppe ein Kompositum, aus dem Kompositum eine Ableitung entwickelt**. Urgerm. **skauniz haiđuz* 'schöne Eigenschaft', daraus regelrecht lautlich entwickelt ahd. [= althochdeutsch] *scōnheit*" (p. 347-348; emphasis added). "Die Entstehung neuer Suffixe steht in stetiger Wechselwirkung mit dem Untergang alter" (p. 349).
>
> c) "Auf die gleiche Weise wie Ableitungssuffixe entstehen **Flexionssuffixe**. Zwischen beiden gibt es ja überhaupt keine scharfe Grenze. ... Das Anwachsen des Pronomens an den Tempusstamm lässt sich z.B. durch Vorgänge aus heutigen bairischen Mundarten erläutern, die schon § 217 besprochen sind. Die Bildung eines Tempusstammes zeigt sich am handgreiflichsten am romanischen Fut.: *j'aimerai = amare habeo*. Doch es scheint mir überflüssig aus der **Masse des allgemein bekannten und jedem zur Hand liegenden Materials** noch weitere Beispiele zusammenzutragen" (p. 349-350; the latter emphasis added).

It is a sad fact, but a fact nevertheless, that today's average linguist knows next to nothing about the history of his/her own discipline. This is why (s)he now stares uncomprehendingly at those of Paul's words that are emphasized in the last quotation. (And if [s]he knows no German, as is today increasingly the case, his/her lack of comprehension will indeed be total.) How can Paul dismiss the 'lexical > inflectional' process in such a nonchalant way? How can he, in 1880, speak of the "huge mass" of grammaticalization phenomena supposedly known to "everybody"? Is it not Meillet who, in 1912, discovered that the French *j'aimerai* descends from the Latin *amare habeo* and, by the same token, discovered the fascinating world of grammaticalization?

The answer to the last question is a thundering 'No!' To justify this answer, we must dig deeper and go farther back in history.

Comparative Finno-Ugric linguistics was established before comparative Indo-European linguistics was. For simplicity, however, let us focus on the emergence of the latter, that is, on Franz Bopp and his 1816 book *Über das Konjugationssystem der Sanskritsprache in Vergleichung mit jenem der griechischen, lateinischen, persischen und germanischen Sprache*. What was Bopp's topic? It was, in the first place, **grammaticalization**, or the process in which the copula had (presumably) become part of the Indo-European verb and the originally independent pronouns had become its personal endings. Without grammaticalization, Bopp would have had nothing — **nothing!** — to talk about. Next, let us document this claim:

> "Der Zweck dieses Versuchs ist, zu zeigen, wie in der Konjugation der altindischen Zeitwörter [= verbs] die Verhältnisbestimmungen durch entsprechende Modikationen der Wurzel [= ablaut] ausgedrückt werden, wie aber zuweilen das verbum abstractum [= copula] mit der Stammsilbe zu **einem** Worte verschmolzen wird und Stammsilbe und Hilfszeitwort [= copula] sich in die grammatischen Funktionen des verbum teilen; zu zeigen, wie dasselbe in der griechischen Sprache der Fall sei, ..." (quoted from Arens 1974: 176). "Wenn ehedem ein Grund vorhanden gewesen, warum *mām mich* und *tam ihn* heisst und nicht letzeres *mich*, und ersteres *ihn*: so ist es gewiss aus demselben Grund, dass nun *bhava**mi** ich bin* und *bhava**ti** er ist* heisst, und nicht umgekehrt" (p. 177; emphasis added). "Ein Beispiel, wo im Lateinischen im Praesens ein verbum abstractum mit einem attributiven Wurzel verbunden wird, haben wir am *possum* ['I can'] < *pot-sum*. — Das Imperfekt wird an den attributiven Zeitwörtern [= verbs other than copula] durch *ba-m, -s, -t* und das Futurum durch *bo, bis, bit* bezeichnet. *Bam* und *bo* leite ich wie *fuvi* [> *fuī* = the Latin 1SG.PERF of 'to be'] von der indischen Wurzel *bhu-* ['to be'] ab" (p. 178).

Dahl (2004) is aware that the study of grammaticalization did not start with Meillet, as can be seen from his remark that grammaticalization "was extensively studied as a phenomenon **even in the 19th century**" (p. 119; emphasis added). At the same time, as the preceding quotations show, his focus is wrong. There would have been no 19th century linguistics (as we know it) without grammaticalization.

Let us go back to Givón's slogan "Today's morphology is yesterday's syntax" (as a reformulation of Figure 1). Paul (1975 [1880]) shows that in many, perhaps most, cases this slogan is in fact misleading:

> "Die Lebendigkeit des Gefühls für das Komposition zeigt sich besonders in der Fähigkeit eines Kompositums, als Muster für **Analogiebildungen** zu dienen. Wenn wir die Komposition aus der Syntax abgeleitet haben, so soll damit keineswegs gesagt werden, dass jedes einzelne Kompositum aus einem syntaktischen Komplex entstanden ist. Vielmehr sind vielleicht die **meisten** sogenannten Komposita in den verschiedenen Sprachen nichts anderes als Analogiebildungen nach solchen, die im eigentlichen Sinne Komposita zu nennen wären" (p. 346; the latter emphasis added). "Zwar [sind] die ersten

> Grundlagen der Wortbildung und Flexion durch das Zusammenwachsen ursprünglich selbständiger Elemente geschaffen, ... aber diese Grundlagen, sobald sie einmal vorhanden waren, mussten auch sofort als Muster für Analogiebildungen dienen" (p. 350).

Thus, it is **not** the case that every compound, derivation, or inflection genuinely results from the gradual process of condensation, which can be characterized as a steady 'forward motion'. Rather, in many if not most cases they result from analogy, which may be characterized as a 'sideways motion'. This shows, incidentally, that Paul's characterization of condensation as the **normal** way that forms emerge may be in need of some qualification. The same applies to the current fascination with grammaticalization, or the view (to put it crudely) that "grammaticalization is everything".

In the passage from Paul (1975 [1880]: 198) analogy was characterized, essentially, as a reaction to repair the damages (*Verheerungen*) caused by sound change. In the passages from p. 346 and 350, no such motivation is needed for new analogical constructions to come about.

Finally, Paul's dichotomy between analogy and ('non-analogical') condensation (*Komposition*) cannot be accepted as it stands. On reflection, it turns out that condensation/grammaticalization too involves its own kinds of analogy. Let us accept the view that grammaticalization has two components, namely reanalysis and extension (cf. Section 7 below). First, reanalysis needs some (analogical) model; and second, extension is of necessity an analogical process, as can be seen from the fact that Hopper & Traugott (1993) simply call it 'analogy'. Reanalysis-as-analogy and extension-as-analogy are distinguished and discussed in Itkonen (2002) and (2005: 109-113).

In what precedes, the term 'analogy' was mentioned several times. But what **is** it, really? This question is answered in Itkonen (2005) in nearly excruciating detail.

Why was this section needed anyway? If you don't know why, you have to read the second motto again.

2) Dahl's (2004) overall view of linguistic complexity/maturity

Dahl (2004) accepts Figure 1, with some modifications. I have put the following continuum together out of materials given on p. 107, 165, and 210.

free > periphrastic > compounding/incorporating > affixing > fusional > zero

Figure 2

In the first place, Dahl (2004) takes this continuum to represent the process of **grammaticalization**. However, it also represents increasing **complexity** or **maturity**. These three terms are regarded as synonyms. The starting point of the

continuum represents the state of maximal simplicity which, in agreement with McWhorter (2001), is assumed to be exemplified by **creole** languages. For instance, a periphrastic 'Auxiliary + Verb' structure is less complex than an incorporating 'Noun-Verb' structure, which is less complex than an affixing (or agglutinative) verb, which is less complex than a fusional (= ablaut) verb. More generally, and in reference to McWhorter (2001), mature phenomena are said to contain (but not to exhaust) the following (pp. 114-115):

— inflectional morphology
— derivational morphology
— incorporating constructions
— lexical idiosyncrasy
— agreement
— specific marking of subordinate clauses.

On p. 165, zero is referred to as "the final output of grammaticalization", and on the continuum of Figure 2, zero indeed represents the logical end point of maturation. But it certainly cannot be said to represent the highest degree of complexity. In this sense, then, maturity and complexity are **not** identical. Moreover, insofar as the process of maturation stops at the fusional stage without reaching zero, it might not be wholly inappropriate to call it *maturatio interrupta*.

In addition to increasing complexity/maturation, the continuum is also claimed (on p. 181) to represent increasing **non-linearity**. This is obviously true of the distinction between affixing vs. ablaut structures. It also makes sense insofar as zero, having no substance, is certainly the most non-linear phenomenon of all. Non-linearity is further exemplified by **hierarchy**. But notice that when (relatively) free word order becomes fixed and begins to express grammatical meanings, this is an instance of increasing linearity, not non-linearity.

3) How is the Dahl-type 'complexity' defined?

The "general idea" is stated as follows: "The complexity of an object [is] measured by the length of the shortest description of that object" (p. 40). Yet, complexity is not a unitary concept but contains the following subdivions:

a) "system complexity" = the complexity in the mapping between meanings and forms (p. 43, 51), which is "the topic of this book". The more a system is complex, the less it conforms to 'transparency' (p. 45). Transparency is identical with the so-called principle of 'one meaning – one form', or the 1M1F principle for short (cf. Anttila 1989 [1972]: 100-102, 129-10; Itkonen 1983: 208-211; Itkonen & Pajunen 2008: Subsection 1A). Hence the 1M1F principle can be identified with 'system simplicity' (although this term is not actually used in Dahl 2004).

b) “structural complexity” = this notion is introduced by means of the word pair *maid* vs. *pai-d*, of which the latter is more complex since it contains more parts than the former (p. 44). Accordingly, the highest amount of structural complexity would be exemplified by the **polysynthetic** word. This cannot be quite right, however, because a (‘fusional’) ablaut verb form like *sang*, which contains no parts, is more complex like the affixing verb form *pai-d*. It follows that the Sioux languages, which, according to Chafe (1976: 8-9), are **both** polysynthetic **and** fusional, must exemplify the most complex type. This is not explicitly stated anywhere, but it is the logical conclusion.

Within structural complexity, moreover, there is an additional subdivision:

b-1) formal complexity (or “output complexity”)
b-2) semantic complexity (or “structural complexity at the morphological level”).

The need for this distinction becomes evident in connection with cumulative (or port-manteau) morphemes like the French *(il/elle) a* which consist of one (very simple) form (= “output complexity”) and of several meanings (= “structural complexity at the morphological level”) (p. 182). Now, on p. 289, “structural complexity” is distinguished from “output complexity”. This is somewhat inconsistent because “structural complexity” was originally defined so as to pertain to “the structure of expressions, at some level of description” (p. 44); and surely the level of “output” is one of the “levels of description”. Thus, output complexity **is** structural complexity.

Finally, there is also “conceptual complexity” (p. 45), but this turns out to be identical with the “general idea” quoted in the beginning of this section.

After all that has been said in what precedes, it is rather surprising that the ‘complexity metric’ so defined is **never** applied. First, it is **never** shown how the shortest one is chosen from among all possible descriptions of a phenomenon X. Second, it is **never** the case that the complexity of two (or more) phenomena X and Y (etc.) is compared by comparing the shortest description of X with that of Y. From the methodological point of view, this is a major disappointment.

Dahl (2004) opens with a presentation of the basic notions of **information theory**. Syntactic information and semantic information are introduced on p. 7. ‘Information’ *tout court* is defined as “reduction of uncertainty” (p. 7, 19). ‘Redundancy’ is defined as follows: “A message is **redundant** if there is a **less complex** message that could transfer the same amount of information” (p. 9; emphasis added). This seems to amount to equating redundancy with formal complexity. Notice that here ‘message’ stands for a formal entity. But it stands for a semantic entity on p. 43, where a language is interpreted as “a system which maps messages to expressions, or if we like, meanings to forms”. Moreover, “we regard the set of messages [i.e. meanings] that can be expressed in the language

under study as given", which is odd because this set must be not just infinite but essentially unpredictable.

Surprisingly enough, no use, or at least no systematic use, is made of information theory in the sequel. Let us consider the notion of **redundancy**. An ablaut form exemplifies 2M1F and — as compared with the standard provided by the 'transparent' 1M1F — is therefore 'complex' in the sense of being **non**-redundant. By contrast, any instance of agreement exemplifies 1M2F and is therefore 'complex' is the sense of being redundant. These are **opposite** phenomena. The only thing that they have in common is that they both deviate (although in opposite ways) from the 'transparent' 1M1F pattern. Therefore it is a fallacy to think that they are part of some unitary notion of 'complexity'. It is exactly the same criticism which shows the traditional 'flexive' (or 'fusional') type to be non-existent (cf. Itkonen & Pajunen 2008: Subsection 1A).

As noted above, there are still other instances of complexity, including lexical idiosyncrasy. This is a graded notion which ranges from inflectional classes (i.e. declensions and conjugations) to genuine exceptions. Now, information theory is called upon to explain the existence of lexical idiosyncrasy, as follows: "Lexical idiosyncrasy does seem to be an irrational and counterproductive property of language. ... It may be argued, however, that lexical idiosyncrasy introduces potentially **useful redundancy** and therefore serves a certain synchronic function" (p. 112; emphasis added).

Grammatical homonymy (also called 'syncretism') is the opposite of inflection classes insofar as a single form (like the Latin *mensae*) may express several grammatical meanings. Again, information theory is put to use: "It could also be argued that syncretism of the *mensae* type, where identical forms differ on at least two dimensions, is in fact an economical way of creating '**smart redundancy**'" (p. 188; emphasis added). **Opposite** phenomena are made to admit of one and the same explanation. Here the 'explanatory' use of information theory is seen to be 'flexible' to the point of becoming vacuous.

What is the **real** connection of inflectional classes and homonymy with ablaut on one hand and agreement on the other? As a special case of cumulative structure, ablaut exemplifies 2M1F in the **syntagmatic** dimension, while agreement, as a special case of extended or discontinuous structure, exemplifies 1M2F in the same dimension. Now, inflectional classes represent the more general phenomenon of **allomorphy**, which means that they exemplify 1M2F in the **paradigmatic** dimension. Homonymy (or syncretism) in turn exemplifies 2M1F in the same dimension. Notice that, as instances of (paradigmatic) complexity, allomorphy and homonymy are redundant and non-redundant, respectively. Now, the only thing that allomorphy and homonymy have in common with ablaut and agreement is that all these patterns **deviate from the 1M1F pattern**, whether in the syntagmatic or in the paradigmatic dimension (again, see Itkonen & Pajunen 2008: Subsection 1A).

To sum up, schematically: (syntagmatic or paradigmatic) 1M1F = **simplicity** vs. (syntagmatic or paradigmatic) non-1M1F = **complexity**. Moreover, (syntagmatic or paradigmatic) 2M1F = **non-redundant** complexity vs. (syntagmatic or paradigmatic) 1M2F = **redundant** complexity. Clearly, information theory is of no use in the present context because, regardless of what definitions may have been given in the opening chapters of Dahl (2004), complexity equals both the increase and the loss of redundancy.

As Panini already realized, zero may mean either invisible presence or genuine absence (cf. Itkonen 1991: 15-16). The former case is an instance of 1M0F. The role of zero is considered in Dahl (2004: 188-189), but much too briefly. Indeed, it can be argued that it is a common mistake in contemporary typological linguistics to underestimate the importance of zero (cf. Itkonen 2008a). This entails, among other things, that **analytic** languages are considered as somehow less interesting or, as in Dahl's (2004) case, as "less mature". This is inconsistent insofar as in terms of Figure 2, analytic languages qualify as the **most mature** ones.

To sum up, there is no genuine definition of 'complexity' in Dahl (2004). At most, it could be said that the following **ostensive** definition is given: "Look at Figure 2! This is what I have decided to call '(increasing) complexity'."

4) The rival conception

Such disciplines as literary criticism, semiotics, and (increasingly) sosiology have looked upon linguistics as a model to imitate. Linguists, in turn, would prefer to see themselves as practitioners of one or another natural science. This phenomenon is far from new:

> "What [the linguist] does need to insist upon is that the character of his department be not misrepresented, in order to arrogate to it a kind and degree of consequence to which it is not entitled — by declaring it, for exmple, a physical or natural science, in these days when the physical sciences are filling men's minds with wonder at their achievements, and almost presuming to claim the title of science as belonging to themselves alone. ... Not one item of any existing tongue is ever uttered except by the will of the utterer; not one is produced, not one that has been produced or acquired is changed, except by causes residing in the human will, consisting in human needs and preferences and economies. There is no way of claiming a physical character for the study of such phenomena except by a thorough misapprehension of their nature, a perversion of their **analogies** with the facts of physical science" (Whitney 1979 [1875]: 310-311; emphasis added).

As suggested by Whitney, one cannot view linguistics as a natural science unless one, first, sees (or thinks that one sees) an **analogy** between linguistics and this or that natural science and, second, tries to strengthen this analogy into an identity. Itkonen (2005: 196-200) discusses some such analogies. In general, they range from bad to very bad.

In the 60's and 70's, the term 'linguistics' was in general so employed as to refer to synchronic grammatical description. At the time it was assumed that the type of **explanation** needed in linguistics was the so-called deductive-nomological (= DN) explanation, which was originally meant to apply to deterministic physical phenomena. The original impulse for writing my 1974 dissertation was to show that this view was (and is) totally false, even if my argument soon ramified in new, and perhaps more interesting, directions. Itkonen (1978) is a revised version of my dissertation. During the 70's I was widely criticized for rebuking the DN conception. Today nobody defends anymore the view that the DN explanation, or any other comparable model of deterministic explanation, is what is required in linguistics (and in particular, in the synchronic-grammatical description). So we have to conclude that I was right and my critics were wrong. If nobody else says it openly, I say it myself.

Today, evolutionary theory has been set up as the model for linguists to imitate, and the *Zeitgeist* requires that linguistics be regarded as a **biological** science. I think this is a fundamental mistake. Language certainly has a biological substratum as well as an individual-psychological one, and both must be, and are, investigated. Nevertheless, and as argued by people like Whitney, Saussure, and Trubetzkoy among others, any given language is first and foremost a **social institution**. The social point of view is explained at length in the contributions to Zlatev et al. (2008). Special emphasis deserves to be placed upon **normativity**, which demarcates linguistic phenomena against 'mere' biological (or physical) phenomena (cf. Itkonen 2008b).

It goes without saying that, from a bird's-eye-view, organisms have grown in complexity. Therefore, if the analogy between evolutionary theory and linguistics is taken seriously, it follows that linguistic change (or 'evolution') must be viewed as exemplifying complexification rather than simplification. This is also Dahl's position (cf. Section 2), even if he is less anxious than some others to emphasize the analogy with evolutionary theory.

It is well known, however, that in the history of diachronic linguistics also the opposite view has had its own defenders. For Whitney, simplification is a necessary characteristic of linguistic change:

> "The tendency to abbreviation for ease, for economy of effort in expression, is a universal and blind one; destruction lies everywhere in its path. ... But we may note for our consolation that [a people] does not lose what it once possessed in the way of inflectional apparatus without providing some other and on the whole equivalent means of expression" (pp. 106-107).

For Paul, sound change can have a salutary effect when it genuinely simplifies the system:

> "Die Aufhebung lautlicher Verschiedenheiten bei funktioneller Gleichheit kann sehr **wohltätig** wirken, weil sie die Bildung der formalen Gruppen **vereinfacht**. ... So fallen z.B. die auf gleicher Grundlage beruhenden althochdeutschen Bildungssilben *-ul, -al, -il* im Mhd. [= Mittelhochdeutsch] in *-el* zusammen, ebenso *-un, -an, -in* in *-en* etc. Zwecklos sind aber auch solche Unterschiede wie die doppelte Bildung des Komparativs und Superlatives im Ahd. *-iro, -ist* — -ō*ro,* -ō*st*, und es ist daher nur ein **Vorteil**, wenn wir jetzt nur *-er*, *-[e]st* und *-ig* haben. Auch der Zusammenfall zweier ganzer Flexionsklasse wie der althochdeutschen Verba auf -ō*n* und -ē*n* in mhd. *-en* ist nur eine **zweckmässige Vereinfachung**" (p. 223; emphasis added).

Under other circumstances, however, sound change produces local simplification but global disorganization. It is in such situations that the therapeutic interference of analogy is needed (cf. the quote from Paul 1975 [1880]: 198).

The following often-quoted passage from Jespersen (1922: 324) deserves to be quoted once again in this context:

> "That language ranks highest which goes furthest in the art of accomplishing much with little means, or, in other words, which is able to express the greatest amount of meaning with the simplest mechanism".

The so-called Natural Morphology, developed in the 80's by Mayerthaler, Wurzel, and Dressler, is cited by Dahl (2004: 115-117) as the principal exponent of the view that linguistic change equals simplification rather than complexification. According to this view, to put it briefly, morphology contains several dimensions or levels: on each of them, change equals simplification, but since the different changes tend to cancel out one another, the end result is not, in general, an overall simplification, but rather a state of equilibrium. This position is formulated very clearly by Vennemann (1988).

As a final example, and one from more recent research, it is worth mentioning that Kusters (2003: 9) feels justified to adopt a position which takes the general truth of the 'change-as-simplification' view for granted:

> "And, in what ways do languages change towards complex morphology? Although these questions are interesting and relevant, I do not examine them farther. ... Therefore, my focus lies on the more accessible simplification processes."

Instead of debating *in abstracto* the strengths and the weaknesses of these rival views, it seems more meaningful to ask what happens as a matter of fact. Do languages become simple or complex? This will be the topic of Section 5. Still, already at this point it is good to realize that if we accept the traditional idea of a **typological cycle**, the whole debate evaporates because both sides are right: Languages become simple **and** languages become complex.

5) Do languages, as a matter of fact, become simpler or more complex?

I do not try to answer this question, at least not in the present context. Instead, I will make a few comments on the data that Dahl (2004) adduces to support his view that linguistic change is complexification rather than simplification.

Dahl is right to claim (p. 285) that the general opinion concerning the nature of linguistic change has been too much influenced by what has happened during the last 2000 years or so to the inflectional systems, and in particular to the case systems, of the Indo-European languages in Western Europe. It is certainly true that, to put it simply, case endings need not automatically disappear and be replaced by adpositions.

First, languages like Lithuanian and Russian have resisted the erosion of case endings much better than English or the Romance languages. To be sure, in the Indo-Aryan languages, not mentioned by Dahl (2004), the original case system has been totally restructured: "There are at least three **layers** of forms with case-like functions ... in most NIA [= New Indo-Aryan] languages, typically made up of inherited synthetic, new agglutinative, and quasi-analytic elements" (cf. Masica 1991: 231).

Second, other language families may offer a different picture. Proto-Uralic is assumed to have had a system of six (or perhaps only five) cases. This number has only exceptionally been reduced (as in a Khanti dialect). It may have remained the same (as in the Samoyed languages). But typically, it has increased (as in Finnish, Estonian, and Hungarian) (cf. Itkonen 1983: 218; 2008c). At least *prima facie*, much the same is true of the Dravidian languages. Proto-Dravidian is assumed to have had a system of at least four cases. Ancient Tamil had six cases, surprisingly with optional and interchangeble endings (cf. Itkonen 2003), while Modern Tamil has eight cases.

All these facts can be accommodated, if we just assume that Proto-Indo-European has occupied a different place on the **typological cycle** than either Proto-Uralic or Proto-Dravidian. Interestingly, Korhonen (1996: 191-194) thought that he was able to see glimpses of the **isolating** structure that presumably characterized the **Pre**-Proto-Uralic stage. To be sure, the fate of the various Uralic languages shows, or seems to show, that there is no 'iron necessity' that forces the various members of a language family to follow the typological circle in exactly the same order (and with the same speed). Of course, some of the differences involved may be due to language contact, but it is far from easy to decide whether this is in fact the case (cf. Section 6). In any case, Dahl is certainly right to claim (p. 291) that there is no **necessity** for any language to go all the way to zero on the grammaticalization scale. For instance, many Uralic case systems exemplify simultaneous decay and renovation.

In Chapter 11, Dahl's argument is crucially based on the stability of the **ablaut** pattern in the preterite of German verbs. However, this argument is far from cogent. Sometimes ablaut disappears, and sometimes it does not. It is

uninformative to label the latter cases as "stable" or "mature", because this terminology just repeats the fact that in these cases (unlike in many others) ablaut has not disappeared. It would be more meaningful to ask how **frequent** this phenomenon is. In the Indo-European context it does not seem to be very frequent. Consider the role of ablaut in 3SG.IND forms of the Sanskrit verb, exemplified by the root *nī-* ('to lead'): ACT.PRES = *nayati*, ACT.PRF = *ninā́ya*, ACT.AOR = *anaisit*, ACT.FUT = *nesyati*, PASS.PRES = *nīyate*. In Indo-Aryan languages this ablaut alternation has been lost entirely. In Hindi, for instance, a new type of ablaut has emerged for the expression of the INTR vs. TRANS or the TRANS vs. CAUS distinction: *marn*ā ('to die') vs. *mārn*ā ('to kill'), *dikhn*ā ('to be visible') vs. *dekhn*ā ('to see'), *dhon*ā ('to wash') vs. *dhulān*ā ('to make wash'), etc. The use of ablaut in the verb morphology was in Classical Greek less pervasive than in Sanskrit, and in Latin even less so. Modern Greek and the Romance languages have lost ablaut entirely. Dahl conveniently ignores all these vital facts.

Notice also that WALS (= Haspelmath et al. 2005) finds it appropriate to adopt the following position: "less frequent inflectional methods like infixation, tonal affixes, and stem changes [= ablaut] are ignored" (p. 110). Thus, on the world scale, ablaut appears to be a marginal phenomenon. And, as we just saw, it seems to be rather infrequently the case that this marginal phenomenon acquires a "stable" or "mature" character. So why bother? One answer to this question will be given in Section 8.

Although there is no necessity to reach zero on the grammaticalization scale (cf. above), some, i.e. **analytic** (or isolating), languages have undeniably come close to doing so. There are two possible interpretations here: either analytic languages have remained unchanged for tens of thousands of years, invariably representing the **immature** creole-like stage; or they descend from more complex languages by means of linguistic change and thus represent, literally, the **most mature** stage. Late Archaic Chinese, exemplified by the *Analects* of Confucius, has been characterized by Li (1997) as "a prototype of isolating languages". Confucius formulated a remarkbale political philosophy which moulded the Chinese mind and also influenced later thinkers to the extent that he has been called "the patron saint of [European] eighteenth-century Enlightenment" (cf. Itkonen 1991: Chapter 3). The idea that the language in which Confucius expressed himself could in any sense be labelled 'immature' strikes me as utter nonsense.

6) Complexity defined in terms of ease vs. difficulty of processing

"[W]e should keep complexity apart from other notions such as 'cost' and 'difficulty', which must always be related to a user or an agent" (p. 39). This is in

line with the intention to distinguish 'absolute' complexity from 'relative' complexity and to focus on the former (pp. 25-26). But this cannot be done.

First of all, there is no simplicity (or complexity) *per se*. For instance, in whatever way simplicity is measured, the results differ depending on whether we are talking about computers or human beings. Therefore simplicity/complexity is necessarily relative in one sense or another. — This topic is important enough to be discussed in more detail in another context.

Second, Dahl repeatedly contravenes his own advice not to refer to difficulty when discussing complexity. To begin with, he exemplifies his notion of "smart redundancy" with assimilation and concludes: "Ease of articulation **and** understanding is thus bought at the expense of the complexity of the system" (p. 10; emphasis added). We already have seen that several complex phenomena are claimed to exemplify "smart redundancy" (cf. Section 3). Maybe this is an accurate characterization, maybe not. The important thing is that the existence of **smart** redundancy (qua characteristic of complexity) is justified by referring to **ease** of processing. Thus it is **not** possible to keep complexity apart from such notions as 'ease' or 'difficulty'.

Notice also the following oddity. Speech production and speech understanding are taken to be analogous here as far as ease vs. complexity is concerned. This is an extraordinary view. It should be the other way around: "In fact, production is quite different from perception. ... [S]peakers and hearers have opposing preferences" (Kusters 2003: 36-37).

Verb-second order, one of the 'mature' phenomena (p. 109, 115), "belongs to the most **difficult** features to master for second-language learners" (p. 113; emphasis added). Here maturity/complexity equals learning difficulty. Grammatical gender too is one of the 'mature' phenomena (p. 109, 115), and "as is well known, second-language learners have great **problems** with grammatical gender" (p. 200; emphasis added). Again, maturity/complexity equals learning difficulty.

In the Indo-European languages of Western Europe there has been much more simplification than complexification. Doesn't this fact suffice to refute Dahl's (2004) overall view? Apparently not. Why not? Because — Dahl assumes (pp. 280-285) — the conditions of language acquisition have been "suboptimal" in Western Europe; and Dahl's theory is supposed to be operative only in situations where the learning conditions are "optimal". It takes no great acumen to realize that this stipulation renders the theory unfalsifiable in principle: if things do not go as predicted, blame the circumstances, don't blame the theory; it is the circumstances (and not the theory) which are wrong.

The connection with learning difficulty becomes evident once again: "The long-term effects of suboptimal acquisition can be expected to involve the 'filtering out' of ... 'difficult' features of language" (p. 282). To be sure, it is immediately added that " 'difficult' and 'complex' should not be automatically

equated here". Maybe they should not be equated, but they are equated nevertheless.

In sum, Dahl fails to give the definition he promises to give, i.e. complexity defined in terms of the length of descriptions (cf. Section 3); and he gives a definition he promises not to give, i.e. complexity defined (rather incoherently, to be sure) in terms of processing ease/difficulty.

There remains an intriguing implication to be explored. By Dahl's criteria, polysynthetic (and preferably fusional) languages represent the highest degree of complexity. This is how Fortescue & Olsen (1992: 214) describe the conditions under which polysynthetic languages have been acquired:

> "The fact that such languages generally seem to have emerged in hunter-gathering societies in rather specific physical environments can hardly be ignored. Adaptation to a more complex world requiring greater referential specificity, greater emphasis on 'things' rather than on actions and on states (for the description of which polysynthetic 'holophrasis' can be extremely efficient), may militate against the preservation of extreme polysynthesis, and there do seem to be pressures of this sort at work in modern Greenlandic."

Considering the nature of the end result, such learning conditions should be called **super**-optimal from the complexity point of view (even if this goes against the etymological meaning of 'optimal'). In the name of consistency, it could — or could it? — then be demanded that the results of **super**-optimal learning be disregarded for the same reasons as those of **sub**optimal learning.

7) Misunderstanding grammaticalization

Verbs like *go* and *come* originally express deictic motions in space. By metaphorical extension, we may speak of someone or something 'coming' from the past and 'going' into the future, which means that the verbs have become grammatical markers of the corresponding tenses (cf. Heine & Kuteva 2002: 72-73, 161-163). This can be schematically illustrated with the following French example:

il vient de nager = 'he comes from swimming' > 'he just swam'

Here the *venir + de + INF* construction has been **reanalyzed** in the way indicated by the English translations. Still, one cannot know whether or not such a reanalysis has taken place, until it is **extended** to new contexts**,** i.e. contexts where the original spatial meaning is inappropriate:

il vient de mourir = 'he just died' (vs. '*he comes from dying')

According to this 'standard' interpretation, grammaticalization is a composite process with two components, namely reanalysis and extension (also called 'analogy', e.g. by Hopper & Traugott 1993). This interpretation also reveals the (meta-)**analogy** between grammaticalization and the so-called hypothetico-deductive method: a new theory is **abduced** to explain the old data, but whether or not this has happened, cannot be known until **new predictions** have been derived from it, i.e. predictions that could not have been derived from the old theory (cf. Itkonen 2002).

Curiously, Dahl's misses the crucial point in this scenario: "But this **undermines** the neat distinction between reanalysis and analogy [= extension], the latter becoming the empirical manifestation of the former" (p. 175; emphasis added). But this is just the point! At least in its first stage, reanalysis (or abduction) is **defined** so as to be 'invisible' until something else happens. Otherwise the distinction would not be "neat" at all; in fact, there would be no distinction. Dahl writes page after page about 'grammaticalization', but one cannot help wondering what he has in his mind when he uses this word.

In the foregoing example, reanalysis involves the weakening of a main verb into an auxiliary. This could be called 'categorial restructuring', in the following sense:

V + INF > AUX + V

There is also a stronger sense of restructuring, or what might be called 'syntactic restructuring'. Paul (1975 [1880]: 296-303) discusses this phenomenon under the label *Verschiebung der syntaktischen Gliederung* ('shift of syntactic structuring'):

> "Noch viel wichtiger ist es, dass gewisse Wörter, namentlich Pronomina oder Partikeln, die ursprünglich dem Hauptsatze angehörten, zu Verbindungsgliedern zwischen diesem und einem psychologisch untergeordneten Satze werden, der bis dahin noch von keinem Partikel eingeleitet war, ja überhaupt noch gar kein grammatisches Zeichen der Abhängigkeit hatte. Diese Wörter pflegen dann als ein Teil des Nebensatzes angesehen werden [= reanalysis]. Auf diese Weise sind eine Menge den Nebensatz einleitende Konjunktionen entstanden, und dieser einfache Vorgang der **Gliederungsverschiebung** ist eines der wesentlichsten Mittel gewesen, eine grammatische Bezeichnung für die Abhängigkeit von Sätzen zu schaffen. Meistens waren die betreffenden Wörter ursprünglich hinweisend auf den folgenden logisch abhängigen Satz (vgl. § 100). Hierher gehört die wichtigste deutsche Partikel *daz* = engl *that*, ursprünglich Nom. Akk. des Demonstrativpronomens.*Ich sehe, dass er zufrieden ist* ist hervorgegangen aus einem *Ich sehe das: er ist zufrieden*. ... Nachdem die Hineinbeziehung in den Nebensatz und die dadurch bedingte **Verwandlung** in eine Konjunktion sich vollzogen hatte, konnte diese Konstruktion ... auch auf Fälle **übertragen** werden, für die ein Nom. oder Akk. des Pron. nicht passte, vgl. *ich bin überzeugt (davon), dass du Schuld hast***; ..."** (p. 299; emphasis added).

First, there is reanalysis in the sense of syntactic restructuring (*Gliederungsverschiebung*):

> *[ich sehe das] [er is zufrieden] > [ich sehe [dass er ist zufrieden]] > [ich sehe [dass er zufrieden ist]]*

Second, there is extension (*Übertragung*):

> *ich sehe, dass er zufrieden ist > ich bin überzeugt, dass du Schuld hast (cf. *ich bin überzugt das: du hast Schuld)*

Categorial restructuring and syntactic restructuring are somewhat different. This is one reason that makes Haspelmath (1998) doubt the existence of a unitary notion of reanalysis (as mentioned in Dahl 2004: 171-174). For my part, I think the similarities outweigh the differences. By emphasizing the former, we achieve a generalization. We also establish the connection with abduction as it is used in scientific discovery, thus achieving an even higher-level generalization (again, cf. Itkonen 2002).

Paul was apparently unaware that *Analogie* as used in morphology and *Übertragung* as used in syntax are one and the same thing. This is not surprising, given that even 100 years later there were strong doubts about the viability of analogy in syntax. In fact, generativists went so far as to deny its existence, using the somewhat dubious argument that if something cannot be formalized, it does not exist. Itkonen & Haukioja (1997) showed, first, that intersubjectively valid intuitions about analogical relationships exist in syntax and, second, that such intuitions can be formalized. The argument is repeated, and based in a larger context, in Itkonen (2005: 2.2, 2.5, Appendix). The same argument is (re)formulated with exemplary clarity by Kac (2008).

It is customary to concentrate on the 'lexical > grammatical' cline, which results in a rather narrow conception of what grammaticalization is about:

> "What is, then, the subject matter of grammaticalization? On a wide interpretation, it concerns the emergence of different ways to express grammatical (as opposed to lexical) meanings, which include e.g. word order and [originally] phonetically-conditioned internal change (~ *Ablaut*)" (Itkonen 2002: 421).

In the same spirit Dahl (2004: 275) notes that "ablaut patterns ... generally seem to derive from prosodic alternations". The important point is that when phonetic/phonological variation is 'harnessed' so as to express grammatical meanings, this process cannot be conceptualized in such a way that there is first something (i.e. something lexical) which then becomes something else. Ablaut emerges, as it were, from **nothing**:

> "Zwischen der inneren Umänderung [= Ablaut] aber und dem Suffix ist der wichtige Unterschied der, dass der ersteren ursprünglich gar keine andere Bedeutung zum Grunde gelegen haben kann, die zuwachsende Silbe dagegen wohl meistens eine solche gehabt hat" (von Humboldt in 1832, quoted in Arens 1974: 214).

8) In praise of ablaut: Resurrecting the German romanticism

Between 1800 and 1870 the following overall view emerged in the German historical-comparative linguistics. The history of languages is divided into two periods that are characterized by progress (*Aufbau*), on the one hand, and decline (*Verfall*), on the other. The former period is contains the following three asceding stages: 'isolating < agglutinative < flexive (*flektierend*)', corresponding to 'mineral < plant < animal'. The latter period contains the development 'synthetic > analytic'. The highest stage, now largely belonging to the past, is characterized by ablaut. — The ideological roots of this view are discussed in Itkonen (2004).

Dahl thinks he can detect an analogy between Natural Morphology and the Aristotelian theory of locomotion and, in an attempt to discredit the former, he notes that "this theory, as is well known, was later discredited" (p. 116). This analogy is far-fetched, or simply bad. By contrast, it is not far-fetched at all to see a genuine analogy between the discredited view that ablaut represents the **highest** developmentary stage and the view that ablaut represents the **most mature** developmentary stage.

9) Final remarks

As far as its ontology is concerned, language is not a monolithic entity. At least the following three ontological levels, roughly corresponding to Popper's three 'worlds', must be distinguished: social-normative, individual-psychological, and physical/biological. Correspondingly, three distinct acts of knowledge have to be postulated, namely intuition, introspection, and observation (or perception). This position has been argued for e.g. in Itkonen (1981), (1983a), (2003b), (2008d). It is of some interest to note that Dahl (2004) has come to endorse the same position:

> "It does seem to make sense to speak of a language as a "Platonic" or if we like, an information-theoretic object ... We thus really need **three** notions rather than the customary two. If we let 'E-language' continue to stand for observable language behavior, and 'I-language' is made more precise by letting it denote the "cognitive representation of language", we can use 'P-language' (P for 'Plato' or 'Popper') as an emergent, information-theoretic object (for **earlier proposals** to the same effect, cf. ... Katz 1981)" (pp. 66-67; emphasis added)..

It is rather incongruous for Dahl to put his off-hand remark, presented on a single page, on a par with Katz (1981) and to treat the two equally as **proposals** to this or that effect. Katz (1981) may have its own defects (cf. Itkonen 1983b),

but it is nevertheless a closely argued book of more than 300 pages, whereas due to its character, Dahl's own "proposal" has no inherent interest.

Let us posthumously ask Katz what in **his** opinion were the "proposals to the same effect". Here is the answer:

> "Montague himself regarded linguistics as a branch of mathematics and a number of linguists and philosophers working within the Montague framework also take this point of view. Esa Itkonen and Jon Ringen have argued that linguistics is not an empirical science but an intuitional science (e.g. E. Itkonen, 'The Concept of Linguistic Intuition', in *A Festschrift for the Native Speaker*, ed. F. Coulmas, Mouton & Co., The Hague 1981, pp. 127-140, and J. Ringen, 'Linguistic Facts: a Study of the Empirical Scientific Status of Transformational Generative Grammars, in *Testing Linguistic Hypotheses*, eds. D. Cohen and J.R. Wirth, Halstead Press, New York, pp. 1-41). Katz has argued in *Languag and Other Abstract Objects* for linguistics to be taken as a branch of mathematics, for it to be an intuitional science, and for its truths to be about abstract objects" (Katz 1985: 15).

A couple of corrections are needed here. What Katz refers to as 'linguistics', is something much more restricted for me, namely so-called autonomous linguistics (in the sense of synchronic-grammatical description of a single language). Labov-type sociolinguistics, experimental psycholinguistics, and diachronic linguistics are certainly empirical disciplines (as argued in Itkonen 1983a). My view of autonomous linguistics as an intuitional science is presented in detail in Itkonen (1978: Chapters 10-11), where I also establish the thorough-going analogy between autonomous linguistics and formal logic (more precisely: a 'philosophical' logic like deontic logic), something that Katz often promised to do, but never delivered.

REFERENCES

('HP' is added to articles that are available on Esa Itkonen's homepage)

Anttila, Raimo. 1989 [1972]. *Historical and comparative linguistics*. Amsterdam: Benjamins.

Arens, Hans. 1974 [1969]. *Sprachwissenschaft. Der Gang ihrer Entwicklung von der Antike bis zur Gegenwart,Band I* (2nd ed.). Frankfurt a/M: Athenäum.

Chafe, Wallace. 1976. *The Caddoan, Iroquoian, and Siouan languages*. The Hague: Mouton.

Dahl, Östen. 2004. *The growth and maintenance of linguistic complexity*. Amsterdam: Benjamins.

Fortescue, Michael & L. Lennert Olsen. 1992. The acquisition of West Greenlandic. D. I. Slobin (ed.): *The cross-linguistic study of language acquisition*. Hillsdale, NJ: Lawrence Erlbaum.

Haspelmath, Martin. 1998. Does grammaticalization need reanalysis? *Studies in Language.*

Heine, Bernd et al. 1991. *Grammaticalization*. The University of Chicago Press.
Heine, Bernd & Tania Kuteva. 2002. *World lexicon of grammaticalization*. Oxford UP.
Hopper, Paul & Elisabeth Traugott. 1993. *Grammaticalization*. Cambridge UP.
Itkonen, Esa. 1974. *Linguistics and metascience.* Studia Philosophica Turkuensia II.
___. 1978. *Grammatical theory and metascience*. Amsterdam: Benjamins.
___. 1981. The concept of linguistic intuition. F. Coulmas: *A festschrift for native speaker*. The Hague: Mouton. HP.
___. 1983a. *Causality in linguistic theory*. London: Croom Helm.
___. 1983b. Review of Katz (1981). *Lingua*. HP.
___. 1991. *Universal history of linguistics: India, China, Arabia, Europe*. Amsterdam: Benjamins.
___. 2002. Grammaticalization as an analogue of hypothetico-deductive thinking. I. Wischer & G. Diewald: *New reflections on grammaticalization*. Amsteram: Benjamins. HP.
___. 2003. A case system with optional and interchangeable endings. HP.
___. 2004. Value-laden history: German linguistics 1800 – 1870. Manuscript.
___. 2005. *Analogy as structure and process*. Amsterdam: Benjamins.
___. 2008a. The blind spot of contemporary typological linguistics: The role of zero. HP.
___. 2008b. The central role of normativity in language and linguistics. In Zlatev et al. (eds.). HP.
___. 2008c. Remarks on the typology of Uralic languages. HP.
___. 2008d. *Qué es el lenguaje? Introducción a la filosofía de la lingüística*. (translated and with an introduction by Araceli Lopez Serena). Madrid: Biblioteca Nueva.
___ & Jussi Haukioja. 1997. A rehabilitation of analogy in syntax (and elsewhere). A. Kertesz (ed.): *Metalinguistik im Wandel*. Frankfurt a/M: Peter Lang.
___ & Anneli Pajunen. 2008. A few comments on WALS. HP.
Jespersen, Otto. 1922. *Language: Its nature, development, and origin*. New York: Holt.
Kac, Michael. 2008. Review of Itkonen (2005). *Studies in Language.*
Katz, Jerrold. 1981. *Language and other abstract objects*. Oxford: Blackwell.
___. (ed.). 1985. *The philosophy of linguistics*. Oxford UP.
Korhonen, Mikko. 1996. *Typological and historical studies in language*. Helsinki: Société Finno-Ugrienne.
Kusters, Christiaan Wouter. 2003. *Linguistic complexity. The influence of social change on verbal inflection*. University of Leiden Dissertation. Utrecht: LOT.

Li, Charles. 1997. On zero anaphora. J. Bybee et al. (eds.): *Essays on language function and language type, dedicated to T.Givón*. Amsterdam: Benjamins.
McWhorter, John. 2001. The world's simplest grammars are creole grammars. *Linguistic Typology.*
Paul, Hermann. 1975 [1880]. *Prinzipien der Sprachgeschichte*. Tübingen: Niemeyer.
Ringen, Jon. 1975. *Linguistic facts*. D. Cohen & J. Wirth (eds.): *Testing linguistic hypotheses*. New York: Halstead Press. [Reprinted in T. Haukioja (ed.) 2006. *Papers on language theory*. University of Turku: Publications in General Linguistics 10.]
Vennemann, Theo. 1988. Language change as language improvement. Vincenzo Orioles (toim.): *Modelli esplicativi della diacronica linguistica.* Pisa: Giardini.
WALS = Martin Haspelmath et al. (eds). 2005. *World atlas of language structures.* Oxford UP.
Whitney, William Dwight. 1979 [1875]. *The life and growth of language.* New York: Dover.
Zlatev, Jordan et al. (eds.). 2008. *The shared mind: Perspectives on intersubjectivity.* Amsterdam: Benjamins.