INTRODUCTORY REMARKS: ON THE SCIENCE OF LANGUAGE IN LIGHT OF THE LANGUAGE OF SCIENCE

Peter Grzybek

The seemingly innocent formulation as to a *science of language* in light of the *language of science* is more than a mere play on words: rather, this formulation may turn out to be relatively demanding, depending on the concrete understanding of the terms involved – particularly, placing the term 'science' into a framework of a general theory of science. No doubt, there is more than one theory of science, and it is not the place here to discuss the philosophical implications of this field in detail. Furthermore, it has become commonplace to refuse the concept of a unique theory of science, and to distinguish between a general theory of science and specific theories of science, relevant for individual sciences (or branches of science). This tendency is particularly strong in the humanities, where 19th century ideas as to the irreconcilable antagony of human and natural, of weak and hard sciences, etc., are perpetuated, though sophisticatedly updated in one way or another.

The basic problem thus is that the understanding of 'science' (and, consequently, the far-reaching implications of the understanding of the term) is not the same all across the disciplines. As far as linguistics, which is at stake here, is concerned, the self-evaluation of this discipline clearly is that it fulfills the requirements of being a science, as Smith (1989: 26) correctly puts it:

> Linguistics likes to think of itself as a science in the sense that it makes testable, i.e. potentially falsifiable, statements or predictions.

The relevant question is not, however, to which extent linguistics considers itself to be a science; rather, the question must be, to which extent does linguistics satisfy the needs of a general theory of science. And the same holds true, of course, for related disciplines focusing on specific language products and processes, starting from subfields such as psycholinguistics, up to the area of text scholarship, in general.

Generally speaking, it is commonplace to say that there can be no science without theory, or theories. And there will be no doubt that theories are usually conceived of as models for the interpretation or explanation of the phenomena to be understood or explained. More often than not, however, linguistic understandings of the term ‘theory’ are less “ambitious” than postulates from the philosophy of science: linguistic “theories” rather tend to confine themselves to being conceptual systems covering a particular aspect of language. Terms like ‘word formation theory’ (understood as a set of rules with which words are composed from morphemes), ‘syntax theory’ (understood as a set of rules with which sentences are formed), or ‘text theory’ (understood as a set of rules with which sentences are combined) are quite characteristic in this respect (cf. Altmann 1985: 1). In each of these cases, we are concerned with not more and not less than a system of concepts whose function it is to provide a consistent description of the object under study. ‘Theory’ thus is understood in the descriptive meaning; ultimately, it boils down to an intrinsically plausible, coherent descriptive system (cf. Smith 1989: 14):

> But the hallmark of a (scientific) theory is that it gives rise to hypotheses which can be the object of rational argumentation.

Now, it goes without saying that the existence of a system of concepts is necessary for the construction of a theory: yet, it is a necessary, but not sufficient condition (cf. Altmann 1985: 2):

> One should not have the illusion that one constructs a theory when one classifies linguistic phenomena and develops sophisticated conceptual systems, or discovers universals, or formulates linguistic rules. Though this predominantly descriptive work is essential and stands at the beginning of any research, nothing more can be gained but the definition of the research object [. . .].

What is necessary then, for science, is the existence of a theory, or of theories, which are systems of specific hypotheses, which are not only plausible, but must be both deduced or deducible from the theory, and tested, or in principle be testable (cf. Altmann 1978: 3):

> The main part of a theory consists of a system of hypotheses. Some of them are empirical (= tenable), i.e. they are corroborated by data; others are theoretical or (deductively) valid, i.e. they are derived from the axioms or theorems of a (not necessarily identical) theory with the aid of permitted operations. A scientific theory is a system in which some valid hypotheses are tenable and (almost) no hypotheses untenable.

Thus, theories pre-suppose the existence of specific hypotheses the formulation of which, following Bunge (1967: 229), implies the three main requisites:

(i) the hypothesis must be *well formed* (formally correct) and *meaningful* (semantically nonempty) in some scientific context;

(ii) the hypothesis must be *grounded* to some extent on previous knowledge, i.e. it must be related to definite grounds other than the data it covers; if entirely novel it must be compatible with the bulk of scientific knowledge;

(iii) the hypothesis must be empirically testable by the objective procedures of science, i.e. by confrontation with empirical data controlled in turn by scientific techniques and theories.

In a next step, therefore, different levels in conjecture making may thus be distinguished, depending on the relation between hypothesis (h), antecedent knowledge (A), and empirical evidence (e); Figure1.1 illustrates the four levels.

(i) *Guesses* are unfounded and untested hypotheses, which characterize speculation, pseudoscience, and possibly the earlier stages of theoretical work.

(ii) *Empirical hypotheses* are ungrounded but empirically corroborated conjectures; they are rather isolated and lack empirical validation, since they have no support other than the one offered by the fact(s) they cover.

(iii) *Plausible hypotheses* are founded but untested hypotheses; they lack an empirical justification but are, in principle, testable.

(iv) *Corroborated hypotheses* are well-grounded and empirically confirmed; ultimately, only hypotheses of this level characterize theoretical knowledge and are the hallmark of mature science.

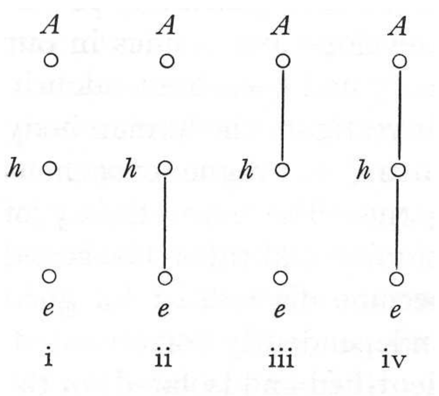

Figure 1.1: Levels of Conjecture Making and Validation

If, and only if, a corroborated hypothesis is, in addition to being well-grounded and empirically confirmed, general and systemic, then it may be termed a 'law'. Now, given that the "chief goal of scientific research is the discovery of patterns" (Bunge 1967: 305), a law is a confirmed hypothesis that is supposed to depict such a pattern.

Without a doubt, use of the term ‘law’ will arouse skepticism and refusal in linguists’ ears and hearts.[1] In a way, this is no wonder, since the term ‘law’ has a specific connotation in the linguistic tradition (cf. Kovács 1971, Collinge 1985): basically, this tradition refers to 19th century studies of sound laws, attempts to describe sound changes in the history of (a) language.

In the beginnings of this tradition, predominantly in the Neogrammarian approach to Indo-European language history, these laws – though of descriptive rather than explanative nature – allowed no exceptions to the rules, and they were indeed understood as deterministic laws. It goes without saying that up to that time, determinism in nature had hardly ever been called into question, and the formation of the concept of ‘law’ still stood in the tradition of Newtonian classical physics, even in Darwin’s time, he himself largely ignoring probability as an important category in science.

The term ‘sound law’, or ‘phonetic law’ [Lautgesetz] had been originally coined as a technical term by German linguist Franz Bopp (1791–1867) in the 1820s. Interestingly enough, his view on language included a natural-scientific perspective, understanding language as an organic physical body [organischer Naturkörper]. At this stage, the phonetic law was not considered to be a law of nature [Naturgesetz], as yet; rather, we are concerned with metaphorical comparisons, which nonetheless signify a clear tendency towards scientific exactness in linguistics. The first militant “naturalist-linguist” was August Schleicher (1821–1868). Deeply influenced by evolutionary theorists, mainly Charles Darwin and Ernst Häckel, he understood languages to be a ‘product of nature’ in the strict sense of this word, i.e., as a ‘natural organism’ [Naturorganismus] which, according to his opinion, came into being and developed according to specific laws, as he claimed in the 1860s. Consequently, for Schleicher, the science of language must be a natural science, and its method must by and large be the same as that of the other natural sciences. Many a scholar in the second half of the 19th century would elaborate on these ideas: if linguistics belonged to the natural sciences, or at least worked with equivalent methods, then linguistic laws should be identical with the natural laws. Natural laws, however, were considered mechanistic and deterministic, and partly continue to be even today. Consequently, in the mid-1870s, scholars such as August Leskien (1840–1916), Hermann Osthoff (1847–1909), and Karl Brugmann (1849–1919) repeatedly emphasized the sound laws they studied to be exceptionless. Every scholar admitting exceptions was condemned to be addicted to subjectivism and arbitrariness. The rigor of these claims began to be heavily discussed from the 1880s on, mainly by scholars such as Berthold G.G. Delbrück (1842–1922), Mikołai Kruszewski

[1] Quite characteristically, Collinge (1985), for example, though listing some dozens of *Laws of Indo-European*, avoids the discussion of what ‘law’ actually means; for him, these “are issues better left to philosophers of language history” (ibd., 1).

(1851–87), and Hugo Schuchardt (1842–1927). Now, 'laws' first began to be distinguished from 'regularities' (the latter even being sub-divided into 'absolute' and 'relative' regularities), and they were soon reduced to analogies or uniformities [Gleichmäßigkeiten]. Finally, it was generally doubted whether the term 'law' is applicable to language; specifically, linguistic laws were refuted as natural laws, allegedly having no similarity at all with chemical or physical laws.

If irregularities were observed, linguists would attempt to find a "regulation for the irregularity", as linguist Karl A. Verner (1846–96) put it in 1876. Curiously enough, this was almost the very same year that Austrian physicist Ludwig Boltzmann (1844–1906) re-defined one of the established natural laws, the second law of thermodynamics, in terms of probability.

As will be remembered, the first law of thermodynamics implies the statement that the energy of a given system remains constant without external influence. No claim is made as to the question, which of various possible states, all having the same energy, is at stake, i.e. which of them is the most probable one. As to this point, the term 'entropy' had been introduced as a specific measure of systemic disorder, and the claim was that entropy cannot decrease in case processes taking place in closed systems. Now, Boltzmann's statistical re-definition of the concept of entropy implies the postulate that entropy is, after all, a function of a system's state. In fact, this idea may be regarded to be the foundation of statistical mechanics, as it was later called, describing thermodynamic systems by reference to the statistical behavior of their constituents.

What Boltzmann thus succeeded to do was in fact not less than deliver proof that the second law of thermodynamics is not a natural law in the deterministic understanding of the term, as was believed in his time, and is still often mistakenly believed, even today. Ultimately, the notion of 'law' thus generally was supplied with a completely different meaning: it was no longer to be understood as a deterministic law, allowing for no exceptions for individual singularities; rather, the behavior of some totality was to be described in terms of statistical probability. In fact, Boltzmann's ideas were so radically innovative and important that almost half a century later, in the 1920s, physicist Erwin Schrödinger (1922) would raise the question, whether not all natural laws might generally be statistical in nature. In fact, this question is of utmost relevance in theoretical physics, still today (or, perhaps, more than ever before). John Archibald Wheeler (1994: 293) for example, a leading researcher in the development of general relativity and quantum gravity, recently suspected, "that every law of physics, pushed to the extreme, will be found to be statistical and approximate, not mathematically perfect and precise."

However, the statistical or probabilistic re-definition of 'law' escaped attention of linguists of that time. And, generally speaking, one may say it remained unnoticed till today, which explains the aversion of linguists to the concept of law, at the end of the 19th century as well as today... Historically speaking, this aversion has been supported by the spirit of the time, when scholars like Dilthey (1883: 27) established the hermeneutic tradition in the humanities and declared singularities and individualities of socio-historical reality to be the objective of the humanities. It was the time when 'nature itself', as a research object, was opposed to 'nature *ad hominem*', when 'explanation' was increasingly juxtaposed to 'interpretation', and when "nomothetic law sciences" [nomothetische Gesetzeswissenschaften] were distinguished from "idiographic event sciences" [idiographische Ereigniswissenschaften], as Neokantian scholars such as Heinrich Windelband and Wilhelm Rickert put it in the 1890s. Ultimately, this would result in what Snow should term the distinction of *Two Cultures*, in the 1960s – a myth strategically upheld even today. This myth is well prone to perpetuating the overall skepticism as to mathematical methods in the field of the humanities. Mathematics, in this context, tends to be discarded since it allegedly neglects the individuality of the object under study. However, mathematics can never be a substitute for theory, it can only be a tool for theory construction (Bunge 1967: 467).

Ultimately, in science as well as in everyday life, any conclusion as to the question, whether observed or assumed differences, relations, or changes are essential, are merely chance or not, must involve a decision. In everyday life, this decision may remain a matter of individual choice; in science, however, it should obey conventional rules. More often than not, in the realm of the humanities, the empirical test of a given hypothesis has been replaced by the acceptance of the scientific community; this is only possible, of course, because, more often than not, we are concerned with specific hypotheses, as compared to the above Figure 1.1, i.e., with plausible hypotheses.

As soon as we are concerned with empirical tests of a hypothesis, we face the moment where statistics necessarily comes into play: after all, for more than two hundred years, chance has been statistically "tamed" and (re-)defined in terms of probability. Actually, this is the reason why mathematics in general, and particularly statistics as a special field of it, is so essential to science: ultimately, the crucial function of mathematics in science is its role in the expression of scientific models. Observing and collecting measurements, as well as hypothesizing and predicting, typically require mathematical models.

In this context, it is important to note that the formation of a theory is not identical to the simple transformation of intuitive assumptions into the language of formal logic or mathematics; not each attempt to describe (!) particular phenomena by recourse to mathematics or statistics, is building a theory, at least not in the understanding of this term as outlined above. Rather, it is important that there be a model which allows for formulating the statistical hypotheses in terms of probabilities.

At this moment, human sciences in general, and linguistics in particular, tend to bring forth a number of objections, which should be discussed here in brief (cf. Altmann 1985: 5ff.):

a. The most frequent objection is: "We are concerned not with quantities, but with qualities." – The simple answer would be that there is a profound epistemological error behind this 'objection', which ultimately is of ontological nature: actually, neither qualities nor quantities are inherent in an object itself; rather they are part of the concepts with which we interpret nature, language, etc.
b. A second well-known objection says: "Not everything in nature, language, etc. can be submitted to quantification." – Again, the answer is trivial, since it is not language, nature, etc., which is quantified, but our concepts of them.

In principle, there are therefore no obstacles to formulate statistical hypotheses concerning language in order to arrive at an explanatory model of it; the transformation into statistical meta-language does not depend so much on the object, as on the status of the concrete discipline, or the individual scholar's education (cf. Bunge 1967: 469).

A science of language, understood in the manner outlined above, must therefore be based on statistical hypotheses and theorems, leading to a complete set of laws and/or law-like regularities, ultimately being described and/or explained by a theory. Thus, although linguistics, text scholarship, etc., in the course of their development, have developed specific approaches, measures, and methods, the application of statistical testing procedures must correspond to the following general schema (cf. Altmann 1973: 218ff.):

1. The formulation of a linguistic hypothesis, usually of qualitative kind.
2. The linguistic hypothesis must be translated into the language of statistics; qualitative concepts contained in the hypothesis must be transformed into quantitative ones, so that the statistical models can be applied to them. This may lead to a re-formulation of the hypothesis itself, which must have the form of a statistical hypotheses. Furthermore, a mathematical model must be chosen which allows the probability to be calculated with which the hypothesis may be valid with regard to the data under study.
3. Data have to be collected, prepared, evaluated, and calculated according to the model chosen. (It goes without saying that, in practice, data may stand at the beginning of research – but this should not prevent anyone from going "back" to step one within the course of scientific research.)
4. The result obtained is represented by one or more digits, by a particular function, or the like. Its statistical evaluation leads to an acceptance or refusal of the hypothesis, and to a statement as to the significance of the results.

Ultimately, this decision is not given a priori in the data, but the result of disciplinary conventions.

5. The result must be linguistically interpreted, i.e., re-translated into the linguistic (meta-)language; conclusions must be linguistically drawn, which are based on the confirmed or rejected hypothesis.

Now what does it mean, concretely, if one wants to construct a theory of language in the scientific understanding of this term? According to Altmann (1978: 5), designing a theory of language must start as follows:

> When constructing a theory of language we proceed on the basic assumption that language is a self-regulating system all of whose entities and properties are brought into line with one another in some way or other.

From this perspective, general systems theory and synergetics provide a general framework for a science of language; the statistical formulation of the theoretical model thus can be regarded to represent a meta-linguistic interface to other branches of sciences. As a consequence, language is by no means understood as a natural product in the 19th century understanding of this term; neither is it understood as something extraordinary within culture. Most reasonably, language lends itself to being seen as a specific cultural sign system. Culture, in turn, offers itself to be interpreted in the framework of an evolutionary theory of cognition, or of evolutionary cultural semiotics, respectively. Culture thus is defined as the cognitive and semiotic device for the adaption of human beings to nature. In this sense, culture is a continuation of nature on the one hand, and simultaneously a reflection of nature on the other – consequently, culture stands in an isologic relation to nature, and it can be studied as such.

Therefore culture, understood as the functional correlation of sign systems, must not be seen in ontological opposition to nature: after all, we know at least since Heisenberg's times, that nature cannot be directly observed as a scientific object, but only by way of our culturally biased models and perspectives. Both 'culture' and 'nature' thus turn out to be two specific cultural constructs. One consequence of this view is that the definitions of 'culture' and 'nature' necessarily are subject to historical changes; another consequence is that there can only be a unique theory of 'culture' and 'nature', if one accepts the assumptions above. As Koch (1986: 161) phrases it: " 'Nature' can only be understood via 'Culture'; and 'Culture' can only be comprehended via 'Nature'."

Thus language, as one special case of cultural sign systems, is not – and definitely not *per se*, and not *a priori* – understood as an abstract system of rules or representations. Primarily, language is understood as a sign system serving as a vehicle of cognition and communication. Based on the further assumption that communicative processes are characterized by some kind of economy between the participants, language, regarded as an abstract sign system, is understood as the economic result of communicative processes.

Talking about economy of communication, or of language, any exclusive focus on the production aspect must result in deceptive illusions, since due attention has to be paid to the overall complexity of communicative processes: In any individual speech act, the producer's creativity, his or her principally unlimited freedom to produce whatever s/he wants in whatever form s/he wants, is controlled by the recipient's limited capacities to follow the producer in what s/he is trying to communicate. Any producer being interested in remaining understood (even in the most extreme forms of avantgarde poetry), consequently has to take into consideration the recipient's limitations, and s/he has to make concessions with regard to the recipient.

As a result, a communicative act involves a circular process, providing something like an economic equilibrium between producer's and recipient's interests, which by no means must be a symmetric balance. Rather, we are concerned with a permanent process of mutual adaptation, and of a specific interrelation of (partly contradictory) forces at work, leading to a specific dynamics of antagonistic interest forces in communicative processes. Communicative acts, as well as the sign system serving communication, thus represent something like a dynamic equilibrium.

In principle, this view has been delineated by G.K. Zipf as early as in the 1930s and 40s (cf. Zipf 1949). Today, Zipf is mostly known for his frequency studies, mainly on the word level; however, his ideas have been applied to many other levels of language too, and have been successfully transferred to other disciplines as well.

Most importantly, his ideas as to word length and word frequency have been integrated into a synergetic concept of language, as envisioned by Altmann (1978: 5), and as outlined by Köhler (1985) and Köhler/Altmann (1986). It would be going too far to discuss the relevant ideas in detail here; still, the basic implications of this approach should be presented in order to show that the focus on word length chosen in this book is far from accidental.

Word Length in a Synergetic Context

Word length is, of course, only one linguistic trait of texts, among others. In this sense, word length studies cannot be but a modest *contribution to* an overall *science of language*. However, a focus on the word is not accidental, and the linguistic unit of the word itself is far from trivial.

Rather, word length is an important factor in a synergetic approach to language and text, and it is by no means an isolated linguistic phenomenon within the structure of language. Given one accepts the distinction of linguistic levels, such as (1) phoneme/grapheme, (2) syllable/morpheme, (3) word/lexeme, (4) clause, and (5) sentence, structurally speaking, the word turns out to be hierarchically located in the center of linguistic units: it is formed by lower-level units, and itself is part of the higher-level units. The question here cannot be, of course, in how far each of the units mentioned are equally adequate for linguistic models, in how far their definitions should be modified, or in how far there may be further levels, particularly with regard to specific text types (such as poems, for example, where verses and stanzas may be more suitable units).

At closer inspection (cf. Table 1.1), at least the first three levels are concerned with recurrent units. Consequently, on each of these levels, the re-occurrence of units results in particular frequencies, which may be modelled with recourse to specific frequency distribution models. To give but one example, the famous Zipf-Mandelbrot distribution has become a generally accepted model for word frequencies. Models for letter and phoneme frequencies have recently been discussed in detail. It turns out that the Zipf-Mandelbrot distribution is no adequate model, on this linguistic level (cf. Grzybek/Kelih/Altmann 2004). Yet, grapheme and phoneme frequencies seem to display a similar ranking behavior, which, in both cases depends on the relevant inventory sizes and the resulting frequencies with which the relevant units are realized in a given text (Grzybek/Kelih/Altmann 2005).

Moreover, the units of all levels are characterized by length; and again, the length of the units on one level is directly interrelated with those of the neighboring levels, and, probably, indirectly with those of all others. This is where Menzerath's law comes into play (cf. Altmann 1980, Altmann/Schwibbe 1989), and Arens's law as a special case of it (cf. Altmann 1983).

Finally, systematic dependencies cannot only be observed on the level of length; rather, each of the length categories displays regularities in its own right. Thus, particular frequency length distributions may be modelled on all levels distinguished.

Table 1.1, illustrating the basic interrelations, may be, *cum grano salis*, regarded to represent something like the synergetics of linguistics in a nutshell.

Table 1.1: Word Length in a Synergetic Circuit

	SENTENCE	Length	Frequency
		↕	
	CLAUSE	Length	Frequency
↱		↰ ↕	
Frequency	WORD / LEXEME	Length	Frequency
↕ ↱		↰ ↕	
Frequency	SYLLABLE / MORPHEME	Length	Frequency
↕ ↱		↰ ↕	
Frequency	PHONEME / GRAPHEME	Length	Frequency

Much progress has been made in recent years, regarding all the issues mentioned above; and many questions have been answered. Yet, many a problem still begs a solution; in fact, even many a question remains to be asked, at least in a systematic way. Thus, the descriptive apparatus has been excellently developed by structuralist linguistics; yet, structuralism has never made the decisive next step, and has never asked the crucial question as to explanatory models. Also, the methodological apparatus for hypothesis testing has been elaborated, along with the formation of a great amount of valuable hypotheses.

Still, much work remains to be done. From one perspective, this work may be regarded as some kind of "refinement" of existing insight, as some kind of detail analysis of boundary conditions, etc. From another perspective, this work will throw us back to the very basics of empirical study. Last but not least, the quality of scientific research depends on the quality of the questions asked, and any modification of the question, or of the basic definitions, will lead to different results.

As long as we do not know, for example, what a word is, i.e., how to define a word, we must test the consequences of different definitions: do we obtain identical, or similar, or different results, when defining a word as a graphemic, an orthographic, a phonetic, phonological, a morphological, a syntactic, a psychological, or other kind of unit? And how, or in how far, do the results change – and if so, do they systematically change? – depending on the decision, in which units a word is measured: in the number of letters, or graphemes, or of sounds, phones, phonemes, of morphs, morphemes, of syllables, or other units? These questions have never been systematically studied, and it is a problem *sui generis*, to ask for regularities (such as frequency distributions) on each of the levels mentioned. But ultimately, these questions concern only the first degree of uncertainty, involving the qualitative decision as to the measuring units: given, we clearly distinguish these factors, and study them systematically, the next questions concern the quality of our data material: will the results be the same, and how, or in how far, will they (systematically?) change, depending on the decision as to whether we submit individual texts, text segments, text mixtures, whole corpora, or dictionary material to our analyses? At this point, the important distinction of types and tokens comes into play, and again the question must be, how, or in how far, the results depend upon a decision as to this point.

Thus far, only language-intrinsic factors have been named, which possibly influence word length; and this enumeration is not even complete; other factors as the phoneme inventory size, the position in the sentence, the existence of suprasegmentals, etc., may come into play, as well. And, finally, word length does of course not only depend on language-intrinsic factors, according to the synergetic schema represented in Table 1.1. There is also abundant evidence that external factors may strongly influence word length, and word length frequency distributions, factors such as authorship, text type, or the linguo-historical period when the text was produced.

More questions than answers, it seems. And this may well be the case. Asking a question is a linguistic process; asking a scientific question, is a also linguistic process, – and a scientific process at the same time. The crucial point, thus, is that if one wants to arrive at a science of language, one must ask questions in such a way that they can be answered in the language of science.

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