

On Coseriu's legacy**

Abstract

This article examines Eugenio Coseriu's overall theory of language and linguistics, based on his 1974 book *Synchronie, Diachronie und Geschichte* (originally in Spanish, in 1958). The topics that are singled out for analysis include the role of consciousness and the nature of functional explanation. Coseriu's ideas are shown to have strong affinities with those of Hermann Paul and William Dwight Whitney. His humanistic conception of linguistics is vindicated vis-à-vis its rivals, i.e. physicalism and Darwinism.

Dieser Aufsatz beschäftigt sich mit Eugenio Coserius umfassender Sprachtheorie, wie sie in dem 1974 auf deutsch veröffentlichten Buch *Synchronie, Diachronie und Geschichte* (spanisches Original 1958) dargelegt wird. Die für die Analyse ausgesuchten Themen umfassen u.a. die Frage der Bewusstheit sprachlichen Wandels und die Frage der Natur funktionaler Erklärungen. Es wird die Nähe von Coserius Auffassung zu der von Hermann Paul oder Whitney aufgezeigt sowie die Ablehnung von Physikalismus und Darwinismus.

Keywords

Eugenio Coseriu, Functionalism, Theory of language change, diachronic linguistics.

1. Introduction

Eugenio Coseriu's numerous publications reflect the wide range of his interests. His 1974 book *Synchronie, Diachronie und Geschichte* (originally published in Spanish, in 1958) perhaps contains the most extensive and most explicit statement of his overall theory. This is why I shall concentrate on this book in what follows. The purpose of my article is not just exegetical. Instead, I try to single out such 'Coserian' themes as are of continuing importance to theoretical linguistics and deserve to be further developed, if possible. In writing this article, I have greatly benefitted from López Serena (2009). Anttila (1989 [1972]) has had a lasting influence on my conception of diachronic linguistics.

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2. Consciousness

What do speakers consciously know (in the sense of 'knowing-that') about their language? One's philosophical position as far as linguistics is concerned becomes evident from how one chooses to answer this essentially simple question. The answer given by a consistent physicalist is 'Nothing' (cf. Jackendoff 2002: 28). But this makes the physicalist guilty of a contradiction because his/her data typically consists of his/her own conscious judgments of the following kind: "Sentence S is grammatical (or ungrammatical)". Therefore physicalism immediately disqualifies itself as a serious candidate for being an adequate philosophy of linguistics (cf. Itkonen 2008a: 16).

Next, I shall examine the attitudes that a few eminent non-physicalists have adopted vis-à-vis the issue of consciousness. In particular, the contributions by Hermann Paul, Ferdinand de Saussure, and Eugenio Coseriu will be singled out in this section and the next one.

For Paul (1975 [1880]), grammatical description applies, in principle, to the set of all the utterances that, having been produced in the social interaction between the speaker-hearers, constitutes a state of language (*Sprachzustand*), or the linguistic usage (*Sprachusus*), of a given community (p. 24). Now, the speech of different persons is characterized by a high degree of uniformity (p. 19). It follows that, in practice, the linguist may confine his/her attention to the speech of just a few persons, or even of a single person (p. 29). This person must be the linguist him-/herself because the psychic aspect of speaking – just like that of any other activity – is directly accessible only to (conscious) **self-observation** (*Selbstbeobachtung*) (p. 30). Thus, the primary object of grammatical description turns out to be the linguist's own knowledge of language (*Sprachgefühl*), with the understanding that what is true of the linguist is (likely to be) true of other speakers as well. Of course, the linguist is duty-bound to extend this data base the best (s)he can.

But this is not the whole story. Any state of language is produced by a psychological system (*Sprachorganismus*), which Paul conceives of as a vast network of associations of ideas (*Vorstellungen*), of varying strength, about forms and/or meanings. This system belongs to the **unconscious** mind (= "*er ist ja etwas unbewusst in der Seele Ruhendes*"), and therefore its character can only be conjectured about. The unconscious categories are emphatically asserted **not** to coincide with those arrived at by means of "grammatical reflection" (pp. 27–31): "*Unser grammatisches System ist lange nicht fein genug gegliedert, um der Gliederung der psychologischen Gruppen adequat sein zu können.*" Here Paul gives an exact formulation to the problem of 'psychological reality' that was raised in the 1970's and continues to be debated even today (cf. Itkonen 2005: 44–52). Finally, Paul takes it for granted that, for ordinary speakers, **linguistic change** lies below the level of consciousness (pp. 50–51).

With de Saussure, things are less clear. To be sure, his point of departure is the same as Paul's: the linguist has to "enter the consciousness of the speakers" (de Saussure 1962 [1916]: 117), which means that the subject matter of (synchronic) grammatical description is constituted by what the speakers consciously know about their own language: "*pour savoir dans quelle mesure une chose est une réalité, il faudra et il suffira de rechercher dans quelle mesure elle existe pour la **conscience** des sujets*" (p. 128; emphasis added). On the other hand, speakers are supposed to be "largely unconscious of the laws of language" (p. 106). In particular, linguistic change is claimed to be non-intentional (p. 122) and to operate in an unconscious manner (p. 127, 227). But this thesis is contradicted, at least on the face of it, by the claim that **analogy**, described as the prime determinant of change, presupposes the

consciousness of a relationship between (typically three) forms: “*elle suppose la conscience et la compréhension d'un rapport unissant les formes entre elles*” (p. 226; emphasis added).

Compared with Paul and de Saussure, Coseriu turns out to be an unequivocal advocate of consciousness and, by the same token, the anti-physicalist *par excellence*. He categorically rejects the view that speakers are not conscious of the norms of their language and argues that, instead, every speaker possesses “a clear and certain knowledge” (“*ein klares und sicheres Wissen*”, pp. 49–50). In express opposition to de Saussure, he even claims that “speakers are fully conscious of the linguistic system and of the so-called laws of language” (p. 51). I shall explore the import of these statements in the next section (see also Sect. 11).¹

3. Different Types of Conscious Knowledge

Conscious knowledge is not a monolithic concept. Rather, it turns out to encompass several distinct, even though related, viewpoints.

A) Pretheoretical vs. Theoretical Knowledge

Having distinguished “grammatical reflection” from psycholinguistic experimentation, Paul seems to imply that the former gradually ‘grows out’ of that type of self-observation whose object is one’s own *Sprachgefühl* (pp. 30–31). This might be called the ‘ascent’ from **pretheoretical** to theoretical knowledge. De Saussure has to deal with the same problem when discussing the (ontological) status of grammatical categories: on the one hand, they do exist in the language as “abstract entities”; on the other, “their study is made difficult by the fact that one never knows whether the [pretheoretical] consciousness of speakers goes as far as the [theoretical] analysis by the grammarian” (p. 190).

On this issue Coseriu is more explicit than Paul or de Saussure. He notes (p. 45) that even if one must start from the knowledge that the speaker has of his/her language, the (pretheoretical) level of speaking must not be confused with the (theoretical) level of linguistics. (The German terminology makes the nature of this opposition more evident: *Sprechen* vs. *Sprachwissenschaft*.) It follows that the number of distinctions postulated by the grammarian necessarily exceeds the number of distinctions known to ordinary speakers (*ibidem*). At first, this seems to be at odds with the previously quoted statement that speakers are “fully conscious of the linguistic system” (p. 51). However, the ‘pretheoretical vs. theoretical’ distinction is accounted for by means of the additional remark that the consciousness of ordinary speakers concerns the **use** (*Anwendung*) of the “linguistic instrument”, whereas its **understanding** (*Verstehen*) is the prerogative of the linguist.²

¹ According to one of the dogmas of analytical philosophy, there can be no ‘foundational’ knowledge but everything is open to doubt and/or revision. Interestingly, this view is plainly, and even trivially, wrong, given the certainty with which we do know our own language, in particular the meanings of the words and sentences we utter: “I should stand before an abyss if I wanted so much as to try doubting their meanings” (Wittgenstein 1969: § 370; cf. note 2; for discussion, see Itkonen 2003: Chapter 5 [= on certainty], 2008c). – Today Jordan Zlatev (e.g. 2008) emphasizes the importance of the role that consciousness plays in linguistics in a way strongly reminiscent of Coseriu.

² The distinction between ‘pretheoretical knowledge’ (also called ‘atheoretical knowledge’) and ‘theoretical knowledge’ has been central to my own language-conception ever since my 1974 dissertation, with the understanding that the core area of pretheoretical knowledge is **certain** whereas theoretical knowledge (or rather, belief) is always uncertain or hypothetical (cf. note 1). Thus, referring to Coseriu’s view that speakers have “clear and certain knowledge”, I once stated that “Coseriu is one of the few linguists who would agree with what I have been saying here” (Itkonen 1978: 150).

B) Pre-Empirical vs. Empirical Knowledge

Coseriu correctly notes (p. 155) that every human science, not just linguistics, is based on the “original knowledge” (*ursprüngliches Wissen*) that people have of themselves. But it is precisely for this reason that ‘pretheoretical’, as a characterization of knowledge, remains rather vague. Coseriu calls it “foreknowledge” (*Vorwissen*) (p. 42). On the most obvious interpretation, this means knowledge that precedes theoretical knowledge (or belief) (cf. Subsection A). But there is also a less obvious, and therefore more interesting, interpretation: “Before undertaking an empirical investigation of language, one has to know **what language is**” (p. 165; original emphasis). This type of “eidetic” knowledge is primary vis-à-vis, because constitutive of, empirical phenomena. For instance, it is pointless to try to accumulate as many nouns as possible unless one **already**³ has a concept of ‘noun’ (p. 167). Or, to use one of my own examples, it is impossible to embark upon an experimental investigation of how relative clauses are produced and understood in English, if one has no idea of what English relative clauses are like. In brief, grammatical analysis investigates the **precondition** of that which is investigated by different varieties of empirical linguistics: the former deals with those concepts (or norms) whose spatiotemporal exemplifications are investigated by the latter. The latter type of study makes use of **observation** and/or **experimentation**, whereas the former, being of **pre-empirical** character, has to rely on **intuition**. It is this distinction which justifies Zlatev’s (2010) claim that grammatical analysis falls within the purview of **phenomenology**.⁴ Personally, I prefer the **hermeneutic** point of view, as formulated e.g. by Apel (1981), but I admit that I can offer no cogent argument in support of this preference. It seems to be just a matter of (philosophical) taste.

Hence, the ‘ascent’ from pretheoretical to theoretical has to be combined with the ‘descent’ from pre-empirical (or conceptual) to empirical. Applying the theoretical point of view to pre-empirical knowledge (as in grammatical analysis) cannot of course change its fundamental nature. The resulting description remains non-empirical.

In a technical sense, pre-empirical (or conceptual) knowledge is *a priori* vis-à-vis empirical knowledge. In Kant’s philosophy, space and time are the *a priori* framework of human perception and thought, which entails that it is impossible to know ‘things-in-themselves’ as they are ‘behind’ this framework or, alternatively, ‘before’ it was established.⁵ In linguistics, pre-empirical knowledge equals knowledge of those norms which spatiotemporal utterances conform to (and occasionally deviate from). These norms must of course be learned, i.e. there is always a considerable time interval before they come to be established. It follows that here we have, in a sense, a situation where it **is**, after all, possible first to perceive the ‘thing-it-self’ in its original condition (i.e. meaningless sound) and then to follow its gradual disappearance behind the ‘veil’ of the *a priori* framework (which is here constituted by the norms of the language in question). In the case of second-language acquisition, moreover, this process can be observed **consciously**.

³ It is a different matter that the concept of ‘noun’ remains constantly open to revision in light of new (typological) evidence.

⁴ This also seems to capture Coseriu’s intention, as indicated by his use of the word ‘eidetic’. (In this context the closest translation of *eidōs* is ‘essence’.) Surprisingly, it is only in a footnote (p. 73) that he makes the methodologically crucial remark that phonology necessitates the use of Husserl-type phenomenological analysis; and – we may add – what is true of phonology, must be true of grammatical description in general. (This happens to be the only reference to Husserl in this book.) It is only logical that a phenomenological Coseriu-interpretation should be advanced by Zlatev (*in press*).

Of course, this metaphor is defective in many ways. But first, it has to be. And second, it is one of the central lessons of Hindu philosophy that what cannot be expressed directly, can be expressed, if at all, only through metaphor: “treading the path of the unreal, one attains reality” (Itkonen 1991: 80).⁶

4. Saussurean Dichotomies

Much of Coseriu (1974 [1958]) can be seen as a running commentary on (or rather, dialogue with) de Saussure. Only the most general aspects of some of the principal issues will be touched upon here.

A) Langue vs. Parole

It is customary to credit de Saussure with having discovered the *langue* vs. *parole* distinction. This is a serious mistake because, as Coseriu notes (p. 17), some such distinction has always been taken for granted. For instance, Hermann Paul and Georg von der Gabelentz already made use of it (cf. Itkonen 2011). Coseriu further points out that de Saussure actually committed the mistake of opposing *langue* to *parole* and endorsing the primacy of the former. Hjelmslev went one step farther, claiming that *langue* could exist even without *parole*, a view that Coseriu finds “incomprehensible” (p. 47; also pp. 229–234).⁷

In the first half of the 20th century some linguists, influenced by idealist philosophy, committed a mistake opposite to de Saussure's, repudiating *langue* and extolling the inexhaustible richness of *parole* (pp. 42–46). Today the same anti-theoretical view is being propagated by representatives of conversation analysis. Coseriu offers a much-needed antidote, noting (p. 18) that every synchronic grammar that has ever been composed presupposes the existence of *langue*. More recently, the same, or a similar, point has been made by Givón (1995), who emphasizes the importance of “taking structure seriously”, in opposition to claims that structure is non-existent or, at most, “emergent”.

On reflection, it is clear that the two viewpoints have to be integrated, as in Hegel's formulation that “language (*Sprache*) is the system of speech (*Rede*)” (p. 17). Rejecting this view commits one to claiming, unreasonably, that speech is (entirely) chaotic.

B) Synchrony vs. Diachrony

“From the ontological point of view diachrony is primary whereas from the methodological point of view synchrony is primary” (Itkonen 2010). This ‘biperspectivist’ formulation

⁵ Exactly the same idea is expressed in Bhartrhari's (400 AD) philosophy: space and time are forms of human ignorance (*avidyā*) (cf. Itkonen 1991: 79–83).

⁶ One more distinction that is relevant in this context is that between ‘observer's knowledge’ and ‘agent's knowledge’ (cf. Itkonen 2003: Ch. 10, 2008b: Ch. 11). Coseriu seems at least to hint at it when he notes (p. 49) that linguistic knowledge is a type of *Machen-Können* (“ability to do”).

⁷ I spent the summer semester 1965 in Tübingen on a grant from DAAD (= *Deutscher Akademischer Austauschdienst*) and devoted a great deal of time and energy to the study of Hjelmslev (1963 [1943]). (Logically enough, my first article, published in 1968, was *Zur Charakterisierung der Glossematik*.) I also followed Coseriu's course on the history of the Romance languages. This course was once interrupted when Coseriu, in a solemn mood, gave a memorial talk for Hjelmslev, who had just died. Clearly, in Coseriu's opinion, whatever shortcomings Hjelmslev's glossematics may have had, were vastly outweighed by its merits.

captures e.g. Paul's intent exactly. Coseriu too uses it when he criticizes de Saussure for having endorsed the primacy of synchrony: "What de Saussure was doing was not ontology but methodology" (p. 21; similarly pp. 9–10, 42, 224). The same idea concerning the two-faced nature of language has been succinctly formulated by Hugo Schuchardt, quoted by Coseriu on p. 10: "*nur die Bewegung ist wirklich, nur die Ruhe ist wahrnehmbar*" ("only the motion is real, but only the state of rest is [directly] perceived"). By contrast, de Saussure postulated the "curious equations *speech = diachrony* and *language = synchrony*" (p. 19).

5. Functional/Finalistic Explanation

Coseriu devotes a great deal of attention to the **explanation** of linguistic change. He most emphatically denies that changes could be brought about by that type of 'effective' causality which is characteristic of the natural sciences. Instead, he espouses **functional** explanation (where 'functional' is explicitly stipulated to be synonymous with 'finalistic', p. 179). According to this type of "explanation by expressive necessity" (*die Erklärung durch eine Ausdrucksnotwendigkeit*, p. 150) changes are brought about by **free will** operating to satisfy **expressive needs**. The most frequently used term is *Ausdrucksnotwendigkeit*, but also the following synonymous expressions are used: *Ausdruckserfordernis*, *Ausdrucksbedürfnis*, *Ausdrucksfinalität*, *Ausdruckszweck*, *kommunikativer Zweck*, *Mitteilungszweck* (= "expressive requirement/need/finality/purpose, communicative purpose [twice]").

The use of functional explanation is justified in a fully traditional way, namely by reference to Aristotle's 'four causes' (p. 178). When someone builds a house, the entire process can be analyzed into the following components: the materials that the builder uses (= 'material cause'); the idea or plan which the builder eventually realizes (= 'formal cause'); the actions which put the house together (= 'effective cause'); the purpose which the house is meant to serve or 'that for the sake of which' it is there (= 'final cause'). It is undeniable that in this scenario the other causes are subordinated to the final one. They lose their identity, if the purpose is extracted.

The preceding account seems *prima facie* to involve a contradiction insofar as changes are claimed to be both free and necessary. The traditional solution, given by Aristotle, was later summarized by Immanuel Kant, as follows: "Who wants the end, wants the means which is both necessary and at his disposal" (for discussion, see Itkonen 1983a: Ch. 2, esp. pp. 49–53). In other words, one is **free** to choose one's goal but, having chosen it, one **must** choose the means to achieve it. This is also asserted by Kant's most illustrious successor: "This pattern of 'beliefs' leading to necessary consequences in action is the basic structure of Hegel's notion of inevitability in history" (O'Brian 1975: 148–149).⁸

Three important qualifications, however, need to be added. First, free will (in the sense of the freedom to choose one's goals) has obvious limitations. In the 1930's it was impossible, even for the most strong-willed person in the world, to want to buy a personal computer.

Second, even in the properly limited sense of 'freedom', it is certainly not true that one is always free to choose one's goals. Under exceptionally constraining circumstances (e.g. whether or not to stay in a building that is about to be blown up), it is entirely predictable which goal (and which action) one will⁹ choose. Those who accept such extreme cases as

⁸ There is this additional twist that the beliefs that Hegel is concerned with are also **about** freedom.

⁹ In principle (but hardly in practice), one could – out of 'existentialist bravado' – stay and let oneself be killed.

prototypical may have been led to think – wrongly – that rational behavior is governed by deterministic laws of the same kind as those of Newtonian mechanics. But this is not true, in general. It must be clearly understood that functional explanations, as applied to human behavior (and as envisaged e.g. by Coseriu), exhibit **non-nomic** necessity. Deterministic laws, by contrast, exhibit, **nomic** necessity.

Third, assuming that a goal has been chosen (in whatever way), it is often, and perhaps even typically, the case that the agent-to-be has at his/her disposal not just one but **several** courses of action each of which, to the best of his/her knowledge, will achieve the goal at (roughly) the same 'cost'. Hence, none of them is necessary, and each is sufficient. Clearly, it is **this** situation which is, in general, characteristic of linguistic change. Therefore it is just **wrong** to claim, indiscriminately, that every (linguistic) change is necessary, and can only be explained as being such. This criticism applies e.g. to Coseriu's following explication of what is meant by "functional explanation", or explanation based on "functional necessity": "a new fact of language has come into being because a few, or several, speakers have realized that it satisfies a given expressive purpose" (p. 132).¹⁰ It is, or should be, self-evident that there is **no** (non-nomic) necessity involved here.

In brief, one should be realistic enough to admit the role that **chance** plays in linguistic change, or in social change in general: it just so happens that from among several alternatives this one is selected, and not that one. It follows that free will may occasionally become indistinguishable from chance (cf. Itkonen 1983a: 93). Whether or not this is indeed the case, cannot be decided *a priori* but must be assessed anew for each change that is taken under scrutiny.

The purport of the two preceding paragraphs may be summarized by noting that when the agent has several equally good alternatives at his/her disposal, his/her action is **unpredictable**, and would be such even if, before (s)he acts, we could read his/her mind like an open book (which we never can). To be sure, (un)predictability is relative to the level of abstraction. Normally it can be predicted with some assurance which **type** of action the agent will do in a given situation.

6) Functional Explanation Is Rational Explanation

When the meaning of Kant's dictum is spelled out, it turns out to assert that there are two, and only two, principal determinants of human action: the **goal** (= G) that the agent entertains and his/her **belief** (= B) about his/her own actions which, *qua* the means-to-the-end, are such as to bring about the goal. This explanatory schema may be summarized as **(G&B) ♦ A** = 'the goal-cum-belief causes the action'; but, of course, its actual structure is more complicated (cf. Itkonen 1983a: 49–54, 2003: 184–186, 2008b: 112–114). The resulting concept of explanation is called **rational explanation**. It explains not only actions which **are** rational, or means adequate to bring about the goal, but also irrational actions, or actions which (wrongly) **seem** to the agent to be rational. For instance, Durkheim's explanation of suicide can **only** be understood as a type of rational explanation, depicting the agent as facing a problem with no other solution: "[Durkheim's] notion of cause ... relies on reconstructing the world of the ill-integrated from within, so as to make suicide a (semi-)rational act" (Hollis 1977: 130; cf.

¹⁰ It could be objected that what is necessary is not the choice of any given action but the (more abstract) fact that **some** action must be chosen. But, quite clearly, this is not what Coseriu has in mind. What he is claiming is that each particular change that actually took place was (somehow) necessary.

Itkonen 1983a: 96–97). This point should not be very difficult to grasp but it has been (and probably continues to be) nearly universally misunderstood.¹¹

On the face of it, the explanatory schema given above can, and indeed should, be criticized for being individualistic, on the one hand, and atomistic, on the other. On both accounts, the remedy is the same, namely placing the goalscum-beliefs in their larger **social context**; and this is exactly what always happens in practice. Language is a social entity, and those goals and/or beliefs that speakers may variously come to entertain are conditioned by interaction both with other speakers and with the environment shared by all the speakers. It goes without saying that e.g. Durkheim-type explanations constantly refer to the society at large.

The terms 'functional/finalistic' and 'teleological' are often used synonymously, but Coseriu makes a clear-cut distinction between the two. For him, 'teleological explanation' entails the assumption of a pre-established and external goal towards which a 'system' has to move. Hence, teleology turns out to be the very opposite of free purposive action and, as Coseriu sees it (pp. 182–183, 196), quite incompatible with linguistic change.

In Itkonen (1982a [1978]: 89) I point out that my notions of 'short-term teleology' and 'long-term teleology' correspond to Coseriu's notions of functional explanation and teleological explanation, respectively, and it is in reference to the former that I make the following remark: "Coseriu (1974 [1958]) presents an account of linguistic change which is, in essence, closely similar to the one expounded in the present section" (p. 96).¹² Furthermore, I introduce **rational explanation** as a more specific explication of 'short-term teleology' (p. 94). When I later return to this notion in Itkonen (1981a, 1981b, 1984), I feel justified to emphasize even more strongly the similarity between Coseriu's position and my own: "The view of diachronic-linguistic explanation as a type of rational explanation was put forward by Coseriu (1958/1974, esp. p. 57, and 158–159)" (1981a: 81–82). "To my knowledge, the concept of rationality offers the only systematic basis for constructing a concept of non-nomic explanation. A similar conception of linguistic change as rational behavior has been defended in detail by Coseriu (1958), and long before him by Whitney (1979 [1875], esp. pp. 143–152)" (1981b: 695). "I have found much general support for my view that linguistic change is rational in the work of Eugenio Coseriu (see especially Coseriu 1958)" (1984: 208).¹³

Functional/rational explanation applies to the whole spectrum of human behavior, from predictable to unpredictable. Does this mean that it explains too much? No. Why not? Because this flexibility accurately reflects the fact that we are above all **purposive** beings, regardless of the variable influences that external circumstances may have on our behavior.

Finally, it may be added that the structure of a living organism is often said to be explained by its 'function'. For instance, the human heart is such as it is because it serves the function of maintaining the blood circulation. Woodfield (1976: 211–212) calls functions of

¹¹ My notion of rational explanation is identical with Woodfield's (1976: Chap. 10) 'teleological description' as applied to (human) actions (esp. p. 182).

¹² This paper is based on a talk that I gave at the Third International Conference on Historical Linguistics, in Hamburg, August 1977. Once the time allotted to discussion was over and the chairman was about to announce the lunch break, Coseriu, who had been sitting in the first row, jumped up and yelled (in German): "If the discussion is not allowed to continue, this conference serves no purpose at all!" The chairman, visibly disconcerted, had no choice but to comply.

¹³ How did Coseriu react to this declaration of a common agenda? See Sect. 16.

this type “natural or biological **ends**” and carefully distinguishes them from **goals**, *qua* “mental entities, living permanently within intentional brackets”.¹⁴

7. But Why Functional Explanations At All? Why Not Non-Functional Explanations?

Perhaps the majority of those who are today working in the field of diachronic/typological linguistics subscribe to one or another form of ‘functionalism’. However, they are **not** willing to endorse the full-fledged ‘humanistic’ version of functional/rational explanation that was outlined in Sections 5–6 above. In order to safeguard their scientific respectability, they would much prefer to have such explanations as conform to the canons of natural science. But this cannot be done, for reasons that will now become apparent.

A) There Are No Deterministic Explanations

It is a simple fact that no deterministically valid ‘laws of human behavior’ have ever been discovered.¹⁵ Ever since the beginning of the 19th century, historians have been looking for them, but without success:

All seemed ready, particularly in the nineteenth century, for the formulation of [a natural science of history]. ... The stage was set, but virtually nothing materialised. No general laws were formulated – not even moderately reliable maxims – from which historians could deduce (together with knowledge of the initial conditions) either what would happen next, or what had happened in the past. ... Neither sociologists nor psychologists ... had been able to create the new mechanism: the ‘nomothetic’ sciences [of history]... remained stillborn (Berlin 1980 [1960]: 110).

Because of its very nature, the subject matter of (historical) linguistics is more recurrent and hence more uniform than that of general historiography. Nevertheless, law-based explanations and predictions of particular facts are equally impossible in both disciplines:

All the laws that have been or will be proposed have a common **defect**: what they assert is only possible, not necessary. ... The laws of general-historical phonetics or morphology do not suffice to explain a single fact ... we are not able to predict a single future development ...” (Meillet 1921: 15–16; emphasis added).

Since then, Meillet’s complaint has often been repeated, and seldom as stridently as by Lass (1980, 1997). With an odd mixture of desperation and stoicism, Lass notes the lack of progress in “post-Neogrammarian historical linguistics”: “we still have no convincing explanations for change” (1997: 386–387).¹⁶ Clearly, Lass could not be “convinced” by

¹⁴ Woodfield adds the following clarification: “I agree that if a goal is achieved, we say that it exists or has been actualised. But this teetering from intensional to extensional usage is a loose *façon de parler*. Intentional objects can never break free from their shackles, for they can never become real objects. What is actualised, strictly speaking, is always some action or state of affairs that **matches** the goal by satisfying a goal-description” (p. 212).

¹⁵ In general, ‘nomic’ (= ‘nomological’, ‘nomothetic’) is equated with ‘deterministic’ by those who emphasize the need for nomic (= lawlike) explanations. Personally, I have nothing against the notion of **statistical** nomicity, as long as it is clearly distinguished from its deterministic counterpart (cf. Itkonen 1983a: 24–31).

¹⁶ Hockett (1979: xv), for instance, exemplifies the same attitude: “... and in reading Whitney I find the joy of communion with a kindred spirit across the gap of a century only slightly abated by chagrin that in so much time we should have accomplished so little” (Hockett 1979: xv). Yet this is nothing, compared with the startling fact that grammatical theory has actually **declined** after Panini’s (c. 400 BC) grammar. For

anything less than fullblown deterministic explanations. However, as Coseriu sensibly notes (pp. 201–203), there is not the slightest reason to believe that deterministic ‘laws of language’ exist, and every reason to believe that they do not. Whether or not (historical) linguistics is afflicted by some sort of ‘defect’ – as has been assumed by a series of outstanding scholars, from Meillet to Lass – it is not the lack of attested deterministic laws.

B) Statistical ‘Explanations’ Are Too Weak

Even if no (interesting) deterministic laws have been discovered, it is quite easy to state **statistical** correlations of varying strength in the linguistic data that has been accumulated over the years by diachronic and/or typological research. However, the **explanatory** value of such correlations remains problematic. Lass (1980), for instance, rejects them out of hand as being both non-causal and nonexplanatory. Clearly, different uses of statistics need to be distinguished here.

First, it is generally accepted that the laws of quantum physics are statistical in character, due to their ontology and not just to human ignorance. Nevertheless, Salmon (1984), for instance, has found it perfectly feasible to apply the twin notions of causality and explanation to quantum physics.

Second, mainstream sociology offers much the same lesson. Already Durkheim (1938 [1895]: 124), while insisting on the necessarily **statistical** nature of sociological data, claimed that the sole purpose of sociology is the discovery of social **causation**. The same is true, in particular, of (variationist) sociolinguistics. Itkonen (1983a: 2.2.4, 6.1) tries to show in what sense e.g. Labov-type variable rules can be conceived to exemplify the notions of statistical causality and explanation.¹⁷ Thus, when dealing with large quantities of (sociological or linguistic) data, it is impossible to focus on any given individual case, and the statistical approach becomes a necessity. Therefore, as far as explanation is concerned, there are only two alternatives: either to accept statistical explanation or to give up any attempt at explanation.

Third, the situation changes if it is possible for the sociologist or the linguist to examine one and the same phenomenon from **two** points of view simultaneously, i.e. both as an individual entity and as an ‘anonymous’ entity contained in a statistical correlation. It is **this** situation which is characteristic of diachronic and/or typological research; and, as shown by Greenberg-type correlations, it goes without saying that the ‘individual’ point of view is always **more explanatory** than the ‘statistical’ one.

Consider Greenberg's (1966: 110) second universal: “In languages with prepositions, the genitive almost always follows the governing noun, while in languages with postpositions it almost always precedes.” Finnish is a language which conforms to this (statistical) universal insofar as the genitive precedes the noun and postpositions (whose governing nouns happen to be in the genitive, for the most part) are much more frequent than prepositions (which do occur, however). But clearly, any grammar of Finnish **explains** the differential relations between Finnish case-inflected nouns and post-/prepositions much better than the mere statement of this general (approximate) correlation. Of course, it is **not** the purpose of a

aficionados of scientific progress this is bad enough. But it gets worse once it is realized that Panini composed his grammar without the help of literacy (cf. Itkonen 1991: Chap. 2; 2011: Sect. 1-E; also 1982b).
¹⁷ In keeping with his general position, Lass (1980: 28) characterizes variable rules as “exceedingly mysterious ... distinctly odd”.

statistical description to explain in its full individuality each and every phenomenon that it subsumes. But this is precisely the point (cf. Sect. 9).¹⁸

C) There are No Formal Explanations

Generativism has always denied any possibility of functional explanations. Optimality Theory continues this line of thinking, offering descriptions in terms of 'constraints' that are assumed to be **formal** in the sense of being innate 'design features' of language. Taken together, the constraints define a notion of 'grammatical optimality'. However, Haspelmath (1999) succeeds quite well in showing that grammatical optimality typically coincides with 'user optimality', which means that it has some sort of **functional** motivation. A whole spectrum of linguistic forms dealt with by Optimality Theory – phonological, morphological, and syntactic – are shown by Haspelmath to serve as the means to achieve such goals as 'saving production energy', 'avoiding articulatory difficulties', 'eliminating threats to comprehensibility', or 'avoiding ambiguity'. Thus, there are no genuinely formal explanations, or at least none have been discovered so far.

As Haspelmath sees it, all these functionally motivated forms have been shaped by **linguistic adaptation**. His next step is to establish an **analogy** between linguistic adaptation and biological adaptation. The number of publications with the same broadly Darwinist purpose has multiplied in recent years.

D) There Are No Darwinist Explanations

An analogy may be either illuminating or misleading (cf. Itkonen 2005: 176–190); and I maintain that the Darwinist analogy is likely to be misleading. Let us consider a typical 'functional' explanation given in the framework of evolutionary theory: The earlier white variant of butterflies died out in the new grey environment of a mining town because it was easily detected and then eaten by birds, whereas the grey mutation survived because it could not be detected. This account contains the two components of any evolutionary explanation, namely random **mutation** (from white to grey) and natural **selection** (carried out by birds-cum-environment). The grey colour of the new mutation turned out to be 'functional' for the survival in the new environment, hence the term 'functional explanation'.

Those who are anxious to apply Darwinism to linguistics point out that linguistic change seems to exemplify exactly the same bipartite structure: first **innovation** (corresponding to mutation) and then **acceptance** (corresponding to natural selection).

Let us see to what extent this analogy (or metaphor) is justified. To do so, let us consider a typical (small-scale) linguistic change, reproduced from Itkonen (1982a [1978]: 107). In Classical Latin the 2SG and 3SG copula forms were *es* and *est*. In Vulgar Latin they coalesced as *es* (or threatened to do so). In Spanish, for instance, the ability to express the meaning distinction between 2SG vs. 3SG was restored (or maintained) by creating a new formal distinction *eres* vs. *es*. **Why** did this happen? It is reasonable to infer, in hindsight, that the coalescence of *es* and *est* was experienced as a **problem** by the speakers, and creating a

¹⁸ Lessnoff (1974: 65) summarizes the situation as follows: "[S]ocial science theory is affected by what might be called **quasi free will**. ... Social science laws have the characteristics that would be the consequence of human free will, if it existed."

new formal distinction was the **solution** which they arrived at. The speakers of other Romance languages faced the same problem and chose slightly different solutions.

Coseriu uses the same terminology, noting that “linguistic freedom meets problems which it has to solve” (p. 169). Nowadays the problem-solving account has been widely accepted, e.g. by such eminent typologists as Tom Givón and Bernd Heine. Of course, it is just a reformulation of rational explanation: to solve the problem is the goal, and the actual solution is the means to achieve this goal. It goes without saying that problem-solving amounts to an **application of intelligence**.

Why is all this so important? Cohen (1986: 125) tells us why: “Hence no evolutionary change of any kind came about through the application of intelligence and knowledge to the solution of a problem. That was at the heart of Darwin’s idea. ... And that is why Darwinian evolution is so deeply inappropriate a model ... for the understanding of [linguistic change]”.¹⁹

It follows that if we accept the problem-solving interpretation of linguistic change, then the Darwinist interpretation is **false**. Moreover, those who accept both interpretations are guilty of **contradiction**.

The implications of what precedes are spelled out by Itkonen (1984) in uncharacteristically conciliatory terms:

Even if one attempt to adapt linguistics to evolutionary theory has failed, it is possible that other similar attempts will succeed; and I have no way of disproving this possibility. Indeed I do not wish to deny that there may be some deep-seated similarity that underlies both biological mutations and linguistic changes. Nevertheless, to me at least it seems undeniable that at present the link between biological and linguistic changes is **metaphorical**, whereas the link between linguistic and socio-psychological changes is so direct as to be almost one of identity (p. 209; original emphasis).²⁰

The last sentence (which may seem puzzling) refers, *inter alia*, to Smelser (1962) and Schelling (1978), i.e. to such types of (explanations of) sociopsychological changes as were the object of discussion at the time. It does **not** mean to assert that linguistic changes are brought about by some ‘unknown’ causal force, or a force which is neither psychological nor social.

Finally, let us mention one more **disanalogy** between biology and linguistics: mutation and selection have **different** sources whereas innovation and acceptance have the **same** source (= the speakers themselves) as well as a common motivation: one accepts only what one might have innovated oneself (cf. Coseriu, p. 131, Itkonen 1982a [1978]: 113).

8. Causality

On the one hand, causality is generally thought to operate **nominally**, i.e. in a law-like manner. On the other hand, human actions are generally thought to be **non-nomic**; at least, as

¹⁹ I have taken the liberty of replacing here “scientific progress” by “linguistic change”. Am I justified to do so? Certainly, because we have already agreed that both linguistic change and scientific change are problem-solving activities. This is, incidentally, what I asserted already in the title of my 1982b paper: “Change of language as a prototype for change of linguistics” (based on a talk given at the Fifth International Conference on Historical Linguistics, in Galway, April 1981).

²⁰ This paper is based on a talk I gave at a conference that was organized by Raimo Anttila and his group of students at UCLA, June 1982. Coseriu too participated and, according to Anttila, gave his first public talk in English. At an informal post-conference dinner, it so happened that, around midnight, Coseriu and I came into a heated argument, aroused not by any kind of professional disagreement, but rather by his evident reluctance to appreciate my irreverent sense of humor. Fortunately, this did not damage our relationship (cf. Sect. 16).

noted in Subsection 7-A, no universally valid 'laws' of human behavior have ever been discovered, and this fact agrees with the assumption of free will (regardless of whether this assumption is in fact true or not). It seems to follow that human actions have no causes. I for one cannot accept this conclusion. Under the normal circumstances, A would not have occurred if the relevant G&B had not been there. Therefore, I think it is quite natural to call G&B the 'cause' (or 'causes') of A. This is in keeping with the view that "intentional action is, on causal theory, defined by its causes [= beliefs and desires]" (Davidson 1973: 151). But this means that in connection with human actions we have to accept a concept of **non-nomic causality** (cf. Itkonen 1983a: 35–38).

This is also Coseriu's position. Identifying causality with effective or nomic causation, he typically declares language to be incompatible with causality: "Language does not belong to the **causal** domain but to the **finalistic** domain which is constituted by phenomena determined by their **function**" (p. 23). This statement is repeated on several occasions, and each time it is justified by referring to the fact that language is essentially **free creation**.

However, if the terminological criteria are relaxed, then Coseriu is quite willing to admit that linguistic changes are produced by "subjective" or "free" causality: "The only genuinely causal explanation of a new linguistic fact is that freedom has created it for a certain purpose" (p. 174; also p. 169, 193). Among his predecessors, William Dwight Whitney seems to be the one who has expressed views most congenial with Coseriu's: "Once more, there 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" (Whitney 1979 [1875]: 144; emphasis added; for discussion, cf. Itkonen 1984, 2009).²¹

Finally, the view that Paul (1975 [1880]) has of causality is seen to provide a useful point of comparison.²² In his opinion, there are two basic types of science, namely nomic sciences (*Gesetzeswissenschaften*) and historical sciences (*Geschichtswissenschaften*). Experimental physics is a typical nomic science. Nomic science does not coincide with natural science, however, because there are also sciences of the history of either inorganic nature (like geography) or of organic nature (like evolutionary theory). All cultural sciences belong to historical sciences. Linguistics is the cultural science with the most exact results (due to the inherent uniformity of its data), which has created the misconception that linguistics is a natural science. Experimental psychology (which is just emerging) is the most important auxiliary science of linguistics. Although of nomic character, it is distinguished from natural sciences by the presence of a 'psychic element'.

Historical grammar is scientific in the sense of exposing the **causal connection** (*Kausalnexus*) in its data. No non-causal account can be genuinely explanatory. Any given state of language is described by a (synchronous) descriptive grammar. Because states of language are **abstractions**, the corresponding descriptive grammars are non-causal and *prima facie* nonscientific.²³ Still, they do qualify as scientific in the secondary sense of being presupposed by historical grammar.

²¹ On this issue Whitney is supported by Michel Bréal, another pioneer of diachronic linguistics (quoted by Coseriu on p. 93). Bréal declares that, after thirty years of study, he finally sees linguistic change as it really is, because he has learned to discard all secondary causes and to concentrate on the only real cause, which is *l'intelligence et la volonté humaine* ("the human intelligence and will-power").

²² The following account summarizes the contents of Introduction and Chapter I.

²³ Incidentally, this is exactly the distinction between non-causal or 'autonomous' linguistics (= Itkonen 1978) and (different kinds of) causal or non-autonomous linguistics (= Itkonen 1983a).

Morpho-syntactic changes are functional (*zweckmässig*); and every language strives at any moment after the goal (*Ziel*) of 'one meaning – one form', but never quite reaches it.²⁴

These are the pieces of the puzzle: causality, non-nomicity, functionality. When they are put together, we get the same overall view of linguistic change as has been outlined in the preceding sections.

A typical functional formulation is as follows: 'X did A **in order to** achieve G'. But this is opaque. Its meaning becomes clear when it is translated in such a way that both the causal component and the functional components become explicit, as follows: 'It is **because** (= causal) X had the **goal** G (= functional) and believed that the action A is a/the **means** (= functional) to achieve G that X set out to do A.'

9. The Fallacy of 'No Extramental Causes'

On several occasions (e.g. p. 63, 178) Coseriu emphasizes that the psychophysiological environment has, and can have, no causal influence on people's behavior. The only important thing is how they **conceptualize** their environment; and this is a matter of free purposive action. Hence, there are no extramental causes of human behavior.

Depending on one's predilections, this view may be rated as either myopic or just false. A simple example will make this point clear. Assume a farmer has been experiencing a drought period. Now (he thinks) he has two alternatives: either to sell his land or to invest in more effective technology. Regardless of which alternative he will choose, it is clear that they both are his responses to the situation as he has conceptualized it. According to Coseriu, the causal story has been told in its entirety as soon as the conceptualization and the response to it have been mentioned. The drought, or the physical and extramental event that started it all, plays no part in the causal story. But this is absurd. Why? Because, apart from defying common sense, it contradicts the 'transitivity of causation': if X causes Y, and Y causes Z, then X causes Z' (cf. Dray 1980: 56).²⁵

In diachronic linguistics, structural changes may be free from any outside influence. But such influence is always the determining factor in borrowings and often in changes of lexical meaning.

10. Non-Nomic Explanation

The second part of Itkonen (1981a) is devoted to answering the following question: "How to construct a non-nomic methodology?" Actually, this question should have been formulated differently. It is not a matter of constructing non-nomic methodology. This methodology is already there. Everybody is using it, and most people are not using anything else. Hence, it is a matter of making people see that this is what is going on. A few arguments to this effect will now follow, in addition to those that have already been given in the preceding sections.

Let us start with a lengthy quotation from Berlin (1980 [1960]: 126–127). The problem is how to identify the **explanatory tie**, expressed by *because*, which is characteristic either of a nomic explanation or of its non-nomic counterpart:

²⁴ Thanks to Trubetzkoy and Jakobson, it has been known since the 1920's that, instead of just playing a disruptive role, sound change has a systematic functionality of its own.

²⁵ Of course, it could be said in Coseriu's defense that he is right insofar as people do inhabit a reality that they have conceptualized in one way or another. But nobody contests this, whereas Coseriu clearly thinks that he is arguing against a widely-held opinion.

Supposing that a doctor informs me that his patient recovered from pneumonia because he was injected with penicillin, what rational grounds have I for accepting this 'because'? My belief is rational only if I have rational grounds for believing the general proposition 'Penicillin is effective against pneumonia', a causal proposition established by experiment and observation, which there is no reason to accept unless, in fact, it has been arrived at by valid methods of scientific inference. ... If, on the other hand, I am told, in the course of a historical narrative (or in a work of fiction, or ordinary life) that *x* resented the behavior of *y*, because *x* was weak and *y* was arrogant and strong; or that *x* forgave the insult he had received from *y*, because he was too fond of *y* to feel aggrieved; and, having accepted these 'because' statements as adequate explanations of the behavior of *x* and *y*, I am then challenged to produce the general law which I am leaning on, consciously or not, to 'cover'²⁶ these cases, what would it be reasonable for me to reply? I may well produce something like 'The weak often resent the arrogant and strong', or 'Human beings forgive insults from those they love'. But supposing I am then asked what concrete evidence I have for the truth of these general propositions, what scientific experiments I or anyone have performed to establish these generalizations, how many observed and tested cases they rest on – I may well be at a loss to answer.

Berlin's point is that it would not be just an instance of misguided pedantry, but plain **wrong** to claim (as positivist philosophers or positivistically minded historians **must**) that explanations of the latter type remain inadequate until they have been established in the same observational/experimental way as those of the former type.

Coseriu is less circumspect. He states flatly (p. 158) that **no** historical fact (whether linguistic or not) admits of nomic explanation. The only possibility is a "particular" explanation (*eine besondere Erklärung*). Of course, it is possible to state any number of **generalizations** about such linguistic changes as have been observed to occur. But generality is not the same thing as nomicity. The former is non-explanatory while the latter is non-existent in diachronic and/or typological linguistics.

The notion of a 'particular' historical explanation in Coseriu's sense may be illustrated by the development of the system of first-syllable vowels in Saami. The original work on this problem was done by Erkki Itkonen (1939, esp. pp. 56–75, and 1946, esp. pp. 278–280), and it is summarized in Itkonen (1995). As reconstructed by Erkki Itkonen, the earliest (= Saami-Finnic) stage of the system was still the same as in Proto-Finno-Ugric. It then underwent no less than four successive changes in Proto-Saami. The third of these five stages was the most harmonious one, exhibiting a "beautiful equilibrium of the system". At the same time, it was typologically the most unusual one, since it contained five height distinctions; and such systems are characterized by Trubetzkoy (1958 [1939]: 101) as "*ganz besondere Raritäten*". The overall development is **explained** in functional terms insofar as each change is seen as being subordinated to *Systemganzheit* ('totality of the system'). Afterwards, this type of explanation, anticipated by Trubetzkoy and Jakobson (cf. note 24), became more widely known thanks to Martinet (1955). Now, it is clear that we are dealing here with a 'particular' explanation because it applies to a unique historical development which in no (non-vacuous) sense falls under any generalization.

On the other hand, the idea of 'generality without nomicity' can be illustrated e.g. by the collection of 'grammaticalization paths' brought together in Heine & Kuteva (2002). They are, precisely in Coseriu's sense, generalizations about observed data, supported by a number of instances ranging from many to very few. It goes without saying that any single process of grammaticalization is better explained in its own right than by reference to the generalization

²⁶ This word refers to the 'covering-law' model of explanation, also known as the 'deductive-nomological' model.

which subsumes it. This just repeats the result of Subsection 7-B (= 'Statistical 'Explanations' Are Too Weak').

11. Problems About The Unconscious

It is commonly assumed to be true by definition that when one proceeds to perform an action that one has decided on, one also **intends** to do so. According to Coseriu, this is also true of linguistic change, under both of its aspects, i.e. innovation and acceptance (or spread). Linguistic change is normally conceived of as unconscious, but in keeping with his general position, Coseriu maintains that the intentions involved in linguistic change are **fully conscious**; and he rejects out of hand any possibility of "mysterious 'unconscious' intentions" (*geheimnisvolle 'unbewusste' Absichten*, p. 194).

This is a surprising view on two (closely related) accounts. First, practically every representative of diachronic linguistics regards linguistic change as unconscious (apart from such exceptional cases as invention of technical vocabulary). Second, reluctance to postulate unconscious entities makes an oddly old-fashioned impression. To be sure, Zlatev (2008) may disagree.

J. F. Herbart (^ 1841) was the founder of German psychology, and according to him "unconscious ideas are just as important for the composition of consciousness as any other material" (Thomson 1968: 32). Heymann Steinthal introduced the Herbart-type psychology into linguistics. He explicitly postulated what Coseriu rejects, namely unconscious intentions ("*mit Absicht, aber unbewusst*", 1972 [1881, 1st ed. 1870]: 164). He also makes the perceptive remark that as long as the intelligent nature of the unconscious remains unrecognized, linguistics is likely to be considered as a natural science. Why? Because most aspects of language are clearly unconscious, and lack of consciousness is taken to be the central characteristic of **natural** objects (p. 166). The other alternative (adopted by Coseriu) is to over-emphasize the role of consciousness in a rather implausible way.

As Steinthal's contemporary, Whitney fully understood the force of this argument: "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 towards that purpose" (1979 [1875]: 146–147).

Paul (1975 [1880]) praises Steinthal for having promulgated the following insight: "All expressions of speech activity issue from this dark space of the unconscious" (p. 25). Interestingly, Paul anticipates Freud in arguing, against Wilhelm Wundt, for the ontological reality of unconscious mental processes on the exact analogy of conscious ones (for discussion, cf. Itkonen 2005: 224–225).²⁷

On this issue I concur with Steinthal, Whitney and Paul, for several reasons. In particular, my notion of rational explanation is plausible only on the assumption of **unconscious** rationality. This assumption was confirmed e.g. by Davidson, Suppes & Siegel (1957) who, in a series of experimental investigations on decision-making, discovered that test persons act in accordance with the subjective possibilities and utilities determined by probability theory, although they neither could have consciously calculated them nor were just repeating what

²⁷ It should be added that, at least for the most part, the unconscious ideas postulated by Steinthal and Paul are such as have been or will be conscious at one time or another. It is not clear whether they would have been willing to postulate the existence of **permanently** unconscious ideas, or ideas that cannot even in principle become conscious.

they had previously learned.²⁸ Moreover, unconscious rationality has been part and parcel of cognitive psychology since its beginning (cf. Neisser 1967: 292–303). “For all we know, cognition is saturated with rationality through and through”, as Fodor (1975: 173) puts it.

The notion of rationality remains surrounded by a thick cloud of misunderstandings. Perhaps it can be dispelled at least to some extent by noting that higher animals too are fully capable of (unconscious) rationality (cf. Itkonen 2003: Chap. 12, 2008b: Chap. 13).

12. Problems About Intentions

‘Intention’ is an elusive notion (cf. Itkonen 1983a: 95–102). First, actions are generally thought to be preceded by intentions. But second, intentions may also be thought to be ‘in’ actions in the sense of constituting them as what they are. Typically, the contents of antecedent intentions and of concurrent ones are **identical** (cf. Wright 1976: 124). Third, ‘intentionality’ (in the sense of ‘directedness’) is generally assumed to be a characteristic not just of actions but of any such propositional attitudes as hoping, fearing, remembering, and so on.

Personally, I have found that in such a philosophical context as e.g. von Wright (1971), it makes perfect sense to assume intentions to be concurrent. But in a context where e.g. acts of speaking or linguistic changes need to be **explained**, intentions just have to be viewed as antecedent and causally effective. Thus, I accept Mackie’s (1974: 293–294) criticism of ‘intentions-in-actions’ as (explanatorily) empty. The only remaining alternative would be to abandon ‘intentions’ altogether and to replace them by goals(-cum-beliefs), as is indeed done in Woodfield’s (1976) teleological description of actions or in my own schema of rational explanation (cf. note 11). That this could be the reasonable thing to do, is suggested e.g. by how, between the early 70’s and late 80’s, Hilary Putnam and Jerry Fodor have changed their minds about what intentions really **are**. ‘Intention’ may well turn out to be a technical term without any genuine referent.

13. Example: The Emergence of the Romance Future

Coseriu illustrates his narrative with many examples, taken mainly from Romance phonology. Yet the most extensive treatment (pp. 132–151), intended to exemplify the methodologically central notion of **functional explanation** (*finalistische Erklärung*), is devoted to the emergence of the Romance future.²⁹

Traditionally, two distinct (but complementary) explanations are given as to why the synthetic future of Classical Latin (e.g. *amabit* = ‘s/he will love’) was replaced in Vulgar Latin by an analytical or periphrastic construction, typically *amare habet* (= ‘s/he ought to love’³⁰), but occasionally also *amare debet* (= ‘s/he ought to love’) or *amare volet* (< *vult*) (= ‘s/he wants to love’). According to the **formal** explanation, the future forms of various

²⁸ To be sure, the well-publicized experimental studies by Tversky and Kahneman have shown that commonsense thinking in many respects violates the principles of probability theory.

²⁹ The importance attached to this example is evident e.g. from the following formulation (p. 178–179): “Eine einzelne finalistische Erklärung (z.B. unsere eigene Erklärung des romanischen Futurs) ...” (= “Any single functional explanation (e.g. own own explanation of the Romance future ...)”).

³⁰ “Als Grundlage der spätleinischen und romanischen Futurverwendung ist jedoch hauptsächlich, wenn nicht ausschliesslich, die Bedeutung des Müssens...” (= “The basis for the late Latin and Romance future is mainly but not exclusively the meaning of *must*...”, Hofman & Szantyr 1972: 314). The emergence of this *INF+ habet* construction had in turn been influenced by the corresponding construction in Classical Greek: *ἀ÷ἄέ +INF* (= ‘can/must INF’ < ‘has INF’).

conjugations often coalesced with other forms, e.g. present (*aget* ~ *agit* = 's/he will drive' ~ 's/he drives') or perfect (*amabit* ~ *amavit* = 's/he will love' ~ 's/he [has] loved'),³¹ which supposedly created the need to establish a new paradigm for the future. According to the **semantic** explanation, the 'nothing-but-temporal' idea of the future, expressed by the synthetic forms, was supposedly felt to be too insubstantial and was therefore replaced by a 'more affective' notion of the future, expressed by the *INF* + *habet* construction. Thus, the original meaning of the new construction was, first and foremost, **moral duty** (*moralische Notwendigkeit*), but gradually it was grammaticalized:³² formally, it became synthetic in its turn, and semantically, it came to express future *tout court*.

As for the (attempted) semantic explanation, Coseriu correctly notes (p. 138) that it exhibits a vicious circle (or rather, a *virtus dormitiva* -type pseudoexplanation). It just states what happened, but reformulates it as an explanation: 'X was replaced by Y because there was a need to replace X by Y'. As for the formal explanation, Coseriu notes (pp. 141–142) – again correctly – that the emergence of new modal-future constructions is attested in many such languages (e.g. in modern French and Spanish) where the older temporal-future forms are in no danger of coalescing with other forms. Therefore, such a danger cannot at least be a necessary condition for this type of change.

What is needed, in Coseriu's opinion, is some **external** event that would be genuinely explanatory. Perhaps surprisingly, he sees **Christianity** as this external, 'triggering' cause. Why? Because this new religion supposedly aroused the sense of moral duty which, as was just noted, is the original meaning of the *INF* + *habet* (and *INF* + *debet*) constructions.³³ Coseriu regards (pp. 148–149) this explanation as directly supported by Saint Augustine's **concept of time** which, he adds, "is different from anything that has come to us from antiquity". More precisely, according to Augustine, each present moment involves three components: **memory** of what is past, **perception** of what is present, and **expectation** of what is future (= "*praesens de praeteritis memoria, praesens de praesentibus contuitus, praesens de futuris expectatio*"; emphasis added).

Coseriu's explanation is open to two serious counter-arguments. First, as noted above, it is the core of this explanation that the Romance future emerged because Christianity had aroused a sense of moral duty in speakers of (Vulgar) Latin; but Augustine's concept of future (as quoted by Coseriu) involves a reference to expectation, and **not** to moral duty. Second, far from being a unique property of Christianity, Augustine's concept of time is identical with Aristotle's corresponding (and, needless to say, **pre-Christian**) concept: "As already observed, there is no such thing as memory of the present while present, for the present is object only of **perception**, and the future, of **expectation**, but the object of **memory** is the past" (*De Memoria et Reminiscentia* 449b, 25; emphasis added).

As far as I can see, these remarks suffice to refute Coseriu's explanation.³⁴ Also, it should not be forgotten that the future construction, exemplified by *amare* + *habet*, is not an isolated phenomenon, but only one piece of a much larger **pattern**, namely the emergence of *habere*

³¹ Itkonen (1982a [1978]: 107) uses this example, together with the development of the copula forms *es/est*, to illustrate the development 'two meanings – two forms' > 'two meanings – one form' > 'two meanings – two forms'.

³² Significantly, Coseriu prefers to equip this term with quotes.

³³ The plausibility of this explanation is enhanced by the fact, mentioned on p. 148, that the *INF* + *habet* construction was occasionally referred to as a "Christian [tense] form".

³⁴ To be sure, Coseriu was realistic enough to anticipate this possibility. The quotation given in note 29 continues as follows: "... kann anfechtbar und sogar falsch sein, doch heisst das nicht, dass ihr Prinzip falsch ist" (= "... can be questionable and even false, but this does not mean that its principle is false").

as an all-purpose temporal/modal auxiliary. For instance, the ACT.IND.PRF/FUT forms and the ACT.COND³⁵ forms of *amare* ('to love') are constructed out of the following building blocks: PASS.PRT = *amatum*, INF = *amare/habere*, PRES/IMPRF/PRF of *habere* = *habet/habebat/habuit*. Thus: *amatum habet* = PRF, *amatum habebat/habuit* = PLUPRF, *amare habet* = FUT, *amatum [habere habet]* = FUT.PRF, *amare habebat/habuit* = COND (also 'FUT-in-the-past'), *amatum [habere habebat/habuit]* = PRET.COND (also 'FUT.PRF-in-the-past') (cf. Harris 1978: Chap. 6).

To put things in perspective, the following qualifications may also be added. First, the 'nothing-but-temporal' synthetic future of Classical Latin had its origin in the Indo-European ('not-just-temporal') subjunctive (cf. Hofmann & Szantyr 1972 [1965]: 309). Second, the meaning of the synthetic future of Classical Latin was actually **not** 'nothing-but-temporal'. In addition to the temporal meaning, this form expressed also modal meanings, including moral duty (*sittliches Sollen*) (op. cit., p. 310–311), which of course prepared the ground for the *INF* + *habet* ('ought to') construction. Third, the traditional Latin grammar considers the change 'ought to' > 'future' as a "development from mood to tense" ("*Entwicklung vom Modus zum Tempus*", op. cit., p. 315).

Cross-linguistically, this type of development is well attested, as noted by Bybee, Perkins & Pagliuca (1994: 258–264). Interestingly, referring to Benveniste (1968), they contest the view that the *INF* + *habet* construction expressed obligation or moral duty. However, all the facts adduced by Benveniste (1968) – and many more – have already been treated by Hofmann & Szantyr (1972 [1965]), and they arrive at a different conclusion (cf. above).

14. Empathy

According to Bybee et al. (1994: 282), what they have to address is the following question: "Thus despite the diachronic perspective of grammaticization, the ultimate question is a synchronic psycholinguistic question – how does meaning change in grammaticization take place in the minds of speakers while they are using language?" By way of an answer, they proceed to identify five distinct processes, namely 'metaphorical extension', 'inference', 'generalization', 'harmony', and 'absorption'. Heine & Kuteva (2007) struggle with the same question, and the list they come up with contains four processes, namely 'extension', 'desemanticization', 'deategorialization', and 'erosion' (pp. 33–44).

But how do we come to know (or, less pretentiously, to have any inkling about) such processes? This is the logically prior question. Coseriu indicates the answer by reformulating the question more narrowly as follows: "For what purpose would I, disposing of this [linguistic] system and being in this historical situation, change A into B, abandon C or create D?" (p. 177; original emphasis).

The ability to which Coseriu is implicitly referring here is called **empathy**. In Itkonen (1983a: 215–217) I first give a rational explanation to the universal "In all languages, if the intransitive subjects have overt case-marking, then the transitive subjects have it too"; and then I explain how it was possible for me to do so:

Although this term may once again seem out of place, one cannot help realizing that such explanations are contrived on the basis of a certain type of **empathy**, or of imagining what **we** would do if the

³⁵ This abbreviation stands for the conditional mood (*le mode conditionnel* in French: *il/elle aimerait, il/elle aurait aimé*). PRT = participle, PRET = preterite.

unconscious goals that we hypothetically assume to exist were our conscious goals (original emphasis).³⁶

Since then, I have validated my thesis by concentrating on the descriptive practice of such luminaries of typological-functional linguistics as Foley, Givón, Haspelmath, Heine, and Mithun, and by showing that the explanations they typically offer rest on empathy, and **nothing but** empathy (Itkonen 2002a, 2003: Appendix 5, 2004, 2008a). The following phenomena have, *inter alia*, been given rational empathy-based explanations: the cross-linguistic rarity of *N-and-N* constructions; incorporation; coordination marking; subordination (= complement-clause) marking; passive constructions; the counterfactual meaning of factive preterite forms; same-subject vs. different-subject converb constructions; the distribution of personal endings in Hua finite-verb paradigms. Finally, the “deeper explanations” suggested by Croft (2003: 69–80, 116–117, 137–139, 178–183) turn out to be based, without exception, on empathy.

On reflection, the outcome of the previous paragraph is self-evident. If (and when) linguistic change – and more generally, language use – is conceptualized as **problem-solving** (cf. Subsection 7-D above), we cannot make any advance at all, unless we first identify the problem and then think of possible ways to solve it. And the **only** way that this can be done is by imagining what **we** would do if we were in the same situation as the the people whose problem-solving behavior we are describing. Coseriu makes the same point (p. 177): this is what linguists actually do, regardless of what they think they are doing.

As might be expected, such processes as (analogical) **extension** and **reanalysis** stand out in this context as preferential objects of empathy. In addition, it is interesting to note that, according to Haiman (1985), also such general principles as **iconicity** and **economy** are potential objects of empathy, or “relatively accessible to conscious observation” (p. 260). The same can be said of **analogy** in general, as documented and analyzed in Itkonen (2005). In many ways, current linguistics still suffers from after-effects of generativism. For instance, the many uses that can be (and are) made of analogy remain a practically unexplored area.³⁷

Empathy is known under many different names, for instance ‘re-enactment’, *Verstehen*, and *Einfühlung*. As far as I can see, all these names aim at capturing one and the same unitary notion. But what **is** it? Here I just give a short version of the more elaborate answer contained in Itkonen (2008a).

³⁶ This idea is summarized in the title of a subsection in Itkonen (2008b: 259): *La empatía deviene explicación racional*.

³⁷ Consider some of the findings in Itkonen (2005): Analogy refutes modularism, because (as argued by generativists themselves) the supposed modules are structurally similar (= analogous), which clearly points to a common origin. Analogy refutes nativism, because the processes of (syntactic) induction/analogy, claimed to be non-existent and certainly unformalizable, are in fact formalized by means of a computer program. (Kac 2008 offers a elegant summary of this argument for anti-Chomskyan syntax.) Blending à la Fauconnier & Turner is based on analogy because systems that are to be blended need to share a common structure (i.e. to be analogous). Construction grammar à la Goldberg is based on analogy because extending construction X to construction Y presupposes that X and Y share a common structure. And this is just the beginning. – Let us consider a more recent example. In order to explain the emergence of hypotaxis, Heine & Kuteva (2007: Chap. 5) postulate two supposedly distinct processes of ‘integration’ and ‘expansion’. But on reflection, they reduce to one and the same application of analogy, based on the following principle: “Der Nebensatz hat die nämliche Funktion wie ein Satzglied” (= “The subordinated clause has the same function as a grammatical role [e.g. subject or object]”, Paul 1975 [1880]: 296; similarly p. 123). *Satzglied* is generally translated into English as ‘grammatical relation’, but this is a mistake. In a sentence like *Gentlemen prefer blondes* the word *gentlemen* is both the subject and the first argument of the ‘Subject-of’ relation whose second argument is the entire sentence; but it is not (identical with) this relation.

Empathy looks in two opposite directions in the sense of being a 'midstation' between introspection and intuition. First, empathy is 'vicarious introspection'. Second, intuition is 'conventionalized empathy'.

Empathy is not genuine but only 'vicarious' introspection insofar as when person A practices empathy with respect to what person B thinks about X, A imagines what B's introspection is about X. The same idea is expressed by Schelling (1960) when he describes coordination games based on cooperation: "In the pure coordination game, the player's objective is to make contact with the other player through some **imaginative** process of **introspection**, of searching for shared clues; ..." (p. 96; emphasis added).

Intuition is about well-established norms.³⁸ Of course, a norm does not come out of nothing but is, rather, the end point of a process of **conventionalization**. Let us consider how the meaning of a linguistic expression X becomes conventionalized. During this process, A's mental act which is directed at X undergoes a change. First, A **introspectively** knows what A means by X. Second, A **empathically** knows (or thinks s/he knows) what B means by X. Third, A **intuitively** knows what X means, i.e. what anyone **ought to** mean by X.

The status of B needs to be defined. To begin with, B is an individual person. General historiography has to deal with 'great men', i.e. with individual persons, but more often it deals with typical or 'anonymous' persons. Interestingly, this is also the position of the school of thought known as 'methodological individualism':

The information it would accept as explanatory may be, and would normally be, about unnamed or 'anonymous' individuals ... – as would be the case ... if we explained the commercial success of the Huguenot trading community in France in terms of the dispositions of its members, being Calvinists, to reinvest more of their profits in their own businesses than their Catholic rivals customarily did, taking into account also the lack of alternative opportunities open to them (for example, buying land or office) owing to their religious disabilities (Dray 1980: 49).

From being applied to some anonymous B, empathetic understanding quite naturally extends to the entire culture which B is representative of:

But ... what influenced later thinkers, ... is the theme to which [Herder] constantly returns; that one must not judge one culture by the criteria of another; that differing civilizations are different growths, pursue different goals, embody different ways of living, are dominated by different attitudes to life; so that to understand them one must perform an imaginative act of '**empathy**' into their essence, understand them 'from within' as far as possible, and see the world through their eyes (Berlin 1975: 210; emphasis added).

I submit that the same is true of diachronic/typological linguistics as well, with the proviso that distinct areas of language/grammar need to be treated somewhat differently. For instance, person B *qua* object of linguistic study is more anonymous, and less culture-bound, in phonology than in the lexicon.

One might deplore the fact that empathy-based explanations have to rely on 'folk psychology', but it cannot be helped. As Coseriu points out (p. 177), this is what happens. This is what linguists do, whether they realize it or not.

Nevertheless, it is good to point out in conclusion that empathy-based explanations by no means exhaust the methodological arsenal that typological linguistics has, in principle, at its

³⁸ I have dealt with normativity for some 40 years, most recently in Itkonen (2008c).

disposal. First, those linguistic categories and processes that are most likely to be universal have obvious roots in preverbal thinking (cf. Itkonen 2002b: 153–160). Second, the closest thing to explaining an entire language/grammar is to show how it fits in with what is known about other languages/grammars. In social theory, this is called **pattern explanation**, and its philosophical justification is provided by the so-called coherence theory of truth (cf. Itkonen 1983: 2.2.6, 4.1.1.3).

15. Human vs. Natural Science

In his Chapter VI Coseriu repeatedly emphasizes the fundamental difference between natural sciences and human sciences. It is well known (and was briefly mentioned in Section 2) that there are those who deny that there is any such difference. What is the truth? Let us find out.

A) A “crude and untenable” distinction?

Lass (1997) admits that there are no deterministic or non-functional explanations; but, at the same time, he criticizes (in Chap. 6) Raimo Anttila and Esa Itkonen for attempting to ‘redeem’ the concept of functional explanation. Thus, as quoted above, he finally reaches the “point zero”: “we still have no convincing explanations for change” (pp. 386–387).

As Lass sees it, Anttila’s and Itkonen’s joint attempt is based on an (alleged) distinction between “natural science” and “hermeneutics”, a distinction which he considers “crude and untenable” (p. 338). Let us see whether this judgment is justified or not. “Hermeneutics”, being a rather vague notion, will be replaced in what follows by “(philosophy of) human science”.³⁹

As was mentioned at the outset, generativism is the school of linguistics that has the most consistently rejected any kind of distinction between natural and human sciences, in favor of physicalism pure and simple.⁴⁰ Against this background it is interesting to note that the view that generativism holds of physics is somewhat crude (even if not downright untenable). When, in the early 70’s, non-generativists started to argue for the existence of two distinct types of linguistics, namely grammatical theory and (experimental) psycholinguistics (cf. Itkonen 2005: 44–52),⁴¹ generativists countered by claiming that any such distinction is

³⁹ Since the late 70’s I have had reason to argue intermittently both for and against views represented by Roger Lass; but it has always been an intellectual pleasure.

⁴⁰ In my 1974 and 1978 books I considered Jerrold Katz as one of my principal (generativist) opponents. Fortunately for all of us, he was intelligent enough to completely change his mind in his 1981 book (cf. also Itkonen 1983b). I was gratified to receive his letter, dated March 15, 1982, where he states, *inter alia*, the following: “It is clear to both of us that we are thinking along very similar lines and in opposition to the same philosophical and linguistic orthodoxy. I hope we can have the opportunity to discuss our work together” (cf. also Katz 1985: 15). As incredible as it may seem, some remnants of the orthodoxy so eagerly counteracted by me and Jerrold are still with us today.

⁴¹ Occasionally, it was possible for some of us to raise the issue of ‘psychological reality’ already in the late 60’s: “Le critère général dont on fait emploi pour construire une description synchronique est la **simplicité**, ce qui paraît être un choix plutôt naturel. Mais l’ambiguïté de la notion de la simplicité mise apart, on peut se demander s’il y a là vraiment une **réalité** qui corresponde à des systèmes aussi abstraits. Il est loin d’être évident qu’en matière de langage et, plus généralement, dans les sciences essentiellement humaines, la simplicité implique nécessairement une **réalité** quelconque. Et d’autre part, si l’on s’avise de ne plus tenir compte de la **réalité**, les descriptions fondées uniquement sur la notion de simplicité donnent l’impression gênante d’une pure manipulation” (Itkonen 1969: 471; emphasis added). I gave up this negative value judgment as soon as I realized that simplicity, instead of being “mere manipulation”, exemplifies the ideal of **axiomatics**, which constitutes one of the two great traditions in the history of linguistics (cf. Itkonen 1991: Chap. 2, and 2011). Thus, language can be approached with more than one descriptive purpose. – Of course,

unknown in the natural sciences. Presumably, it is pointless to distinguish between descriptions which do, and those which do not, search for 'psychological reality', because in physics, for instance, all theories are equally about the 'physical reality'.

But this is not true: "For [Einstein's] predecessors, the Lorentz transformation was merely a useful tool for linking objects in relative motion; for Einstein it was not a mathematical tool so much as a revelation about the nature itself" (Clark 1973: 120; quoted in Itkonen 2005: xii).

Thus, the difference between Einstein and his predecessors was that, unlike the latter, he claimed **physical reality** for his theory. Thus, contrary to the generativist view, the situations in physics and in linguistics are clearly **analogous**. And the story continues. Even a layman can see with one glance that, if taken literally, Einstein's famous formula $E = mc^2$ entails that even a tiny bit of mass somehow equals a tremendous amount of energy. But when Einstein was asked about the exact meaning of this formula, he reportedly answered that it is "just mathematics". But the younger generation, replicating Einstein's much earlier response to the Lorentz transformation, was bold enough to claim **physical reality** for this formula, with well-known consequences.

Interestingly, Darwin teaches exactly the same lesson. He claimed **biological reality** for what his predecessors had regarded as merely a useful means for presenting the data:

Some authors look at [the Natural System] merely as a scheme for arranging together those living objects which are most alike and for separating those which are most unlike; or as an artificial means for enunciating, as briefly as possible, general propositions ... But many naturalists think that something more is meant by the Natural System; ... I believe that something more is included; and that the propinquity of descent – the only known cause of the similarity of organic beings – is the bond, hidden as it is by various degrees of modification, which is partially revealed to us by our classification (Darwin 1998 [1859]: 312–313; emphasis added; quoted in Itkonen 2005: xii).

Let us recapitulate the preceding argument. First, if linguistics contains a certain distinction, there is no way that this fact can be refuted by appealing to the (alleged) fact that physics or biology has no analogous distinction. But it turns out that they do have it. This result may be interpreted as lending additional (even if, strictly speaking, unnecessary) support to the initial claim that linguistics has this distinction.⁴²

But (as might be expected) this is not the whole story. There is a **further analogy** between linguistics and physics. The normative system investigated by grammatical theory (or 'autonomous' linguistics) is the conceptual precondition for those spatiotemporal or empirical phenomena that are investigated by psycholinguistics, sociolinguistics, and diachronic linguistics. Within the overall theory of physics, an analogous role is played by **protophysics** which, invented by Paul Lorenzen in the 60's, investigates the norms of measuring length, weight, time, etc that are presupposed by each and every physical description. Starting with Itkonen (1978), I have stressed the methodological importance of protophysics as a point of

it would not have been possible to write an article on Merovingian Latin without some amount of familiarity with the topic of Section 13.

⁴² Katz (1981) argues that an 'optimal grammar' should free itself of any psychological constraints (as well as of neurological or sociological ones) and pursue only 'linguistic reality', an endeavour which he explicitly identifies with endorsing the ideal of descriptive simplicity or – we may add - axiomatics. Accordingly, it would actually be **wrong** for a linguist to strive after psychological reality. Now, I do consider 'optimal grammar' as a meaningful notion which has, to my knowledge, been best exemplified by Panini's grammar (cf. note 16). But I see absolutely no reason for privileging grammatical theory in this way at the cost of the other linguistic subdisciplines (cf. Itkonen 1983b and note 41).

comparison; and, during the more than 30 years that have elapsed since then, it has become eminently clear to me that the huge majority of linguists could not care less.

Up to now, I have been dealing with **similarities** between the natural sciences and linguistics *qua* human science. It is precisely at this juncture that we come to see the **differences**. The norms investigated by protophysics are those of physicists, never – **never!** – those of their research objects. By contrast, the norms investigated by grammatical theory are those of **both** research objects (= ordinary speakers) **and** researchers (= linguists). This is the crucial difference. You may or may not think that it is crude. But at least it is tenable.

Thus, the difference between natural vs. human science is ultimately based on the fact that research objects and researchers overlap. In the domain of autonomous linguistics this is shown by **normativity** (cf. note 38). In the domain of non-autonomous or causal linguistics it is shown by **empathy**. In diachronic/typological linguistics we ask ourselves what we would have done if we had been in the same situation as those who we are investigating. More generally “the scientist merely recapitulates the bio-organism” (Givón 2005: 204). By contrast, it is not reasonable to assume, for instance, that physicists ask themselves what they would have done [*sic*] if they had been in the same situation as a planet or a subatomic particle. If they do ask such a question, they can mean it only metaphorically, and the answer they eventually give will most probably be of no help.⁴³

In conclusion, one should keep in mind the general truth that similarities and differences are relative to the **level of abstraction**. As the level of abstraction rises, different scientific disciplines come to look more and more alike:

The testing of a logical system, or of a grammatical system, as to its completeness [= ‘**all** valid formulae or correct sentences’] and soundness [= ‘**only** valid formulae or correct sentences’] has its analogue in the twofold character of empirical testing. A natural-science theory may be confirmed in either of two ways: Either there is an event, and it turns out that the theory explains it; or a prediction is deduced from the theory, and it turns out that there is an event corresponding to it. These two procedures ... might be called ‘explanatory confirmation’ and ‘predictive confirmation’. ... The level where these similarities between the different types of ‘testing’ can be stated is *eo ipso* that quite abstract metascientific level where the differences between natural science, human science, and formal science have been neutralized. At this level, ‘testing’ simply means finding out whether the description is as it should be; and experimentation is of course only one way of doing this (Itkonen 1975: 37).

In other words, it is self-evident that there must be such a level of abstraction where the difference between norms and (non-normative) regularities disappears. But, while fully admitting this, it is **more interesting** to concentrate on such a level of abstraction where this difference obtains. Why? Because the existence of such a level of abstraction is often denied. Similarly, it is self-evident that there must be such a level of abstraction where the difference between physical measurement and empathy disappears. But again, and for the reason just indicated, it is **more interesting** to concentrate on such a level of abstraction where this difference obtains.

B) An “Outdated” Distinction?

In the late 19th and the early 20th centuries there was a sizable group of German historians and philosophers who, referring to the ‘method’ of *Verstehen*, argued for the independence of

⁴³ Let us repeat, once again, that while empathy-based explanations do play a central role, they are **not** the only type of explanation available to diachronic/typological linguistics.

human sciences (*Geisteswissenschaften*) vis-à-vis natural sciences. Therefore it may be comprehensible that when, in the early 70's, a linguist (like myself) started to argue for the viability of this distinction, he invariably met with the remark that it is 'outdated'.

There is an important lesson to be learned here. At that time, practically everybody claimed that all explanations (or 'explanations') contained e.g. in a grammar of English must conform to the same 'deductive-nomological' model as Newtonian mechanistic explanations. In this sense, then, this view was certainly **up-to-date**. At the same time, it was absolutely **false** (as is today conceded by everybody).⁴⁴ Now, when the choice is between 'up-to-date but false' and 'outdated but true', one should not hesitate. And on reflection, of course, it turns out that the latter alternative only looked, but in fact was (and is) not, outdated.

C) Towards A Synthesis?

A genuine synthesis between dissimilar ingredients necessitates 'harmonizing' them, not eliminating their mutual boundaries. An overall view of language can be achieved, if at all, only in a semiotic framework which accommodates the phylogenetic and ontogenetic aspects in addition to the diachronic/typological ones. Such a large-scale approach is represented e.g. by Sinha (2002), Zlatev (2002), and Givón (2006).

16. Concluding On A Personal Note

For the fourth and last time, Coseriu and I met at the 14th International Congress of Linguists, in East Berlin, August 1987. I was pleasantly surprised (cf. note 20) when, offhand, he introduced me to his entourage in the following terms:

"Er ist einer von den ganz wenigen Leuten, die mich wirklich verstehen!" (= "He is one of the very few people who really understand me!"). I sincerely hope that in writing this article I have not betrayed his trust.

⁴⁴ As against the Newtonian paradigm, Givón (1984: 24) correctly pointed out that "biology is a much more realistic metaphor for linguistics as is physics".

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