

EINFÜHLUNG (= ‘RE-ENACTMENT’) AS THE BASIC METHOD OF TYPOLOGICAL LINGUISTICS

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This article discusses the methodology of linguistic typology. It is argued that a rational explanation is the only true explanation for typology. Language can be understood to be a means to achieve the goals of the speaker. Only a rational explanation may reveal how exactly linguistic forms serve the function of language. Therefore, this kind of rational explanation is the only way we can understand linguistic phenomena in a large scale. In addition, even though particular linguistic changes are unpredictable, they can be understood, and thus explained, as parts of a larger whole, which has a rational basis.

Methodologically, the rational explanation is a way to understand linguistic phenomena without trying to wrongly imitate the methods of hard sciences, and still keep the explanations explicit and theoretically rigorous. The article discusses the nature of rational explanation in the light of several examples.

Keywords: *linguistic typology, explaining, rational explanation*

I have argued earlier that the possibility of linguistic typology is based on **analogy**, first analogy between particular oral languages and second, and more abstractly, analogy between oral and signed languages: we identify the common function served by different structures (= a common sentence-meaning expressed by different sentence-forms), and we note the basic (structural) **similarity** between the latter. The **differences** (which constitute the subject matter of typology

proper) obtain at lower levels of abstraction (cf. Itkonen 2002a, forthcoming).

In this paper I shall examine more closely the method (or, more neutrally, the process) which we apply in recognizing the aforementioned analogy and, more generally, in practicing linguistic typology. Let us start with some representative illustrations of the method:

“When an entity is first introduced into discourse, it is usually identified by a full independent noun phrase. In subsequent discourse, the noun phrase may be omitted, since pronominal affixes refer to the entity in question. Sometimes, however, the pronoun alone is not sufficient to qualify verbs of wide scope. Yet a separate noun phrase re-identifying the entity,

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now old information, would sidetrack the attention of the listener. The **solution** is incorporation” (Mithun 1986: 381; emphasis added).

“The incorporated noun for ‘body’ can become a **useful means** of qualifying a verb pertaining to the physical aspect of a person or animal without backgrounding the individual” (Mithun 1986: 384; emphasis added).

“Verbs function regularly as nominals in Cayuga, but they cannot be incorporated. [...] Yet the verb *-k* ‘eat’ incorporates its patient if at all possible. This presents a conflict: the noun phrase should but cannot be incorporated. Incorporation of the noun stem alone provides a **solution**” (Mithun 1986: 387; emphasis added).

Here we see the typological-functionalist method at work. The linguist **explains** a certain phenomenon of a language L as being the **solution** to a problem that the speakers of L were facing. The same approach is represented by Heine, Claudi & Hünnemeyer (1991: 29): “grammaticalization is the result of problem solving”.

Now, this approach presupposes that the linguist (implicitly, for the most part) imagines what was going on, unconsciously, in the speakers’ minds, i.e. what were their (unconscious) goals and their (unconscious) beliefs concerning the available means for attaining the goals. And this, in turn, is nothing but the so-called method of *Einfühlung* (or *Verstehen*), or what Collingwood (1946) called ‘re-enactment’: “For the historian, the activities whose history he is studying are not spectacles to be watched, but experiences to be lived through in his own mind” (p. 218; for discussion, cf. Itkonen 1978: 139–140, 193–194).

Let us add the following clarification:

“This person – the agent – has something he wants to do, some end in view. What he does

will obviously depend on what he believes about ways of achieving this end, and our procedure depends on the assumption that he arrives at that belief by way of an argument.” [...] “Anyway, [...] we must assume a common rationality, and argue from what we would do to what others would do. If we are considering the explanation of past actions, this might aptly be described, in R.G. Collingwood’s terms, as ‘re-thinking people’s thoughts’” (Gibson 1976: 113, 116).

Notice that the method of *Einfühlung* is based on the hypothesis that there is an analogy between the **unconscious** goals-*cum*-beliefs that the historical persons (e.g. the speakers of L) entertained and those goals-*cum*-beliefs which the historian (or the linguist) **consciously** postulates as being those **unconscious** goals-*cum*-beliefs that he himself would have entertained if he had been in the same situation as the persons he is investigating. This method may seem unreliable but – I claim – there is no alternative to it. Notice also that, in the case of linguistic typology, there is a huge amount of cross-linguistic evidence (accumulated in the same ‘unreliable’ way, to be sure) that guides us in hypothesizing about the unconscious goals-*cum*-beliefs in a given situation; and the hypotheses may always be revised in light of new evidence. The method may also seem overly simple but – I claim – this cannot be helped. And even a simple method, when judiciously applied, may lead to quite complex and interesting conclusions. To me, it seems utopian (to put it gently) to assert that the methods we apply to cross-linguistic data are, or can become, those of quantum physics or of evolutionary biology, for instance (cf. below).

In the light of the results we have just achieved, let us consider one more representative example:

“The use of perfective – *past* or *perfect* – morphemes in the marking of subjunctive clauses is widely attested but at first glance somewhat **puzzling**. The subjunctive is the quintessential irrealis sub-mode, while *past* and *perfect* are quintessential realis sub-modes” (Givón 2001: 362; emphasis added).

“Given the wide-spread use of erstwhile perfective markers to code subjunctives, an **explanation** is indeed in order. The one that comes to mind most readily is the **connection** with *counter-fact* clauses. Such clauses, so it seems, tend to be universally marked by a combination of irrealis and either past or perfect” (Givón 200: 363; emphasis added).

“The grammaticalization pathway exploits the **continuum** between realis-indicative, simple irrealis, subjunctive-irrealis and counter-fact ... If the past or perfect did penetrate this paradigm initially at point (54f) [= Counter-fact: ‘If she had come, I would have left’] – the **beach-head** of counter-fact – then its spreading upwards to points (54e,d) [= Past subjunctive: ‘If she came, I would give her anything’ and Past-modal subjunctive: ‘If she should/would/could only come, he would leave’] is but one more instance of gradual subsequent extension from the initial beach-head along the functional continuum of grammaticalization. The three main steps in this gradual analogical extension may be summed up in the hypothesis: ... a) past/perfect Y counter-fact; b) counter-fact Y past subjunctive; c) past subjunctive Y subjunctive. ... Finally, one must reiterate that perfective forms do not enter this paradigm alone at the counter-fact beach-head, but rather combine there with some irrealis marker [such as ‘if’]” (Givón 2001: 365–366; first emphasis added).

The linguist is confronted here with the following problem: A occurs with D, but this fact is **puzzling** because – prima facie – A and D are unrelated. An **explanation** for the co-occurrence of A and D is sought (and, perhaps, found) by assuming a **continuum** $A > B > C > D$, in which each particular step

is comprehensible in itself and in which the movement forward is propelled by ‘analogical extension’, to use Givón’s term. Exactly the same method is repeatedly applied by Heine et al. (1991) (see e.g. Fig. 3.1 on p. 68, Fig. 3.2 on p. 87, Fig 4.8 on p. 114, 6.1 on p. 151).

Let us spell this out. At the start, the linguist does not understand the shift $A > D$ that has taken place in a language L; in order to understand it, he postulates the shifts $A > B$, $B > C$, and $C > D$, each of which is such that he understands it in a ‘direct’ way; now he also understands the shift $A > D$, even if only in an ‘indirect’ way. But why is he capable of doing this? Because he can imagine **himself** performing each of these shifts, with only the difference that his imagining is conscious while all those shifts that were actually performed by speakers of L were unconscious. So we see that, once again, the (typological) linguist is practicing the method of *Einführung*. Notice, however, that in this case the problem facing the linguist is clearly different from the problem that was once facing the speakers of L: his (‘global’) problem is to understand the shift $A > D$, whereas the speakers were confronted with a succession of (‘local’) problems which were, by hypothesis, solved by moving from A to B, from B to C, and from C to D. Notice also that in the present context it is literally the case that **understanding equals explanation**. Once Givón had made the shift ‘perfective marker’ > ‘subjunctive marker’ understandable to himself, then *eo ipso* (he thought that) he had explained it.

Considered in the concrete detail, linguistic changes are unpredictable. Considered at a higher level of abstraction, however, linguistic changes – and even successions of particular linguistic changes – can be more or less predictable. It can be argued that several ‘pathways of grammaticalization’ are to-

day known well enough to be in some sense predictable (cf. Givón 2001: 365). By contrast, the semantic changes investigated by **etymology** are typically rather unpredictable. Consider the following example, discussed by Anttila (1992): How is it possible that a person whose family name is Olenin acquires (in the Estonia of the 50's) the nickname 'Tipstu'? Taken globally, this change is certainly wholly incomprehensible. But analyzed into the following succession of local changes, it becomes wholly comprehensible:

*Olenin > Ostalin > Pik Stalina > Pikstu >
Tipstu*

One only needs to know that 'Pik Stalina' is the name of a mountain that was mentioned in the geography class of those schoolboys who invented the nickname. (This example was brought to Raimo Anttila's attention by the distinguished Estonian linguist Mati Hint.)

The analogy between Givón's example and Anttila's (and Hint's) example should be clear. It should also be clear that the method used in the two cases is the same, namely *Einfühlung*. And also in Anttila's case, a phenomenon is **explained** once it has been **understood**. Compared with Givón's example, Anttila's example is closer to the standard historical explanation in the sense that the explanation that has been achieved cannot be used as a basis for prediction (except at such a high level of abstraction that 'prediction' acquires a nearly metaphorical sense). In recent years, representatives of cognitive linguistics have popularized the concept of unpredictable, 'associative' logic that generally underlies successions of semantic changes, but within etymology this concept has been known all the time (cf. for a rich illustration, cf. Anttila 2000).

Up to now, I have identified typological-

functionalist and/or etymological explanation with understanding based on *Einfühlung*. But what kind of explanation is it, *au juste*? The quotation from Gibson (1976) already hinted at the answer: it is **rational explanation**. This is a concept that I have treated on some 300-odd pages in Itkonen (1983). Newton-Smith (1981: 241) sums it up as follows: "The explanation works by displaying the action as being what [the agent] believed to be the best means to the goal (or the means most likely to realize the goal)."

Several objections can be, and have been, raised against the concept of rational explanation. Here I mention only the most familiar one: this concept presupposes the existence of **unconscious** rationality, but – it is claimed – rationality requires **conscious** deliberation. When spelled out, this objection amounts to the claim that people can have no unconscious goals or beliefs (cf. Searle 1992). Because such a view eliminates – *inter alia* – the mental life of small children, I submit that it should not be taken too seriously. It is better to stick to the following rather traditional position: "There may be – perhaps there must be – some end of this hierarchy of rational decisions. But the end is not in sight. For all we know, cognition is saturated with rationality thorough and thorough" (Fodor 1975: 173).

All objections against rational explanations that I am aware of are of 'philosophical' nature (in the pejorative sense of this word), and they are really beside the point. Why? Because this is the type of explanation that is being used as a matter of fact. Linguists may have all kinds of wild and aberrant notions about what they are doing, but what they are doing **as a matter of fact** is apply the concept of rational explanation in all domains of 'causal' linguistics, i.e. in psycholinguistics, sociolinguistics, diachro-

nic linguistics, and linguistic typology. I documented this claim in Itkonen (1983), and today, equipped with the experience that I have accumulated during the last 20 years, I could document it much better and in much greater (even excruciating) detail.

What about linguistic typology? In my 1983 book I considered the following implicational universal: 'In all languages, if the transitive subjects have overt case-marking, then the intransitive subjects have it too.' This is the (sketch of) **rational explanation** that I proposed for this universal:

"People have a universal need to make distinctions between things that are important to them. Communication is important, and therefore linguistic units used in communication are important too. ... It is immediately evident that there is a greater need for differentiation in case {N, N, V} than in case {N, V}. Now, if the required differentiation is to be achieved by overt case-marking, and not (only) by word order, then it goes without saying that there is a greater need to have {N-S, N-O, V} than to have {N-S, V}. And because greater needs are by definition satisfied before smaller ones, it follows that if a language has {N-S, V}, we can 'predict' that it also has {N-S, N-O, V}, but not vice versa" (Itkonen 1983: 216–217).

Next, let us consider a closely related example (where X = the only argument of the intransitive sentence, Ag = agent-expression, and Pat = patient-expression). There are five logical possibilities of **argument-marking** (which is to be understood as a more comprehensive concept than 'case-marking'), but – apart from a few exceptions – only two of them are realized in the world's languages:

- 1) X = Ag ≠ Pat (NOM-ACC system)
- 2) X = Pat ≠ Ag (ERG-ABS system)
- 3) X ≠ Ag ≠ Pat
- 4) X = Ag = Pat
- 5) X ≠ Ag = Pat

What is the **explanation** of this observed distribution of argument-marking? It is rather similar to the one that was given above to the differential marking of subjects in intransitive vs. transitive sentences. It is clear that, if the **goal** of communication is mutual understanding, then – if there has for instance been a killing – we must be able to tell and to be told who killed and who was killed; and the only **means** to achieve this goal is to distinguish in some way between Ag and Pat. Therefore any language with the $Y \neq Z$ distinction is **rational** in the literal sense of this word; and this includes the types 1), 2), and 3) above. By the same token, any language with the equality $Ag = Pat$, i.e. any language of the type 4) or 5), is **irrational**; and this explains why there are no such languages. (Notice that a language with $Ag = Pat$ would be a language in which it is impossible, even in principle, to differentiate between who kills and who is killed.)

Next, let us concentrate on the rational language-types 1), 2), and 3). Once a language has made the $Ag \neq Pat$ distinction, it still has a choice concerning X: should X be the same as either Ag or Pat, or should it have a marking of its own? The former alternative is clearly the more **economical** one: it clearly saves some mental energy if X is assimilated either to Ag (resulting in the NOM-ACC system) or to Pat (resulting in the ERG-ABS system). Again, this **explains** why the types 1) and 2) are nearly ubiquitous in the world's languages. But since the type 3) too satisfies the requirements of basic communicative rationality, some languages can afford to adopt it although, because of its less economical character, it must qualify as slightly less rational than the types 1) and 2). For instance, in Diyari (= South Australia) the following word-classes exemplify the type 3): dual and plural common nouns, female proper nouns, singular per-

sonal pronouns, and 3rd person dual and plural personal pronouns (cf. Austin 1980).

In the light of the preceding discussion, it is easy to agree with Comrie's (1981: 119) following assessment concerning the distribution of argument marking and its explanation:

"Whatever may be the value of functional explanations in general in linguistics and language universals in particular, here we do have a good example where the predictions of the functional approach appear to fit in very well with the observed distribution of case [sic] marking systems across the languages of the world."

Finally, I should note that there is one genuine objection that can be raised against the concept of rational explanation, as presented up to now. The instances of rational explanation seem rather **atomistic**. Should they not be integrated into some sort of overarching system? Indeed they should. This is the next step that has to be taken. At this stage, particular rational explanations will be subsumed under the concept of **pattern explanation**. To put it roughly, a set of phenomena are 'pattern-explained' when – instead of just being disconnected items of a list – they are shown to constitute a **coherent whole**. This concept was originally developed for 'behavioral' or social sciences by Kaplan (1964) and Diesing (1972). It has been applied to linguistics in general and to etymology in particular by Itkonen (1983: 35–38, 205–206) and Anttila (1989, 1995). Notice also that, just as 'rational' does not mean here 'what is rational' but 'what **seems** rational to the agent', so in the present context the notion of 'coherent whole' does not entail 'maximally coherent in an absolute sense', but 'as coherent as the evidence permits'. It is well known that unconscious rationality operates locally, not globally: a ra-

tional change in one subsystem may have non-rational consequences for some other subsystem. This aspect of linguistic change has been emphasized e.g. by Vennemann (1988).

Kaplan (1964) and Diesing (1972) contrast 'horizontal' pattern explanations with 'vertical' explanations, which they – in conformity with the received view in the 60's and 70's – identify with the 'deductive-nomological' or 'covering-law' model of explanation and take to represent such deterministic areas of natural science as Newtonian mechanics (cf. Itkonen 1978: 2–16). Rescher (1979) regards science as 'cognitive systematization', and he too makes a distinction between a 'vertical' approach and a 'horizontal' one, but in a somewhat different sense. For him, the 'vertical' approach equals traditional axiomatics (rather than nomological explanation), while the 'horizontal' approach equals a 'coherentist' network-model of cognitive systematization. As long as such distinctions are maintained, it is obvious that pattern explanation is non-axiomatic and non-nomological; thus, it remains clearly distinct from natural-science explanations, as argued in Itkonen (1983). If, with the waning of the deductive-nomological model, the emphasis is shifted onto 'explanatory unification' (cf. Kitcher 1998), however, then it is possible to accept the basic similarity between pattern explanations and natural-science explanation, albeit at a quite high level of abstraction. – These issues cannot be adequately discussed in the present context, but they surely will need to be so discussed in some other context.

In what precedes, I have used such reifying expressions as 'the language L chooses the alternative A'. Obviously, expressions about what a language L does must sooner or later be translated into expressions about what speakers of L do (or forebear to do). From

the perspective of the language-system, particular speakers actually **do** very little: first, they learn the language, and second, while speaking and being spoken to, they primarily keep the language as it is and secondarily participate at, or ‘support’, some changes. What is, then, their contribution from the standpoint of rationality? It is the fact that they **forebear to disrupt** the rationality of language. More precisely, particular speakers may act as irrationally as they wish, but their actions are of no consequence for the language as whole. This is guaranteed by **social control**:

“The collective aspect [of linguistic change] is provided by **social control**, which manifests itself in the fact that only innovations not exceeding certain rather strict limits have a chance of being accepted. [...] The linguistic community could be said to act as a ‘rationality filter’ on innovations [...]” (Itkonen 1983: 211, emphasis in the original; on the role of social control, cf. also Itkonen 1978, Sect. 5.4 ‘Rules of language and social control’).

Now I have presented and defended my thesis about the importance of *Einführung*. Given that *Einführung* is one of the basic concepts of the **hermeneutic** philosophy, I have also reasserted the relevance of the hermeneutic point of view that was a central concern in Itkonen (1978) (cf. also Itkonen 1993). Therefore, I could stop at this point. Yet I will not do so. Why? It is possible for someone to accept everything that I have said so far and still to claim that all this is preliminary to the real ‘scientific’ explanation. Therefore, I now proceed to claim that what I have said is all that there is to be said, at least in the present state of our knowledge. To be sure, better rational explanations, integrated into more comprehensive pattern explanations, will be given in the foreseeable future. But no ‘more scientific’ types of explanation will be invented, not in the fore-

seeable future, and – most probably – not ever. Prima facie, making such a prognosis may seem to betray a lack of historical perspective. Therefore, I would like to point out that for some 20 years now I have been mulling over some 2’400 years of linguistics (cf. Itkonen 1991, 2000); and if this does not give you (or rather, me) some historical perspective, then nothing will.

Haspelmath (1999) illustrates the two-stage approach which I just hinted at. First, he shows that purportedly **formalist** explanations (couched in the terminology of optimality theory) are in reality **functionalist** explanations in disguise, since they refer to such goals as ‘saving production energy’, ‘avoiding articulatory difficulties’, ‘eliminating threats to comprehensibility’, and ‘avoiding ambiguity’. But second, he proceeds to claim that functionalist explanations of this kind should be **reduced** to genuinely **biological** explanations.

In explaining why I think this research program is unfeasible, I shall repeat and summarize the argument of Itkonen (1999). Today, grammaticalization is generally understood as a process consisting of ‘reanalysis’ and ‘extension’. The former is an instance of **abduction** while the latter is an instance of (analogical) **generalization** (cf. Itkonen 2002b). Abduction and generalization are cognitive processes, ultimately serving the goal of problem-solving, which intelligent beings like humans **must** perform and non-intelligent beings like genes **cannot** perform. Or, as Cohen (1986: 125) has put it: “Hence no evolutionary change of any kind came about through the application of intelligence and knowledge to the solution of a problem. This was at the heart of Darwin’s idea.”

The proponent of biologism is faced with two unpleasant alternatives: either to abandon the insights of present-day diachronic

linguistics (i.e. to abandon the talk of abductions, generalizations, and the like) or to abandon the insights of evolutionary biology (i.e. to abandon the view of genes as non-intelligent beings). In practice, this leaves only the first alternative. Choosing it amounts to destroying diachronic linguistics as we know it. This alternative is clearly rather unpalatable. So why do the biologically-minded linguists (whose number seems to be increasing day by day) insist on choosing it anyway? Because they cannot think of any other way to heal their unbearable inferiority complex *vis-à-vis* the representatives of the 'hard sciences'.

For the sake of completeness, I mention one more alternative open to those who cannot resist the lure of biologism. Instead of literally reducing linguistics to biology, it is (or seems) possible to accept a non-reductionist analogy between linguistics and biology, namely an (alleged) analogy between **genes** and **memes**. (Notice that there is, at least in principle, a difference between reductionism and non-reductionism.) Dennett (1993 [1991]: 202), for instance, argues that "meme evolution is not just [...] a process that can be metaphorically described in terms of these evolutionary idioms, but a phenomenon that obeys the laws of natural selection exactly". But this is unconvincing. In a genuine analogy between A and B (e.g. between bird and fish), both A and B can be described independently and retain their inherent interest even if we take the analogy away; but if we subtract from memes their analogy with genes, nothing is left except the tautology 'ideas may or may not spread' (for discussion, cf. Aunger (ed.): 2000).

At an earlier stage of the discussion, I mentioned the concept of 'historical perspective'. Can we not invoke the history of linguistics to shed some additional light on the quandary into which biologism seems to have

landed at least some of our colleagues? We most certainly can. Consider the following quotation:

"A grammar of the language L is essentially a theory of L. Any scientific theory is based on a finite number of **observations**, and it seeks to relate the observed phenomena and to **predict** new phenomena by constructing **general laws** in terms of hypothetical constructs such as (in physics, for instance) 'mass' and 'electron'. Similarly, a grammar of English is based on a finite corpus of utterances (observations) and it will contain certain grammatical rules (laws) stated in terms of the particular phonemes, phrases, etc., of English (hypothetical constructs). These rules express structural relations among the sentences of the corpus and the indefinite number of sentences generated by the grammar beyond the corpus (predictions)" (Chomsky 1957: 49, emphasis added; originally in Chomsky 1955/1975: 77).

From the methodological point of view, this quotation may not be fully explicit, but it is easy to flesh it out. On p. 16 of Chomsky (1957) all statistical considerations are ruled out, so the 'general laws' mentioned in the quotation must be **deterministic** in character. In his class 1968–1969 Chomsky mentioned that he accepted philosophy of science as presented in Sheffler (1963); and Sheffler (1963) still assumed that Hempel & Oppenheim's original notion of **deductive-nomological explanation** is an adequate explication of deterministic explanation (cf. p. 46: "In sum, aside from the case of statistical premises, we have seen reason to retain the deductive pattern as a model for explanation").

My first public talks in the late 60's and early 70's had two main topics: the necessity of iconicity (or 'isomorphism') and the critique of the applicability of the deductive-nomological model in linguistics, and more specifically in grammar-writing. Concen-

trating on the latter, it is interesting to note that in general I met with vehement opposition, from generativists and non-generativists alike. If I am not mistaken, the vehemence has subdued, to some extent, during those 30 years that have elapsed since then. It is no longer quite as fashionable as it used to be to claim that writing a grammar of English amounts to explaining and predicting observable, spatio-temporal events on the model of Newtonian mechanics. What is fashionable nowadays, is to pretend that what we are doing is either synchronic neurology or diachronic (= evolutionary) biology (cf. above).

Of course, it cannot be cogently argued that since Newton was a wrong model, Darwin too will be a wrong model. But what can be rather cogently argued, I think, is the following. The **impulse** to use Darwin is just as unjustified as was the impulse to use Newton. What, then, is the nature of this impulse? It is, first, to feel hopelessly inferior to the representatives of the 'hard sciences' and, second, to think that one can get rid of this harrowing feeling by blindly imitating one's betters (or those who one considers such), come what may.

Let us add one more qualification. That one should abandon Newton and Darwin as models does not mean that one should abandon scientific rigour. However, I do think that such areas of today's linguistics as 'conversation analysis', 'critical linguistics', and 'qualitative sociolinguistics' are unconscionably sorely lacking in scientific rigour; and for my own small part, I have tried to remedy this situation with Itkonen (2003). What I have been saying may be summarized as follows. Description must be as rigorous and scientific as possible, and description must be adequate to its subject matter. Should these two goals ever come into conflict, it is the latter which overrides the former.

POSTSCRIPT

Comments by Urpo Nikanne have made me realize that the term 'rational explanation' may still retain unfortunate connotations. To get rid of such connotations once and for all, it is good to emphasize that rational explanation applies not only to **human** behavior, whether rational or irrational, but also to **animal** behavior, e.g. the behavior of rats. About to enter into a discussion of experiments on rats, Dickinson (1988) gives the following general definitions:

"An **intentional** account of behavior is justified if that behavior can be shown to be dependent on, in the sense of being a **rational** consequence of, a set of beliefs and desires about the world" (Dickinson 1988: 307; emphasis added).

"To explain an action in terms of the agent's [e.g. the rat's] beliefs and desires is to demonstrate that the action is **rational** with respect to the content of those mental states [...]" (p. 310; emphasis added).

After reviewing the evidence, Dickinson (1988) reaches the following conclusion:

"So it turns out that instrumental behavior [by rats] will support anintentional characterization in terms of beliefs and desires after all. Not only do we perceive manifest intentionality in instrumental action, its goal-directedness does depend upon experiencing the evidence that would support a belief about the consequences of the action [...]" (Dickinson 1988: 321).

In the main body of his text, Dickinson remains agnostic as to the full ontological reality of such mental states as 'beliefs' and 'desires' that he has to postulate in order to explain behavior by rats. In conclusion, however, he gives up this attitude:

“Finally, it would be disingenuous if I did not acknowledge that the common intentionality shown by my own actions and those of certain other animals leads me to attribute mental states to these animals” (Dickinson 1988: 323).

The nature of ‘beliefs’ postulated by Dickinson (1988) is further specified by Thinus-Blanc (1988):

“I shall consider some spatial behaviors, in animals, that cannot be explained (at least at present) without referring to **cognitive maps**” (Thinus-Blanc 1988: 372; emphasis added).

Such maps are mental representations that emerge when an animal gets acquainted with its environment: “exploration is a cognitive activity that leads to the constitution of maps or internal models of the investigated situations” (p. 389). Having a cognitive map equals having a belief about the environment; and such beliefs are crucially involved in attempted (rational) explanations of behavior by, e.g., rats or hamsters:

“The choice of a new **solution** involves covering a part of the field never experienced before, and this choice represents the **best means** of getting the food as quickly as possible” (Thinus-Blanc 1988: 382; emphasis added).

Once again, the action to be rationally explained is conceptualized as a result of problem-solving activity.

It is not without interest to note that the position that I am advocating here was endorsed also by Darwin, but – *Nota Bene* – by Darwin qua psychologist, not by Darwin qua evolutionary biologist:

“Darwin viewed metaphysical objections to extending human mental qualities to the animals as ‘arrogance’. [...] The whole point of Darwin’s position was to indicate homological resemblance [i.e. analogy] between human and

animal behavior, and it followed that it was no more absurd to speak of a higher mammal showing fear, reasoning power or pleasure than to call the structure on the end of a chimpanzee’s forelimb a hand. The difference was one of degree not of kind. There was one continuous ‘thinking principle’ throughout the animals which Darwin viewed as being contingent on the presence of a nervous system [...]” (Howard 1982: 66–67).

Popper once said that there is only a difference of degree between Einstein and an amoeba. My position is less extreme: there is only a difference of degree between Einstein and a rat. Why? – Because the behavior of both admits of rational explanation.

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EINFÜHLUNG: KIELITYPOLOGIAN PERUSMETODI*Esa Itkonen**Yleinen kielitiede, Turun yliopisto*

Tämä artikkeli käsittelee kielitypologian metodiikkaa. Perusväite on, että rationaalinen selitys on ainoa todellinen tapa selittää kielitypologisia ilmiöitä. Kieli voidaan ymmärtää välineeksi, jolla puhuja pyrkii saavuttamaan tavoitteensa. Vain rationaalinen selitystapa voi paljastaa, kuinka kielelliset muodot kielen funktiota palvelee. Tällainen rationaalinen selitys on ainoa tapa, jolla kielellisiä ilmiöitä voidaan laajasti ymmärtää. Tämän lisäksi, vaikka yksittäiset kielen muutokset ovat ennustamattomia, ne voidaan nähdä osana suurempaa kokonaisuutta ja näin ymmärtää – ja siten selittää – rationaalisesti. Metodologiselta kannalta rationaalinen selitys on keino ymmärtää kielen ilmiöitä pyrkimättä väärällä tavalla matkimaan kovien tieteiden metodeja – ja kuitenkin säilyttää selitykset eksplisiittisinä ja teoreettisesti tiukkoina. Artikkelin käsittelee rationaalisen selityksen luonnetta useiden esimerkkien valossa.

Avainsanat: kielitypologia, selittäminen, rationaalinen selitys