

Trends in Linguistics

Studies and Monographs 213

Editors

Walter Bisang

(main editor for this volume)

Hans Henrich Hock

Werner Winter

De Gruyter Mouton

Language Usage and Language Structure

Edited by

Kasper Boye

Elisabeth Engberg-Pedersen

De Gruyter Mouton

pp. 45-71

ISBN 978-3-11-021917-3
e-ISBN 978-3-11-021918-0
ISSN 1861-4302

Library of Congress Cataloging-in-Publication Data

Language usage and language structure / edited by Kasper Boye,
Elisabeth Engberg-Pedersen.

p. cm. – (Trends in linguistics : Studies and monographs ; v. 213)

Includes bibliographical references and index.

ISBN 978-3-11-021917-3 (hardcover : alk. paper)

1. Structural linguistics. 2. Language and languages – Usage.

I. Boye, Kasper, 1972– II. Engberg-Pedersen, Elisabeth, 1952–

P146.L368 2010

410.1'8–dc22

2009051323

Bibliographic information published by the Deutsche Nationalbibliothek

The Deutsche Nationalbibliothek lists this publication in the Deutsche Nationalbibliografie;
detailed bibliographic data are available in the Internet at <http://dnb.d-nb.de>.

© 2010 Walter de Gruyter GmbH & Co. KG, Berlin/New York

Printing: Hubert & Co. GmbH & Co. KG, Göttingen

∞ Printed on acid-free paper

Printed in Germany

www.degruyter.com

Usage, structure, scientific explanation, and the role of abstraction, by linguists and by language users

Arie Verhagen

1. Complementation and scientific argumentation¹

1.1. A short history of recent thinking on complementation

As noted by Boye and Harder (2007), the analysis of usage and structure of linguistic constructs of the type *John thinks that grammar is dull*, i.e. involving a finite complement clause, has become, over the last ten years or so, a central topic in the debate on the precise relationship between usage and structure. One reason why precisely this topic is worth debating so much is that complementation constructs belong to the prototype of the central structural-syntactic concept of “subordination” (*that grammar is dull* is considered a straightforward case of one clause being subordinated to another (*John thinks X*, *X* being filled by another clause)). Three lines of investigation of actual language use first developed independently, and each led to the conclusion that complements are not really “subordinate”, at least not in the full sense of the word, to their matrix clauses.

The first line concerned conversation. An early publication, already indicating the direction of the consequences of the research quite clearly, was Thompson and Mulac (1991), which concludes that the distinction between matrix clause and complement clause is subject to “erosion” in conversational English. Arguably the most important recent result is Thompson (2002), which claims that the large majority of complementation constructs in (adult) spontaneous conversation does not involve subordination, but rather reusable “fragments” of language which express an “epistemic/evidential/evaluative” (“e/e/e”) stance of the speaker towards what is expressed in the complement clause.

The second line focused on language processing, esp. in text production and text understanding. Using psycholinguistic methods and Dutch data, Schilperoord (1996) showed that a combination of one complement clause and its matrix clause is indistinguishable in the production process from a typical simplex clause, i.e. it is produced as a unit (while subsequent complement clauses to

the same matrix are produced as separate units). Schilperoord and Verhagen (1998) and Verhagen (2001) connected this phenomenon to the role of complementation constructs in the structure of texts (cf. Mann and Thompson 1988),² concluding that matrix clauses are generally a kind of perspectival operator on the content or “force” of the complement clause, which has the consequence that a matrix of a complement is never conceptually independent, in contrast to the “main” clauses of clausal adjuncts.

The third line of research concerned language acquisition. Following up on a longer tradition in acquisition research, Diessel and Tomasello (2001) provided strong evidence that the large majority of children’s early complementation constructs consist of fixed expressions, especially *I think, look, I guess*, and a few other ones, which are attached to other clauses as markers of subjective stance and as interactional cues. Besides these, there are a few other early cases that look like complementation, but they are arguably manifestations of independent constructions. Thus, children do not start to make complement-like utterances by combining two clauses that could be produced independently; there is no general rule for structural subordination in young children’s linguistic competence. Utterances that would allow for an analysis as a complement being embedded in a matrix clause in children’s speech, are actually the product of a number of different processes, a large part involving fixed expressions functioning as modifiers of what the child is communicating. Any general rule for complementation in adults thus has to be the result of later development, not something that is built into the capacity for language at birth. In fact, the claim can be said to be: what Diessel and Tomasello (2001) found in young children’s speech actually mirrors adult usage rather accurately.

Although, as I said, these lines of investigation were initiated and developed independently, they were not totally isolated from each other. Peter Harder wrote a review of Schilperoord (1996) (Harder 1997); Diessel and Tomasello (2001) and Thompson (2002) refer to each other; the latter also mentions Verhagen (2001). Attempts to integrate these ideas and findings were also undertaken more or less at the same time and to a considerable extent independently, in Boye and Harder (2007) and in Verhagen (2005/2007), but once again, these refer to each other, too. Moreover, each of these latter two publications also aimed at extending the new insights to include instances of use that, although a minority, do not lend themselves so readily to the view that they would consist of combinations of a (complement) clause with a more or less fixed expression indicating the speaker’s stance, and not involving subordination at all. To this end, both approaches used some concept of a cline. Boye and Harder distinguish between “lexical” and “grammatical” patterns of complementation, allowing gradual change, through grammaticalization, from the first type to the latter. I

used the construction grammar concept of a network, with specific, relatively fixed phrases located at the “lower” level of the network, but connected to more and more abstract templates (correlated with type frequency) at “higher” levels, with the more specific templates having special properties in addition to, and possibly contradicting, the ones inherited from the more abstract templates; *both* of these may (especially in adult, written language use) productively license new expressions.

Although these approaches are much in the same spirit, they are not, I think, completely identical, at least not conceptually. Not surprisingly, I prefer the network approach, one reason precisely being that it avoids the distinction between “lexical” and “grammatical”, which easily gives rise to the idea of the difference being one of strictly distinguishable categories, rather than a continuum, although the text of Boye and Harder makes it quite clear that they also intend it as a cline. Still, I assume, or at least hope, that what I have to say will translate relatively easily into terms that may come more naturally to Peter Harder. What is crucial for my purpose now, is that these two approaches are indeed aiming at integration and extension, i.e. at developing a single, coherent, and consistent conceptual framework that allows apparently contradictory views on the phenomena to be combined.

1.2. Structure and usage, emergence and reduction, skyhooks and cranes

In the present volume, Fritz Newmeyer provides a new contribution to the debate. As far as I know, it is the first contribution from a formal linguistics side, and as such already an important and welcome step, given the central character that complementation plays in all approaches to grammar. Moreover, Newmeyer uses corpus data as his empirical basis and in that way helps to bridge the gap that otherwise often emerges in debates between formal and functional linguists and that easily blocks any further progress: disagreement on what constitutes the relevant set of facts. Thirdly, his discussion reveals a number of new aspects of complementation that are interesting in themselves, and also relevant to the discussion on the relation between usage and structure (I will return to some of these below). Thus, it is a valuable contribution that can help advance our understanding of the relation between usage and structure in this central domain of grammar.

However, Newmeyer himself uses his discussion as a basis for something different, viz. to claim that usage facts such as those considered by the studies mentioned above, hardly shed any light on issues of grammatical analysis. In

the end, he agrees with usage based linguists that (at least) “frequency is an important factor leading to the shaping and reshaping of grammar”, i.e. usage is recognized as at least one causal factor in the explanation of linguistic structure. This is an important piece of common ground that makes a fruitful discussion between Newmeyer and functional linguists in principle worthwhile. However, he adds: “but appeals to frequency should never be used as a substitute for careful grammatical analysis”, and grammatical analysis is to be based on observations of another kind of phenomena than linguistic usage. This is the methodological position of the “autonomy” of grammar, that makes a debate between the formal linguist Newmeyer and functionalists much more problematic again, as I will demonstrate.

I will try to show that one can agree fully with Newmeyer (and, by implication, with Boye and Harder, with whom Newmeyer agrees) on some form of autonomy of a grammatical (sub)system, i.e. a set of (conventional) rules for formulating and using complementation constructs in a particular language (say English), and at the same time fully maintain the idea that structural phenomena can be reduced to properties at the level of usage. This may sound more surprising than it actually is, but the logic is one that is generally found in all kinds of scientific endeavors. To take an example from physics: the structural difference between solids and liquids does not exist at the atomic level (no single atom is either liquid or solid), but it can be explained in terms of properties of atoms, and what happens when lots of them are close together in certain environments. So the higher level of structure has properties that do not exist at the lower level. But it also constrains what can happen to any lower-level element (i.e. atom) that is contained in it – the structure of a crystal determines where an atom can(not) move, which electrons can move from one atom to another, etc. That is, although higher level phenomena can be completely explained in terms of lower level ones, higher level phenomena, once they have come into existence, become a new, autonomous explanatory factor, not only for higher level phenomena themselves but also for some lower level ones. Examples abound in other sciences as well. Natural scientists have no problem recognizing the autonomy of chemical structure or biological systems relative to the quantum properties of electrons or the chemical properties of complex carbon compounds, respectively, while maintaining the idea that the former are ultimately reducible to the latter. Or to take an example closer to home: economists have no problem recognizing the autonomy of a monetary system once it has come into existence – including its power to affect the “real” economy, as shown by the present day credit crisis – while maintaining the idea that it is ultimately based on a constellation of more elementary processes of production, consumption, and trading of goods.

In short, reduction (to more basic units and processes) and emergence (of properties, at a higher level of organization, that do not exist at a lower level) are generally two sides of the same scientific coin. The really relevant question is: What does one propose as the way to get from the lower to the higher level of organization? In the words of Daniel Dennett: “We must distinguish reductionism, which is in general a good thing, from *greedy reductionism*, which is not. The difference, in the context of [evolutionary] theory, is simple: greedy reductionists think that everything can be explained without cranes [=subprocesses existing in the lower level phenomena that, in certain circumstances, produce higher levels of organization AV]; good reductionists think that everything can be explained without skyhooks.” (Dennett 1995: 81/2). Skyhooks are special mechanisms, not themselves based in lower level phenomena, invoked to explain properties at a higher level.

Clearly, invoking a skyhook does not explain a scientific problem, but rather gives it a name. In the history of science, progress often precisely consisted in the discovery that certain concepts were skyhooks, and that they could be dispensed with. Examples are such discoveries as that we can do *without* a special kind of substance called “caloric” in thermodynamics, a special kind of “vital force” in biology, etc., and actually *explain* processes of heating/cooling (including phase transitions), and life. Such a recognition may involve the discovery of a crane at the same time: the way in which a lower level process produces new properties not originally present at the lower level itself (kinetic energy of molecules explaining temperature, the biochemistry of organic molecules explaining life processes, etc.). But even if it does not, the better scientific strategy is to consider the problem at hand as in need of further investigation, not to invoke an unknown element or process, and give it a name that suggests it provides an explanation.

In this perspective, one might wonder why there should be a debate of this kind in linguistics at all. Communication and processing – i.e. *usage* of signals by senders to influence other organisms on the one hand, and to make inferences about other organisms by receivers on the other – have all the properties to make them prime candidates for the status of the elementary level of phenomena with respect to which human language should count as a higher level of organization (in an evolutionary perspective, communication and the processing of signals are certainly more widespread than, and prior to, language). So shouldn't it be the default assumption that a linguistic system comes into existence as such a higher level of organization on the basis of communicative and cognitive processes, and at the same time that, once in place, it also acquires certain properties that are crucial for understanding how it works and that do not exist at the more elementary level? There is all the more reason to ask this question

(as a rhetorical one), since there are relatively well understood processes that are perfectly suited as mechanisms, already in existence at a lower level than that of linguistic structure, to cause the emergence of higher level structures: routinization (an individual psychological process) and conventionalization (a social one – especially relevant in our own species, given its capacity for empathy and cooperation);³ thus they are definitely cranes, not skyhooks. These processes also provide an immediate basis for explaining the emergence of a certain degree of autonomy: routines are executed automatically whenever certain simple criteria are met, and they “block” the development and performance of non-routinized procedures even if the routine might, in a specific circumstance, be less optimal than some novel course of action. In the same spirit, conventions, based on mutual expectations in a population of individuals, make themselves the preferred procedures to follow by providing a guarantee for at least *some* communicative success within the population, independently of the answer to the question what would be the “optimal” form of communication when only considering the communicative problem at hand. All of this is, in my mind, so straightforward that the continuing debate in linguistics – with Newmeyer’s contribution as the most recent testimony – raises the question: “What is the point?”.

2. The danger of special evidence for structure: jumping to abstractions

2.1. Evidence: usage vs. “tests”

The point seems to be that some linguists, including Newmeyer, only want to go a (small?) part of the way of emergence and reduction; it appears he does not want to abandon all skyhooks. On the other hand, it is also true that Thompson puts less emphasis on the emergence of higher level structure in some of her publications than in other ones, and to the extent that this evokes the conclusion that no abstract structure emerges at all from usage, this may and should invite the same diagnosis, albeit “from the other end”: perhaps she does not recognize the need of any cranes for explaining linguistic structure.⁴ In any case, it appears that Newmeyer does not believe in the scientific ideal of producing an explanation of linguistic structure based *completely* on the more elementary level of usage phenomena (communication and processing), i.e. without invoking some idea of “structure” that does not ultimately come from usage, but is essentially independent (hence a skyhook, in my view). Rather,

he ultimately prefers to conceive of usage and structure as if they were two kinds of phenomena in parallel with many-to-many-connections between them, rather than as phenomena on different *levels*, as Boye and Harder do (despite his explicit endorsement of their paper). This is more evident in some places of his chapter than in others; one where it is relatively clear, for example, is section 3.2. Here he puts “fragments” and “constructions” in opposition to each other, while the general usage-based view would hold that they are just more and less specific instances (i.e. with different levels of generality) in a continuum of basically the same kind of things: stored linguistic units used in processing and production. Several remarks in the text and the footnotes of this section present the same difference as if it were one between “rules” and “lists”, again in such a way that one cannot appear as a natural extension of the other, as (many) usage-based approaches would have it.

As for Thompson, it looks as if Newmeyer attributes to her a “greedy reductionist” approach, when he criticizes her for assuming only “fragments” and not any more general (abstract) constructions. If this is indeed Thompson’s position, then the criticism would be justified (though I would diagnose it somewhat differently). However, I do not think things are quite so clear. Notice the wording of passage he quotes from Fox and Thompson (2007: 318): “small-domain, even sometimes lexically specific, formats which exist in a dynamic family-resemblance relationship to one another that can be modeled in terms of a continuum”. This contains the scalar modifier *even* with *lexically specific*, so the point seems to be that the authors primarily defend the relevance of “small domain” as opposed to “large domain” – and a small domain still involves a generalization, at least that is the only way I can read it; notice also the appeal to the notion of *continuum*. The point rather seems to be the proposal to replace the idea of a *single* uniform construction, with that of a *family* of patterns jointly characterizing relative clause phenomena. Cf. also the quite explicit statement at the end of the conclusion: “Our findings suggest that speakers make use of a wide range of practices – some entirely pre-stored, others partially pre-stored and partially composed based on low-level formats, others not at all pre-stored – and this diversity must be acknowledged and described in our syntactic theories. Focusing too heavily on one practice – whether it is the pre-stored, monoclausal end or the entirely compositional end [–] misses the diversity that underlies the practices by which speakers use ORCs in conversation.” (Fox and Thompson 2007: 319).⁵

As will be(come) clear, I also believe that recognizing the emergence of abstract structures is a crucial part of the story of language, but the degree of abstractness is, indeed, easily overrated, with sometimes serious consequences. Still, the main relevant point here is that fragments and more abstract construc-

tions can be seen as points on a single continuum, and should not be put in opposition to each other.

The basic independence of structure in Newmeyer's view is closely connected to his idea that the most relevant evidence for structural analysis does not consist of usage facts such as frequency or processing data, but rather in the application of certain metalinguistic "tests", of the type: if a constituent X allows replacement by a constituent of type Y, then it is itself also of type Y; or: if a sentence displays an anaphoric relationship between constituents A and B (what Newmeyer calls "binding relations"), then it has a structure of type Z – and others like these. This is a well known and much used method in grammatical research, but that fact does not, of course, make it completely unproblematic. On the contrary, the frequency of use of the method may easily blind us to its dangers. One point that is relevant in this connection, is that the logic of using the outcome of tests as determinants for grammatical analysis invites abstractness. To say that the structure of some sentence is the same as that of another sentence with an anaphor in it, implies abstracting away from possible differences related to the use or non-use of an anaphor. To replace a part X in a sentence by another part Y and, if the result is still good English, thereby determine the structure of the original sentence, implies abstracting away from any structural and functional differences that might be related to the difference between X and Y, etcetera. Now there is nothing wrong in principle with this kind of tests; all evidence is in principle equally welcome, of course. Moreover, they provide a component of the most basic procedure of grammatical analysis, viz. distributional analysis (what are the linguistic environments – encompassing both form and meaning – in which an item occurs, and which items are compatible with what in a certain environment? Cf. Croft 2001; this volume). But when used entirely on its own, this kind of evidence has a certain weakness, because tests in principle only shift the burden of proof: that a particular test may be used to answer a question on structure is something that itself requires justification. Ultimately, it must be possible to justify the use of a test in *another* way than with yet another test; a grammatical analysis based *solely* on metalinguistic tests remains fundamentally shaky, for principled reasons.

An additional danger is that the logic of this method allies easily with the scientific pursuit for generalization and abstractness (finding as few "laws" as possible to account for as many data as possible), which may lead to (mis)taking small generalizations for grand ones (cf. Croft's (this volume, section 2.6) "unwarranted generality assumption"). I think it is telling, in this perspective, that Newmeyer is especially perplexed by the lack of attention for abstractness in Thompson's analysis. While one can in principle see his point (but see my comments above), we should not, as it were with a swing of the pendulum, identify

structure with maximal abstractness. However, this is precisely what happens in Newmeyer's chapter. That is why it provides an excellent opportunity to fill in this particular pitfall for grammatical analysis.

2.2. An illustration: Is "that" a marker of subordination, or of something less abstract?

It is time for an example. In section 2.2 of his chapter, Newmeyer sets out to show that "complements are subordinate" (the title of the section). He starts by making a terminological distinction between "conversationally subordinate" and "syntactically subordinate", and then argues that evidence for the first is irrelevant for deciding on the second. Rather, evidence for the latter should come from distributional evidence of the "test" type mentioned above. The first point concerns the fact that (in English) a complement clause may be introduced by the element *that*, but this may also be left out. Newmeyer states "The complementizer *that* is uncontroversially a marker of subordination", and then goes on to observe that it is used in 16% of the cases in his corpus. Now, the default assumption should be, according to Newmeyer, that clauses with and without *that* have the same structure, and in any case he sees no discourse evidence to the contrary, so the 84% complement clauses without *that* are also structurally subordinate.

The way he presents it certainly gives the reader the impression that he considers this a very simple and straightforward argument. However, it may be uncontroversial that *that* is a marker of *complementation*, but this is not at all clear for (structural) *subordination*. The point is that the latter is a much more abstract notion, expressing the idea of a clause being a proper subpart of a phrase (cf. Newmeyer's schematic representation (2)). This concept generalizes over several types of clauses connected to others; some of these are marked by *that* or "zero", but there also many other types, characterized by distinct conjunctions such as *if*, *whether*, *because*, *since*, and many more. Thus the notion "subordination" abstracts away from *all* of the differences between these conjunctions and the clause types they mark, while "complementation", although an abstraction itself, refers to a more restricted set of cases. Thus, *that* is most certainly *not* a general marker for all clauses considered "subordinate" in English. It is of course true that the conjunctions mentioned all mark clause combinations, but that it is empirically valid to generalize over these different and differently marked patterns of clause combinations as a unified and linguistically significant category, is something that requires independent demonstration. In other words, only if one already *assumes* that complementation is practically the most typical

case of (structural) subordination, could the observation about *that* count as an argument for the latter.

In fact, for each conjunction, the question is what properties of the pattern involved are best described and explained at which level of abstraction: does some property of, say, an *if*-clause correlate with (is it perhaps a consequence of) its being specifically an *if*-clause, or with its being subordinate? The same holds for *that* as an – indeed – uncontroversial marker of complementation: it is not a priori known whether an alleged higher level of abstraction like subordination is involved in the explanation of any of the properties of the complementation pattern, and if so, of which ones – given the specificity of *that* for complementation, it provides no evidence (neither in favor of, nor against) the relevance of the more abstract notion of subordination.

What is happening here, is what I call “jumping to an abstraction”. The level for which the evidence is considered relevant is actually chosen higher than what is, upon closer scrutiny, warranted.

3. Abstraction after abstraction after abstraction, ...

3.1. Complements as arguments

The over-abstraction described in the previous section is not an isolated phenomenon. Before giving his own arguments in favor of the subordinate status of complements, Newmeyer addresses Thompson’s view that complement clauses should not be analyzed as arguments of the verbs of complement taking predicates, specifically not as “objects” of these predicates. Thompson had supported her view by pointing to a number of differences between complement clauses and phrases that constitute the prototype of direct objects: noun phrases with a lexical head (e.g. *his friends*, *a beautiful picture*, *this highly poisonous chemical*), functioning as the patient argument of a transitive verb (e.g. *betray*, *buy*, *destroy*). On the one hand Newmeyer disputes a number of Thompson’s specific claims, and on the other, he considers the logic of the argument “flawed”.⁶

The first point is another case of jumping to an abstraction. Thompson had suggested that certain verbs taking complement clauses do not take noun phrase objects, two of these verbs being *realize* and *wonder*. Newmeyer now produces counter-examples from his corpus with these (and some other) verbs: *I realized the seriousness of it* and *I wonder that myself*. For claims about the specific verbs, these examples provide compelling evidence, but do they also invalidate the claim about a much higher level of generality, viz. complement taking predicates in general? Certainly not immediately. They can only be presented as such under

the assumption that these few examples do indeed generalize to other cases, both noun phrases and complement taking predicates (CTPs), but this has to be made plausible, to say the least, and not simply assumed. But Newmeyer does not provide evidence to this effect.

The point is that it is actually very hard to substantiate combinability of CTPs with “noun phrases” *at that level of generality*. Very many CTPs can be combined with the personal and demonstrative pronouns *it*, *this*, and *that*, but not even all of them (in English, the convention is to say *I think so*, rather than [?]*I think it*, while the Dutch equivalent of the latter is fine). Some CTPs can also be combined with certain abstract nouns (cf. the example with *realize*), while others disallow this ([?]*I wonder the seriousness of it*), and yet other verbs allow concrete (*You promised me this book* vs. [?]*I realized/wondered this book*) or even animate noun phrases (*I know my neighbors well*). What should the criterion be for saying that CTPs can take “nominal objects” – again: at that level of abstraction? We could be very liberal: as soon as a CTP can take one element, say the demonstrative pronoun *that*, from the set of elements considered “nominal”, we say it can take (some) nominal objects; this is what is usually done both in traditional grammar and in (at least classic) generative linguistics. We could also be very strict: as long as some CTP cannot take *any* kind of noun phrase, we claim that “CTPs cannot take nominal objects”. Both positions would in fact be equally arbitrary, instances of what Croft (2001; this volume) has aptly called “methodological opportunism”. What makes them problematic is that they both try to formulate a relation at a very high level of abstraction: one between the entire class of CTPs and the entire class of nominal objects, whereas the reality of the distribution of linguistic elements in fact strongly suggests that the actual relations do not exist at that level of abstraction, but at various “in-between” levels, of specific (semantic and distributional) classes of CTPs, and specific (semantic and distributional) classes of “nominal” phrases. Stating these latter relations is what should really be the contents of the descriptive starting point of any linguistic analysis. This work has not been done yet, as far as I know, but I dare to predict that it will reveal much more complexity than a small set of general rules (something that should not actually surprise us, since we are dealing with phenomena that have been produced by evolutionary processes –both genetic and cultural).

3.2. Grammatical phenomena restricted to complements

Newmeyer’s positive arguments for the position that complement clauses are structurally subordinate are threefold:

- a) *that* is a marker of structural subordination and it is used in 16% of the cases in the corpus; the default assumption should be that clauses with and without *that* have the same structure, so the 84% complement clauses without *that* are also structurally subordinate;
- b) *each other* cannot be used as the subject of a (simplex) main clause, but it can as the subject of a complement clause (*uh joking that that each other are homosexual . . .*), which shows that the complement clause is not a main clause but subordinate;
- c) similarly, the mandative subjunctive can only be used in complement clauses (*my wife's always insisted that somebody else do it*), not in a (simplex) main clause, which shows that the complement clause is not a main clause but subordinate.

This suffices for Newmeyer to formulate the following as a kind of intermediate conclusion: “In a nutshell, whatever the discourse status of sentential complements may be, the evidence is that they are structurally subordinate to their CTP.”

I have already discussed argument a) in section 2.1 as a first illustration of jumping to an abstraction. One thing that I concluded at the end of that section was that it is not a priori known whether an alleged higher level of abstraction, such as subordination, is involved in the explanation of any of the properties of the complementation pattern, and if so, of which ones.

This point is also relevant for arguments b (*each other* as subject) and c (the mandative subjunctive). Concerning b), if we find utterances like *They all knew whether each other were thinking about them* and *They were all laughing because each other were joking about them*, then this will certainly constitute evidence that the possibility of *each other* as subject is a matter of the clause being subordinate –i.e. that subordination is the right level of abstraction to account for this phenomenon. But if we don't, then the evidence does not support this conclusion; then it only indicates that it is a possibility for complements. And if we were actually to find that *each other* as subject only occurs after the complementizer *that* (i.e. if we never find something like *uh joking each other are homosexual . . .*), then this could, of course, even be used to argue for a structural difference between complements with and without *that*. I refrain from drawing a conclusion about this specific point here,⁷ but what I do want to claim is that the phenomenon of *each other* occurring as subject as presented by Newmeyer does not yet *suffice* for the conclusion that the complement clauses involved are *subordinate* –it is jumping to too high a level of abstraction too soon.

A specific conclusion about the mandative subjunctive (argument c) is possible, though. As Newmeyer explicitly remarks, the occurrence of this phe-

nomenon is dependent on a specific set of predicates, and these are all complement taking verbs. That is, the phenomenon does not occur in other clauses than complements; a construct like (*My wife did not want to do it, because somebody else do it*) presumably is not English. Thus, this is not just jumping to a higher level of abstraction than justified by the data, it is clearly the *wrong* level. One may even use this distribution to propose a specific subconstruction of the class of complementation constructions. Newmeyer does not formulate the generalization, but what the relevant predicates have in common clearly is that they indicate directive speech acts (used to get the addressee to do or to think something); in fact, the use of the mandative subjunctive *imposes* such an interpretation: verbs like *suggest* and *propose*, as lexical items, have more uses than that of indicating a directive speech act (e.g. *I suggest that this is an independent construction*), but in the frame of a complement with an uninflected verb, they *have* to be taken as indicating a directive speech act (*I propose that it be recognized as such*).

At the same time, it is quite clear that the evidence adduced by Newmeyer shows that complementation is a special grammatical phenomenon. Specifically: it shows that it is incorrect to view the complement clause as identical to a simplex main clause, and the CTP as no more than a simple add-on that is otherwise inconsequential. On the contrary, it clearly supports the idea that a combination of CTP and complement clause has special properties, as a whole as well as in parts, that do not follow from the properties of clauses and CTPs as such: the whole is more than the sum of its parts. So to the extent that other studies have given the impression that the relationship is so simple, this evidence provides a welcome correction. Still, it does not suffice, in my view, to establish what Newmeyer claims for it: the cognitive reality of abstract structure independent of any functional considerations. I interpret this evidence as showing that the opposition between viewing complementation either as a combination of usage fragments –completely retaining their identity, and gaining nothing, in the combination– or as licensed by a general, abstract grammatical process of subordination, is a false dichotomy. Rather, this evidence supports the view that complementation is to a large extent an autonomous grammatical construction, “autonomous” here not in the sense of “independent of meaning or function”, but “(partly) independent of other linguistic items (words and grammatical constructions)”.

3.3. Different CTPs, and in different types of discourse

The arguments discussed so far were all presented by Newmeyer from his own initial assumption that a distinction can and must be made between “conversational” and “structural” subordination. He has also added a point about the relevance of certain data, and it is interesting enough to warrant a separate discussion. Summarizing again, it comes down to the following:

Thompson has explicitly excluded the CTP *say* and other verbs of communication from her analysis, but they are not at all rare; *say* is next in frequency to *think* and it also frequently exhibits the phenomenon of the omission of *that*. Yet, “[s]ince *say* and other verbs of communication do not express an e/e stance, there is no argument even in Thompson’s own terms that their complements are not structurally subordinate.”

Although this is not stated explicitly, the observations on frequency and similar grammatical behavior suggest that the structure of the CTP-complements of *think* and *say* should be considered similar, if not identical. So if the complements of verbs of communication must be considered subordinate (and this is obviously what Newmeyer is suggesting), then so must the complements of *think* (and other verbs of cognition), by analogical reasoning.

This point is better and more serious than any of the others discussed so far. However, the usage-based literature already contains some solutions to the challenge that it seems to pose. Several authors have observed that the grammatical similarity between *think* and *say* as complement taking expressions has a functional parallel. Boye and Harder (2007), for example, observe not only that for a number of languages, the translation equivalent of English *say* must by analyzed as “grammaticalized” in the same way as *I think*, they also write: “What is common to all these CTPs is that they express (or imply) epistemic meaning [...] –in particular, evidential meaning” (Boye and Harder 2007: 583). Notice the addition of “(or imply)” to this characterization as opposed to Newmeyer’s formulation, indicating that what is important in a functional grammatical generalization, are the *inferences* that an expression licenses, beyond what it may strictly speaking “express”.⁸ The speaker who says *X says/said Y* provides evidence for *Y* (as information coming from source *X*), and in principle endorses it, albeit as defeasible. Notice that for a speaker, expressing her own stance (*I think Y*) is a good way to induce a certain stance in the addressee since the speaker is a good source of information about her own thoughts, while using someone else’s stance to present something to an addressee is better done by *He says Y* (rather than *He thinks Y*), since speaking is a public rather than a private activity and thoughts are not directly accessible to others, so saying “says” provides stronger

support for the addressee to endorse the statement than “thinks”. The generalization is that in such cases, both *I think* and *He says* are used as stance-inducing elements and not as utterances that can be separately addressed, in Boye and Harder’s terminology. Interestingly, these authors also draw attention to a number of instances, mentioned in the literature, of the basic verb of communication in several languages having developed into a grammatical marker of evidentiality. This approach can also be straightforwardly applied to English *say*, which then no longer appears as something that is functionally totally different from a verb of cognition (basically *think*), but rather as a member (not completely identical) of the same family of expressions, structurally and functionally.

Thus, on the one hand Newmeyer is right in drawing attention to the fact that the frequency of use and the grammatical behavior of *say* as a CTP parallels that of *think*, and that this should have consequences for the account of CTPs. But on the other hand, he is wrong in jumping to the conclusion that the commonality must be that they govern structural subordination while their functions are radically different. There is a clear alternative, viz. making a *limited abstraction*, that also has the advantage of providing a basis for understanding parallel functional *and* grammatical features of the CTPs involved. At the same time, this does imply that *some* abstraction is being made, and that the claim that the specific function of *I think* and a few other expressions of mental states and processes is the function of complementation constructs *in general* (even in conversation) cannot be sustained – it would come down to mistaking a frequent and prototypical exemplar (specific case) for an entire category (involving some abstraction). To put it concisely: whereas Newmeyer too quickly jumps to abstractions that are too high, Thompson should allow at least some abstraction over parallel instances of use of different CTPs.

As I have indicated, the amount of abstraction necessary to include verbs of communication into the analysis is not all that enormous. An additional advantage of this view is that it allows for natural extension into the domain of written discourse. As I showed in Verhagen (2005, chapter 3), the speaker-based CTPs of the type *I think*, *I guess*, constitute a minority of the CTPs in newspaper texts at best. Still, it would be wrong to conclude that the majority of CTPs in these texts are not functionally similar to the e/e/e-type that is (at the least) more characteristic of conversation (granting the point of Boye and Harder that the CTP may more often be part of the speaker’s “point” than Thompson was allowing for), and that all that they would share with conversational CTPs could be a common abstract syntactic structure. On the contrary, the function of the large majority of CTPs in newspapers turns out to be a relatively moderate generalization of their conversational function. They mark and identify the perspectives from which the reader is invited to construe the contents of the complement

clauses, and just as in conversation, the latter consistently provide the “point” of the discourse segment at hand. An adult language user who has learned to produce and interpret sequences such as *The director expects that the problem will be solved by tomorrow, but others believe that it may take a bit longer* still takes the issue of when the problem might be solved as the point to consider, and the CTPs as indicators of the perspectives from which the point is presented, i.e. as a kind of evidential markers, to steer his own considerations in one direction or another (cf. Verhagen 2005: 94–98; Verhagen 2006: 327–331, for arguments supporting this view, in terms of discourse coherence). Thus, this function is a more abstract variant of the e/e-kind of marking Thompson is considering, in that the perspectives that can be expressed may extend beyond the speech situation itself –but there is certainly a “family resemblance”, to say the least.⁹

In written discourse, the form of complementation constructions may also be said to be more abstract than in much of conversation. While a small number (between 5 and 10) of lexical items accounts for 80% of CTPs in conversation, newspaper texts exhibit a far greater type frequency: no less than 42 different lexical items account for less than 70% of the CTPs, and about 30 out of 100 CTPs has a token frequency of 1 (cf. Verhagen 2005: 103). This allows for the conclusion that the pattern PREDICATE+COMPLEMENT CLAUSE has developed, at least in adults and in written discourse, into a productive template that may itself license novel expressions,¹⁰ constituting a formal *and* functional abstraction over a large number of different specific expressions (different predicates; both first, second and third persons; different tenses). Certainly some degree of abstractness, but not with usage and structure living in separate quarters (whether interacting with each other or not).

3.4. And long-distance movement?

In opposition to the idea that memorized concrete “fragments” of language use have an important role to play in grammar, Newmeyer defends the view that “[t]he complexity and abstractness of syntactic knowledge that is revealed by conversational speech is stunning”, and he illustrates this with instances of “long-distance *wh*-movement” and “deeply embedded gaps in relative clause constructions”. An example of the former is: *so what do you think that um we should do*, and of the latter: *the rules that they need to be following*. Newmeyer describes the interpretive demands that such constructs place on an addressee in a way that is clearly intended to evoke this alleged complexity: “it is necessary to hold in mental storage a place for an unexpressed direct object in a different clause [viz. the objects of *do* and *following*, respectively – AV] and to link

a fronted *wh*-element or lexical antecedent to that place”, which implies “a sophisticated engine for representing and accessing grammatical knowledge”.

However, the CTPs of all three of the instances of “long-distance *wh*-movement” given by Newmeyer are word by word identical, including the non-occurrence of audible pauses –each is: *what do you think*. This is not a coincidence, but a general feature of such structures as found in actual usage, this time both in conversation and in newspaper texts. On that basis, Dąbrowska (2004) and Verhagen (2005, 2006) have proposed that such constructs should not be analyzed as instances of an abstract pattern of one clause subordinated to another (and then filled in with specific lexical material, of which one element, the *wh*-element, is moved out of its own clause to the front of the main clause), but as directly licensed by a much more specific template for formulating questions with an explicit perspective marker (*viz.* the addressee’s, the content of whose mind is being activated by the speech act “Question”), roughly: *Wh-do-you-think* (actually: the second person, question-marking counterpart of the first person, epistemic formula *I think*). An important corpus-based argument for positing such a separate lower-level item is the fact that the type and token frequencies of CTPs in such questions differ radically from the *general* distribution of CTPs in a corpus (Verhagen 2005: 124–126). As I mentioned above, Dutch newspaper texts exhibit a high type frequency for CTPs, indicating that they have generalized into an abstract template for formulating complementation constructs. But in so-called long-distance questions, 80% of all instances have the same predicate (*denken*, ‘think’), while the total number of CTPs occurring in the pattern is no more than 4 (besides *denken*: *zeggen* ‘say’, *willen* ‘want to’, and *vinden* ‘find, think’ – the most basic and most general verbs of cognition, communication, and volition). Searches in even larger corpora bring to light that occasionally some other verbs are also used, but they do not at all change the dramatic difference between the patterns found in complementation in general on the one hand, and in “long-distance” cases on the other. Dąbrowska (2004), and especially (2008) support the important role of formulaicity in the patterns underlying the processing of such constructs by means of experimental evidence.

It will be clear that the implications of this view for the claims about the complexity of processing (“holding a place in memory for an unexpressed phrase in another clause”) are far reaching. If the CTP actually has the status of a kind of lexical item (a kind of idiomatic operator dedicated to the formation of a question with a specific type of function), then the point more or less evaporates. Processing such a sentence may still be a complex task, but not one that is *more* complex than processing ordinary *Wh*-questions (in English, Dutch, and related languages) – in which the characteristic item is always at the front of its clause.

To avoid one possible misunderstanding: to claim that the CTPs of “long-distance” *Wh*-questions are a kind of complex, stored formulaic items is not the same as saying that *all* of their properties can and must be explained in terms of that status. What the claim does mean is that the complexity which such sentences appear to exhibit when linguists describe them in terms of the most abstract structure – viz. at the level needed to generalize over constructs of this type and declarative complementation constructs (including those found in elaborate written discourse) – need not really be present in the way the constructs involved are actually used, processed and stored by (other) speakers of the language (I will return to this issue in general terms in section 4). There *may* still be properties that can only be explained with reference to more abstract, structural and/or functional, properties, but such a claim needs separate justification, so to speak. A recent example is the study by Ambridge and Goldberg (2008), claiming that a general (not strictly lexical) principle of backgrounding in discourse is involved in the acceptability of “long-distance” *Wh*-questions. I think it is likely that besides such a principle, if it receives further support, lexical factors also will turn out to co-determine acceptability judgments on such constructs (cf. Dąbrowska 2008), but in any case such studies are on the right track in not *assuming* a particular level of abstraction as the right one to account for the facts, but in actively seeking for relevant evidence instead.

I will only briefly say something about the other type of “long-distance” phenomena mentioned by Newmeyer: relative clauses of the type *the rules that they need to be following*. Newmeyer describes the complexity of these cases and the “long-distance” *Wh*-cases in a single sentence (quoted above). So he sees them as having the same syntactic structure, and he is thus, again, implicitly jumping to an abstraction, abstracting away from all the differences between them. The most important difference here is that all of the relative clause cases involve a *non*-finite complement to a “light” (partly grammaticalized) verb with a modal meaning (in a broad sense): *want to*, *need to*, *should(n’t)*, *like to*. One can only see these clauses as structurally similar to “long-distance” *Wh*-sentences if one sees them as involving two clauses, one projected by the tensed “light” verb, the other by the non-finite verb, i.e. by abstracting away from any differences between these expressions and finite complements to CTPs.¹¹ Now although this is a position in formal syntax that is not unique for Newmeyer,¹² it should be clear by now that it may well be another unwarranted abstraction (as I think it is).¹³

4. Conclusion and discussion: abstract structures vs. capacity for abstraction over usage

It is clear from the way Newmeyer presents his critique, that he feels that Thompson, and other “radical” usage-based linguists, underestimate the abstractness of the representation of linguistic structure that ordinary speakers (of English) have at their disposal. While I doubt that this critique is entirely justified, I have tried to show in this contribution that Newmeyer himself greatly *overestimates* abstractness. Now the linguistic analyses that seemed to reveal a high degree of abstraction in linguistic knowledge contribute a considerable part to the basis for claims about the general capacity of humans to deal with abstract structures (including, for instance, the number system). So the question arises: does the claim that linguistic abstractness is often overestimated imply that humans also have far less capacities for dealing with abstractions than traditionally thought? The answer is: Not at all. As a way to explain why not, let us consider the question what may have caused the discrepancy between the high degree of abstractness that grammarians saw in apparent “long-distance” *Wh*-questions and the specificity demonstrated by investigating actual usage. A probable explanation is stated nicely in a recent article by Dąbrowska (2008: 419/20):

This [...] could also be a result of differences in linguistic experience. Many linguists spend a considerable amount of time constructing examples of the structures they are interested in and reading papers containing such examples. [footnote omitted] Since LDD [=Long Distance Dependency] questions have been the object of very intensive research, it is likely that linguists (or at least linguists who work on LDD constructions, or discuss them with their students) have been exposed to more instances of this construction than most ordinary language users, and, crucially, the instances they have encountered are much more varied [...]. As a result, they are much more likely to develop more general representations of these constructions, and accept unprototypical instances of them.

That is, the linguistic knowledge of linguists, including (perhaps ironically) adherents of autonomous linguistics, is just as much usage-based as that of “ordinary language users”. But the experience of these groups of individuals differs, and that is what accounts for the difference in abstractness of the representations produced: the same *capacity* for abstraction simply produces different *results*, i.e. differences in abstractness of representations, in different *circumstances*. The point is that abstract structures are not just “dealt with”, but actually *created by* the human capacity for generalization. Nowadays, since virtually all children in our societies go to school to learn to read and write, practically everyone’s experience with instances of declarative complementation constructs is so var-

ied that it gives rise to a well-entrenched abstract routine for complementation in practically every member of the population. But there is neither a need to assume that all languages of the world and of every era have to have these abstract *structures*, nor to assume that speakers of languages without such abstract structures would have (had) a more restricted *capacity* for abstraction, linguistic or otherwise.

So on the one hand, linguists do not have a “perverted” kind of linguistic knowledge, and at the same time, ordinary language users do not in principle lack the capacity to form the same abstractions – we could train them as linguistics students, for example, and the more their linguistic experience will overlap with ours, the more their capacity for abstraction will start providing them with representations similar to ours. Consider the example of “long-distance” *Wh*-questions discussed in section 3.4. In a usage-based approach, there are good reasons to assume that for ordinary speakers of Dutch, the representation licensing these constructs is a rather specific one, on a low level of the network of complementation construction (the second box from the left with bold lines in figure 1; cf. Verhagen 2006), in which the top node represents the abstract template for complementation.

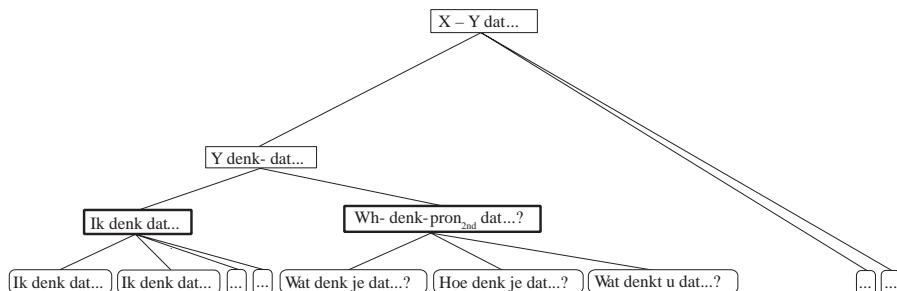


Figure 1.

No basic cognitive capacities have to change in order to change the representation and the structure of this network in such a way that there is (also) a well-entrenched abstract template for “long-distance” *Wh*-questions. Expanding linguistic experience, especially increasing the variety in the cases encountered, would suffice. The judgments of trained linguists testify to this possibility, since there are no reasons to assume that the *basic* capacities of linguists differ dramatically from those of other language users. In fact, I claim that exactly the same mechanism is actually responsible for the fact that a productive template for declarative complementation *is* part of the linguistic knowledge of most members of (Western) modern language communities. So what all of this actu-

ally shows is that it is sometimes very misleading and dangerous to talk about “the” structure of a sentence as if it were something invariable across speakers, whereas in fact each speaker constructs her representation on the basis of her experience, so that differences are bound to arise, despite uniformity of basic cognitive capacities.

A parallel with the number system may be useful here. As is well known, recursion in our number system is a product of cultural evolution. The development of the place-value system for writing numbers and doing arithmetic was a gradual development, as was the “invention” of zero, which made it possible to distinguish between 11, 101, and 110, and which also greatly simplified the task of performing calculations on paper (rather than with an abacus). To see the lack of things as, in some way, “the same” as a value represented by a number, and to represent and use it in calculations in the same way as “true” numbers, involves a considerable abstraction. The inclusion of zero in the line of numbers thus basically coincides with an increase in abstractness of the concept “number”. In turn, it provided the basis for further abstractions, leading to the inclusion of negative numbers (turning the previously recognized ones, besides zero, into positive numbers), etcetera. But of course, peoples using the system of Roman rather than Arabic numerals had no lesser *basic capacity* for abstraction.

The parallel is even more interesting, in that it is clear that once a recursive number system, including the use of zero, is in place, it in turn truly expands the cognitive abilities of its users. Thus, it creates a system with emergent properties, i.e. properties that do not exist in the more elementary level phenomena out of which it evolved. Surely, we do not want to say that the results of these expanded abilities (e.g. the concept of zero, or infinity, or rational numbers, . . .) have been part of all humans’ cognitive make-up for millennia before they were “discovered” a few centuries ago, when it became possible to start *using* them (in those parts of the world that had access to the discovery . . .). The only view that makes sense is that these concepts are *truly* new structures, having emerged on the basis of still the same basic capacity for abstraction (among other things), which happens to apply to new circumstances all the time because of cultural evolution and therefore can give rise to truly novel results.

These are a few well known facts from another cognitive domain than language. I see no objection whatsoever against assuming a parallel story for complementation in grammar. Humans have largely similar capacities for abstraction, but these only gives rise to an abstract, grammatical template for marking perspectives under certain circumstances, a crucial one being sufficient variation in experience with CTPs; where and when such a system arises, it provides its users with a useful, flexible and consistent tool for subtly managing points of view in complex texts. However, for some reason, the lessons that these facts

contain have not really been taken up in linguistics, or at least not in large and prominent parts of linguistics. The name of the reason is, I submit, “structuralism”, and then especially the idea that structure is logically prior to, and independent of usage (communication and processing), i.e. the idea, attributable to Saussure, that structure is the “object proper” of linguistic theory. This is what is usually meant when formal linguists talk about “autonomy” of structure – not the kind of autonomy that I have been presenting in this chapter, and that Newmeyer also appears to adopt at least at some points. I agree entirely when he says that autonomy is totally compatible with the leading idea of usage-based linguistics. But when he then says that usage “shapes” and “reshapes” structure, (only) “to a considerable extent”, and especially when he goes on to repeat that the not-so-surfacey constituent structure of a phrase is to be determined on the basis of totally different things than usage, then I conclude that Newmeyer actually still adheres to the more radical interpretation of autonomy, viz. that some aspects of structure can only be explained in terms of principles dedicated to grammatical structure as such, i.e. skyhooks (if this interpretation is wrong, then I think I can only say to Newmeyer: “Welcome to the usage based club!”). This kind of autonomy is another one than the idea of properties at a higher level of organization that are not present at a lower level but nevertheless causally explained by the latter; in grammar, these properties include open slots in templates, which are, by definition, the product of abstraction over instances of use, and the causal mechanisms include, besides abstraction, routinization and conventionalization (manifested in, among other things, grammaticalization) – these are cranes for building structure out of usage. In this sense, in fact, autonomy and abstractness are consequences of the emergence of structure from usage. They are completely included in a usage-based approach, instead of requiring an explanation on independent, possibly non-reducible principles. Moreover, the usage-based approach also provides a basis for preventing overestimating the role of abstractness of structure, and for understanding why it may be as limited and variable as it turns out to be.

Notes

1. It was a draft of Newmeyer’s chapter in the present volume, distributed and discussed on the internet (esp. the Funknet discussion list), that made me decide to change the topic of my contribution to the present volume. I thank Fritz Newmeyer and the editors for giving me the opportunity to see the final version of his chapter, Bill Croft for sending me his chapter, and the editors again for useful comments that helped me to clarify certain issues. The usual disclaimers apply.

2. This publication, of which Sandra Thompson was also a co-author, may well be regarded as one of the first suggesting (albeit then still without a theoretical interpretation) that complements are not (as) subordinate (as adjunct clauses). Another one that should be mentioned here is Halliday (1985), who replaces the traditional distinction between ‘coordination’ and ‘subordination’ with a tripartite distinction between ‘coordination/parataxis’, ‘hypotaxis’ and ‘embedding’; cf. also Matthiessen and Thompson (1988) for a link between Halliday’s proposal and text analysis.
3. This is the strategy that was and is most forcefully developed in linguistics in Ron Langacker’s work: start from a few generally recognized and well known mechanisms, and show that complex linguistic structure arises out of them and out of their interaction. Two relevant publications, also for alternative views on claims made by Newmeyer that I do not address here (e.g. on ‘bracketing paradoxes’), are Langacker (1997) and (2000).
4. However, I read less of a straightforward contradiction between Fox and Thompson (2007) and Ono and Thompson (1995) than Newmeyer does; cf. my comments in the next paragraph.
5. In other places, Fox and Thompson (2007) formulate as their goal to reverse the focus of attention from the more general to the more specific patterns, not to deny the existence of abstract patterns. Thus, their point concerns the *balance* between generality and specificity, but they, Newmeyer, Boye and Harder, me and, e.g. Croft (this volume, section 2.5) all agree that each of the extremes of maximal computational or maximal storage parsimony is unwarranted. Paradoxically, while Newmeyer considers Fox and Thompson (2007) even more radical than Thompson (2002), the terms ‘abstract’ and ‘general’ as characterizations of grammatical patterns, do occur in the former paper.
6. As for the latter claim, I can only say that there also seems to be some ‘theoretical opportunism’ among different advocates of formal linguistics. Newmeyer ignores a rather long history in generative grammar of considering complement clauses as objects, as well as theoretical issues precisely about the status of complement clauses in a theory that includes the so-called theta-criterion, which implies that each structural position is either an A (=argument, possible target of NP-movement) or an A’ (=non-argument, possible target of *Wh*-movement) position. Newmeyer makes it seem as if no formal grammarian in their right mind would ever have thought of assigning complement clauses the status of argument of the matrix predicate, but this is precisely the position that the formal linguists Hinzen and Van Lambalgen (2008) take in their critique, working with assumptions from Chomsky’s ‘minimalist program’, of Verhagen’s (2005) analysis of the syntactic status of complement clauses, which like Thompson’s starts by denying complement clauses argument status (cf. also Verhagen 2008a).
7. A brief search on the world wide web suggests that at least strings of the type “whether/if each other” and “since each other” do occur, but looking at some of these occurrences as a non-native speaker, I often had a hard time figuring out whether such a usage was anaphoric, as I assumed it should be in the interpre-

tation meant by Newmeyer, or a non-anaphoric phrase meaning “each one out of two”.

8. This insight is elaborated and turned into the foundation of an alternative approach to grammar and semantics in Verhagen (2005); see also Verhagen (2008)b, and the discussion between Hinzen and Van Lambalgen (2008) and Verhagen (2008)a.
9. An even higher degree of abstraction would, finally, consist in a generalization over verbs as CTPs and as transitive event descriptions (based on pairs such as *He wrote that the problem would be solved soon* and *He wrote a message*), ultimately allowing a CTP to be used, in specific circumstances, to describe a transitive event, effectively turning the complement clause into an argument of the verb (e.g. *He wrote that the problem would be solved, waited a few seconds, and then pressed Enter to send the message*). In the Dutch corpus consulted by Verhagen (2005: 112), this type of usage of CTPs amounts to less than 5%. Notice that this suggests that the capability to see and use a CTP as a transitive verb (generalizing over nominal and clausal complements), is a product of linguistic experience, and should thus develop later in life than the capacity to use CTPs and transitive verbs as such; Brandt et al. (submitted) present experimental evidence in support of this view.
10. Notice that this is another and more powerful kind of criterion for establishing a *rule* of grammar than tests that establish similarities and differences.
11. This passage in Newmeyer’s text, especially his use of the phrase “*lexical antecedent*” (my emphasis) might allow for an interpretation in which the relative clause is monoclausal, with the ‘light’ verb and the non-finite verb together constituting a single clause. There would then still be a long-distance dependency, because the (relativized) gap would be directly related to the lexical head outside the clause containing the gap. But such an analysis would contradict the generally accepted view, in traditional, structural, generative and functional linguistics, that the head NP is anaphorically linked to the element at the front of the relative clause, and that it is the latter that is grammatically associated with the gap. If we assume this, and would also analyze these expressions as monoclausal, then they would actually not have the same structure as ‘long-distance’ *Wh*-sentences. Hence my interpretation of Newmeyer’s text at this point.
12. It is no coincidence that the underlying structures posited in generative syntax often resemble the historical source of the phenomenon studied, with the transformations removing large parts of the structure that is actually absent in the modern structure – effectively describing long social, historical chains of events as somehow simultaneously represented in the minds of individual speakers.
13. In cognitive and functional linguistics, it is commonly observed that combinations of verbs with non-finite complements exhibit both formally and semantically more integration with the complement-taking verb than finite complements, which is even used as a typical example of iconicity in language structure. See Kemmer and Verhagen (1994) for an argumentation that causal verbs with non-finite complements do not constitute complex sentences, but exhibit yet another autonomous kind of construc-

tion, which is actually best analyzed as an analogical extension of the (di)transitive template licensing simplex clauses.

References

- Ambridge, Ben and Adele E. Goldberg
2008 The island status of clausal complements: Evidence in favor of an information structure explanation. *Cognitive Linguistics* 19: 357–389.
- Boye, Kasper and Peter Harder
2007 Complement-taking predicates: Usage and linguistic structure. *Studies in Language* 31: 569–606.
- Brandt, Silke, Arie Verhagen, Michael Tomasello and Elena Lieven
submitted German children's productivity with simple transitive and complement-clause constructions: Testing the effects of frequency and variability.
- Croft, William
2001 *Radical Construction Grammar. Syntactic Theory in Typological Perspective*. Oxford: Oxford University Press.
- Croft, William
2009 Ten unwarranted assumptions in syntactic argumentation. (This volume).
- Dąbrowska, Ewa
2004 *Language, Mind, and Brain: Some Psychological and Neurological Constraints on Theories of Grammar*. Edinburgh: Edinburgh University Press.
- Dąbrowska, Ewa
2008 Questions with long-distance dependencies: A usage-based perspective. *Cognitive Linguistics* 19: 391–425.
- Dennett, Daniel C.
1995 *Darwin's Dangerous Idea. Evolution and the Meanings of Life*. New York: Simon and Schuster.
- Diessel, Holger and Michael Tomasello
2001 The acquisition of finite complement clauses in English: A corpus-based analysis. *Cognitive Linguistics* 12: 97–141.
- Fox, Barbara A. and Sandra A. Thompson
2007 Relative clauses in English conversation: Relativizers, frequency, and the notion of construction. *Studies in Language* 31: 293–326.
- Halliday, Michael A.K.
1985 *An Introduction to Functional Grammar*. London, etc.: Edward Arnold.
- Harder, Peter
1997 Review of Schilperoord (1996). *Studies in Language* 21, 3, 667–674.

- Hinzen, Wolfram and Michiel van Lambalgen
2008 Explaining intersubjectivity. A comment on Arie Verhagen, *Constructions of Intersubjectivity. Cognitive Linguistics* 19: 107–123.
- Kemmer, Suzanne and Arie Verhagen
1994 The grammar of causatives and the conceptual structure of events. *Cognitive Linguistics* 5, 115–156.
- Langacker, Ronald W.
1997 Constituency, Dependency, and Conceptual Grouping. *Cognitive Linguistics* 8: 1–32.
- Langacker, Ronald W.
2000 A Dynamic Usage-Based Model. In: Michael Barlow and Suzanne Kemmer (eds.), *Usage-Based Models of Language*. Stanford: CSLI Publications, 1–63.
- Mann, William C. and Sandra A. Thompson
1988 Rhetorical Structure Theory: Toward a functional theory of text organization. *Text* 8: 243–281.
- Matthiessen, Christian and Sandra A. Thompson
1988 The structure of discourse and “subordination”. In: John Haiman and Sandra A. Thompson (eds.), *Clause Combining in Grammar and Discourse*. Amsterdam: John Benjamins, 275–329.
- Newmeyer, Frederick J.
2009 What conversational English tells us about the nature of grammar: A critique of Thompson’s analysis of object complements. (This volume).
- Ono, Tsuyoshi and Sandra A. Thompson
1995 What can conversation tell us about syntax? In: Philip W. Davis (ed.), *Alternative linguistics: Descriptive and theoretical models*. Amsterdam: John Benjamins, 213–271.
- Schilperoord, Joost
1996 *It’s About Time – Temporal Aspects Of Cognitive Processes In Text Production*. Amsterdam/Atlanta, GA: Rodopi.
- Schilperoord, Joost and Arie Verhagen
1998 Conceptual dependency and the clausal structure of discourse. In: Jean-Pierre Koenig (ed.), *Discourse and Cognition. Bridging the Gap*. Stanford: CSLI Publications, 141–163.
- Thompson, Sandra A.
2002 “Object complements” and conversation: towards a realistic account. *Studies in Language* 26: 125–164.
- Thompson, Sandra A. and Anthony Mulac
1991 The discourse conditions for the use of the complementizer *that* in conversational English. *Journal of Pragmatics* 15: 237–251.

- Verhagen, Arie
2001b Subordination and discourse segmentation revisited, or: Why matrix clauses may be more dependent than complements. In: Ted Sanders, Joost Schilperoord and Wilbert Spooren (eds.), *Text Representation. Linguistic and psychological aspects*. Amsterdam: John Benjamins, 337–357.
- Verhagen, Arie
2005 *Constructions of Intersubjectivity. Discourse, Syntax, and Cognition*. Oxford: Oxford University Press.
- Verhagen, Arie
2006 On subjectivity and “long distance *Wh*-movement”. In: Angeliki Athanasiadou, Costas Canakis and Bert Cornillie (eds.), *Subjectification: Various Paths to Subjectivity*. Berlin/New York: Mouton de Gruyter, 323–346.
- Verhagen, Arie
2008a Intersubjectivity and explanation in linguistics: A reply to Hinzen and van Lambalgen. *Cognitive Linguistics* 19: 125–143.
- Verhagen, Arie
2008b Intersubjectivity and the architecture of the language system. In: Jordan Zlatev, Timothy P. Racine, Chris Sinha, Esa Itkonen (eds.), *The Shared Mind: Perspectives on Intersubjectivity*. Amsterdam/Philadelphia: John Benjamins Publishing Company, 307–331.