

Part 3 - supplement

Oslo 23 September 1998

The faculty of arts
University of Bergen
Harald Hårfagresgate 1
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CONCERNING DOCTORATE CANDIDATE JOHN GROVER'S COMMENTS TO THE COMMITTEE'S EVALUATION

We have received John Grøver's comments to our evaluation of the dissertation. The candidate's comments is a voluminous document of 64 pages. The committee has considered these comments, but finds that it has to maintain its original conclusion that the dissertation is not acceptable for the degree Doctor Philosophia. For this decision, we have considered the dissertation in accordance with Prescriptions for the degree Doctor Philosophia at the University of Bergen, where it is specified in §3 first part that "the work must be on a high level when it comes to definition of problems and concepts, methodological and theoretic founding, documenting and way of description, and contribute to develop new knowledge". These requirements must obtain for each of the five parts of the dissertation.

The candidate emphasizes in his letter to the university that the dissertation has a strong inner unity: "All the parts are concerned with the same ultimate goal of arriving at a formalization of the socially encoded knowledge in the domain of the so-called 'Cantor machine'". We therefore found it opportune to take this assertion as our point of departure in order to test in one concrete example how the candidate mastered the task as far as definition of problems and concepts, methodological and theoretic founding are concerned. The analysis in section 2 of our evaluation shows that the candidate has

not mastered the task. The examples in section 3 also shows that the candidate's documentation and analysis of concrete examples are far from meeting the requirements that the Prescription §3 specifies. We furthermore noticed the lack of connection between the candidate's examples and analyses and his attempt at a theory of Cantor machine computability. In total, it is unclear and unfounded what "the domain of the so-called 'Cantor machine'" is meant to be.

The candidate notices in his answer comments to our evaluation that the discussion of the Cantor machine is an entirely peripheral part of the dissertation (page ?? in the comments). Peripheral or not - the candidate's handling of theory and examples in connection with the Cantor machine, the logical paradoxes and the triadic signs is of a kind that justifies our conclusion.

Neither can the candidate's comments (pages 81 ff) to the evaluation when it comes to "The cuneiform episteme" alter the committee's decided view that the bulk of Grover's assertion on Sumerian, Akkadian and Hittite, and on the cuneiform script, remains undocumented.

When it comes to the committee's comments to the dissertation part 1, the candidate has objections to the committee's use of the term 'sound symbolism'. With this term, the committee intends the phenomenon that a single speech sound or a single phonetic feature, for example [continuant], is paired with a meaning, for example [motion], that is, what Grøver calls 'submorphemic signification'. The committee is not negative to the idea that this phenomenon has a role to play in language of the kind Grøver argues for in part 1, but finds that the reasoning fails on a number of points. The committee find no reason to change its evaluation of part 1 of the dissertation: "an original, interesting and ambitious (possibly too ambitious) work, but half finished".

Grøver comments on page ?? on the committee's formulation on this part of the dissertation when it comes to further reworking of it "within the framework of accepted linguistic theory" that this is a limitation which he finds to be discordant with the requirement that

a dissertation is supposed to develop new knowledge. The committee sees that the formulation opens for misunderstandings. The intention was not that a dissertation's thesis cannot deviate from established theories, but that established requirements to method, reasoning and foundation, such as we know them from for example modern linguistics, must be adhered to if a dissertation is to be accepted.

The committee maintains its conclusion that the dissertation is not worthy of being defended for the degree Doctor Philosophia.

On behalf of the committee

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Bergen 1 October 1998

To the faculty of arts
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The degree Dr.Philos. - on the committee's reply to my comments

I refer to letter from the committee for my dissertation as a reply to my comments to the evaluation. The committee's answer is dated 23 September 1998, the day after the meeting in the faculty board on 22 September 1998. The next meeting in the board is 1 December 1998, exactly one year after I handed in my dissertation on 2 December 1998 (minus one day). As far as I understand, my comments to the evaluation did not reach the committee in time to reach the meeting in the faculty board on 22 September, not the least because faculty director Bjørnset wrote back to me and asked for having my comments printed out on A4 paper because he did not like the A5 paper which I had used. I should also add that committee member Braarvig commented - when I called him in the early summer to ask if he had received the letter which followed the dissertation - that it is not abnormal if it takes one full year between the dissertation is handed in and the viva.

As Fenstad says in his reply, my main objection to the committee's critique of vol.III is that they only discuss 5-6 of the 340 pages in the book (in other words, a peripheral part of it), and that they assert on this basis that the dissertation does not hold good for the degree. "Peripheral or not...": *It is entirely essential that these*

are only 5 of the 340 pages in the book! Fenstad presents no further critique when it comes to vol.III than that these five pages are not standard theory. He refers to the Precription §3 first part concerning the requirement that the dissertation must be on a high level: "These requirements must obtain for each of the five parts of the dissertation". This is the basis for rejecting vol.III. Fenstad rejects vol.III because he has not seen the analysis on these five pages before. *This is the only reason for the rejection of vol.III.* It is not serious at all.

I may as well refute the critique of these five pages as well, if Fenstad wants to have it in more detail. My model presupposes that natural language consists in hierarchic levels with *essentially incommensurable languages* on the various levels. These languages consist in arbitrary linguistic signs which are the result of formalizations of culture. The signs thereby also have a social reference to institutions and to knowledge-spaces defined by the information technology (including the natural information technology). Tarski's theory of truth receives a new and unexpected - and suddenly meaningful! - interpretation within my model, in the form that every level give the conditions for truth through the formalization of culture which has outputted the constituents on that level - something which relegates the question of *truth* to that culture-space where truth must be tested in the last analysis. I talk of human truth in a broad sense of it, and I see the concept of truth in logic as a formalization of this human truth. This is what is new in my interpretation of Tarski: The conditions of truth in Tarski's model comes to be a formalization of human and social truth in such a way that it can be applied to the social space in a new and productive way. (Even when it comes to truth in the sense of the theory of science). The future Cantor machine can make use of Tarski's model in precisely this way, because the conditions of truth can, in the last analysis, be reduced to a result of the process of cultural formalization (through the thousands of years of culture history) that has shaped the languages on the various levels and that has conditioned the (presumably) non-Turing-computable relation between the various incommensurable languages. This explanatorily strong interpretation of Tarski's model of truth emerges *only* in light of the model that I have presented in the dissertation. It is in this

light that one must read the five-six pages which (presumably) Fenstad criticizes. But because (presumably) Fenstad has not understood what the dissertation is about, he fails in his traditionalist critique. He makes the mistake of rejecting the entire vol.III because he has not seen this interpretation of Tarski before (this is a kind of $E = MC^2$) - an interpretation over five-six of the 340 pages, and it seems as if he has not understood much of the semiotic discussion in the remaining 335 pages. That is the basis for the rejection. It is entirely unacceptable.

Further to the third paragraph in Fenstad's reply: My discussion of the logical paradoxes has the same explanatory framework as point of departure, such as I have discussed in my comments.

Further to the same paragraph: When it comes to 'the triadic sign', that is the main contents of vol.I, which I must presume that Fenstad has not read. This volume is a pioneering work which solves a lot of problems in the theory of language acquisition. It contains an elaborate discussion of the architecture of 'the triadic sign'. I must add that *this model of the triadic sign is my own*. It is a part of the results in the dissertation. There are other triadic signs on the market, but they do not have the same explanatory force as my model has.

As to the fourth paragraph of Fenstad's reply: There should be no need for 'documenting' my discussions (my 'claims', as they interpret it) of the cuneiform script. I discuss the *code* in the traditional interpretation of cuneiform (and I could perhaps have referred to some introductory books, but that should not be necessary), and I base my discussion on general considerations over this. To repeat it once more: Hittite is accepted as an Indo-European language because there is a narrow stripe of consistency which emerges when one assigns one single phonological interpretation to the many-valued cuneiform signs in Hittite. (At least, it is somewhat consistent). *Because the morphemic linguistic sign by Saussure is considered to be unmotivated*, this consistency means that Hittite is verified. One can predict forms, and one finds these on new clay tablets. This is also the most important empirical support for Saussure's laryngeal theory. My model in vol.I shows that this consistency can be a result of other factors, and thereby the received proof for Hittite (and much of Saussure's other basic claims)

vanishes. This is all I say about Hittite. There is no need for documenting this by showing empirically that the interpretation does not hold: Such a 'documentation' is of course also impossible. My documentation is the theory of submorphemic signification set forth in vol.I, and there is no need for providing further relevant 'documents' beyond vol.I of the dissertation on this point. (These consequences for Indo-European, Hittite and the laryngeal theory are, in addition, yet another example of the explanatory power in my odel - something which the committee ignores or has not understood). The committee has not understood the contents of my discussion of the cuneiform script: I use it exclusively for showing that it *may* represent a level under the alphabetic script in such a way that the level must be considered incommensurable with the alphabet. That supports my model. For all parts of the model, it will be the case that it is impossible to prove that the previous model is wrong: One here faces a paradigm shift in my model, and then it is a matter of Gestalt switches. Fenstad has obviously not yet seen the light, *but he should have*.

As to the fifth paragraph in Fenstad's reply, on 'sound symbolism': This is either a meaningless comment or the term is misunderstood. Fenstad defines 'sound symbolism' in terms of a certain speech sound paired with a certain meaning. But this obtains of course for all signification in natural language irrespective of level, so this cannot be the proper content of the term 'sound symbolism'. There is a deplorable lack of knowledge revealed in this paragraph.