THE DOCTORATE

The doctorate

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Contents

Preface

In this book, I publish the material for a doctorate which was not accepted: It contains one lecture in the theory of science (an obligatory part of the doctorate in Norway) together with the evaluation from the committee which judged it; it also contains the evaluation from the committee which judged the dissertation; in addition to this, it contains my answers to both committees. The purpose of this is partly to focus on what is evaluated in the doctorate (not only scientific matters, rather culinaric, if you see my point), partly to make a booklet which I can enclose when I apply for work, research funding etc. - in defence of my scientific qualifications.

At the end of the book I also discuss the matter whether the committee which did not accept my dissertation, even if it should have been accepted on scientific grounds, have made themselves guilty to §§ 247 and 248 in the Norwegian Criminal Law - which qualifies for up to three years in prison. This is also a very interesting question which is worth some official attention. For example: If this kind of rejection qualifies for such sizable punishment - what forks are not at stake when a dissertation is rejected? It is well possible that some committees will find the delicacies just too tempting to resist it, in particular if they feel sure that they will not be arrested by the police afterwards. This is also an important point which ought to receive some attention when the doctorate is discussed.

The first rejection took place in 1996. I was enrolled in the socalled 'dr.art.' programme at the University of Bergen (at the faculty of arts) and had written the obligatory lecture in the theory of science which everybody qualifying for this degree must get accepted and must perform for an audience before they get the degree. In the traditional 'dr.philos.' (the PhD), there are two lectures - one you can choose yourself, and one for which the topic is given to you. In the new 'dr.art.' degree, the one you choose yourself is restricted to the field of the theory of science. Earlier, one had the sensible arrangement that the candidate wrote the lecture and performed it for the audience and for the committee. If it was not accepted, one would at least have an audience present to control the committee's judgement. This was altered a few years ago: Now the candidate has to send in the lecture for acceptance by committee committee in advance, and if it is accepted, then it can be performed (without any risk of rejection). The committee normally consists of three representatives from the near surroundings - one of them has to be the candidate's obligatory supervisor. It is inevitable that there is a considerable risk for unnecessarily many rejections of the first attempt, and that power is exerted to a considerable extent. I sent in my lecture in the beginning of October 1996 after having made the appointment in advance, and I expected, in accordance with the promises, that it would be a matter of short time (a few days) to have it accepted. I waited for six weeks before I received the rejection (the first committee evaluation reproduced below). I wrote back that there was nothing in the evaluation which could give reason to a rejection, and I argued for my view (my letter given in part 1). There was no answer from the committee, and I contacted all the relevant parts of the university instutition in order to get help to receive an answer from the committee: The institute, the faculty, rector, the institute board (which was to give the final acceptance of the lecture). Nobody could help me in getting the answer: They all pointed to the committee and suggested that I discuss these things with them. I was even subjected to a formal complaint to the head of department from a member of the institute board (Mortensen) due to 'unwanted cor-respondence' from me, because I had sent the lecture to her as a member of the institute board which was supposed to give the formal acceptance. After another six weeks I received a laconic letter from the committee (by the obligatory supervisor), stating that they 'no longer worked with the acceptance'.

I had consequently not succeeded in getting through. I therefore decided to head for the dr.philos. degree (the PhD) instead of this dr.art., since that allowed me to give the lecture first and then have it accepted afterwards. I handed in the dissertation on 2 December 1997. In February, after having made the University Council and rector aware of the fact that time limit for appointing of committee was much overdue, I got the message that the committee was appointed. Not much happened before the end of July (almost eight months after the dissertation was finished) when I received the evaluation from the committee - the second part of this book. My comments to this evaluation, written as a letter to the faculty who is to give the formal acceptance of the dissertation (I suppose the comments first will go back to the committee), are also reproduced below.

After these two rejections, I am left with the impression that the doctorate degree represents no serious evaluation on scientific criteria, but that it is used by the university institution for all kinds of purposes. I suppose that is what this book is about. The doctorate degree is of course worthless as a scientific academic degree if it does not reflect scientific academic level.

The book also includes the lecture in the theory of science which was rejected by the first committee. This is also included as a chapter in "The theatre of the heart" (1997), which was the third part of the dissertation. This dissertation, which the second committee evaluated, is of more than 900 pages and cannot be included here. The evaluation comments only on three of the five parts. I suppose these will be published fairly close to the form which they had when I handed them in - just in case somebody at a later stage should be interested in reading the material. (The first part - vol.I - is anyhow available at the university library in Norway).

I have reproduced the material as close to the form it had, including potential typing errors and other misunderstandings. The few footnotes are added in the translation only and was not in the original. In order to make the material readable, I have replaced references to the pages in the committee evaluations with references to the pagination in the present book.

The Norwegian original contains an appendix with a letter from me to the Faculty of Arts at the Uni-versity of Bergen, where I ask for a new administ-rator of the committee because they had used an excessive amount of time in their evaluation. The letter, which was sent the day before the evaluation was completed, is difficult to translate into English since it also points to a series of institutional arrangements which can be seen to have reference to my name.

Part 1

The lecture in the theory of science

JOHN GRØVER:

Kuhn's paradigms and the simultaneity of scientific discoveries

In this lecture, I discuss two concepts pertinent to the history of philosophy which it is my intention to show the mutual relevance of. The first is the one which has persisted in philosophy, and, even more, in religion, throughout all ages and cultures in various forms, and which can be associated with a concept of a shared social consciousness in a community, whether this be interpreted in the form of shared knowledge in the broad sense of CULTURE, or whether it is taken in a more narrow philosophical frame, from platonism in antiquity to the idealism of the previous century, and, more recently, in the novel views advocated by Rupert Sheldrake, David Bohm, and others. We can easily trace it in mythological reality and in any outlook which considers FATE pertinent to knowledge, which even today is a widespread conception in the subjective and everyday interpretation of reality (in such forms as 'why does this always happen to me...'), and the question of FREE WILL has followed philosophy through all ages. Archetypes, whether in Jung's form or such as Eliade conceived of them already in 1949/52 (in 'The myth of the eternal return'), when he identified them with exemplary models and paradigms, is perhaps the most obvious theoretical formulation of the notion of a shared consciousness. The concept which is most relevant in the present context is, nevertheless, Thomas Kuhn's concept of scientific paradigms from 1962, whether this concept be carried over from Eliade or not.

A second concept I am about to discuss is another one of Thomas Kuhn's, which he deals with in a paper from 1959, entitled 'Energy conservation as an example of simultaneous discovery'. It is reprinted in the 1977 collection entitled 'The essential tension'. This paper discusses the phenomenon of simultaneous discovery in science, when at least two scientists make the same discovery simultaneously, and independently of each other. Kuhn's example is the closely connected hypotheses on energy conservation which were advanced independently by altogether 12 European physicist, in three groups of four each, in the years from 1830 to 1850. On the cluster of these 12 independent scientists making very similar discoveries independently of each other, he remarks that "the history of science offers no more striking instance of the phenomenon known as simultaneous discovery" (p.69). He ends the paper with the question: "Why, in the years 1830-1850, did so many of the experiments and concepts required for a full statement of energy conservation lie so close to the surface of scientific consciousness?" (p.104).

This question, from 1959, was answered in a general form three years later, in his Structure of scientific revolutions, from 1962. It is the connection between these two papers, that I will elaborate on here. I will refer to his 1962 paradigm concept as 'shared consciousness', and to his 1959 concept as 'simultaneous discovery'.

The awareness of a shared consciousness was a prominent feature in nineteenth century idealist philosophy - which also Kuhn seems to suggest in his 1959 paper. I find it convenient to consider the period <u>between</u> the end of the nineteenth century and the appearance of Kuhn's philosophy of science (that is, roughly the period 1900-1960) as an intermediate stage reserved for a highly specialized cultural development. That development is, needless to say, the development of the information technology in the computer and the cultural formalization needed to interpret it. The mid point in this period between 1900 and 1960 is the thirties, with Gödel at the centre in 1931. My interpretation is this: In order to develop the new technology and its supporting epistemology, the metaphysical parts of scientific epistemology had to be abandoned. The new technology, even if it trespassed the old information technology vastly, could not be expected to formalize God. So, in order to interpret the new computers, the boundary to the knowledge which was to count as computable was defined in a first form almost exactly at the turn of the century, by the discoveries of new logical paradoxes and Cantor's proof for the existence of a transfinite domain essentially out of reach for the enumerative powers of those natural numbers which came to be the foundation for the new computers. It is probably important to notice that it was Cantor's explicit intention to study the theological interpretation of mathematics when he defined his transfinite numbers. With this boundary to the computable, a compartmentation of the knowledge-space was introduced, such that everything inside the boundary came to count as computable, and everything outside was - if not principally uncomputable, so at least - not computable in that sense of it which Alan Turing defined formally in the thirties. Metaphysics was pushed well beyond this boundary, and the logical positivists attempted to capture knowledge in protocol sentences - which in effect would have made everything within this scope computable.

Then, in 1931, Gödel proved that any such formal system, have it only the slightest complexity, will be incomplete and inconsistent. This achievement can be interpreted as signalling that <u>all</u> theory beyond a certain level of complexity ultimately is tied up to a realm beyond the scope of the theory itself, and consequently that any theory within the Turing boundary must be tied up knowledge outside this boundary. In the present framework, Gödel's proof entailed, in 1931, that the boundary could no longer be pushed any further. After this, the prerequisites for an epistemologically valid interpretation of the computer technology were made in the thirties. Tarski defined the new semantics, the new computability boundary was defined, and the computer was developed as the technical tool to handle this new knowledge within the scope of the Turing boundary.

In this interpretation, the socalled cognitive revolution in the latter half of the 1950's is <u>not</u> the beginning of a completely new era: It is, rather, the beginning of the return to a reopening of that not all too spacy knowledge which the new computability had confined us in, to reassociate it with a larger metaphysical attachment to a shared social consciousness, which it had been the marxists' task to keep in store while the logicians did the mechanical work.

This is where I see Thomas Kuhn. In the early sixties, computability within the scope of the Turing boundary had already started to grow a little boring, and Kuhn took a bold leap forwards and called his essay the <u>Structure</u> of scientific revolutions. In assuming that such revolutions, which represent transitions between mutually incommensurable paradigms and consequently should not be computable by any Turing machine, while still maintaining that they possess sufficient structure to be addressed in the manner he does, Kuhn has assigned to them a computability on a higher level than the Turing-computable. Hence if a Thought Filter Machine works in the background of Kuhn's paradigms, it must be in the form of what we could call a Cantor machine. Let us assume that anything which can be transmitted from one scientist to another in the form of a consistent scientific theory is also definable in the form of a Turing machine. This is today generally acknowledged in the practice of computer implementation and testing of hypotheses. A paradigm is then a sort of Cantor machine governing scientific thought beyond the scope of the Turing machine. This is how I interpret the cognitive revolution in the latter half of the fifties: The new science addresses the knowledge-space beyond the Turing-computable.

An example will make this clearer: After the onset of the computer technology, linguistics has been split into two branches: 1) Computational linguistics, which works with elaborating grammars in algorithmic form, implementable in computers, and 2) all the rest of linguistics, being left with a vague and seemingly uncomputable domain which comprises both the computer-implementable grammars in addition to its more vague domains. We should perhaps expect to find that any successful scientific theory of grammar should be implementable in computer programs, and that this is a convenient test on the successfulness of a grammatical theory: If we do expect to find this, then linguistics as a science is just a short-hand label for computational linguistics. If not, we must ask what distinguishes ordinary natural-language linguistics from the branch of computational linguistics. My answer follows from the above: Natural-language linguistics adds something to the branch of computational linguistics to the extent that it works with the linguistic representation of the social space, in particular those aspects of it which is concerned with the shared consciousness, or, in Kuhn's terminology, with paradigms. The non-computational linguists work with describing the units and nature of the signification in this social space (in the mind of what Orwell called the Big Brother). Linguistics subsists currently as an important representation of the era of cognitive science just as much by virtue of the role which Chomsky assigns to it in his Syntactic structures from 1957, when he explicitly rejects any discovery procedures for the working linguist. We are not, according to this programme, expected to account for how we make our scientific discoveries. This is where we transcend the Turing machine and enter the domain of the new Cantor machine. The linguist of natural languages is supposed to work by means of intuition and inspiration. This means that the non-computational linguist is supposed to address the mind-space which transcends what can be captured in protocol sentences. I suppose that this explicit internalization of grammar, which is the hallmark of Chomskyan grammar, heralds the entering of the new space for science, transcending the realm of the Turing-computable languages of rational explication. If this is not only the retreat to the previous century, which it probably isn't, then it is the beginning of a reformulation of that old perennial problem of the shared mind and consciousness which Western culture had to relegate to the communists while working out the design of the new computers.

This reformulation is the project which also Kuhn embarks upon around the same time. In his 1959 paper on the 12 simultaneous discoveries in physics in 1830-50, he advances a hypothesis of three factors relevant to the simultaneities which he finds plausible. These factors are:

1. The availability of conversion processes, such as the technical conversion of heat to work and vice versa.
2. The general concern with engines, as the technological mastering of the conversion processes
3. The philosophy of nature in the ninenteenth century.

Kuhn discusses these three factors in turn, and repeatedly refers to the views advanced by Faraday and Grove as essential to all of them. The years 1830-1850 saw a remarkable convergence of those sciences which contributed to the understanding of energy conservation through the mastering of the concept of physical work. Kuhn quotes a paper written by Mary Sommerville from 1834, about the connection between the new physical sciences and the convergence among these which had lasted from about 1829 (according to her). This produced a network of disparate scientific disciplines converging on these phenomena of physics. Kuhn says about the first factor, the availability of conversion processes:

"Faraday and Grove achieved an idea very close to conservation from a survey of the whole network of conversion processes taken together. For them conservation [i.e, of energy] was quite literally a rationalization of the phenomenon Mrs.Sommerville described as the new 'connexion'" (p.75).

This is the point which runs through Kuhn's paper, and which has perhaps the strongest explanatory force relative to his later concept of 'disciplinary matrices': The idea of energy conservation, which was conceived of simultaneously by this group of 12 different independent scientists scattered over Europe, could in itself be traced to the rationalization of this network of converging but disparate scientific disciplines. The simultaneous discoveries had an empirical basis, but the phenomenon of simultaneity of discovery must be traced to the rationalization which shifts the theories from their empirical physical basis to their exact interpretation in mathematical form. This essentially lends an aspect of idealism, which is the third factor, to Kuhn's interpretation as well.

I return to the point below, but I will mention that in the context of his discussion of the German 'Naturphilosophie', Kuhn cannot resist the temptation to make notice of the concern which several of these physisists also nourished relative to the phenomenon of the light colour of venous blood in the tropics (a slighty surprising - but possibly important - point), in light of the conclusions which could be drawn as to the relation between oxidation and loss of heat from the body, relevant to the concept of physical work. This conversion process (relation between the colour of blood in the veins and the surrounding physical temperature conditions) is discussed by Kuhn in the context of the third factor, the natural philosophy, concerning the view that object and subject converge in an idealist reality wherein such conversion processes may take place.

He takes recourse to the approach of Faraday and Grove and the rationalization of the scientific network of the highly disparate approaches not only when he discusses conversion processes and natural philosophy, but also when he considers the second factor, the concern with engines. His discussion leads to the conclusion that the historical phenomenon of simultaneity of scientific discovery must be traced to this shared component in the three factors.

When the rationalization of the network of the converging empirical scientific disciplines leads to simultaneity of scientific discovery, we should indeed expect to find that the simultaneity phenomenon is most prominent in the most rationalizing discipline itself, that is, in mathematics. The connection between mathematics and the concept of a shared consciousness can in fact easily be traced in the philosophy of mathematics. There are three main schools of thought generally acknowledged as prominent when it comes to the ontological status of mathematical objects. The majority of mathematicians probably subscribe to the classical platonist or logistic schools, in modern form represented in Bertrand Russell and the tradition from him. These mathematicians assume that the objects of thought which the mathematical symbols and expressions refer to have independent existence in some platonic realm, and therefore have existence independently of whether they are addressed by humans or not. Hence when this is accessible to all matheaticians, we have a counterpart to the shared consciousness in it. The two other prominent schools are the intuitists, who presume that mathematics derives from a primordial intuition which has a universal status, and the formalists, which was the progressing problemshift in the twenties, but which suffered particularly from Gödel's proof, signalling a social dependency on all sufficiently complex formal systems.

Hence we may safely assume that most mathematicians are inclined to acknowledge an epistemological level beyond the individual mind as essential to the nature of mathematics, and therefore that, if anywhere, the hypothesized shared consciousness should be found as pertinent to mathematics, as the primary example from the sciences. Accordingly, simultaneous discoveries should be a prominent feature in the history of mathematics, and it is here that we find a possible test condition for the hypothesis: If there is a shared consciousness relevant to science, major revolutionary shifts in the history of mathematics should be characterized by simultaneous discoveries.

The story is quickly told: Leafing through the textbooks on the history of mathematics soon convinces the reader that this indeed is the case. Unfortunately, I have, in spite of extensive searches, not been able to find any systematic studies of the phenomenon of simultaneous discovery in the history of mathematics, but I have made my own superficial studies. I cannot present any statistics from this, but I can suggest that the overwhelming number of reported cases met with in the literature will provide any such study with the needed basis for making what I assume will contribute to the supporting evidence for the hypothesis of such a characteristic feature in the science and history of mathematics. If the study is not already made, I suppose it will be.

I will here restrict myself to the mention of a few important and fairly well-described cases essential to revolutionary shifts in mathematics. The most famous example is the priority dispute which followed the grand paradigm shift inherent in Leibniz' and Newton's discoveries of the infinitesimal calculus in the latter half of the seventeenth century (around 1670-80). The opening of this field of mathematics counts as a true turning point in the history of Western mathematics, and one may well consider everything preceding it and everything succeeding it as two radically different compartmentations of the history. It is crucial that this profound discovery, which entails working with infinitesimally small values approaching zero, was made by two outstanding mathematicians at the same time, but in different countries. This case is telling by its profound importance and the far-reaching effects of the priority dispute which followed. It is well described in the literature.

Another very telling case is the discovery of the non-Euclidean geometry after some 2000 years of mathematical uneasiness by Euclid's fifth postulate. It is true that there had been some increased attention to the problem of deriving the fifth postulate from the other four in the decades preceding the solution of the problem, and this could have contributed to the eventual solution, but the simultaneity of it, by Nicolai Lobachevski in Russia and János Bólyai in Hungary, consisting in omitting the fifth postulate entirely and thereby creating a novel geometry, is nevertheless impressive in light of the 2000 years preceding it. The story is, though, possibly telling for another important aspect as well: Carl Friedrich Gauss has traditionally been credited with priority to this discovery, but he never published the finding. This may, as insinuated by some historians, possibly be traced to the young János Bólyai as a source of his discovery. The father of Bólyai was also a mathematician and a close friend of Gauss, and the son told his father in 1823 about some plans he had for solving the fifth postulate problem by just omitting the postulate and creating a completely new geometry instead. Some time after, Gauss let it sift out to the mathematical world that he had come across a solution to the problem (even if he humbly refrained from publishing it), but the young János was occupied with other tasks and could not find the concentration and opportunity to work out his theory, even if his father started to urge nervously on him. Then, in 1829, seven years later, it happened that Nicolai Lobachevski, who likewise had common acquaintances with Gauss, published the same solution, albeit in a Russian journal which was sufficiently remote to keep the solution out of European attention for still some years. It was nevertheless in that same year 1829 that János suddenly got the spur to write his theory down. His theory was published in 1832 as an appendix to a mathematical work of his father. This revolutionary turn in nineteenth century mathematics was the solution to a puzzle which had rid the mathematical world through 2000 years. János later discovered that he could have been deprived of the priority to the discovery, and he did not manage to publish anything more in mathematics in the course of his lifetime. Unfortunately, many of János Bólyai's mathematical works are still unpublished. The librarian at the manuscript collection in Budapest has promised to send me a list of the titles.[11]

This story may be telling for a certain logic of discovery characteristic for the attachment to and impact from a hypothesized shared social consciousness which recurs with considerable anecdotal similarities in a somewhat related process, taking place a few years later, but still in that same period of time which Kuhn deals with in his 1959 paper. This was the discovery and elaboration of the modern hypercomplex numbers in 1843-44, an event which led to a true revolution in algebra. Sir William Hamilton, who as a child was a prodigy and was said to have mastered 13 languages

1. I later received a letter from the librarian. The manuscript collection in Budapest has now got copies of the 13000-15000 pages left after Bolyai in Tirgu Mures, Romania. Most of these are still unpublished. Elemér Kiss has recently tried to tidy up a little in the messy material.

when he was 13 years old, got his famous 'flash of genius' on an evening stroll together with his wife, after fifteen years of fruitless pondering on the problem of complex numbers. He suddenly received the solution in the form which he termed quaternions (with three imaginary components instead of one), and he immediately inscribed the solution into the bridge stone wall he happened to pass in the same moment. Meanwhile, in Stettin, Hermann Grassmann sat working with his grand 'Audehnungslehre', a theory of socalled extension which defined a much more general and far-reaching algebra, but in important respects identical in the sense of making use of numbers as classes of numbers. It is from Grassmann's work, which was started in 1840 and published in 1844, that the modern algebra derives. Hamilton's more narrow solution soon became famous, while Grassmann's work fell into oblivion - and he finally left mathematics and turned to the study of Indo-European linguistics instead, where he wrote a Sanscrit dictionary still in use, and discovered the wellknown Grassmann's law. Today, though, Hamilton's quaternions are quite 'out', while Grassmann's algebra is certainly 'in'.

These three cases of simultaneous discovery all rest on the turning points to large and revolutionary paradigm shifts in mathematics. It would be an interesting topic of study to investigate all the major paradigm shifts in the history of mathematics, from the point of view of verifying whether such simultaneity of discovery occurs systematically in these turning points. Kuhn's 1959 paper, taken together with his theory of paradigms or disciplinary matrices, seems to suggest that such simultaneity should expectedly be found in such cases, the more prominent the more important the discovery is. We may well stop and ponder the question: If indeed there is such a logic of discovery in the exact sciences, what is the rationale behind it?

I will not attempt to answer this question here, and neither does Kuhn provide any explicit suggestion to its solution, but I will point out that a certain ambiguous use of language in his paper seems to be telling for an important aspect of the problem. In the majority of cases where he refers to the concept of physical work (that is, in the technical sense of it), he uses the somewhat strange way of expression 'THE CONCEPT WORK' - that is, instead of the expected form 'THE CONCEPT OF WORK'. This ambiguous term refers both technically to physical work (the concept work) and to the idea of work with concepts (the concept work). I find, by a close reading of the paper, that this ambiguity is far from unimportant for the question. The most striking use of it is found on the pages 86-87 in the 1977 collection, where the following wording is found in the context of discussing the eighteenth century's pendant to the concept of work, in the concept of the socalled VIS VIVA (that is, 'living force' as a technical term), and the reinterpretation of it in the first half of the nineteenth century:

"Nor was this new dynamical view of the concept work really worked out or propagated until the years 1819-39, when it received full expression in the works of Navier, Coriolis, Poncelet, and others. All these works are concerned with the analysis of machines in motion. As a result, work - the integral force with respect to distance - is their fundamental conceptual parameter" (p.86f, my emphases).

This and similar passages clearly indicates that the formulation THE CONCEPT WORK, running through the paper, is deliberately intended to have the ambiguity of meaning which he later assigns to the Gestalt switches of his paradigm shifts in 1962. The ambiguity of this expression carries in fact a considerable part of the explanatory force in this paper.

As suggested in a paper by David Bloor, we may turn to Wittgenstein for support of the contention that mathematics is the concept work: In the Remarks on the foundations of mathematics (book V section 46), Wittgenstein says: "Mathematics form a network of [social] norms". Bloor interprets this in the sense that the ontological status of mathematics is the same as that of a social institution. Consequently, mathematical work has repercussions in the social space. This seems to bring us somewhat closer to a rationale behind the phenomenon of simultaneous discovery: We make the daring leap of assuming that revolutionary work with mathematical concepts is also revolutionarily present in the (more or less) platonic regions of the shared consciousness, which consequently means that the work can be perceived by other mathematicians working in the same regions of mathematics, even if they are in considerable geographical distance from the source of this revolution. Furnished with this bold working hypothesis, we can turn to an interpretation of the phenomenon of simultaneous discovery with more incisive tools. For example, we can hypothesize that the young János Bólyai, in order to work out his non-Euclidean geometry, for some reason needed somebody to collaborate with in the platonic realm. I will not speculate on the reason, beyond the somewhat trivial assumption that if mathematical concepts are essentially social, or coincide with social concepts, then the elaboration of new mathematical concepts may also require social collaboration in some form or other. János first tried his father, who could not help him much, but who leaked the project to Gauss, who neither was really cooperative. However, Gauss seems to have hinted to the mathematical world that some support was needed here. János did not find the metaphysical support he needed untill 1829, when somebody occurred in those same metaphysical regions, when Lobachevski in Russia, completely unknown to János, started working in the same parts of the mathematical paradise. This gave to János the incentive and socio-metaphysical support which his contemporaries thought he needed, and he quickly wrote his hypothesis out.

In this interpretation, I assume that the revolutionary paradigm shift in geometry was to be socially implemented in the form of a simultaneous discovery with contribution from at least two mathematicians. It adds to the irony that these two minds, even if the present interpretation seems to assume an interdependency in the metaphysical realm, nevertheless had to be independent in the observable realm, since neither his father nor Gauss could function as the needed support for this revolutionary discovery.

These traits recur strongly also in the way the story about Hamilton and Grassmann usually is told. Gauss seems in fact to have had a metaphysical finger in this story as well: From his own scattered hints and his own copies of the letters which he had sent to friends, it seems as if he had discovered equivalents to quaternions even before William Hamilton started pondering the problem. Hamilton probably started the concept work in 1828, but without much success. Then, after Grassmann had started to work out his impressive 'Ausdehnungslehre' in 1840, Hamilton got the flash of genius in 1843, just before Grassmann, who never got a chair at a university, had finished his work. Hamilton hurried to the local mathematical society and announced his discovery. Again, it seems as if Hamilton could not really make the discovery without Grassmann's work and metaphysical impact.

There is divine irony in a farcical priority dispute which arose shortly after the Dubliner Sir William Hamilton had announced this revolutionary discovery of the quaternions. Another man with the same name - Sir William Hamilton, a Scottish philosopher with a strong interest in logic and metaphysics - had been lecturing on the quantification of predicates since 1839. In 1846, around the time when Grassmann sent his 'Ausdehnungslehre' to Cauchy in Paris, the metaphysical Hamilton got a letter from the Englishman Augustus de Morgan (one of the socalled founders of symbolic logic), who was just about to publish a work on the quantification of predicates, and who wanted some additional information from Hamilton before the publication. The metaphysical Hamilton sent him some of his papers, which de Morgan made use of in his publication, and a furious priority dispute arose: The metaphysical Hamilton accused de Morgan for plagiarism, in making use of the material he had sent him. The metaphysical Hamilton got so upset by the ensueing priority dispute that he almost lost his mind, as the story-tellers know to phrase it. De Morgan succeeded in prolonging the horrible dispute untill the metaphysical Hamilton finally retired from the world in 1856, and he even managed to prolong it after that, against the students and supporters of the metaphysical Hamilton. This lasted untill 1862, when finally everybody was tired of the nonsensical and noisy dispute, and it silenced.

Then, in 1862, Grassmann, who had been left in oblivion in Stettin, made a second edition of his 'Ausdehnungslehre'. He wrote in the preface to this and to a later reprint of the first edition that he was very disappointed by the neglect and complete silence which had followed the first edition, and by the fact that he never got an academic chair, and that, in 1854, he had written to the Academy in Paris with a request for considering a possible plagiarism by Cauchy, to whom he had sent his 'Ausdehnungslehre' around 1845-46. However, the committee never answered his letter, and the request was left in silence. After the publication of Grassmann's second edition, de Morgan in London (who, of course, knew who the mathematical Hamilton was) once again tried to start the dispute with the metaphysical and now expired Hamilton, but after a few unsuccessful attempts, he gave up. Grassmann then left mathematics and turned disappointed to the study of languages instead.

This sad and absurd interpretation, associating Grassmann with the metaphysical Hamilton, as a kind of shadow representative in the platonic regions of the exact sciences, or as the one whom the mathematician Hamilton mistook for himself in 1843, seems perhaps far-fetched in the light of traditional constraints on historical interpretation, but it should be tolerable as a possible account under the assumption of a platonic or social realm accessible to all, such as most mathematicians are inclined to believe in. It is my contention that linguists who are not computational linguists must be ready to acknowledge such slightly absurd accounts, if the bold working hypothesis finds empirical support from studies in the history of science.

There is also a story about priority disputes from considerably more paradisial regions than has been discussed so far, with obvious relevance here. This is the one found in the Bible, in Genesis chapters 25-35, in the story of Esau and Jacob, with their female counterparts Rachel and Leah. The story of the twins Jacob and Esau is well known, how Jacob acquired Esau's birthright with a dish of stewed circular lentils (and a little bread) when Esau came hungry home from the fields. It was this which later made it possible for Jacob to acquire his father's blessings in Esau's place with some shade of legitimacy. He made use of young goat fleece to imitate Esau's hairy skin, in order to deceive his father of the blessing which rightly should have befallen Esau. Jacob took Esau's blessing from their father, as had the divine providence already foreshadowed long before. Rebecca told him that the priority dispute which came to follow was too harsh for Jacob to remain in Isaac's land, so he had to flee to his father-in-law Laban, where he stayed for fourteen years. He was supposed to stay for seven years and then marry Laban's daughter Rachel. But on the wedding night after these seven years, he got Rachel's elder sister Leah instead, and when, in the next morning, he reproached Laban for having made a mistake, Laban just answered that they usually married the older daughter before the younger one in his country. So Jacob had to work another seven years to get Rachel as well. This mistake was, of course, a copy of his own replacement of Esau's birthright with his own.

The sojourn in Laban's land is a copy of the events in his father's land: When Jacob is about to return to Isaac's country, Rachel steals her father Laban's god images, as a counterpart to Jacob's own stealing of his father's blessing. When they approach Isaac's land and are about to meet Esau again, he wrestles for a whole night with an angel who finally touches and displaces his hip. Again, this is Esau's representative in the social/angelic realm (a kind of platonic region, we may assume), somewhat like the metaphysical Sir William Hamilton who occurred instead of Grassmann in the priority dispute.

This aspect of TWO instantiations of the same person runs through all of this story about the twins Esau and Jacob. The story is not interrupted by Jacob's stay in Laban's land, but, rather, it is here that it touches directly on the three factors which Kuhn suggests as essential to such cases of simultaneity. Before they leave Laban, Jacob is allowed to pronounce his wish for wages for the fourteen years of labor. He suggests the following conceptual division line as defining for his wages: Every goat in Laban's flock which is striped or speckled or spotted, and every lamb which is dark-coloured, shall be his, and every animal which does not possess this geometrical pattern in Jacob's flock shall be considered stolen. But Laban removes all these animals from the flock before Jacob gets the chance to select them, and Jacob is left with nothing. All animals in the flock are suddenly without these geometric patterns.

Now Jacob creates the technology which is the counterpart to a new revolutionary discovery. He tends Laban's flocks, and in the course of this work, he takes rods from poplar, almond and plane trees and peels the bark in streaked geometric patterns to expose the white wood underneath. These patterned rods he handles like a machine to generate the conversion process he needs: He puts the streaked rods into the watering trough when the strong animals are in heat, and - somewhat miraculously - these strong animals conceive young which possess precisely this defining geometric property on their sheepskin, as transferred from the pattern on the peeled rods. However, when the weak animals are in heat, he does not put the peeled rods into the water trough, which has the consequence that the young of the weak animals do not acquire these defining geometrical patterns on their sheepskin.

That is, Jacob puts the rods in and take them out somewhat like a mechanical engine. Jacob then separates the strong young with this property from the weak young without it, and gradually his share of the flock is shifted into a strong and healthy and growing part, while Laban's share of the flock weakens and sickens away. This is the conversion process which shifts the wealth from Laban to Jacob.

This technology possesses exactly the three defining factors which Kuhn lists as relevant to the simultaneity of discoveries:

1) The selective exposition of the peeled rods before the strong animals in heat only, and their removal before the weak animals. This is what Kuhn calls THE CONCERN WITH ENGINES.

2) The fact that this selective exposition produces a transference of strength or power from Laban's share to Jacob's share of the flock. This factor is what Kuhn calls THE AVAILABILITY OF CONVERSION PROCESSES.

3) The idealism ('Naturphilosophie') in the Jacob story appears in the moment of transference of similarity from the sensory impression of the streaked rods onto the young of the conceiving animal (that is, the animal doing THE conceiving or CONCEPT WORK). This is exactly the same point which Kuhn makes, when he takes recourse to the observation that venous blood (that is, supposedly, the blood in the veins, and not the blood of Aphrodite) is lighter in tropical areas than normal, due to less loss of heat from the body. This was Kuhn's sheepskin in his 1959 paper: Indeed, Jacob made use of the same trick when the animals were in heat (that is, sexual heat rather than tropical or technical heat). This represents the moment of identification of subject and object which is typical for the idealist absolute: Subject and object are identifiable when the objective rods leave their imprints on the young of the perceiving and conceiving animals. Hence, the transference of similarity from the peeled rods to the sheepskin represents THE NATURAL PHILOSOPHY WITH IDENTIFICATION OF OBJECT AND SUBJECT.

These three factors have an important role to play when Jacob is away from his twin Esau, who has an essentially HAIRY skin in the story: When Jacob appeared before Isaac, who could not see him, he was disguised in young goat's skin to imitate Esau. The flocks he tended in Laban's land is a consciousness or subconsciousness he initially shared with Esau, and which he gradually acquired.

Jacob's household eventually gave him their lentil-circular earrings together with their images of foreign gods, which he all buried under an oak at Schechem. Jacob then had his name changed to Israel: He was the head from which all of Israel's twelve tribes descended, indeed the root vertex of the Jewish society.

Hence, the Jacob story represents the Biblical version of the relationship between simultaneous discoveries and paradigms. The priority dispute which followed the mistaken blessing of Jacob is a part of the authority relegation in the major revolutionary paradigm shift in the early mythological history of Israel: Isaac gave his authority over to his follower in the moment when the nation of Israel was born, and divine providence had already at the outset secured that he would give it to the wrong man. Isaac 'trembled very exceedingly' when he discovered the mistake.

It is pertinent to the history as well as to the theory of science to observe that the Jacob/Esau story has remarkable similarities not only with important events in the history of mathematics, but also with the central moments in Kuhn's 1959 paper, making way for his concept of paradigms a few years later (consult for example Margaret Masterman's overview of this concept in the Lakatos/Musgrave collection, including even the concept of 'an anomalous pack of cards'). The acknowledgement of a shared social consciousness wherein paradigms produce their thoughts would be a considerable leap forwards, if this entails that we are allowed to make such inferences on historical dependency as in the example with Bólyai and Lobachevski, or with Hamilton and Grassmann. If we <u>can</u> indeed approximate such knowledge scientifically, then we have entered a radically new space of knowledge - presumably to be identified with the new knowledge-space inaugurated by Chomsky.

I have not touched much upon the corollaries for the theory of science which follow from the assumption of such a shared social consciousness, governing conjectures and refutations as well as the general confidence which the scientific community takes in empirical and theoretical results. No doubt, the <u>mastering</u> of this knowledge-space will belong to future, but we should nevertheless be able to perceive the traces of this shared consciousness and its knowledge even today. I would like to make some comments on how I believe that we can interpret the assumptions advanced here in everyday situations and scientific work. It goes without saying that the paradigms in Kuhn's sense hold a certain sway over our thoughts in manners which we are normally not really aware of: We aquire the thoughts we do, because these are in conformity with the current paradigm, and we simply do not think of other thoughts. I think that we sometimes can come across evidence for the paradigm emerging as a shared consciousness in rather trivial situations. Assume that you are writing a letter and go to mail it in the mailbox. As you approach it and prepare for the letter to be put in, you suddenly experience this vague but still strong resistence to it. You feel that you should not mail the letter, even if it looks perfectly appropriate to do so, and the addressee supposedly will appreciate the letter. The irrational resistence nevertheless convinces you that there must be something wrong, and you give in to the incentive and put the letter back in your bag again. The resistence never gets an explanation, and you never send the letter.

I believe that many people would behave in this manner, and give in to such irrational incentives without knowing why. Many would do so without even knowing about it consciously, and many would also think that such resistance signals a kind of genuine noblesse, a sort of aristocratic remains which still heralds a residual dignity for mankind on earth. There are of course many ways to interpret such 'signals', if we may call them so, but in the present framework, I am inclined to interpret them as expressions for the current paradigm, holding its members with a kind of split and conquer strategy. This point is, I believe, of importance for the theory - and, in particular, for the practice - of science. We can, in fact, contribute to an understanding of the working of the hypothesized shared consciousness even today by relating in a more conscious and mature manner to this social paradigm. An example from my own experience is telling: I experienced over and over again in the course of my first year as a doctorate student that queries and questions to the university institutions remained unanswered, somewhat in the manner of the letter I just spoke about. In this period, and, in particular, in the period when I came to work on this lecture, I had, due to the obscurity of the state I was assigned in the system, to send a number of requests to various instances in the university system. I counted that the accumulated span of time which I had to wait for answers to these questions ran to something close to four years altogether, summed up in the course of only the first year of my project, and distributed over, say, ten such requests. The resistance and silence was so immense that I had to conclude that irrational powers were at work. Repeated requests led to nothing, and the practical difficulties which ensued from this came to hamper my scientific progress. I conjecture that this was the effect of the scientific paradigm control in its self-preserving function, and the irrational resistance was to be explained along the lines of the letter which was never sent. Probably, the administrators and colleagues who were supposed to answer would have been overcome with irrational anxiety (produced by the Big Brother) if they had ignored the resistance and dropped the letter in the mailbox. The overt rationales behind the delays may still have been well-founded in the traditions of the university institution, but could, I believe, hardly stand a test of scrutiny as to its scientific purpose.

This is a kind of problem which the theory of science has perhaps neglected too much, but it is clear that a contribution which allows for a heightened consciousness as to the impact from a shared consciousness also may improve science in this very practical and immediate sense of it, in addition to the far wider perspectives which open by a future formalization of this knowledge-space (the Cantor machine), wherein conjectures and refutations are thoughts in the mind of the more or less capricious community. In order to master the knowledge of the shared social consciousness, we must probably transcend that Western tradition which goes all the way back to patriarchal times of conquering persons rather than the scientific problems we are out to solve. For example, competition and career are the worst possible drives for gaining such scientific knowledge. Wittgenstein and Kuhn seem to converge on the view that exact sciences, preparing the path for the less exact sciences such as physics, come down to a matter of social consciousness. This is, I believe, a very important point to observe for the theory of science.

Finally, I would like to mention that, in immediate succession to my preparatory work with the Jacob story in this lecture (from the beginning of September), the university was visited by Kjartan Slettemark, who made a performance at the university square on the 19th of September. I failed to see the performance, and I had no knowledge of it in advance, but I afterwards saw the report in the university newspaper, which convinced me that he had worked with similar concepts in the course of my work: Together with the two sisters Karin and Marie Grønlund (representing something like Rachel and Leah Grove), he painted twelve pictures, in close parallel to the pictures which Rachel stole from her father, and to the twelve tribes of Israel. The slogan reported from the performance was: I AM STUEREN, to be interpreted: I AM THE STEWER, that is, Jacob with his lentil stew. After having read the report in the newspaper, I understood the importance of Slettemark's contribution in the shared social consciousness to my own preparatory work to this lecture, and the institutionalization it received during the performance. This is evidently also a part of the function of his performance art and his art in general, addressing the same social knowledge-space as I have been concerned with here.

The university director bought the twelve pictures, and Jacob, Rachel and Leah left Bergen without them.

My thanks for Slettemark's contribution and for your attention.

Bergen 21.11.96

For John Grøver
Postboks 320
5001 Bergen

Dear John Grøver!

I send this letter on behalf of the committee for your lecture in the theory of science for the dr.art. degree. All members, Lars Johnsen, Marianne Skånlad and I myself, agree that the lecture, in the form it has now, cannot be accepted. Below, I have explained the reason for this, in a reasoning which we all agree upon. (Before this letter was sent to you, a copy was sent to the other two members, who have accepted the contents). In addition to this, I include an expert opinion from senior scientific officer Mickal Walecki at the Institute of Informatics, whom we have used as an expert on the things you write about mathematical/logical foundation research.

You show extensive learning in your lecture, but a main problem for you is that you draw analogies and see connections where we hardly see any. You want to embrace everything, but thereby you run the risk of embracing nothing!

Most serious in this respect is your attempt to see parallels between the science-theoretic problem which you deal with in the first part of the lecture - how one may understand the fact that important scientific discoveries sometimes (often?) are made by different scientists at approximately the same time -and the story about Jacob and Esau in the Old Testament. To pose the first question is legitimate. But the connection to Jacob and Esau seems extremely farfetched, as I also gave expression to in the telephone conversation with you at the end of the last week. Does the Bible tell of scientific discoveries, or even of such simultaneous ones? We can see absolutely no connection between the original problem and what you write from page 28 onwards, which has the effect that the first and the last part falls entirely apart in our view.

But even within the pages up to page 28, there are clear traces of the same inclination to mix phenomena which obviously seem different. Your way of conceiving Kuhn's concept of paradigms is, in this respect, symptomatic. On page 1-2, you see a parallel between Kuhn's concept of paradigms and the concept of archetypes, but to us, this seems strange. Paradigms, such as Kuhn conceives of them, change, while the archetypes are thought of as constant representations in our subconscious ideas. Furthermore: Even if scientific paradigms according to Kuhn have unformulated elements, a kind of conditioned scientific spinal reflexes, they nevertheless contain consciously formulated scientific ideas which hardly can be compared with archetypal ideas. - On the other hand, Kuhn's concept of paradigms has been too closely related to ideas within mathematical/logical foundation research when you, on page 14, see an analogy between Kuhn's conception of science and Cantor's mathematical ideas. Kuhn's concept of paradigms is all too informal to allow for a comparison with strict mathematical concepts.

Another important concept which remains indistinct and somewhat fluid in your discussion is the concept of 'shared consciousness'. In order to account for the concept, you sway between extremes which hardly are suitable 'bedfellows'. On page 24f. you write: "We make the daring leap of assuming that revolutionary work with mathematical concepts is also revolutionarily present in the (more or less) platonic regions of the shared consciousness, which consequently means that the work can be perceived by other mathematicians working in the same regions of mathematics, even if they are in considerable geographical distance from the source of this revolution". How this is to be understood, is left very open. - What are 'the platonic regions'? - Do platonic ideas give rise to a form of extra-sensory perception in disparate researchers? But on the other hand, you relate the cleft consciousness of mathematicians to presumptuously solid social structures. You thereby seem, just above on the same page (page 24), to accept the reductionist science-theorist David Bloor, when he compares mathematics with a social institution. - The problem is here, as other places, that very different ideas are wiped under the same rug.

Another important objection is that you do not give scientific evidence for central assertions. It is important for you to show that there is a conscious ambiguity in Kuhn's use of the expression 'the concept work' (cp. page 23f.). You interpret Kuhn to mean that this expression of his means both the concept of physical work (in a technical sense) as well as work with concepts. The quote you bring from Kuhn just below, seems to us to contradict this hypothesis. The use of the expression "the concept work" which occurs in the first line in the quote, is later on in the same quote clearly connected with the expression "the analysis of machines in motion". That should indicate that only a solid physical inter-pretation of "the concept work" is intended. It is plain enough that Kuhn, in the same quote, also makes use of the word "work" about intellectual work. But the point is that there seems to be no ambiguity in any of the occurrences - "work" means either the one or the other. Apart from this, it is seldom that scientific discussions consciously strive towards ambiguity, therefore your hypothesis lacks "initial probability". Furthermore, you do neither give any deeper sense to the point that this alleged ambiguity of meaning (in "the concept work") is related to his concept of Gestalt-switches in "the Structure of Scientific revolutions".

When it comes to your main question, simultaneous discoveries of identical theories or concepts in the sciences, this is in itself interesting, and that you at the outset relate them to Kuhn's concepts, seems sensible. But your explanation to the phenomenon seems to be somewhat hovering. An unmotivated displacement of the problem is also taking place. One thing is that there occurs researchers, independently of each other and approximately at the same time, who reach the same important discoveries. But you seem, entirely unmotivated, to inflate this phenomenon to metaphysical dimensions. Compare what you say on page 25f.: "In this interpretation, I assume that the revolutionary paradigm shift in geometry was to be socially implemented in the form of a simultaneous discovery with contribution from at least two mathematicians". I am not entirely sure, but it seems as if you here mean to say that there had to be two persons who simultaneously contributed to the scientific turn from Euclidean to non-Euclidean geometry. In any case, it would have been valuable for you to direct the critical searchlight in the opposite direction: how common is it really that several researchers make the same discovery at the same time? Aren't there many examples of unique discoveries, such as only one researcher has contributed to, or even discoveries which are made once, then are forgotten for a long time and then rediscovered by another researcher? Before you critically have investigated the spread of your explanandum phenomenon - the fact that the same creative ideas appear in more than one researcher at the same time - you can hardly say anything well-founded on how important it is, or how pressing it is to find an explanation to it. You, on the contrary, seems to take it for granted that such simultaneous discoveries are the rule, in any case in mathematics, without giving any rationale for it.

This does not mean that I suggest that you should give up this as a topic for your lecture in the theory of science - only that you ought to streamline your conceptions of the problem. You are doubtless both knowing and clever, but I think that it is most fair to tell it right out: If you are to have the lecture accepted, you must grasp yourself critically in your neck. One thing which finally ought to be mentioned is this: You should avoid including references to your relations to the university administration in a scientific work. That makes it too private.

Sincerely

Erik Brown

Walickis expert opinion

As to "Kuhn's paradigms and the simultaneity of scientific discoveries", by John Grøver.

I was asked to give an expert opinion relative to the more specific mathematical and logical aspects of the lecture. I therefore look apart from what looks like the 'main message', from approx. page 28 onwards, where the relationship between 'simultaneous discoveries' and 'paradigms' is transferred from a science-theoretic and -historical context to a somewhat more private and esotherical sphere.

I would sum up the contents of the first 10 pages as follows:

1. Introduction of the concepts of 'simultaneous discoveries' and 'paradigma' ("shared consciousness").

2. Indication that "computability" can be considered as a paradigma in the period 1900-1960, along with a distinction between
   2a. "Turing-computability" vs.
   2b. "Cantor-computability"

3. Discussion of the three factors which Kuhn recognize in 'simultaneous discoveries', with the main thesis that such processes emerge in a process of 'rationalization' of "network of converging but disparate scientific disciplines" (which seem to refer to 'shared consciousness').

4. Mathematics is supposed to be the most 'rationalizing' science, and a widespread presence of 'mathematical platonism' shall inicate that 'shared consciousness' is an essential part of mathematics.

5. Therefore, one should easily find 'simultaneous discoveries' in mathematics, and a series of three classic cases follows.

6. The seemingly evident - but unpronounced - thesis, viz., that 'shared consciousness' is a basis for 'simultaneous discoveries', receives an inter-pretation in the discussion of "the concept work" and the alleged ambiguity in this expression such as it is used by Kuhn.
7. The main thesis, on page 25, says that "the revolutionary work with mathematical concepts is also revolutionarily present in the platonic regions of the shared consciousness, which consequently means that the work can be perceived by other mathematicians..."

Let me briefly comment some of the points:

7.
Since the concept of "the platonic regions of the shared consciousness" remains entirely unexplained and unspecified, it is difficult to say what should be the author's contribution in clarifying the relation between the 'paradigms' and the 'simul-taneous discoveries' in mathematics (and elsewhere). The author does, though, indicate a possible influence on individuals through this sphere, but this influence has a character of completely "irrational powers at work". It is furthermore asserted (page 25f.) that a paradigm change in geometry should be implemented "in the form of a simultaneous discovery with contribution from at least two mathematicians". The implied necessity of 'simultaneity' suits the author's few examples well, but it does not suit well to a series of other situations which also could be called "paradigm shifts", wherein no 'simultaneous discovery' took place (for example the discovery/introduction of Euclidean geometry, irrational numbers, Galois theory, Cantor's transfinite numbers, Frege's logic, etc.).

6.
The paragraph on 'the concept work' was almost entirely incomprehensible to me and I could not determine it relative to the rest of the argument. The quote which should illustrate the difference between two alleged interpretational alternatives illustrates nothing.

2.
The presentation of the basic thoughts and results of logic reveals no fundamental deficiencies. There are only a few minor things which remain unclarified:

i) On page 12, it is asserted that "in order to interpret the new computers, the boundary to the knowledge which was to count as computable was defined ... at the turn of the century". At this time, nobody had as yet any ideas of "the new computers", so unless the author here wants to indicate some kind of developmental determinism a la Hegel or Marx, it sounds a little strange.

ii) Furthermore on page 12: "it was Cantor's explicit intention to study the theological interpretation of mathematics". One should be aware that Cantor excluded Russell's paradox (before it was discovered) precisely by not mixing mathematics and theology - according to Cantor, the universe of all objects could not be considered a mathematical object, because this kind of universe would be a potential object for God but not for mathematics.

iii) Page 13: "...were made in the thirties. Tarski defined the new semantics, the new computability boundary was defined, and the computer was developed as the technical tool to handle this new knowledge...". If by "new semantics" defined by Tarski is meant "formal semantics", then this semantics has little to do with the definition of computability. 'Computer' was not developed "in the thirties".

iv) Page 14: "...anything which can be transmitted from one scientist to another ... is also definable in the form of a Turing machine. This is today generally acknowledged in the practice of computer implementation...". This sounds directly insensible to me. People who work with (theoretical) informatics are perhaps particularly aware of the obvious limitations in computers.

These and a couple of other places indicated for me that the use of terms such a "computability", "Turing-machine" etc. is not always to be understood literally in a technical sense, but that they function more as labels for more general phenomena which the author tries to approach. In this connection, there also comes the distinction between

2a. "Turing machine" and

2b. "Cantor machine"

which is fairly unclarified since 2a cannot be understood as a technical concept. In an honest attempt, I have strived to interpret 2a. as 'normal science', what is 'inside the paradigm', and 2b. as a kind of metalevel, or precisely a potential source for paradigm shifts. But, e.g., the example on pages 14-15 indicates that 2a. nevertheless should be interpreted in a technical sense. It would be helpful with a somewhat more clarified exposition of the concepts and distinctions one works with.

My general impression is that the author has satisfying knowledge of existing mathematical concepts and historical examples. To the extent that one can talk of presentation of any original ideas, these are at best obscure and inaccessible.

Michal Walicki
senior scientific officer

John Grøver
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27.11.96

To the Committee for acceptance of my lecture in the theory of science, University of Bergen.

I refer to the letter from the committee, authored by Erik Brown on 21.11.96.

I have now read through the letter and find no important objections to my lecture in the theory of science. I conclude that the committee by Brown disagrees with my conceptions on a few points, but that is of course inessential for the evaluation of the work.

The intimate relationship between Kuhn's paradigms and archetypes should be well known and is not so difficult to understand. I have made use of Eliade's concept of paradigms. A few quotes from Eliade (1949/52):

"[...] the same 'primitive' ontological conception: an object or an act becomes real only insofar as it imitates or repeats an archetype. Thus, reality is acquired only through repetition or participation; everything which lacks an exemplary model is 'meaningless', i.e., it lacks reality. Men would thus have a tendency to become archetypal and paradigmatic" (p.34).

"Hence it could be said that this 'primitive' ontology has a Platonic structure; and in that case Plato could be regarded as the outstanding philosopher of 'primitive mentality', that is, as the thinker who succeeded in giving philosophic currency and validity to the modes of life and behavior of archaic humanity" (p.34).

For his archetype concept, Eliade discusses the convertion of historical events to mythical form, encoded in archetypal format, and he is explicit on the point that archetypes encode the anhistorical memory of the collective in the social space:

"If certain epic poems preserve what is called 'historical truth', this truth almost never has to do with definite persons and events, but with institutions, customs, landscapes. Thus, for example, as Murko observes, the Serbian epic poems quite accurately describe life of the Austrian-Turkish and Turkish-Venetian frontier before the Peace of Karlowitz in 1699. But such 'historical truths' are not concerned with personalities or events, but with traditional forms of social and political life [...] - in a word, with archetypes. / The memory of the collectivity is anhistorical" (p.44)

The committee's objections about 'mixing' of concepts such as social institutions, archetypes and platonic levels of knowledge is, therefore, not my problem.

Neither can one, such as the committee attempts, assert that paradigms cannot be encoded in archetypical format because these can be said to be too 'constant representations in our subconscious ideas': This is equivalent with asserting that scientific theories cannot be replaced by other theories because they are logically consistent - and, as we know, the logic changes not so often in its foundations.

The traditionally most accepted forms of collective consciousness which I discuss in the lecture are church and nation. The church can for most western nations be traced back to Israel. The large social/political <u>paradigmatic</u> revolution which the foundation of the nation Israel entails is <u>essentially</u> a story about twins.

There are particularly three areas which can bring some empirical support to the hypothesis which I discuss:

1) The history of the exact sciences
2) Archetypical representation of historical myths
3) Social institutions

I discuss empirical material from all of these in my lecture: 1) From paradigm changes in the history of mathematics, 2) from the mythologically most relevant source in the Bible, which clearly supports the hypothesis in being about twins, an 3) the discussion of the social state by the official institution which the university is. THE COMMITTEE IS WRONG when it claims that I discuss my private relations with the university adminstration. I have discussed the handling of matters in a scientific institution, in relation to the hypothesis of a social repesentation of the paradigms. This empirical material is ideal for the problems I discuss.

The lecture is creative because it brings these empirical fields under a common understanding. The committee is on a low level when they accuse me of 'embracing too much'.

When it comes to geographically scattered discoverers: Kuhn himself discusses these problems on the background of spread, explicitly formulated in the first sentence of his article: "[...] the hypothesis of energy conservation was publicly announced by four widely scattered European scientists - Mayer, Joule, Colding and Helmholtz - all but the last working in complete ignorance of the others".

When it comes to 'the concept work': This expression is trivially ambiguous, and the narrative effect must be understood by the reader him/herself (I have not enough space for that in the lecture). I assign to it the same systematic meaning in Kuhn's article as Chomsky assigns to the construction "flying planes are dangerous"[2] in Syntactic Structures. The connection with the interpretation of the new cognitive knowledge-space should be self-evident. I point to the 'the word work' in my quote, where Kuhn uses the word 'work' in six different meanings (in the article in as much as eight different meanings right after each other) in the course of approximately the same number of lines.

All this is fairly trivial, and I see no reason why the comittee should have to reject the lecture on this basis. I would have liked to expand the discussion of most of these points, but it is the scarcity of space (the 45 minutes which I had to my disposal) which limits the lecture. If I remove some of the points from the discussion, the committee will understand even less. I think that I have found an ideal balance under the present circumstances.

2. The quote from Chomsky (p.87) should of course be: "They are flying planes"

The simultaneity of the discoveries is the very point: The core of it emerges as a result of the discussion. The key to it is found by Kuhn himself: It is in the interpretation of 'the rationalization of converging disciplines' from 1959 as a pre-stage to his later 'disciplinary matrices' in the Postscript from 1969. This interpretation of simultaneity of discovery is in the lecture given immediate relevance for the understanding of the concept of paradigms, and it is a concrete and substantial contribution to the theory of science.

Michal Walicki (his navn was misspelt in Brown's letter) seems to have a much clearer understanding of the lecture than the committee, but even here are his few substantial objections reduced to nothing (his objections to the points 6 and 7 have been discussed above):

2 i) The developmental determinism can be the collective consciousness with extension in time and space, or only a wisdom at hindsight which explains why the new logical paradoxes were necessary in order to interpret the computer culturally.

2 ii) This objection is a mistake: Cantor's own paradox says that the set of all sets is both larger than and equal to its own power set.

2 iii) Tarski's argument for the infinite hierarchy of metalanguages rests on the need to avoid semantical paradoxes in the infinite object language. This argument thereby leads to an infinite series of infinite languages, and it is thereby bound up to computational problems related to the potential uncountability of the set of all these languages (the set of Turing-computable languages is, as is well known, computable).

2 iv) the quote is incomplete. The entire sentence plus the following sentence should speak for itself, and should not be difficult to understand in the context. On the contrary, it enhances the understanding of the relationship between the Turing machine and the socalled Cantor machine, which Walicki has not entirely understood. The difference must be sought on the background of my concept of archetypes and paradigms and the social encoding of

knowledge, and cannot be immediately understood in the extension of ordinary 'computation theory'.

In other words: There are no serious objections from Walicki either. I will contend that my understanding of the relationship between the collective and the individual consciousness should indicate that the boundary to Turing-computability is tied up to individual consciousness. If nobody has said this before me (unknown at least to me), then it is a substantial novelty in my lecture, which directly touches onto the interpretation of Church's thesis. It is also an exact hypothesis with much empirical content, and I cannot understand how it is possible to make a better lecture in the theory of science.

I feel that the committee has pulled the level at the Faculty of Arts even further down by its letter of 21.11.96. They have no reason to reject my lecture in the theory of science.

John Grøver

Copy to:

The committee (Marianne Skånland, Erik Brown, Lars Johnsen)
Michal Walicki
Vice-dean

Part 2

The dissertation

The committee: Evaluation of dissertation for the PhD degree - John Grøver

1. On the dissertation

John Grøver has presented a dissertation in five volumes for evaluation for the degree of dr.philos. at the University of Bergen. The five parts of the dissertation have the following titles:

Vol.I	Submorphemic signification
Vol.II	Epistemes, language and information technology
Vol.III	The theatre of the heart (plus supplement with colour illustrations)
Vol.IV	A pilot study for a poetic science
Vol.V	A waist of time

The dissertation is thus not presented under a common title. Neither is there a common introduction nor summary. But the author emphasizes in his letter to the University that the dissertation has a strong internal unity: "All the parts are concerned with the ultimate goal of arriving at a formalization of the socially encoded knowledge in the domain of the socalled 'Cantor machine'". It is therefore natural to let our evaluation of the dissertation take the candidate's presentation of the socalled Cantor machine as our point of departure.

2. The candidate's suggestion for definition / description of the Cantor machine

Let us with the author take as a point of departure the case where two computational processes are running in parallel on two machines, which well may be interconnected. In what the author calls a closed system - and for him a neural network is a typical example of this - such computations have the same capacity as computations on a Turing machine. But suppose that each of these processes requires interaction from a user in the form that these users, from time to time in the course of the computation, actively must perform some action (e.g., answer a question, choose between various alternatives, etc.) as a part of the computational processing. We can then have a process which transgresses the limitation inherent in absolute Turing-computability.

This is standard theory, and there is much literature on socalled relative Turing-computability. The special suggestion which the author makes is to consider a computation relative to what he calls "the collective consciousness"; see e.g. the description on pages 7 and 8 in Vol.III. The concept of a collective consciousness, as "a level of shared knowledge which individuals communicate with and may consult when they are making choices (Vol.III, page 4)", is of course not an unknown concept, neither in anthropological analyses of the concept of culture, nor - to take an example from philosophy - K.Popper's concept of World 3. But here the concept is given a particular shape. We must distinguish between the author's vision and motivating description of the collective consciousness and his technical construction of the collective consciousness as a definition of the Cantor machine.

Motivating for the author is the collective consciousness, or at least parts of the collective consciousness, as a common 'platonic region', which individuals can communicate with "even if they are in considerable geographical distance from the source of this revelation (Vol.III, page 34)". This motivation stands centrally in the author's interpretation of Kuhn's dissertation "Energy conservation as an example of simultaneous discovery" and in his discussion of the non-evclidean geometry. But one thing is motivation and interpretation, another thing is the author's attempts at definition and technical construction as a basis for a precise theory. Grøver himself emphasizes strongly that formalization is possible and that there exists a "computational device", which he calls a Cantor machine.

In the author's attempts at formalization, Cantor's diagonal argument stands centrally. He connects this with Tarski's analysis of the concept of truth in formalized languages (- see also a commentary in 4.2.2 to the author's assertion on the relationship between Tarski and Gödel). Tarski was led to a hierarchy of languages, where a language on level n+1 expands the language on level n by giving a formalized definition of truth to this level n language. The author then wants to identify what he calls the diagonal language to such a Tarski hierarchy, as a version of "the collective consciousness" for use in a Cantor machine processing; see in particular the pages 52-53 in Vol.III. The assertion that "the achievement of Cantor was, as I see it, that he proved the existence of the collective consciousness, and showed that it could be formally described (Vol.III, page 9)" seems right away entirely unfounded. But one understands the author's intentions when one sees the "collective consciousness" tentatively constructed as a diagonal language.

But even if one understands the author's intentions, the analysis and constructive description are all too imprecise to meet scientific critic. The author concludes (Vol.III, page 71):

The collective consciousness can be represented in a list with all the indexes to the persons which are members of the community of this collective consciousness, annotated with the types which are or may be represented by archetypes. By varying these types, the social state can be modelled. If we take grammar types to be archetypes, we can conclude that the collective consciousness can be conceived of as a Cantor machine constituted by NAMES and ARCHETYPE ANNOTATIONS to these. This creates a link between the cyclic archetypal memory of the community and the phonology of the names of its members".

In the author's descriptions, we find words and intentions, but no technical analysis and construction, e.g. of how the alleged structure of the "collective consciousness" as NAMES and ARCHETYPE ANNOTATIONS explicitly are to be constructed as a "diagonal language". The author points to, and seems to want to build on, fields such as linguistics, logic and informatics. These are fields with well established requirements to knowledge and method; we cannot see traces of these scientific requirements in this part of the dissertation. Neither can we see that the last assertion in the above quote has been given any scientific rationale through the suggested construction.

3. The collective consciousness: from theory to examples

So far, we have adhered to the author's general analysis. But theory should be applicable in concrete situations, and the author himself puts much emphasis on the explicit analyses which the dissertation contains:

"The distributed object which is studied here is particularly interesting in the sense that it seems to be interpretatable in the framework of one literary text: Rainer Maria Rilke's fourth Duino elegy. This gives an excellent opportunity to study some linguistic properties of the collective consciousness (Vol.III, page 4)".

For the further discussion, and in order to give full justice to the author, we must read this paragraph and his further analysis in light of the last sentence in the quote in paragraph 2 above, where he postulates "a link between the cyclic archetypal memory of the community and the phonology of the names of its members".

We see in our evaluation hardly any reason to dwell long by the author's detailed expositions of the art exhibition BRUDD, names on the office doors in Allégaten 34 and episodes during an institute lunch in the autumn 1995. Guided by Rilke's elegy, the author finds connections, satiated with contents, between the various events. Since names and descriptions are the central elements in the collective consciousness, meanings are revealed by the study of transformations and symmetries by these names. The author can e.g. explain the somewhat special circumstances at the institute lunch by the following transformation. The lunch was held in

FONETISK LABORATORIUM[3]

If we move the initial F to the end of the name, we obtain an expression "close to" (- keep in mind the comment on the meaning of phonology in the above quote)

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[3] PHONETIC LABORATORY

ÅND-ETISK LA PÅ BORDET TRIUMF[4]

According to the author, ÅND-ETISK ['spirit-ethic'] will here refer to himself, TRIUMF will necessarily refer to the letter which he wrote to the faculty about the meaning of the nameplates in Allégaten 34 -a letter which was "put on the table" in the institute lunch referred to; see Vol.III, page 166.

The meaning of the nameplates are, according to the author, not the least in the hidden symmetries; e.g., the following name from an office door on the second[5] floor in Allégaten 34

SIV-ELLEN KRAFT

can be transformed by replacing KRAFT with the Latin VIS to

SIV EL LE(n) VIS

which is a form which displays a "meaningful" mirror symmetry; Vol.III, page 144. The parallel between the art exhibition BRUDD and the presence of certain persons in the institute lunch is revealed by the following kind of analyses; Vol.III, page 176:

"ØYVIND ANDERSEN --> NØYVIND ANDERSE, a form which is not immediately transparent in itself, but which, in the context of the outdoor DRIVE INN in the exhibition, reasonably easily can take the role of representing the picture of the naked behind in DRIVE INN, if only we accept the slight dialect form NØYEN to means NAKED, a change which brings the altered name form close to NAKEN ENDE-SE, meaning "NAKED BEHIND-LOOK"."

This much as far as the concrete analyses are concerned. We should mention that the author in the last chapter of Vol.III returns to the analyses of the socalled "triadic signs" from Vol.I, and tries to argue for how symmetries in this sign are of particular importance. He

4 SPIRIT-ETHIC PUTS TRIUMPH ON THE TABLE

5 The office was on the third floor ('fjerde' in Norwegian)

calls this sign for a "crystal" and is probably concerned with a possible analogy to the study of crystals and their symmetry groups. But, again, where the crystallographer has precise models, computations and experiments, we find in this analysis only speculative constructions - which are not even properly founded in the author's own theory of Cantor machine computability.

4. Discussion of other parts of the dissertation

In this paragraph, we will discuss the contents of Vol.I and Vol.II in the dissertation.

4.1 Vol.I: Submorphemic signification

In the volume Submorphemic signification, John Grøver presents a new theory on the role of sound symbolism in language. Grøver's claim is that a universally based sound symbolism, or submorphemic signification, as he calls it, plays a central role in early child language, viz. in the course of the single word stage, and that traces of this can be seen also in the adult language. On basis of a comparison of a number of languages, Grover claims that such traces can be seen in the pronoun paradigms. He claims that the generalizations which Joseph Greenberg has made concerning pronoun forms among the world's languages are effects of sound symbolism.

This volume, the earliest of the various parts of the dissertation, differs from the other parts (with the exception of chapter 1 in Epistemes, language and information technology) in having a welldefined and initially not unsensible hypothesis, and by and large is free of the grandiose, revolutionary ambitions which characterize the other volumes. We have therefore chosen to treat it quite extensively, as an individual work, even if Grøver emphasizes the integration of it in the total unity of the dissertation. Considered as an independent work, we find that this volume has the preconditions for being developed to an independent dissertation, even if we also have some strong objections.

The idea that sound symbolism plays a larger role in early child language than in adult language, seems reasonable. Grøver's idea is that a universal, 'innate' sound symbolism so to speak constitutes the first step into language. He argues that one should consider the single word stage as consisting of two stages, wherein also the socalled word spurt around 1 1/2 year marks the boundary. During the first stage, the sound symbolism is the only form of signification: Differentiation of the phonological domain goes in parallel with the differentiaation of the semantic domain. At around the age of 1 1/2 year, the child goes from submorphemic to morphemic signification.

Grøver refers to precursors, primarily to Roman Jakobson, who discussed the idea that the sounds in mama and papa are semantically motivated, where [p] represents 'the other', 'non-ego' - an idea which Grøver later develops. But apart from this, the dissertation lacks a discussion of the scientific literature on sound symbolism after Jakobson, a thing which obviously should have been included in a dissertation about such a controversial theme.

Grøver discusses the difficulties with founding the theory empirically. He suggests that it may even in principle be impossible to understand the very early semantic categories, but he suggests that we can 'occasionally glimpse into such clusters of meaning'. In chapter 2, Grøver nevertheless succeeds in scraping together observations from various parts of the literature, in addition to his own observations and speculations, in support of the theory on the sound symbolic stage. The most original and thought-provoking empirical support presented by Grøver comes from the regressive consonant harmony which is typical of early child language (gokke instead of dokke, ngengen instead of sengen, and so forth): If a binary phonological feature distinction (e.g. ±dental) corresponds to a semantic binary distinction (e.g. ±pleasant), then e.g. dokke can be excluded for semantic reasons, as a self-contradiction! Unfortunately, this does not fit well into Grøver's time schedule. Grøver predicts that the consonant harmony will disappear in the latter part of the single word stage. In reality it lasts much longer, past the two-word stage.

Grøver receives another relatively direct support from the phenomenon of phonological selection, that is, some children's (apparent) preferance for or dislike of certain speech sounds. Another and considerably less direct support builds on the phenomenon of attachment, which reaches its peak around the age of 1 1/2 year, and thereby coincides in time with Grøver's sound symbolic stage. Grøver has a kind of explanation to attachment: In the period when "the child's social self is as yet non-distinct from its parents", the child is dependent on the parents for "mastering the space of signifi-cation" (pp.92f.). Signification is not yet conventionally based, but is founded in a limited and in a certain sense undifferentiated social world (the family). It remains somewhat obscure why submorphemic signification should be a precondition for this. The same kind of dependence should presumably arise even if the signification were morphemic, as long as it is experienced as 'personal'?

Grøver seems to exclude entirely the possibility that psycholinguistic tests can prove the existence of the universal sound symbolic stage. This seems unnecessarily pessimistic.

In chapter 3, Grøver makes some fantastic attempts to 'glimpse into' very small children's sound symbolic world. On basis of some examples of single word utterances in context, taken from the literature, Grøver argues for, among other things, an analysis of the meaning of [b]: approximately 'intrusion' or 'separation' (cp. Jakobson's hypothesis). Other ideas are that continuants represent motion, and nasals 'transference across boundaries'.

The reasoning in chap.2 and 3 builds on an observation which goes to back to Roman Jakobson, viz., that the linguistic development proper starts with the first pronounced word around the age of ca.10-12 months. What precedes this, such as babbling, belongs to a prelinguistic stage. Grøver has looked entirely apart from, or not had knowledge of, all the research which shows that the linguistic development starts much earlier. A good summary is found in Peter Jusczyk (1997) The discovery of spoken language. This book did not exist when Grøver wrote Submorphemic signification, but a large part of the research which is summarized there is published before 1995. We now know that children recognize sound patterns in their mother tongue in the latter half of the first year of life, and that they to a certain extent can recognize word forms, etc. The babbling gradually exhibits more and more traits of the mother tongue. There is now evidence that children start understanding words before they reproduce any themselves. Grøver ignores almost entirely the difference between receptive and productive competance, which is central to the new research on the very early development of children's language. It is obvious that Grøver's argument for a sound symbolic stage in the course of the first half of the single word stage would have to follow entirely different lines if it is indeed the case that children's mental world is richly differen-tiated, both phonologically and conceptually, already when the first word is pronounced.

Chapter 4 is a traversal of pronoun paradigms in various languages from various families and regions. Grøver's idea is that traces of the early submorphemic signification can be recognized in particular in pronoun forms, which, as is known, express very fundamental social distinctions, precisely such things as 'me vs. others'. A problem which Grøver leaves out of discussion, is that children typically do not make use of pronouns before quite late in the development, after the single word stage. It therefore remains obscure why the early submorphemic categories should be recognized precisely among the pronouns. Grøver shows that the distinction between 1st and 2nd person often correlates with + or - nasal, partly with vocalic/ consonantal. Nasal sounds occur particularly often in 1st person, something which fits into Jakobson's papa-mama hypothesis.

A serious weakness in this chapter is that it remains obscure which standard it is the forms/ oppositions which he finds among the pronouns deviate from in such a way that it calls for an explanation. Maybe the distinctions ±coronal or ±nasal are as prominent in the other subsystems of the grammar (e.g. among other functional categories such as tempus, case or gender morphs) as they are in the personal pronouns?

In chapter 5, Grøver presents an argument based on a reinterpretation of Greenberg's theory of genetic affiliations among language families, which in its turn is based on (among other things) precisely these pronoun forms. Grøver's argument is as follows: Greenberg has shown that similar pronoun forms exist all over Eurasia (1st person -m- as against 2nd per-son -t-), while a different pattern obtains for The New World (1st person -n- against 2nd person -m-). This together with certain other continent-specific traits leads Greenberg to the conclusion that the Eurasian families which exhibit these forms are related, and likewise for the corresponding American language families. Greenberg's theory has been rejected, by several Americanists and others. Grøver accepts their argument against the theory of genetic relatedness. But in that case, there must be some other explanation to the patterns which Greenberg observes. Grøver's answer is this: The explanation is partly in a universally based sound symbolism, partly in language contact. Grøver claims to have shown in chapter 4 that there are universal tendencies in the distinction of person among the pronouns. But then how do one explain the regional distribution of forms, which Greenberg has shown? If genetic related-ness is excluded, the explanation can only be language contact. The forms with -m- for 1st person and -t- for 2nd person must have spread in Eurasia through cultural contact among related languages. This can possibly be defended. But it leads Grøver to a strange argument:

The development of phonological categories goes in parallel with the development of social categories / semantic categories. There are universal tendencies in this development, but it gives space for 'cultural parametrizing' (e.g., 1st vs. 2nd person correlates with + and - coronal, but whether 1st person is + or - coronal is seemingly a cultural parameter). Words acquired earlier are most affected by cognitively based universal categories, and they are also the carriers of the most prominent cultural parameters. They are the culturally most independent words. Also, Grøver concludes, the similarities in the core vocabulary between two dissimilar languages means that they derive from the same culture - not (necessarily) from the same language.

If Grøver were right, we should find that languages/people in close contact generally would tend towards having similar core vocabularies, but also possibly dissimilar peripheral vocabulary (since this is not culturally meaningful to the same extent). But this is not the way things are. This is a part of the reason why we draw the traditional boundaries between different languages. The cultural parameters in Finland, take Helsingfors as an example, are fairly similar in the Finnish-speaking and the Swedish-speaking parts of the population. According to Grøver, we expect that the Finnish-speaking and the Swedish-speaking population shall have the same core vocabulary - but they have not. The similarities subsist primarily in the peripheral vocabulary. If their core vocabularies had been similar, we would probably speak of the same language. This is standard linguistic theory.

The phenomenon of bilingualism must be a problem for Grøvers theory. How can a child acquire two languages with different core vocabularies, (almost) simultaneously, but only one culture (if we assume that a family in e.g. Helsingfors normally share one common culture)?

The final impression is that Submorphemic signification is an original, interesting and ambitious (possibly too ambitious) work, but half finished. Grøver argues with inventiveness, perseverance and with considerable proficiency for his hypotheses. Sometimes, however, the boundary to pure speculations is transgressed (chap.3), and far-reaching conclusions are in several cases drawn on basis of weakly founded presuppositions (e.g. chap.4). There are also big holes in Grøver's knowledge on the field. One problem with the kind of crossdisciplinary project which this work represents, is that the researcher, rather than being an expert in one field, becomes somewhat of an amateur in several fields. The advantage is supposed to be the possibility of new ideas and new ways of seeing things arising through cross-fertilization among the various fields, and one may say that this is what has happened in the present case.

4.2. Vol.II: Epistemes, language and information technology

A goal for the author in this volume is to show a particular kind of parallelism between linguistics, logic and information technology; the analysis in this volume is primarily meant as a basis for the general theory in Vol.III.

4.2.1 The history of linguistics and information technology

In this work, Grøver wants to show how linguistic analysis is tied up to history, that is, European history in general, and how it expresses the general cultural-historical situation, or the various epistemes.

Grøver thus makes a large-scaled attempt to establish a linguistic historical periodicity by describing western linguistics as a function of culture in the broadest sense, but primarily also as a function of the information technology, that is, the phonemic script, the printing press and the digital computer. He here points out that in archaic Greek philosophy, such as in the logos philosophy of Heraclitus, there is total isomorphy between language, thought and reality, a view on language which is continued by the Stoics and their theory of the stoikheion, the "element", where the elements of the word are supposed to correspond to elements of the reality which it represents. Grøver furthermore describes how this view comes to be a part of the crisis in linguistics, in the sense that it becomes apparent that there is no complete analogy between word and reality, and shows how the dispute on analogy vs. anomaly results from the discovery that the element cannot directly be the fundamental unit ("basic unit") in language. It is by Augustine, however, and Christ, as the carrier of logos, "the word", that the word replaces the element as a linguistic basic unit, such as Grøver describes Hellenist and later Roman linguistics. It is a historical principle for Grøver that the crisis in collective knowledge contributes to create new epistemes: "There is one important characteristic of all the turning points we have considered here, that they are accompanied by crisis in knowledge". With the renaissance and the printing press, the sentence becomes the basic linguistic unit, such as Grøver interprets the grammarians of that time. This is a result of, among other things, the debate on universals in scholasticism. Furthermore, in our time, the formation of the computer technology correlates with the Chomskyan grammar and the languages as basic linguistic units. Grøver's hypothesis is that the development of linguistics correlates with the general development of cultural history (p.5): "The hypothesis suggests a close relationship between the development of linguistic analysis and the general cultural development, in particular as this is related to epistemological problems".

This somewhat interesting hypothesis has a very high-level goal, a goal which probably must be too high for such a short treatment as the present one. One cannot let unmentioned that the description of the various epochs and their characteristics come to be very schematic, and the description of linguistic thought in the most important grammarians through all the western history of grammar becomes, for the present purpose, too scant. Grøver has made far too little use of original sources: It is, after all, necessary to make a close reading of the grammarians' own texts in order to give a satisfying account of their theories - this in particular when, as is the case with Grøver, they are subjected to a detailed interpretation. Grøver makes almost exclusively use of secondary sources. This is the case e.g. for Grøver's treatment of the stoikheion concept: It is true that the case of sources for the understanding of the Stoics is very difficult, but since Grøver uses this concept almost as a parameter through all of his historical description (e.g. pp. 51, 54, 67, 71-2, 107) - not only in casu stoicorum - one should have expected that he had explained, even inter-preted, this concept far better by offering himself and the reader a close reading of the primary sources. The author is to some extent aware of this: "The stoikheion model in antiquity may still need a more thorough investigation than has been carried out so far" (p.107).

The last part of this paragraph discusses Chomsky's linguistic theory. The author has obviously knowledge of these theories, at least from the period before 1960. But we should point out from the use which Grøver in later paragraphs makes of these theories (e.g., in the context of the logical paradoxes, see paragraph 4.2.2), that theoretical linguistics today is far more than Chomsky pre-1960 and by no means gives reason for the conclusion which the author wants to make.

It must be said that Grøver in the present part of the dissertation has an interesting hypothesis, but it is regrettable that the argument for giving substance to this hypothesis has not been more profound and well-documented. It must therefore be concluded that the first part of Epistemes, language and information technology in its present form does not suffice for scientific requirements - the work appears as far too unfinished for that.

4.2.2 The history of logical paradoxes

Seen in the light of the theory in Vol.III, we understand the importance which the author assigns to the logical paradoxes. But the exposition in the dissertation is incomplete.

Most importantly, it is a defect that the author does not seem to know the current research literature in the field, and thereby is not acquainted with the technical analysis of the paradoxes, e.g. with the role which the liar paradox, transformed into a socalled self-referential proposition on provability, plays for the technical formation of the proof of Gödel's incompleteness theorem. On the background of this context (and others), we find it entirely unfounded that the author looks apart from the semantic paradoxes when he is about to prove a 'deeper' connection between linguistic theory and logic.

His incorrect assertion (Vol.III, page 52) that Tarski built on Gödel's theorem shows that the author elsewhere as well seems to have only superficial knowledge of the relevant literature. Neither are we convinced that his assertion (Vol.III, page 22) that "Gödel proved that any such formal system, have it only the slightest complexity, will be incomplete and inconsistent" is but a slip of the pen. The author has nowhere in the dissertation documented the necessary technical competence which is a prerequisite for a scientifically appropriate discussion of computability, self-referentiality and partiality which is a necessary foundation for an analysis of the logical paradoxes.

The decisive weakness in the paragraph on the logical paradoxes is, though, concerned with the attempt to show a certain kind of "identity" between the paradoxes and Chomsky's classification of formal languages. That NESTING, that is, the recursive of character by certain types of formal languages, and the element relation of set theory both give rise to partial orderings, is not a sufficient basis for recognizing a content connecting these two phenomena. Equally unfounded is his assertion that a connection between CONTEXT and "the notion of the highest number producing the paradoxes in the case of Burali-Forti (ordinals) and Cantor (cardinals) (Vol.II, page 133)". There could be more comments to make against the author's treatment of the paradoxes, e.g., that his analysis of the socalled Zeno paradoxes is not of a standard one expects to find today, but enough is said to conclude that this paragraph does not exhibit an appropriate scientific level.

4.2.3 The cuneiform episteme

In part II of "The history of logical paradoxes and the cuneiform episteme", under the titles "Cuneiform" and "Epistemes and cyclic time" (p.159-211), Grøver attempts to develop further his idea of epistemes and correlations between the cultural history and the history of linguistics from part I, "The history of linguistics and information technology". Grøver here erects a grandiose historical construction, wherein Christ seems to be the centre of history, and wherein historical periods before and after Christ 'mirrors' each other in a cyclic perspective. In this way, the transition from the Sumerian period to the Akkadian corresponds to modern times: "It is not difficult to see the parallel to mathematical logic from Leibniz to the creation of the computer". These speculations are neither sufficiently documented nor made probable, and neither is his assertion that "we also see a parallel deconstruction of the cuneiform episteme down to the point zero by Christ, to be rebuilt as a church, and to be replaced technologically by the new script". Now it is correct that the cuneiform script went out of use just before the birth of Christ, but it had, as a defining "episteme" over larger cultural areas, lost its real authority long before. Historical research must be carried out with far more sobriety and caution when it comes to periodizing than is the case here, in particular when these periods are as weakly documented with relevant examples as in the present case.

Grøver claims that the study of Sumerian, Akkadian and Hittite to a large extent has failed, because the cuneiform script is, as he asserts, so ambiguous that it is impossible to reach sufficient consistency in the interpretation. He refrains, though, from discarding earlier research traditions entirely, but claims that this has come into a wrong track. The fact that isolated cuneiform signs can be read with various phonetic values, and that words which are identical in Latin transcription can have as a textual basis words written with various cuneiform signs, is for Grøver an indication that these words also have various meanings, and that the variation in the way of writing expresses a submorphemic meaning which we cannot capture in our transcription, and thereby neither in our understanding. Grøver refers not to the fact that the cuneiform signs comes to be much more definitive and less ambiguous, formally as well, when they are read in context: Two cuneiform signs in context will often define each other. This contradicts Grøver's idea. Grøver claims that there are possibilities for large new discoveries on basis of the already available cuneiform sources, he even puts a question mark by the fact that Hittite is an Indo-European language, because it has so many Sumerian pictograms and Akkadian words. In addition, he means that Sumerian and Akkadian may be one and the same language - this is incorrect, even if Akkadian has many Sumerian loanwords and the Akkadian culture has inherited parts of its semantic and religious universe from the Sumerians. Grøver also takes his point of departure in the case that Sumerian can be read in a "crossword pattern", and assigns a particular code to this (p.185): "Cuneiform, as it is interpreted here, is a crossword code which must be read horizontally and vertically at the same time". This is addition to the fact that each single word in later languages written in cuneiform can be written with various wedges, can have some importance: Babylonian scribes interpreted words and assigned to them a manifold meaning by reading the cuneiform signs both as logograms and as phonetic signs. It is, though, highly probable that the variation of cuneiform signs in the same word, crossword patterns, and the holes in the tablets, had but an ornamental function. It is, though, worth a study, a study which Grøver does not carry out, and which he has not proved himself capable of carrying out. Nevertheless, he proposes very substantial conclusions on the Sumerian and Akkadian languages, conclusions which cannot be characterized as anything but arbitrary assertions in order to underpin a main interest which also is very obscure: viz., the speculations on a particular kind of cyclic time or history conception. Grøver's claims in the work discussed here lack virtually any documentation - his documentations are but isolated quotes from assyriological secondary literature.

5. Conclusion

The two smaller additions (Vol.IV and Vol.V) do not alter our common conclusion, that the dissertation is not worthy of being defended for the degree Doctor Philosophiae; we refer in this context to the requirements in the regulations, in particular the first part of §3 in "Regulations for the degree Doctor Philosophiae at the University of Bergen".

There are critical insufficiencies in the author's attempts at definitions and constructions in the general part; see particularly Vol.III. It is a general trait in the author's treatment that the methods which he makes use of in the attempts to make the general theory concrete, lack sufficient scientific foundation.

We will nevertheless contend that the material in Vol.I may be further reworked, but then within the framework of accepted linguistic theory; see the discussion under 4.1.

Oslo/Tromsø 27 July 1998

Jens Erland Braarvig
Jens Erik Fenstad
Anders Holmberg

John Grover: Comments to the committee's evaluation

I am delighted to observe that the committee nowhere produces a serious critique, but is content with attacking misunderstandings and irrelevances. My dissertation consists of five parts, but vol.III, IV and V are not discussed in any relevant way. Vol.IV and V are not touched upon at all, while the committee only discusses approximately 5 out of the 340 pages in vol.III. Vol.I and II receive more attention, but the objections are not of a kind that needs to be taken seriously.

I will here go through all the parts of the evaluation in a just discussion.

1. About the dissertation

The five parts of the dissertation are listed. There are no references to number of pages. Since this can have a certain importance, I give them here: Vol.I = 276 pages. Vol.II = 220 pages. Vol.III = 340 pages. Vol.IV = 69 pages. Vol.V = 46 pages.

The committee prefers to take the 'Cantor machine' as a point of departure. This is an entirely peripheral part of my dissertation, a concept which points to the future control of computations in the social space on basis of the social/cultural formalizing which our time is about to enter. In the letter which I enclosed with the dissertation, I made it very clear that there is a strong internal coherence between the five parts, and that they just as well could be bound in one volume under a common theme. With this I meant to say that they should be considered as one volume which thus should be read from beginning to end. The evaluation says that there is no summary in the dissertation. This is not correct: The summary of the four first volumes (900 pages) is given in the beginning of the fifth (46 pages). It is this summary which must count as a summary of all the parts of the dissertation. The committee does not discuss the last part - seemingly because it would have been a waste of time to discuss also "A waist of time".

When I, in the letter, also pointed out that the dissertation is "concerned with the ultimate goal of arriving at a formalization of the socially encoded knowledge in the domain of the socalled 'Cantor machine'", this of course means not that I have made an attempt to construct the Cantor machine. My dissertation is primarily about the formation of the arbitrary linguistic sign, and is, as such, a semiotics. The committee has nowhere in its evaluation discussed this most important part of the dissertation. The concept 'arbitrary' is used only once in the evaluation (on page 77), and then in a completely different sense (meaning 'unmotivated choice'). My comment in the letter to the committee says of course that the dissertation is concerned with this cultural development (in the computer episteme) towards a society wherein the socially encoded knowledge (= the domain) is formalized in such a way that the future Cantor machine can work on it. That is as yet far into the future. The reference to the Cantor machine serves to point to the domain, not the machine. It is the process of formalization I am concerned with: My thesis is that the new technology leads to a formalization in a new domain, and it is the study of this domain and its formalization that I am concerned with in the dissertation. It is therefore not a good idea to let the 'evaluation of the dissertation take the candidate's presentation of the socalled Cantor machine as our point of departure' - when I don't present any such. I discuss some technical concepts which may be relevant for this machine on approximately 5 out of the 950 pages (in addition to a discussion of the various 'states' of the sign in the last chapter of vol.III, which must count as something else), but that is all. Neither does the summary on the pages 3-5 in "A waist of time" suggest any such machine. I here reproduce the three first pages of vol.V, those pages where the dissertation is summed up as follows (references to the titles of the other volumes are here replaced with vol.I vol.II etc.):

A summary at the beginning of vol.V:

This study (vol.V) is a continuation of vol.I, II, III and IV. The background can be summed up as follows.

Vol.I interprets early child language in terms of a triadic sign with one social, one observational, and one phonological component. The single-word stage is submorphemic, presumably with a featural rooting of the signification. At the transition from the single-word stage to the onset of syntax, a transitory period of attachment to caretaker is characteristic. In this period, the child relegates control of signification to the joint attention, which I here interpret as an instantiation of the collective consciousness. It is through the guiding of this collective consciousness that the new level of knowledge is attained, which suggests that the significational consistency on the new level allows for erasure of the internal consistency of the submorphemic level, which comes to be replaced by the new knowledge. It is because this development into the arbitrary morphemic signification seems to require a transitional attachment that we can hypothesize that the two levels are significationally incommensurable. This does not imply that submorphemic signification vanishes from adult language, but it implies that it loses internal consistency. Its cultural manifestation is shown in vol.I in the presence of universals in submorphemic signification in personal pronouns and in the cross-linguistic patterns shown by Greenberg ('Language in the Americas', 1987).

Vol.II is a historical study which concludes that there is a parallel development of the formal description of the levels of language, invention of new information technology and the nature of logical paradoxes. In addition, it is shown that Christianity (in extension from the Old Testament origin in the invention of the alphabetic script) represents the morphemic signification, while the preceding cuneiform episteme represents a submorphemic signification on some level or other. It is also suggested that cuneiform predominantly encodes cognitive categories which need not have a consistent phonetic representation. An account which rests on a fundamentally mirror-symmetric distribution is also suggested to have considerable explanatory potential, and it is this which will be pursued here. The present study (vol.V) does, therefore, support the analysis in vol.II as well. Another central idea discussed in that book is the presumption that an arbitrary sentential sign will develop in the continuation of the computer as a revolutionary advance in information technology, with a shift of computational domain from the symbolic, which it shares with the alphabetic script, to the social domain. It follows, in the continuation from vol.I, that there will be a corresponding period of social attachment to the shared consciousness (a stage of puppets? or apes?) which eventually will output an arbitrary sentential level which is significationally consistent, and which leaves the sentential level, such as we know it today, internally inconsistent, even if word syntax is retained developmentally and therefore must be interpretable in language. This arbitrary sentential sign will refer to complexes distributed in space and time. Vol.II suggests that it is likely that this also will run in parallel with a religious revolution, and that the alphabetic script eventually goes out of use when the essential contents of it is relegated to this new religious representation.

Vol.III investigates what probably are instantiations of the new arbitrary sign in empirical data covering certain correlations with the narrative structure in Rilke's fourth Duino elegy. It is found that units of signification in these correlations varies from a few words up to segments of more than 60 words, normally covering one or a few sentences. The dominant segment sizes are found in the present data (vol.V) as well. The important result in vol.III is that it provides empirical support for the assumption of a sentence level sign. In addition, it presents data for the relevance of parallel-reading of related narratives - a way of reading which also recurs as relevant in the present data (vol.V) as a regulative principle for parameter values.

Vol.IV presents further empirical data which investigates the interface between the individual signifying mind and the collective consciousness - that interface where the arbitrary sentential sign is supposed to find its reference. The study supports empirically the conclusions in vol.II for the sign wherein the diachronic and synchronic dimensions receive a unified account, and this, again, supports the empirical investigation in vol.III. An argument is presented and analyzed which shows that it reaches internal logical consistency only on basis of the postulation of a signification on a universal featural level which allows for communication between the subjective mind and the collective consciousness.

The present study (vol.V) attempts to show how the arbitrary sentential sign can be detected in signal files. It rests essentially on the conception of the split triadic sign from vol.I, such as this is interpreted in vol.II with the split in the phonological component, and on the role of the mirror structure.

This is the summary of the dissertation at the beginning of vol.V. It is possible that the committee did not arrive at this summary before they started making their evaluation. It is, anyhow, difficult to recognize the content of this summary in the evaluation produced by the committee. It looks almost as if two different works are at stake. The reason may be that the committee has read the dissertation with strong preconceptions.

2. The candidate's suggestion for the definition/ description of the Cantor machine

The committee starts with my example of the two parallel computations running on two machines, with users interacting with the processes. It is mentioned on p.57 that there is a standard theory with relative Turing-computability when users enter the process. (That does not affect my theory as long as the collective consciousness is not conceived of as computable). Then there is mention of the collective consciousness (there is reference to anthropology and to Popper's third world - I appreciate that)...

...and then a puncture occurs. Here the committee should have come to the most essential concept in the dissertation, viz. to the <u>arbitrarity in signifi-cation</u>, defined in terms of users consulting the collective consciousensss in what I define to be a parallel social distributed processing when they are about to carry out the semantic interpretation of the sign. Instead of discussing this most essential concept, the committee presents a disappointing performance in 'witchcraft' which seemingly has the function of imitating some of the most central concepts of the dissertation (vol.III) in the construction of the evaluation, rather than discussing the arbitrary sign explicitly:

"We must distinguish between the author's visions and motivating description of the collective consciousness..." This concept of 'motivating description' is not easily understood: It isn't mine, rather the opposite. If 'motivated' is the opposite of 'arbitrary', then the 'motivating description' should turn me into a sign in the social space. The 'visions' and the 'motivating description' do perhaps rather constitute pendants to the three components (the observational = the 'visions', the social = the 'motivation', the phonological = the 'description') in the sign which I discuss in the dissertation (this is, for example, the basic concepts in chapter 9). This is furthermore contrasted to "his technical construction of the collective consciousness as a part of the definition of the Cantor machine". It is probably pages 52-57 (plus a paragraph on p.71) which are referred to here - which means that the committee has skipped the core concept which they had arrived at in their discussion of vol.III: The arbitrarity, which is the key to the entire dissertation.

The committee continues in the next paragraph (on p.58) with a mention of the 'platonic region' which individuals communicate with "even if they are in considerable geographical disctance from the source of this revelation (Vol.III, page 34)". This is a deplorable quote: My dissertation page 34 says 'revolution', not 'revelation'. It may be meant as a joke, but that woud signal that this is not a case of a serious discussion. This impression is supported by the continuation, where it is referred to "Kuhn's dissertation...": It is an <u>article</u> of Kuhn which is at stake here (in my dissertation), not "John's dissertation..." ['Kuhn/John' is a near-minimal pair in Norwegian]. Furthermore, "his discussion of the non-evclidean geometry...": I have not discussed the non-Euclidean geometry anywhere, not in any other sense of it than my scarce mention of the discoverer János Bolyai - not only as one with the binary code 1001, but also as the author who was not so successful with the launching of his theory, even if he was a good fencer. I suppose it is Fenstad who is writing here. "Non-EV-clidean" is normally spelt "non-EU-clidean" - Fenstad should know that. It looks like even more 'interpretation' on my name: The first name in the form "Kuhn's/John's dissertation", and possibly the surname in "non-EV-clidean" geometry.

It is not difficult to recognize the narrative elements from the last chapter in the book, summed up around pp.320-323. Besides the ordinary components of grammar in the phonological component, I also discuss concepts on systematic ambiguity in time, space and mirror-symmetry in the social component. In addition to that, also the concepts of social cognition, personal name and (visual) perception in the observational component (p.322). It is obvious that these are concepts which the committee makes use of in their discussion (the author's "visions" and "motivating description", references to my name by ambiguity etc.), but without telling that these are my concepts from the last chapter. It seems in fact as if the committee has startet the discussion at the beginning of Vol.I, followed the text a few pages ahead, and then, rather than discussing the essential concept of arbitrarity which I discuss from page 10 onwards, jumps to the last pages of the book, to the 'mirror image' a few pages before the end (p.321-323), and makes use of the concepts which I discuss there in his own exposition in such a way that it looks as if this is the committee's own concepts.

It is not difficult to recognize the components of social cognition, personal name and (visual) perception (p.322). In addition, there are elements from 'spatial/temporal determinacy' (p.322) in the erroneous quote 'considerable distance from the source of the revelation'. Now only the indeterminacy of the mirror symmetry is missing (p.322 in vol.III of the dissertation) before all the components in my discussion <u>at the end of the book</u> are made use of. This is found under section 4.2.2 on page 74 of the evaluation where there is a quote from the reference which is made to Gödel in the dissertation (I refer to Gödel only very few times, in very marginal contexts, and it is one of these which is exposed): "Neither are we convinced that his assertion (Vol.III, page 22) that 'Gödel proved that any such formal system, have it only the slightest complexity, will be incomplete and inconsistent' is but a slip of the pen". This point is completely uninteresting in the context: The only relevance I can find is that there is a reference to a <u>slip of the pen in the context of the name Gödel</u>. The reason why this little comment becomes a mirror image for page 58 in the evaluation is found in terms of two aspects: 1) The use of slips of the pen on page 2, where 'revolution' becomes 'revelation', my first name is subjected to confusion with Kuhn's ('dissertation' erroneously instead of 'article') and my surname is possibly connected with the "non-EV-clidean": That brings the names Kurt Gödel, Kuhn, and John Grøver on a common form by means of slips of the pen, and one thereby obtains a form of 'ambiguity' through the comment on slips of the pen in the vicinity of the name Gödel (which otherwise is irrelevant) on page 74 which thereby also refers to page 58 in the evaluation. 2) One thereby obtains that point 2 in the evaluation receives a mirror image in point 4.2.2, or, more generally, the three points 1, 2 and 3 receive their mirror images in the points 4.2.1, 4.2.2 and 4.2.3: The slip of the pen in point 4.2.2 then has a mirror image in point 2 (I guess that both these paragraphs, plus point 3, are written by the same member of the committee, probably Fenstad). This explains also the strange subdivision of the evaluation, where vol.III is discussed in point 2 and 3, vol.I is discussed in point 4.1 and vol.II in 4.2.1, 4.2.2 and 4.2.3. The mirror effect which is obtained by the name Gödel in addition to the slips of the pen in point 2 thereby introduces a element which makes the evaluation an image of my theory, albeit in a way which is not much clarifying. It is interesting to add that the comment on a possible slip of the pen is found almost exactly in the middle of paragraph 4.2.2, while the mid point in point 2 is just before the reference to 4.2.2 in the next paragraph on page 58 (approximately where Cantor and Tarski are mentioned), in immediate proximity to the errors. I suppose it is possible to close-read the points 2, 3 and 4.2.2 (all of them probably written by Fenstad) in search of such parallels, with rotation about point 3, and thereby as an interpretation of my discussion of text-parallels in vol.III. - But without reference to the source. To the extent that the member of the committee has borrowed his exposition method from my work, there should of course have been a mention of it.

All of this discussion on page 58, <u>which should have been about the concept of arbitrarity</u>, must anyhow be considered uninteresting.

It may also be pointed out that a further effect is obtained through association of my name with KURT GÖDEL through these ambiguous obscuricisms, probably in the form KURZ GÜRTEL/GIRDLE, as in the waist of the hour-glass, with referance to vol.V in the dissertation with the title "A waist of time". The committee does not discuss this vol.V in the dissertation (that would perhaps have been 'a waste of time', according to the evaluation page 78), but they have this reference instead.

The paragraph on page 58 mentions Gödel once more (with reference to the ambiguous point on the other side of the dissertation) before it switches to Tarski and further through a somewhat sudden change to Cantor's diagonal proof. It is claimed (page 59): "The assertion that 'the achievement of Cantor was, as I see it, that he proved the existence of the collective consciousness, and showed that it could be formally described (Vol.III, page 9)' seems right away entirely unfounded". This claim in vol.III is not entirely unfounded. On the contrary, I argue quite extensively for precisely this claim in vol.II of the dissertation, pages 193-196. It is possible that I, with this discussion, make a slight fool out of the symbolic logic, which has invested so much in the reliability of precisely this tottering proof, but such a case will then have to be an 'archetype annotation' to the logician. Why hasn't the committee discussed this argument instead? Here it is a matter of concrete details and claims (from me) that the essential proof does not hold good - but the committee ignores this constructive element and complains that I do not repeat the argument from vol.II in vol.III (on page 9). I suppose these things are a little sensitive: See, for example, the standard work Boolos & Jeffrey; "Computability and logic" - the overview of the inner dependency between the chapters in the book, given on one of the very first pages. Absolutely all of the book, which is about the fundamental modern concepts and prerequisites for the theory of computability in logic, rests fundamentally on chapters 1 and 2, which are about the diagonal proof and enumerabiity only. It is this single tottering proof on which logic has gambled all its saving money. On pages 193-196 in vol.II, I discuss this in a simple, easily understandable and lucid way.

Many linguists are allergic to arguments against Saussures arbitrarity, synchrony and diachrony (for example, the committee seems to suffer from such allergics), and many logicians/mathematicians will be alergic to arguments against this proof. <u>These two things are interconnected</u>. This is important for the totality of my approach, but it seems as if it is not welcomed by the committee. Why don't they discuss these interesting aspects rather than presenting foggy talk on obscure 'intentions' and the author's 'motivation'?

There are complaints that one finds 'words and intentions, but no technical analysis and construction'. There are also analyses and constructions in my dissertation, but I do not proceed any further than to this tottering proof, also because it is here that a key to the arbitrarity can be found. I make use of no further basic logical concepts than just this diagonal and the countability, and give these a redefinition which provides a basis for a 'rough sketch' of the formal definition of the collective consciousness. I thereby do not enter into a technical description of the machine. I go the other way round, which is much more fruitful: I show how a collective narrative can be postulated on basis of these two fundamental concepts, and I proceed from the principle that it is poetic language which is the carrier of the collective narrative. In the study, it is Rilke's fourth Duino elegy which has the function of being the collective narrative. This is also exemplified by all the parallel readings, which has a considerable empirical value, but which is not mentioned by the committee at all. Poetic language is supposed (by me) to encode more parallels than any other language (lithurgical and religious language are then presumably also a kind of poetry in this sense of it, such as I point out on pages 59-60 in Vol.IV which the committee not even touches upon). I thereby see a bridge between poetics / theory of religion on the one hand and formal logic on the other side, but I do not construct the formal machine at once. (I may perhaps never do so at all). These are fruitful cross-scientific approaches. When the committee complains that I am not sufficiently formal or technical, this is just to create obstacles. I work with semiotics, not with logic.

On page 3, one asks for established requirements for knowledge and method within the fields of linguistics, logic and informatics. The summary of the dissertation given in the beginning of this commentary can be consulted to see if this is an essential point. I suppose that the real requirement is that I am expected to accept the outnumbering interpretation of Cantor's proof and Saussure's distinction between diachrony and synchrony as well as his arbitrarity as a basis for the discussion - such that e.g. what is discussed in Boolos & Jeffrey's book can be saved and the mathematical logic keeps its trousers on. The point of departure for the committee is possibly all too different from mine in this respect. I am concerned with dissolving the strong demarcation between diachrony and synchrony which Saussure introduced, and thereby also the concept of arbitrarity, and to show the parallel weakness in Cantor's concept of countability. The committee is obviously concerned with guarding these two concepts such as they have come to receive a fundamental role in much existing intellectual achievements.

The entire dissertation, 950 pages, is about the arbitrarity (while the committee does not mention the arbitrarity with a single word), and I have, as mentioned, discussed uncountability explicitly on those places where it is necessary. Section 2 in the evaluation (pages 57-60), which should have been about just these two fundamental concepts, appears as an entirely failed critique.

3. The collective consciousness: from theory to examples

In this section of the evaluation, nothing is said - absolutely nothing, even if it occupies two pages of the evaluation. The committee extracts three examples, exposes them, and withdraws discreetly after having pointed to the 'triadic sign'. There is talk about a mysterious crystal with strange capacities...

The question may be posed whether the purpose of this 'section 3' is to create a rotation point for the mirroring of section 2 in section 4.2.2, as a parallel to the mirroring which was introduced in the discussion of vol.III where the end of the book was discussed instead of the beginning. It is interesting here to point out that the exact middle point in vol.III (332 pages of text) is the doorplate with the wording FONETISK LABORATORIUM (page 166) in the chapter on the institute lunch, mentioned on page 61f. in the evaluation.

I am criticized for imprecision. On page 62: "Where the crystallographer has precise models, computations and experiments, we find in this analysis only speculative constructions".

Once again, my feeling is reinforced that the boundary between the committee's evaluation and my vol.III purposely is blurred (it seems as if the committee's evaluation indeed is a 'speculative construction'). It is nevertheless an important difference in that I am explicit where the committee hides its cards.

I may add, just to mention it, that I am studying the semiotics in subjective meaning assignment, and then no association will be too absurd. It is like blaming the small child for being imprecise in its babbling and speculative in its meanings, or to let the dream interpreting psychoanalyst criticize his client for being speculative in his interpretations or for jumping unsystematically from the topic to the next. The examples exposed by the committee are the results of my associations. It is of course irrelevant whether other people have the same associations (I suppose they don't), and this is fundamental for the model in the dissertation, which has to do precisely with the boundary between subjective and collective knowledge.

To the extent that the evalution on this point not only serves to blur the identity of the evaluation with my work, it reflects a pure unwillingness against understanding the subjective in meaning - one asks for cogency and precision in the description, as if I had attempted to construct a machine. It emerges clearly from the dissertation that it is subjective signification which is at stake, and then the commentary at the end of section 3 is just representing a bad or irrelevant evaluation. The committee ignores entirely what vol.III is about, and takes it as an attempt to define a Cantor-machine. I am not trying to define the Cantor-machine. On the contrary, I am studying the interface between subjective and collective knowledge as interesting for the understanding of the concept of arbitrarity. The critique which the committee presents has nothing to do with these things, in particular since it nowhere mentions what vol.III really is about. The deplorable lack of poetic sense which is revealed in the evaluation is compensated for with irrelevant requirements on formal rigor.

Well. Galois threw the sponge in the examinor's head during the entrance examination for École Polytechnique because the pedantic examinor was stuck in tradition and called for cogency and precision. Galois was not admitted to the school.

The requirement for precision and formal rigor (cp. the evaluation's formal imitation of vol.III) is particularly interesting in light of the absurd constructions which have been erected on basis of Cantor's diagonal proof for the uncountability. There exists a huge production of scientific papers which exclusively rests on this bizarre proof, and all of them are hyper-formal. It is a kind of institution-alized split: One accepts anything as a tottering foundation, and indulges in extreme formalisms on top of this needle-point. It is these formalisms which the committee invites me to participate in. I refrain from that and concentrate on finding out of the foundations instead.

Conclusion: The committee has not evaluated vol.III of the dissertation in any satisfying way. To the extent that it has entered into an evaluation of it at all, beyond the attempts to mirror the <u>form</u> of vol.III, it has evaluated it as a dissertation about a machine, and not as a dissertation in semiotics. It is also regrettable that the committee uses my concepts from chapter 9 to turn things around on page 58 (including the mirror-symmetric point on page 74), such that it looks as if these concepts of mine are the committee's concepts which I have not thoroughly understood.

4. Discussion of other parts of the dissertation

4.1 Vol.I: Submorphemic signification

It is difficult to take the discussion of vol.I very seriously, since the committee member, from the first sentence and throughout the entire discussion, talks of the book <u>as if it were about sound symbolism</u>. It is not about that at all. A strong critique is made against this view which I do not represent at all. On the contrary, it appears very clearly from the book that the submorphemic constituents are to be considered on a par with the morphemic ones, except that they are smaller. If the morphemic ones are arbitrary, then the submorphemic ones are so as well. I sum up the theoretic discussion in the book on pages 119-120 in the following way:

"The chapter set out with surveying some of the attempts which have been made for periodizing the single-word stage, and it was concluded that the large individual variation and the lack of consensus on such periodizing points to the need for a more flexible model of linguistic development in the period. A two-component model was proposed, consisting of

1) an unspecified grammatical (syntagmatic) component which can take submorphemic or morphemic units as input, and

2) a lexical component consisting in the expansion of the scope of conventionality imposed on the symbolic units, to expand submorphemic to morphemic units, possibly concomitant with the erasing of an already existing phonotactic signifying structure, in the course of the second year.

On basis of a number of fairly different individual developmental patterns, it was suggested that this two-component model can account for a high degree of variability in the development, including early and late onset of syntax, large and small single-word corpora, and pivot syntax".

When I wrote the book in 1993, it was as yet too early to discuss anything but morphemic and submorphemic constituents. In vol.III, IV and V of the dissertation, I discuss also the arbitrary sign on the sentence level, but it was as yet too early for that in 1993. I was concerned with the relation-ship between <u>two languages</u>: The submorphemic language and the morphemic language. In addition, the developmental relationship between them - where the attachment component has an important role in guiding the formalization which is needed to arrive at the morphemic level in a successful way. (Page 120: "A model of attachment was elaborated, relying heavily on the assumption that attachment serves the function of preserving and supporting the development of event representation on a submorphemic level, and guide the transition into conventionality", where 'convention-ality' here means 'morphemic conventionality'). In the rest of the dissertation, this is expanded to include also the sentence level (empirically), and theoretically it is expanded principly an infinite set of languages over and under the morpheme language (which is the only language which traditional linguistics knows).

These things emerge clearly from the conclusion in vol.I, which is summed up with only these two components: The grammatical, which takes lexical units as constituent input, and the lexical, which erases grammatical structure and converts strings to arbitrary signs.

(They work in opposite directions, so to speak: The grammatical is the competence of the individual, while the lexical is the competence of the collective). Since these two are the only components, it follows by itself that even the submorphemic constituents are arbitrary signs, only smaller than the morphemic. However, it also follows from the dissertation as a totality that every arbitrary sign is also a motivated sign with internal structure on the level below - the difference lies in where the semantics is processed - encapsulated in the individual or on the boundary between individual and collective consciousness. It is essential to the model in the dissertation that signs are both arbitrary and motivated: That will be as when (in my model) a sentence with a semantics arrived at through processing of the internal structure converts to an arbitrary sentential sign in interaction with the collective competence: The sentence node as a sign can then be assigned one single arbitrary semantic interpretation which depends on the state in the social space, and it is assigned a sentence type which corresponds to one of the four Chomsky grammars (the pendants to the word classes on the morpheme level). It is obvious that both must exist simultaneously, but that does not make out of the morpheme level a 'sound symbolic' level. The principle of a parallel arbitrarity and motivatedness at the same time is a prerequisite for the model with theoretically infinitely many languages over and under the morphemic language level. This is captured also by the two-component model in the conclusion.

It is entirely mistaken to discuss this as a case of 'sound symbolism', i.e., a kind of onomatopoeia ('gargle', 'gurgle' etc.). There is no talk about this at all, and the critique against the book falls on this. On page 64f. in the evaluation, it is stated: "In chapter 2, Grøver nevertheless succeeds in scraping together observations from various parts of the literature, in addition to his own observations and speculations, in support for the theory on the sound symbolic stage". Except that I do not recognize these things about 'own observations and speculations', it is this chapter which leads to the conclusion which I just quoted (from pages 119-120). The evaluation gives such a strong impression of rash reading with preconceptions that it must be permitted to ask whether the committee member is qualified to give an evaluation in this context.

This is supported also by the examples with phonological and social selection (pages 64-65). For example, it is said (on page 65) that the model will meet problems in explaining how regressive consonant harmony can last into the two-word stage. It has not: If the lexical component erases existing structure, then the existing harmony will of course remain for a while after the single word stage. The model presupposes that the lexical component imposes a new communicative competence on the child relative to the collective consciousness (the child discovers a new communication which cannot be predicted from the grammatical processing of submorphemic constituents), but that does not mean that the submorphemic processing suddenly disappears and never returns. As it emerges from the introduction to vol.III, the difference between the grammatical and the lexical component entails strictly speaking just that the child discovers a semantic processing which is not computable by means of the grammatical component: A new processing is introduced which transgresses the grammatical competence, and this is the discovery of the new level. When the child establishes this as a new defining processing, the earlier submorphemic competence is replaced (more or less slowly) by the new. When the child has grown sufficiently into the new, the new morphemes are consolidated as a lexicon which can be subjected to grammatical processing in the same way as the submorphemic could. This means that the grammatical competence once again takes over, but now on larger constituents. But this does not mean that the entire 'lexicon' suddenly is reshaped into a morphemic lexicon over the night - even if it still has a somewhat low status in the community of linguists...

The answer to the question at the end of page 65, on whether attachment should apply also to the morphemic signification, follows from what I just said: Yes, attachment certainly applies also to the morphemic level. This is another potentially groundbreaking aspect of the first part: My draft for a theory of 'attachment' supposes that this is an essential part of the mechanism which governs the interaction between the individual and the collective consciousness in the arbitrary linguistic sign, and, consequently, that it is systematically tied up to natural language. It furthermore clearly states that the extreme degree of attachment around the middle of the second year of life is due to the transition from one level to another and serves to guide this. Attachment in any case works throughout the whole life and is a characteristic of all societies. My draft for a theory of attachment is new in the sense that it sees attachment relative to arbitrarity and control of the development across the language levels. I sent my book to Inge Bretherton, who also presupposes a connection between attachment and language - but only for arbitrary morphemic signification - a couple of years ago, but I haven't heard anything from that scholar since.

Page 66: Does Grøver reject the possibility that psycholinguistic tests can show the existence of 'the sound symbolic stage'? No, I don't say anything like that at all. It is obvious that psycholinguistic texts can say much interesting, but I do not discuss 'sound symbolism'.

Further on page 66: On children's featural semantics. See LANGUAGE March 1998 for a new article which says almost exactly what I say in the relevant paragraph (this is perhaps the reason why it is mentioned by the committee?): Michael Shapiro's article on "Sound and meaning in Shakespeare's sonnets" explores a correlation between the phonological features SONORANT and OBSTRUENT on the one hand and the semantic features FREEDOM and CONTRAINT on the other, in some of Shakespeare's sonnets. I quote from the comparable paragraph in my vol.I page 171-172:

"To summarize, the tendency which is detected here seems to suggest that (in this [child's] vocabulary) non-coronal constrictions are associated with the establishment of spaces, labials with the boundaries to them, velars with their inside (as containers), while coronals seem to perform a generally ostensive function within these spaces. Continuants are suggested to be associated with motion, fricatives unmarked, nasals with the additional feature of transference across boundary. Vowels could be correlated with a mimetic representation of 'space size', to have high vowels represent body-space and open vowels house-space, although these analyses (as such analyses necessarily have to be) seem somewhat uncertain in the absence of any information about the social context of use for the words. Whether such a concrete signification extends beyond this limited vocabulary to the two-word stage (for Douchan) has not been considered here, and, indeed, it would be premature to suggest that these concrete features generalize to other children and to other languages".

I don't know if Shapiro has read my book, but he emphasizes in the article that there hardly exists literature on the topic. It is obvious that Shapiro's article can be read as support for my vol.I, to the extent that literary texts (poetry) are relevant for the understanding of signification on other levels than the morphemic level. In particular, Shapiro's article is a strong support for vol.IV of the dissertation, which the committee does not discuss at all, and which was blatantly rejected as an application to the Norwegian Research Council last year, the committee headed by Taraldsen. It is obvious that I am more in line with LANGUAGE than with Tromsø (I suppose it is Holmberg who has written the evaluation of vol.I). I suppose nevertheless a more advanced model when it comes to social encoding of knowledge than what Shapiro represents, in the sense that it is not the iconicity in itself which is my concern - even if that could be recognized in the particular material which I discuss in the book.

Page 66f. shows clearly that the committee has not understood what I am saying (it once again looks like over-rash reading): "We now know that children recognize sound patterns in their mother tongue in the latter half of the first year of life, and that they to a certain extent can recognize word forms, etc. The babbling exhibits in gradually increasing degree traits of the mother tongue" (page 66). This is precisely my principle. The committee attempts either to turn things around and make their ideas mine in claiming that I say the opposite of what I do and then criticize me for it, or it has not read the dissertation. It is furthermore said: "Grøver ignores almost entirely the difference between receptive and productive competance, which is central to the new research on the very early development of children's language. It is obvious that Grøver's argumentation for a sound symbolic stage...". I do not ignore this. It is discussed on for example page 148, but I do not consider it essential for my model. - In addition, I do not argue for a 'sound symbolic stage'!!!!!

Page 67: It is claimed that I avoid the problem that children are not eager to introduce conventional pronouns in their single word vocabulary, and the committee member therefore does not understand "why the early submorphemic categories should not be found precisely among the pronouns". On the contrary, of course: I suppose that the feature level is relevant for encoding of person, and when children express themselves with semantic constituents on feature level, they simultaneously express things relevant to personal pronouns. Then what should they need personal pronouns for?

And now for a bad one: On page 67, last paragraph, it is claimed that there is "a serious weakness" in the chapter that it does not specify how this can be found among the pronouns but not in other categories, such as tempus, case, gender. Here it looks as if the committee member has 'glimpsed into' the fact that my model eminently suits e.g. Chomskyan minimalism, which was developed around the time of my model (around 1993 - minimalism tries to minimize the distance between phonetic and logical form). This is precisely what I say in my book, and it is from that book that the committee member gets this idea - but I understand well that the committee member would have liked to have been the origin of it. To call it 'a serious weakness' is entirely failed.

The summary of the chapter on Greenberg (page 68f.) looks OK, except for the strange sentence "If genetic relatedness is excluded, the explanation can only be language contact" (page 68) - I say the opposite, that it is cultural similarity and not geographical contiguity or historical relatedness which is the reason for the correlations in the core vocabulary. Page 69 is therefore without importance here. I discuss (pages 257-258 in vol.I) how Finnish can be similar to the Amerindian language Penutian in the core vocabulary as a consequence of cultural similarity which certainly is not due to geographical proximity or historical relatedness. It means that one has to presuppose that they share cultural properties which condition the core vocabulary in a way which generates similar forms - a coincidence which consequently means that there is a cultural parametrizing with considerable constraints in this lowlevel semantics. The question which is raised on page 69 - why Finnish isn't more similar to Swedish in its core vocabulary - is not my problem. It looks irrelevant in this context. It goes without saying that there is no reason to expect that they should be similar because they are so different in other respects, even if they should happen to share some cultural features which we do not know much about when it comes to the lowlevel semantics. I have discussed the feature VOICEDNESS in vol.IV of the dissertation (a slightly ingenious analysis which is ignored by the committee): It is obvious that we cannot make any conclusions on how Finnish and Swedish would relate culturally to this kind of knowledge. The committee member concludes by invoking standard linguistic theory, which says something different from me. I should take that as support for the originality in my work.

Page 69, last paragraph: Bilingualism is not a problem for my theory, but it was at the outset, while I still identified myself with linguistics. (I no longer do). One can well pose the question and be stuck in it, but it is better to solve the large problems first, and then the answer comes by itself afterwards.

It is, furthermore, not mentioned in the evaluation of my analysis of the personal pronouns and the interpretation of these on basis of Greenberg's data that this was written in 1993 (more precisely from the end of the 80's onwards), reviewed by publishers in 1994 and 1995 and self-published in 1995, that is, before the article by Nichols & Peterson: "The Amerind Personal Pronouns" was received and printed in Language June 1996. This article on Greenberg's data and personal pronouns in Amerindian languages is so similar to my analysis that there is a question of a slight 'priority dispute'. I sent the book to the editor Mark Aronoff last year and pointed out the correlation. He wrote back that he recommended that I make an article out of chapters 4 and 5 (on the pronouns and Greenberg's data), and that it is exactly such stuff as my analysis that he would like to print in Language. That should be telling of a certain priority in the research, an originality which should be credited in connection with a doctoral dissertation. It should also, I would say, indicate that the scientific level is good enough. Instead, the committee comes around with its narrow-minded critique of what they think is oldfashioned 'sound symbolism'. I don't feel that it is much worth.

Therefore, I do not appreciate the conclusion on page 70 either, which builds on the preceding discussions - and which do not hold much water. It is claimed that I have big holes in my knowledge, but it is not stated where. I have certainly not read all the books in the world, but that is irrelevant here.

Conclusion on the evaluation's discussion of vol.I: The evaluation does not suffice at all for its conclusion.

4.2. Vol.II: Epistemes, language and information technology

This book is essential for the understanding of all the other parts of the dissertation. It is somewhat important to keep this in mind.

4.2.1 The history of linguistics and information technology

The summary on pages 71-72 of the first part of vol.II is certainly not bad.

The comments start on page 72, approximately the middle of the page: I am criticized for making use of secondary sources and for insufficiencies in this vast reconceptualization of the entire Western cultural history which I have squeezed into 100 small pages. I return to this criticism below.

Page 72-73: One asks for a close reading of the Stoics in order to throw more light on the socalled 'stoikheion' concept. (The committee member is probably aware of the fact that the 'stoikheion' concept was central throughout antiquity, not only for the Stoics). I studied the important Stoics somewhat in the spring this year (maybe the committee member has perceived the vibrations): This did not affect the analysis in vol.II. My studies of the original texts last Spring once again reinforced the impression that there is a massive influence from reception history in the traditional interpretation of the central concepts, which of course makes it almost impossible to rely on translations, since these by and large serve to encode reception history in the texts. It is therefore a delight to discovery that my analysis holds well also with a closer reading, and I think that the same will be the case for all the periods I discuss.

Page 73, end of second paragraph: It is asserted that "theoretical linguistics today is far more than Chomsky pre-1960 and does certainly not give reason for the conclusions which the author attempts to make". This objection must be wrong, quite simply. I conclude in the relevant part, page 106, in the last paragraph on the modern grammar: "We can now make a fair guess at what is the ultimate purpose of generativism: It implies an internalization and a concomitant formalization of the structure with the goal of attaining motivatedness of the signification, and it thus prepares, just as the enlarged basic symbol in the antiquity and nominalism in the middle ages, for a further expansion of the grammatical structure".

This should be clear enough: This is what is at stake everywhere, but now it has come to be so self-evident in terms of cognitivism (since the fifties) that we no longer think of it as an internalization. I am not concerned with local fluctuations and other ephemeralities. I must nevertheless point out that one can read this conclusion, written in January 1993, as a postulation of the Chomsky minimalism, which works exactly towards a minimization of the distance between logical and phonetic form. The committee's critique is regrettable from this point of view.

Page 73, further: I read the doctorate dissertation of Tor Bastiansen Trolie which was accepted for the dr.art.-degree in 1997 - it was a book of 210 pages plus 4 pages references (title: "Skuespilleren i kontekst - en skisse til et vitenskapsteoretisk altenativ"). These were A5-pages, with margin and distance between the lines so large that a normal printout would (I calculated) be less than 90 pages. It can of course be argued that the size of the book is not really what is important: If only the idea is good enough, and there is an element of original scientific thought, it can be defended for the doctorate degree. (I did, it is true, not see the idea when I read it, but I do not know the field, and the committee for it must probably have seen it).

Okay, then I point to vol.II in my dissertation. It is considerably larger than Bastiansen Trolie's dissertation, but the first half of my book is constituted by such a large and original scientific thought that it holds well out the next millenium. The idea is that historical epistemes constitute units which have parallels in linguistic structure which are so obvious that one can justify to give them one and the same epistemological status. This entails that Saussure's fundamental idea - that synchrony and diachrony must be strictly separated in the linguistic analysis - falls apart. One can thereby compute on historical epistemes such as one computes on constituents which are stored in the individual memory, the difference being that such computation on epistemes will be a computation on constituents in the collective memory.

In vol.II, the first half, I show that the history of linguistics can in fact be read in precisely this way and that the epistemes which are constituted in the development of the formal analysis of grammatical levels coincide with the development of information technology.

I show this by historical analysis. It is irrelevant that I do not rewrite all historical research in the thousands of volumes which are required - I have no time for that. I propose the idea, I show that there is a historical basis for it, and I relate it to the other parts of the dissertation, which it then comes to be an integrated part of by showing the basis for a linguistic structure in the collective consciousness.

My critique of Saussure's distinction between diachrony and synchrony is a critique of a dogma which was introduced in linguistics around the same time as uncountability was introduced in mathematical logic. The distinction is tied up to the dogma of arbitrarity in a systematic way - it may take some time to see this if one hasn't seen it in advance (perhaps the committee hasn't), but now I must ask that the committee does not start criticizing me of not discussing this explicitly in my dissertation (for example: 'a serious weakness in the dissertation is that it does not discuss the relationship between the distinction of synchrony and diachrony and the arbitrarity principles such as this was formulated by Saussure...').

Now I am not against arbitrarity in itself. On the contrary, this thing about the arbitrary linguistic sign on the sentence level is an important matter for vols.III, IV and partly V, and the arbitrary linguistic sign on the submorphemic level is it for vol.I in the dissertation. Neither am I against the mathematical concept of uncountability - I am only against this idea of 'bigger and bigger infinities' which one has been treading the water with in waiting for permission to talk about a collective conscious-ness with a grammatical competance. Neither am I a defender of either rationalism or empiricism in linguistics, but of both of them. My dissertation shows how one can arrive at an understanding of signification which transgresses these schools which mainly serve as mainsprings in the academic life (often in the hats of the participants): I consider the linguistic system as constituted by both empiricist and rationalist analysis, the linguistic sign as both arbitrary and motivated.

I have not found anybody else who may have proposed a similar theory on the relationship between historical epistemes and the grammatical competence in individual cognition. It is my pioneering work, and it will come to be the foundation for the theory of grammar in the next millenium. The committee member ought to burst into the corridor with vol.II lifted high and shout to his colleagues that the solution is found. Instead, it is muttered on page 73: "...an interesting hypothesis, but it is regrettable that the argument for giving substance to this hypothesis has not been more profound and well- documented. It must therefore be concluded that the first part of Epistemes, language and information technology in its present form does not suffice for scientific requirements - the work appears as far too unfinished for that". Hence it is complained that 'the argument for giving substance to this hypothesis has not been more profound and well documented'. Well, that is what will be done in the next millenium, when history is reconceptualized on a large scale. I have in fact used a hundred pages of the dissertation to formulate the idea which the committee member hardly would have come across if I had not said it first. I have also shown how and why such a reconceptualization is possible. It is here a matter of a far-reaching paradigm shift, and then just a few pages more does not suffice anyhow. Some thousand volumes are needed to meet the committee's critique. Those will be written, but by the normal science which is to install the new paradigm.

Now to compare with Trolie's dissertation: Only this single small part of my dissertation should be 'an idea good enough' to suffice for such a doctorate as Trolie's dissertation suffices for. Only vol.II of the five parts of my dissertation is longer than Trolie's dissertation.

4.2.2 The history of logical paradoxes

I guess that this is written by Fenstad, and that the division of labour has been that Fenstad should criticize vol.III and the chapter on the logical paradoxes in vol.II. That can be the reason for the initial comment that this chapter is seen relative to vol.III. In reality, the chapter on the history of the logical paradoxes is an additional empirical support for my hypothesis in the first part of vol.II - it does not contribute to define a formal architecture. On the background of the rest of the dissertation, <u>which must be read as a totality</u>, it will be predicted from the reading of the first part of vol.II that the hypothesis can be tested by studying the history of the logical paradoxes. If a corresponding development is found, the hypothesis is supported. So it is, shows the chapter. It is primarily this which is the purpose of the chapter.

Page 74, second paragraph: The committee member would have liked to see my theory applied to Gödel's incompleteness theorem. There is certainly much to gain for logical theory in my reconceptualization which entails that there is a grammatical structure in the collective consciousness, but I haven't discussed Gödel in any interesting way at all.

It is pointed to a 'deficiency' in that I should not be 'acquainted with the technical analysis of the paradoxes, e.g. with the role which the liar paradox, transformed into a socalled self-referential proposition on provability, plays for the technical formation of the proof for Gödel's incompleteness theorem' (page 74). These are, again, irrelevances from the committee, and my hypothesis that they here focus on things which can be traced to my name is reinforced by this passage. They once again come along with Gödel. Provability in connection with self-referring propositions is not a part of my discussion. On the contrary, one can see the proposition in the evaluation as self-referring on provability in itself.

It is asserted in the same paragraph (p.74), the last part: "On the background of this context (and others), we find it entirely unfounded that the author looks apart from the semantic paradoxes when he is about to prove a 'deeper' connection between linguistic theory and logic". This ambiguous use of 'entirely unfounded' means either 1) that I should not have looked apart from the semantic paradoxes - my hypothesis would have been going well even with these in the luggage, or 2) that I should have given a reason for the exclusion of the semantic paradoxes but have not done so (that should, then, be criticized). Now I do give a reason for the exclusion of the semantic paradoxes on pages 129 and 132 in vol.II: "We cannot make more than four kinds of paradoxes with the two binary-valued features NESTING and CONTEXT. [...] In the present context, I restrict myself to the four classes suggested by the Chomsky hierarchy and the two parameters NESTING and CONTEXT" (page 129). "Ramsey (1925) suggested that the paradoxes be divided into 'semantic' and 'logical' paradoxes, the former including reference to natural language, the latter being restricted to logical concepts only. This seems to make sense also in the present context, since the ones which can easily be subsumed under the nesting/context analysis also fall under the class of logical paradoxes in Ramsey's definition. I thus exclude the semantic paradoxes of Berry, Richard, Zermelo-König and Grelling, which all rest essentially on the role of the code of natural language" (page.132).

This is the reason why I exclude the semantic paradoxes. The assertion that this is 'entirely unfounded' comes in addition to the regrettable assertion on page 59 that it should be an 'entirely unfounded' assertation that Cantor proved the existence of the collective consciousness, possibly written against the committee member's knowledge if he has read all of vol.II.

It is furthermore asserted in vol.III (not vol.II, which is discussed here) that I advance, on page 52, the incorrect statement that Tarski built on Gödel's theorem. I do not say that: On page 52, I refer to the fact that Tarski worked in the climate which followed Gödel's theorem in the beginning of the thirties. I here allow for a quote from Popper, "Conjectures and refutations" (1989:269), which should be telling:

"Gödel, by his two famous incompleteness theorems, had proved that one unified language would not be sufficiently universal for even the purposes of elementary number theory: although we may construct a language in which all assertions of this theory can be <u>expressed</u>, no such language suffices for formalizing all the proofs of those assertions which (in some other language) can be <u>proved</u>. / It would have been best, therefore, to scrap forthwith this doctrine of the one universal language of the one universal science (especially in view of Gödel's second theorem which showed that it was pointless to try to discuss the consistency of a language in that language itself). But more has happened since to establish the impossibility of the thesis of the universal language. I have in mind, especially, Tarski's proof that every universalistic language is paradoxical (first published in 1933 in Polish, and in 1935 in German). But in spite of all this, the doctrine has survived; at least, I have nowhere seen a recantation".

It should be much more interesting to point to the large science-theoretic explanatory potential in my model of the hierarchical languages in this connection than to refer to a slip of the pen in connection with the name Kurt Gödel (I see, as mentioned above, a slip of the pen in connection with Kuhn/John in an earlier paragraph in the evaluation). As Popper remarks, there is a large need for such a model. I have made one for natural languages. One should have burst into the corridor with the dissertation lifted high... Instead, it is muttered about sounds symbolism etc.

The purpose of my discussion of the paradoxes and grammar types is exclusively to point to what character the various logical paradoxes of the epistemes exhibited, and to suggest how one can impose a double binary opposition over them in order to obtain an analytical tool to start handling these historical episteme boundaries in a more precise manner. In the analysis of the culture history (vol.II), I work with two binary oppositions which in fact are strongly related to those I make use of in vol.V ('A waist of time') for signal analyses. I make, nevertheless, no attempts in the dissertation to compare the various epochs in a way which allows for a beginning computation on basis of them. If one has invited Fenstad to evaluate the contents of the Cantor machine on basis of computation theory, I suppose one has invited him on the wrong premises. I do not deal with that. It is irrelevant for me in the dissertation what labels are given to the two binary oppositions and what paradoxes receive which values. My intuitive discussion serves to present the paradoxes and to show that there is a connection between the emergence of new historically important paradoxes and the transgression of constituent boundaries in the theoretical grammar. In addition, they are brought on a form which makes them compatible with the analysis in vol.V, and thereby they come to be implementable in a concrete cognitive connection. On page 74f. in the evaluation it is said that 'the decisive weakness' in the chapter on the logical paradoxes concerns the attempt to show an identity between paradoxes and formal languages. This cannot be the case, such as I see it, since it is without importance for the discussion <u>in the dissertation</u> whether Burali-Forti's paradox is + or -NESTING, + or - CONTEXT, etc. In addition, my relevant signal analysis in vol.V is an original contribution to studies of linguistic structure in signals, and has at the outset nothing to do with traditional analyses of formal languages. The analysis of the paradoxes in the dissertation serves to illustrate a possible dimensionality which I make use of in the two most basic oppositions I could find - what can be interpreted as paradigmatic and syntagmatic oppositions. To claim that this should be a 'decisive weakness' is unreasonably formalistic.

It appears clearly from the book that the purpose with the analysis of the logical paradoxes is to arrive at a common analysis of historical episteme shifts, logical paradoxes and constituent boundaries in grammatical structure (in individual and collective cognition). For example, on page 200 I say, underscored: "<u>A grammatical node represents a logical paradox</u>". This - that grammatical nodes are logical paradoxes - is also a highly interesting point for the exposition, and it is regrettable that the committee member does not rather indulge in enthusiastic discussions of the contents of my exposition instead of ignoring it as if I had not even touched upon it. I have searched quite extensively to find literature which could support my thesis on the connection between grammatical nodes and logical paradoxes, but I found nothing better than Curry's vague allusion from a seminary at the beginning of the sixties. If I am the first to state this explicitly in such a way that it doesn't sound like a triviality, then I have made a huge bomb for linguistic and logical theory. Fenstad should then burst into the corridor with the dissertation lifted high and shout to his colleagues that the solution is found. Instead, it is muttered on page 74 that it is "entirely unfounded that the author looks apart from the semantic paradoxes when he is about to prove a 'deeper' connection between linguistic theory and logic".

At the end of 4.2.2, page 75, it is referred to my analysis of Zeno's paradoxes. The analysis has, according to the committee, not the standard which one expects to find today. I agree that the standard seems to have declined since the dr.art.-degree was introduced. My discussion of Zeno's paradoxes is one of the clearest examples of the explanatory potential in the model. The history of philosophy has been concerned with these for more than two thousand years, and I have not yet heard about anybody else who have found my simple and ingenious solution, which builds on the presupposition that the boundary between incommensurable knowledge-spaces (subjective and collective consciousness) is the basis for the paradoxes. They can, for example, be captured with more formal precision in the form of my two components in vol.I, the grammatical and the lexical, or with the two binary oppositions (mid' and 'half') for signal analysis in vol.V, page 17, where it is stated about the signal analyses: "There is an obvious similarity with the classification of Zeno's paradoxes discussed in Grover (1997e)". The four wellknown Zeno paradoxes are solved in a simple manner by my analysis, and one avoids all the busy talk of geometric vs. arithmetic series etc., which only serves to reduce them to a narrow string level without any gain at all. My analysis presupposes a psychological aspect which is characteristic of the knowledge-space which the cultural history had developed at that time. I nevertheless assume that the binary features NESTING and CONTEXT can be used as descriptive tools for the understanding of it, if for nothing else so at least because they are as general as possible. This is how one must consider this chapter. I show that it is possible to read the history of paradoxes in this way, but I make no claims to have made formal definitions - not the least because I try to give a common frame of understanding for social, sensory, phonetic and historical data.

One consequently cannot assign much importance to the critique of my chapter on the paradoxes as long as the critique rests on requirements for formal rigor. I emphasize several places in the dissertation that I work towards a poetic science and that it is poetry which is the literary representation of the collective narrative (poetic language is collective narrative). This is the kind of 'computability' which I work with.

In addition, the attention to Gödel in the evaluation should perhaps be criticized. My very few allusions to Gödel are totally marginal, and if anything meaningful can be read out of the similarity between the names Kurt Gödel and John Grøver, such as appears from the beginning of the evaluation, it has anyhow no serious function. The discussion of the paragraph on the semantic paradoxes contains not really much more than a few references to Gödel, some talk on a few unimportant aspects of the identity between modern paradoxes and language types, in addition to unreasonable complaints on my discussion of Zeno's paradoxes. What is wrong with my discussion of Zeno? - Then I could even give an anwer to it, but it is not explicated what it is that is wrong.

Conclusion: Enough is not said to allow for the conclusion that the chapter is not on a scienti-fically acceptable level, as long as my scientific aspirations are not in the field of logic.

4.2.3 The cuneiform episteme

It is asserted that I deal speculatively with Sumerian/Akkadian periodizing. I fully agree with that, of course, in particular since one knows, after all, very little about Sumerian - in fact, it is not even certain that there ever was anything like a Sumerian people. As I refer to on pages 191-192, the only reason that one assumes Sumerian as a group distinct from the Akkadians is that the language on these clay tablets, that is, the cuneiform signs, suggests two different codes. It is fairly broadly agreed little is known of Sumerian after all - Erica Reiner in Chicago (an hyper-expert in the field) says that we know, after all hardly anything about the Sumerian language. Many will claim that Sumerian is not dechiffered at all. A historical periodizing of Sumerian/Akkadian, such as e.g. the idea that Akkadian 'conquered' Sumerian in the year 1945 (that is, before Christ..), appears in the light of this as speculations which serve to interpret Sumerian/Akkadian as a mirror image to our own modern history on the other side of Christ. It is my point that this is speculative - not the committee's.

The cuneiform script went out of use around the birth of Christ. Then the alphabet had existed for more than a thousand years, and the cuneiform script would certainly have been on the retreat for hundreds of years, so it is certainly not a matter of an alphabet which suddenly was invented around the birth of Christ, and then all the old clay tablets and the cuneiform script were thrown into the garbage. I have not suggested anything like that, but it looks as if I have suggested something like that from the evaluation page 76: "Historical research must be carried out with far more sobriety and caution when it comes to periodizing than is the case here...".

Page 76, the last lines: I criticize the traditional interpretation of cuneiform, but the committee reverts that cuneiform is less ambiguous than I claim because the signs are contextually contrained - an isolated word (a sign group) is transcribed (to alphabetic script) in one way in one context and in another way in another context. This is supposed to be to the defence of the traditional interpretation. In reality, this only means that there is redundancy in the code and that this redundancy is assigned a semantics which by and large derives from the pioneers in the cuneiform research only. Their students learnt from them, and so forth, and in this way, the redundancy has come to be determining for an unambiguous interpretation. Consequently, it is hard toil to learn cuneiform, and therefore, as it goes, it is also very difficult to criticize the interpretation (one has to be an 'expert' in order to criticize). I am not into such things: I only say that this redundancy just as well can be telling of distributive properties which the cuneiform signs refer to (such as situation elements or cognitive elements or something like that) rather than to speech sounds.

In accordance with the basic idea in the dissertation, the cuneiform script should be an information technology for signification on the level below the cultural 'morphemic level' which is religiously interpreted by Christ. It should then, as such, differ from the encoding in the alphabet. One should nevertheless expect to find arbitrary symbols on a lower level, but it is obvious that it would not be phonological features which were represented by the wedges, since the features are the construction units of the alphabet script. Rather, one should expect to find that the dominating episteme before the alphabet episteme would exhibit the feature as the unit outputted from the processing - which would mean that a redundant group of cuneiform signs should encode a series of semantic units which together conspire to units which later are converted to a series of phonological features. The arbitrary units of the cuneiform code would then be smaller than the phonological units, and distributed in a cyclic manner throughout the code and captured in terms of the redundant distribution. This is what I say in vol.II of the dissertation. The assertion on top of page 77 that "this contradicts Grøver's idea" is entirely mistaken.

This of course means also that it is possible to arrive at a certain function from sign to sound, even if it goes through rdundancy patterns over larger areas and the function from sign to phoneme is far more complicated that in the alphabetic script.

I say in the dissertation that I am open for the possibility that the pioneers could have their intuition in order, and that the reading of the clay tablets in fact could imply a sympathetic understanding of the semantics in this area which for us is quite unknown (cp. the problems with understanding early child language semantics). That would of course imply a vast relief, since we then could be enabled to find out of this enigmatic lowlevel semantics by making use of the existing interpretations. One may perhaps hope that this to some extent can be possible - since that would be the key to the submorphemic level, but it is, at the outset, wise to retain some scepticism.

I base my discussion in the book on such general considerations on the cuneiform script as a code and the deplorable consequences (and large progress) which such a scepticism may lead to. It is meaningless within such a framework to discuss whether the Sumerians in fact inherited religious ideas from the Akkadians or not, such as the committee does on page 77. There is also a notable imprecision with the reason why Hittite should appear in a doubtful light: I do not say that this is because there are so many Sumerian and Akkadian signs and words in Hittite cuneiform. I say that this is because the cuneiform code is extremely ambiguous at the outset: There is a large number of possible transcriptions to alphabetic script of a 'Hittite' cuneiform text if one reads it in Akkadian. When, in addition, it is presumed that Sumerian and Akkadian loanwords are written in Sumerian and Akkadian, one is left with a narrow stripe of indisputable consistency in the interpretation which is supposed to be the proof of Hittite. My point is quite simple: If the consistency which is the proof of Hittite can be caused by something else, then Hittite dissolves to almost nothing. It is precisely this alternative motivating factor which I have shown the existence of in the dissertation, in vol.I on the submorphemic signification with a possibly universal basis - such as in the discussion of how it can come about that Finnish and Penutian exhibit so many essentially similar traits even if they are historically unrelated. That puts Greenberg's data in a new light as well. This important observation on Hittite is something which the committee should have noticed - instead of pretending that I discuss what the use of all the strange Sumerian and Akkadian signs in Hittite can possibly mean.

The committee discusses 'crossword patterns': This is, as far as I know, my concept, which corresponds to my idea that the arbitrary lowlevel units must exhibit a form of periodicity (which they deviate more or less from) within the segmentations which we assign a delimited semantics to in the later alphabetic script. "Crossword patterns" is a consequence of my theory, and something which I myself have identified in cuneiform data (otherwise I would not have discussed it). It is my idea (and my discovery in the data), which is not discussed by anybody else than me. It then sounds strange that the committee concludes that 'crossword-pattern' probably had only an 'ornamental function', as if this should be wellknown things. "It is, though, worth a study, a study which Grøver does not carry out, and which he has not proved himself capable of carrying out". This is to go far beyond the boundary to the acceptably for a committee member of this kind: He has got the idea from me, and then he cannot suggest that I am not 'capable' of carrying out such an investigation. I suppose I may allow myself to point out that one should keep an eye on the later scientific production of this committee member in this field. If there later should occur results on redundancy based on such 'crossword patterns', these must be traced to my dissertation.

On page 77-78: My considerations on a cyclic time and history conception are not speculative. They are based on these considerations of the cuneiform code and on my model in the dissertation in total. They are not based on the cuneiform culture's own documents: The interpretation of the script is far too uncertain to allow for that. It is I who have a healthy scepticism towards the speculative cuneiform interpretation, not the committee.

Close to the end of this part of the evaluation, there finally occurs the word I had been waiting for all the time: '...arbitrary...'. Unfortunately, the word is used in an entirely irrelevant way (about my 'arbitrary assertions', that is, assertions without any basis), and this is perhaps characteristic of the committee's work.

The committee has not discussed the chapter on the cuneiform script in light of the dissertation as a whole. I would not have included my considerations on cuneiform had they not contributed substantially to the understanding of the dissertation as a unit.

It is obvious that the cuneiform script has much to contribute with in the understanding of a series of important problems in the interpretation of the epistemology of information technology. I would unfortunately not be much surprised to discover that the University of Bergen or the Norwegian Research Council initiate research projects on cuneiform on basis of the interest which has been aroused by my dissertation, while I go hungry without research funding. This is seemingly the kind of flea market which the committee suggests when it, after having read the dissertation and got this good idea concludes: "It is, though, worth a study, a study which Grøver does not carry out, and which he has not proved himself capable of carrying out". Things like this are not permitted in an evaluation of this kind.

5. Conclusion

There is not much in the evaluation which is worth taking up. The general impression is that the committee has read my dissertation, has got some good ideas from it, and then tried to criticize me for not discussing these good ideas - which they have got from me.

The worst is nevertheless that the committee totally neglects what the dissertation is about - the arbitrarity in signification and the dissolution of the distinction between diachrony and synchrony (see the summary of the dissertation in the beginning of this commentary). It neglects thereby the extremely large explanatory potential in it.

It furthermore neglects all of vol.IV, all of vol.V and almost all of vol.III. This means that almost half of the dissertation is not seriously discussed at all. Put differently; The committee has considered only half of the dissertation. It even looks as if not all of them have read the entire half: The one who criticizes vol.III has obviously not read the last part of vol.II. There are also things to worry about when it comes to the parallels between the sections 2 and 4.2.2, together with 3 which seems to serve as a rotation mechanism. I am therefore hesitant as to whether I should assign any importance at all to these parts of the evaluation.

The dissertation is essentially about the arbitrary linguistic sign on submorphemic, morphemic and sentence level, with extension to theoretically infinitely many levels above and below the morphemic. Vol.I is about the submorphemic level and the empirical basis for assuming the existence of it. Vol.III and IV on the arbitrary sign on the sentence level. Vol.V on the arbitrary sign interpreted within the framework of optimality theory. I present an entirely new interpretation of the concept of arbitrarity in the dissertation, an interpretation which entails a fundamental reassessment of Saussure's basic concepts on arbitrarity, synchrony and diachrony. This is not mentioned with a single word in the evaluation. Vol.II shows how it is possible to arrive at an interpretation of diachrony which puts it on a line with synchrony.

It is also regrettable that the committee has not understood the relevance of my model for modern linguistic theory. Optimality theory is given a very interesting interpretation in vol.V. I have also mentioned minimalism a couple of times above: The conclusion of the section on Chomskyan grammar from January 1993, vol.I with minimization of the distance betwen logical and phonetic form, as well as the arbitrary sentential level (whereupon every sentence is assigned a 'language type' of Chomsky type 0-3) are examples of this. The Chomsky minimalism has not yet arrived in a state where it can let Saussure's arbitrarity go, but it will arrive there soon. Two quotes from Chomsky are illustrative: 1) He presup-poses ('The minimalist program', 1995:221) that 'telepathy' would erase phonology: "UG [Universal Grammar] must provide for a phonological component that converts the object generated by the language L to a form that these 'external' systems can use: PF [Phonetic Form], we assume. If humans could communi-cate by telepathy, there would be no need for a phonological component, at least for the purposes of communication". It is obvious that

Chomsky's concept of telepathy is not the same as my concept of a collective consciousness with grammatical form, but it is sufficiently close to it to allow for a compa-rison of the Chomsky minimalism with my model, which assumes that it is in the boundary to the collective consciousness that phonology is created, where phonology is a result of the individual's interaction with the collective consciousness. 2) Chomsky's lexicon is filled with exceptions to the rules, and the reason why he still clings to Saussure's arbitrarity is still that he finds no way to divide up the morphemes. The main goal for the minimalist program is nevertheles to minimize the distance from phonetic to logical form, and there is no doubt that the program has to ask itself whether it can come under the morpheme boundary in order to go beyond it. In a few years time, the minimalists will arrive at a sentence level sign of the kind which I work with in vol.III, IV and V, with 'word class' as one of the four language types and where there is a minimal distance from the phonetic to the logical-conceptual. They will not arrive there without dissolving the morpheme: If one postulates a level over the morpheme level (with such a minimal distance), then one has no other choice than to postulate one under it as well. Then one ends in reality up where I am now.

There is a peculiar rigidity in this field: A lexical unit is, for Chomsky, in addition to several other things, a phonological matrix which is associated with a set of semantic features. It goes without saying that there must be much redundancy in the distribution within an ordinary morpheme (the lexicon is, after all, finite), and then one can easily make a minimalist grammar as a mapping from the phonological matrix to the logical-conceptual even within the morpheme. One may well ask whether Chomsky opens for a submorphemic signification when he says of the lexicon that it contains units which consist of "a phonological matrix of the familiar kind expressing exactly what is not predictable, and a comparable representation of semantic properties, about which much less is known" ("The minimalist program", page 236). There is no reason to assume that this should posit the morpheme as a last impenetrable barrier. And if one has come below the morpheme, then one can of course continue as far as one wants. It goes in that direction whether one likes it or not, and then, sooner or later, one ends up with an arbitrary sentence level sign - which the computers can represent, but which could not be represented in the episteme of the alphabetic script.

It is highly regrettable that the 'submorphemic' in vol.I is interpreted by the committee as a case of traditional 'sound symbolism', that is, a kind of onomatopoeia, and the arbitrary sentential sign is not discussed at all. Then one is still left with Saussure's morphemic symbol which the committee will not let go. Then the historical model in vol.II comes to be quite uninteresting as well - what then is the purpose with these epistemes and discussions of paradoxes? It is probable that this attitude which the committee takes cannot be much more than a traditional resistance against theory which is ahead of its time.

I should add that <u>arbitrarity</u> in signification normally means that there is no internal structure in the symbol. Motivatedness means that there is internal (grammatical) structure. I presuppose that all signs are motivated and arbitrary. What is new in my model is that arbitrarity means that the semantics to the arbitrary symbol is given in the interaction between the individual and the collective conscious-ness (which is essential to the model), that is, in the boundary where the 'attachment' works. It is on this boundary that the lexical component (vol.I page 120) works. Arbitrarity should consequently not be considered as a case of absence of internal structure, but as a matter of degree of interaction between individual and collective consciousness. If the cognitive processing can be encapsulated informa-tionally in the subject, then one has a case of motivated signification. Otherwise it is arbitrary. This has traditionally been captured in the concept of arbitrarity as conventional signification. The difference is that such conventional signification has been presumed to be stable, while my model presupposes that it changes all the time, and thereby is a real component in the collective processing. I furthermore presuppose that such signs exist on all levels, even if the morphemic level is most fashionable in individual cognition (in adult language) nowadays, as it has been for some centuries.

This means, again, that I reintroduce the diachronic aspect in the synchronic competance. A traditional conventional arbitrarity on a Saussurean basis does of course presuppose nothing more than a signification which is reasonably stable: It certainly varies over years (at least over centuries), and this is Saussure's diachrony, which according to his students' reproduction must be strictly and systematically separated from the synchrony. When I reintroduce diachrony on a local level, which means that conventional signification changes over milliseconds as well as over centuries, I obtain the theory with the extreme explanatory potential which I have proposed in the dissertation (and which the committee does not touch upon at all). It is here that it makes sense to talk of epistemes as centuries (vol.II) in the same breath as one talks of it as milliseconds (vol.V, which the committee does not discuss). This is also the reason why it makes sense to analyze the history of paradoxes and to arrive at the conclusion that 'logical paradoxes are grammatical nodes'. This is my scientific achievement, which I have shown empirically and theoretically.

The main conclusion in my dissertation can therefore perhaps be reduced to precisely this - that the diachrony is so local that one must revise the concept of arbitrarity to include interaction between individual and collective cognition also as a part of the concrete language production. I say in the conclusion to vol.II, pages 209-210:

"The model which has been discussed here entails a collapse of the rigorous distinction emphasized by Saussure between the synchronic and the diachronic levels of language. It suggests that a joint account of the formalized knowledge of historical epistemes with the formal properties of grammars must be sought, ultimately in a joint cognitive interpretation which also lets us understand the nature of memory, culture, history and language in a manner which allows for a deeper understanding of the interface between the individual mind and the shared consciousness of the community. / This is the natural way to go for the computer episteme we are moving into".

The committe has not understood anything of this. They have not with a word mentioned the arbitrarity in its evaluation. Neither have they understood why or how this is the solution to a series of current problems in the transition into the computer empisteme. There is an enormous explanatory potential in the dissertation, but the committee has not understood this - probably because they are stuck in the thought of the alphabet episteme. This emerges e.g. from the last sentence in the conclusion of the evaluation (page 15: "We will nevertheless contend that the material in Vol.I may be further reworked, but then within the framework of accepted linguistic theory"), something which reveals a deplorable lack of understanding of what my dissertation really is about. In 'Standard regulations for the dr.philos. degree', it is stated on pages 1-2 that "The dissertation shall be an original scientific work [...and] contribute to the development of new scientific knowledge". The committe cannot insist that the dissertation is presumed to be within the framework of accepted linguistic theory and at the same time claim to have made a serious evaluation.

My attempts at formal analyses serve to introduce a common analytical tool for a series of different domains - the grammatical/lexical, paradoxes and grammatical structure, the signal level, in addition to the historical episteme level. The analysis serves to show that one <u>can</u> arrive at a shared analysis of these in a cognitive model. I include the last part of the conclusion to vol.II (pages 210-211):

"Furthermore, the study has pointed to how we may arrive at a practical interpretation of cultural knowledge embodied in language, when this is assigned a cyclic semantics which also correlates the various levels of the hierarchical structure of language with the diachronic properties which have given the particular shape to language. Given this cyclic instantiation of sedimented history (in archetypal format), we can arrive at a very practical interpretation of the unified sign wherein the diachronic and the synchronic dimensions meet. It is here that the subjective mind has its interface to the collective consciousness. In a wider perspective, it is this conception of signification and cognitive processing which ultimately will open up for a formalized processing of those aspects of knowledge encoded in the social space which transcend Turing-computability, and which finally may lead to the construction of the 'Cantor machine' with a computational power transcending the Turing machine. This of course is the direction of development of the evolving computer age".

The dissertation is, as appears from this, a dissertation in the field of <u>semiotics</u>. This is not the least the case for vol.III, which it looks as if Fenstad has been the critic for. It is true that I postulate a future Cantor machine, but I make no hasty attempts to make the technical specifications to the machine. The critique in the conclusion of the evaluation, page 15, that "there are critical insufficiencies in the author's attempt at defini-tions and constructions in the general part; see particularly Vol.III" seems to aim at precisely this - that I have not constructed the machine sufficient-ly well. Section 2 in the evaluation, with the title "The candidate's suggestion for definition / description of the Cantor machine" (pages 57-60) is obviously about this, but it consist, in addition to the 'witchcrafting' in the faulty name forms ('Kuhn' instead of 'John', 'EV-clide' instead of 'EU-clide' as a slip of the pen in the context of 'Gödel', as in the evaluation page 74), of an attempt to make a formal critique of my discussion on pages 52-57 plus the small concluding paragraph on page 71 in vol.III. These shall, as I say on page 52, be a 'rough sketch of what will be [underscored here] the basic architecture of the Cantor machine working by archetype mechanics". I make no attempts to define this, even if I discuss some possible concepts on pages 52-57. This should emerge clearly also from the last chapter in vol.III (with the title "The arbitrary sign and the Cantor machine"), which shows that the state in the social space varies with wind and weather and with who comes and goes (not very surprising, I suppose).

The committee criticizes all of vol.III ("The theatre of the heart") only by these 5-6 pages which are discussed in their section 2 (the evaluation pages 57-60). In addition, the committee includes the totally empty 'discussion' in section 3 (the evaluation pages 60-62), where indeed nothing at all is said (it is probably meant to be 'telling'). It does, though, look entirely apart from e.g. my conclusion at the end of the chapter on "The arbitrary sign and the Cantor machine", where I state:

"Similarly, it is not a massive amount of correlations in the present book between the infinite set of observations in Bergen 1996 together with the elegy of Rilke which gives credibility to the analysis: It is, rather, the relevance of the few observations which are made which gives substance to it. A statistical assessment of all observations made in Bergen 1996 will probably not output Rilke's elegy as the underlying collective narrative. We should not even try to fit all observations into this narrative. This is what makes the science exemplified here a case of poetic science: A new space of human recognition is opened beyond the space of machine-recognition, and it is in this space that the new science must work. The value of singular poetic observations, such as Elisabeth Aas' work, is much larger for this kind of study than a statistical corroboration of a hypothesis based on a large set of observations. / The pivotal poetic observations serve to define, in a constrained manner, the collective consciousness" (s.331).

I cannot see that the committee's evaluation implies that "a new space of human recognition is opened beyond the space of machine-recognition", where the new science which I outline in the book is supposed to be situated. On the contrary, the committee attempts as well as it can to pull it all back again to the alphabet-epistemic machine level, and criticizes the dissertation for being unfit for that level.

If Einstein had come with his relativity theory to have it accepted for the doctorate degree at the University of Bergen, a similar committee would hardly have accepted it: "It is too long here and too short there. In addition, we have not heard about this $E = MC^2$ before. 'We will nevertheless contend that the material may be further reworked, but then within the framework of accepted theory'".

The committee's resistance against seeing anything but the traditional morphemic symbol with a syntactic structure over it seems strangely deep-seated. It is probably the same kind of resistance which has met vol.I as a book since 1994, when I first started my attempts to have it published. I suppose these are problems which touch onto religious matters, and which thereby are felt as cultural resistance (experienced as cultural resistance against the publisher who publishes it - or against the committee accepting it). It is possible that the committee has nothing principled against a submorphemic or sentence level sign, but why is it then so difficult to see it? I have described these over more than nine hundred pages and it should be easy to understand. The reason for the resistance is perhaps that people must change their attitude to others if they are to accept the model, since the discovery must be that the solution to the logical paradoxes is to be found in the social and the cultural and not on the symbolic string level. The logician is thereby personally affected. That can explain some of the academic resistance.

This committee has understood so little of what the dissertation really is about that it has not even understood to renounce the evaluation work. It may be that the dinner was too good for that.

I suppose I will have ask for a more mature committee - not in age but in mind.

Judicial matters

As it appears from my evaluation of the committee' work, the dissertation should doubtless have been accepted. The committee says, though, that it is not on an acceptable level.

It has furthermore taken too much time (cp. the regulation) to reach the fairly superficial evaluation which in actual fact concerns only almost exactly half of the dissertation.

According to the regulation, the evaluation should have been completed several months earlier. Helge Dyvik was the administrator for the committee, even if I, in a letter to the faculty and to the committee, asked that he should not be a member of the committee.

I handed in the dissertation to the university in the beginning of December 1997, while there were still 8 months left of my university scholarship. I did this to ensure that the viva could be held within the scholarship period, such that I would have reasonable chances for new employment/funding after the expiry of the scholarship.

I received the committee's evaluation a couple of days before the end of the scholarship, and today I am unemployed - just as happened after I had written the first part of vol.II in 1992-93, a groundbreaking work which had the effect that I was excommunicated.

According to Norwegian Criminal Law § 247,

"anybody who in words or deeds acts in a way which serves [...] to impose [...on another] loss of the trust needed for his position or profession, or who contributes to that, is punished with fines or prison up to one year".

Furthermore, the Criminal Law § 248 says:

"If a person guilty in accordance with § 247 has acted with conscious knowledge of the liability, he is punished with prison up to three years. Under particularly extenuating circumstances, fines can be used".

Now it is asserted in § 249:

1. Punishment in accordance with [...] § 247 is not made use of if proof for the truth of the accusation can be provided.

3. Punishment in accordance with [...] § 247 is not made use of for one who has been obliged or compelled to make a statement or who has made a statement in order to ensure a justified maintainance of own or others' interests, if it can be shown that he in every respect has acted with sufficient care.

[These translations from the Norwegian law are my own]. The question is now whether the committee's evalution is subsumed under these paragraphs. It is true that this part of the criminal law is found under the chapter of 'defamations', and it seems to be the case [at least in the Norwegian interpre-tation] that this is not really a case of anything such (even if there in fact exists something called 'honorary doctorate' which perhaps suggests that there may be a smaller distance from the doctorate to this part of the criminal law than is the case for other academic degrees, but the present case is about the ordinary doctorate and not the honorary one). There is, though, no doubt that § 247 can be interpreted in such a way that it can be seen as relevant here, since the committee in words have behaved in a manner which contributes to deprive me of the trust which is needed for my position or profession. I am left unemployed after the scholarship period and I will have to refer to the dissertation work which I have carried out through the last three years. A potential employer will ask for the dissertation and the doctorate degree and I must refer to the fact that it was not accepted by the committee consisting of these-and-those persons. This entails loss of the trust needed for my position or profession, and my economic loss can be considerable.

If the committee's evaluation can be taken as a 'proof' for the assertion on page 78 (the assertion that the dissertation cannot be accepted for the doctorate degree), then the committee is of course not guilty. If, however, the 'proof' does not hold good, then it is of course guilty.

Such as I consider the evaluation, the 'proof' does not hold good (even if it is Fenstad who has made it), and then it does in fact look as if the committee has made itself guilty of a crime (a statement) which can qualify for up to three years in prison. An unbiased committee could for example compare Tor Bastiansen Trolies dissertation from 1996 - "Skuespilleren i kontekst - en skisse til et vitenskapsteoretisk alternativ" (a dissertation which I read around the time of the viva in 1997) and my dissertation and ask if my dissertation is indeed worse than Trolie's. If there is no doubt at all that it is not worse, then the 'proof' does not hold good.

Poetic matters

Strictly judicial matters were discussed in the previous section, but I think that it would perhaps by clarifying to introduce also point 4a from § 249. This is a poetic consideration which may have more resonance in the committee's subconsciousness than in their rational logic. This is precisely where the new science must be situated, according to my conclusions. The point in § 249 goes as follows:

4. Proof of the truth for an accusation is not accepted
   a. for punishable act which the accused has been acquitted for by a domestic or foreign sentence

What kind of punishable act could it be that I should have committed here? That would have to be the act which the committee attacks from the beginning of the evaluation - an act which consists in having described colleagues and others as puppets in a puppet theatre in the collective consciousness, which, according to the dissertation, should have grammatical stucture. Maybe the committee considers this justifiable when it formulates its accusation against me - since I could have made 'big fools' out of people with my description (I have not). Venerable institutions appear in a ridiculous light in this human comedy which the candidate claims to have proved scientifically... Or is this a matter of the morpheme boundary as a religious institution? Then there could even be an element of blasphemy in this dissertation...

Kafka is an appropriate interpretational framework for these matters. Both 'The process' and 'The castle' can be read as eminent descriptions of the case. The problem is now: What happens if a domestic or foreign court of justice acquits me of the accusation that I make fools of people with my descriptions (or whatever the accusation may be)? Then the committee loses its right to prove the truth of the accusation! Then the committee has nothing for its defence and must probably in jail - they will at least be liable to this according to the law. Braarvig, Fenstad and Holmberg must go in jail for perhaps as much as three years.

It is possible that this can be a factor of social importance in the further treatment of my dissertation. What will it imply if a new committee is appointed, or what will an acceptance in the faculty board against the first committee's evaluation mean in the light of this? Will it entail a domestic acquittal for the accusation of a punishable act?

Can it be tolerated that this committee is thrown into jail? Can it, in other words, be tolerated that my dissertation is accepted for the doctorate degree?

I think that these are the things which are really at stake here - and they feel so 'right' because they are based on the law. If the committee had understood these things better, I suppose they would not have made the evaluation in the way they have. I must therefore ask that my work is considered as a scientific work and not as an accusation against society.

It is possible that the solution can be found by an elegant judicial trick which I myself have made. Since the point a. under the fourth part of § 249 modifies the concept 'proof for the truth of an accusation' in the first part of this paragraph, one can quite simply move this point a. up to the first part, and obtain the following wording:

1. Punishment in accordance with [...] § 247 is not made use of if proof for the truth of the accusation can be provided

a. for punishable act which the accused has been acquitted for by a domestic or foreign sentence

This means that a new committee can accept my dissertation without this implying that the first committee must go in jail. It is thereby opened up for the acceptance! Not only that - one must hurry up and have the dissertation accepted as soon as possible, in order that the first committee shall not have to go in jail in the meantime. Fenstad can then, after a refreshing bath, just continue making proofs that I have accused society for this and that, and I can continue my own work without the committee's interruption.

John Grover 8.8.98

Part 3

Some further remarks

Some further remarks

The contributions to this book has shown that there are cases where it is impossible to have a doctorate dissertation accepted however good it is. Similarly, one can think of cases where it is possible to have it accepted however bad it is.

The lecture in the theory of science for the dr.art. degree

In the first part, the problem was that the committee did not respond to my letter which refuted all the relevant objections from the committee. I relied on standard theory. If it makes any sense at all to talk of 'acceptance on scientific criteria', the committee would have to admit that their objections did not hold good. They should then have responded with a letter stating that they were wrong and therefore, with these new facts in their hands, had changed their minds and accepted the lecture. Instead, they just refrained from answering. Not only that: I sent the committee's evaluation and my answer (which should indicate that I was justified in asking for an answer) to most relevant parts of the university, but nobody could help me with obtaining an answer from the committee. The least the committee could have done was to answer that they still did not understand anything and therefore could not change their conclusion. Instead, I received, three months after I handed in the lecture, the message that they 'no longer worked with the acceptance'. I was thereby invited to give up without having received an answer.

As I point out at the end of my answer to the committee, the lecture should be as optimal as is possible for such a science-theoretic lecture. Within the framework of a 45 minutes' lecture, I had shown that Turing-computability is limited to individual cognition, and that this boundary can be transgressed only by postulating a collective consciousness with a grammatical competance which allows for such processing. That should be a clearly formulate topic with a clearly delimited discussion, an exact hypothesis with much empirical content, and it should be one of the most important topics for current theory of science. Still, the lecture was rejected.

I considered it absurd to appeal in order to get a new committee with a fresher view on the things (after all, the new committee could be as unfresh as the first), and I therefore chose the option with the dr.philos. degree instead.

The dissertation for the dr.philos. degree

The theoretic conclusion in my lecture in the theory of science is the background for the dissertation, and it is possible that the general science-theoretic climate is not yet mature enough for this. There are many mathematicians occupied with breaking through the boundary constituted by Church's thesis, but I suppose it is still the case that one has to stay on the individual mat in order to get research funding. Collective processing is still reserved for politics and social control through the use of power. Therefore the resistance.

My critique of the committee's evaluation suggests that it is on the boundary to being unserious, or well beyond this boundary. It is natural to ask what can be the reason for the committee making such an evaluation, with such heavy expenses. It can here be relevant to mention some peculiarities which may have had some influence on these matters. While I was waiting for the committee to finish their work, I could not avoid noticing that there occurred some other committees which looked as if they had parallel fields or ways of working. First I caught some interest in the new Power committee (with the task of reporting on the power structure in Norway), which was composed around the same time as the present committee. It was Arvid Hallén in the Norwegian Research Council who, in the winter 1997, had been asked by the ministry to suggest a composition of this Power committee, and it was, as far as I know, his suggestion which was followed in the composition of that committee. This was just at the time when I had some correspondance with Hallén in connection with the fact that my project application to the Research Council 1997 had disappeared mysteriously, without a trace, after it had been received in the council. The application (a pilot project and a project description) was exactly vol.IV in the dissertation, which I had handed in to the University of Bergen in December. (Vol.IV has, by the way, also disappearaed mysteriously from the present committee's discussion in the evaluation). In February, I received, around the same time, three interesting letters: The first was a muscular letter from Hallén who pointed out that the Research Council can make use of secret consultants when considering applications, and they will <u>never</u> tell anybody that they have done so or who they have used as consultants... The second letter was from the Faculty of arts in Bergen, with a message that the committee was now appointed (with the members who have written the above evaluation). The third was from the university director in Bergen, Rommetveit, who wrote that the committee was now appointed (I myself had written to rector to make him aware of the fact that the appointing of the committee was too much delayed, so that may have been the reason). Later I discovered, not the least because of this, that there were three parallel committees in work from approx. March onwards: The one was the new Power committee (suggested by Hallén) which should revise the first 'Hernes' report on the power structure in Norway. The second was the doctorate committee. The third was a committee with Rommetveit and Norman (who is now running for the rector position at the NHH in the autumn election) from Bergen together with rector at the University in Tromsø: This committee should, according to Rommetveit in the university newspaper, make a report on higher education (to a revision of the 'Hernes' report on higher education) to the 1st of April 2001. I could not help reading in parallel: Here there were two from Bergen and one from Tromsø, in parallel with the committee with two from Oslo and one from Tromsø. The 'Hernes' reform should be revised by this committee (for higher education) as well as by the Power committee (for the power structure). The doctorate committee came to be a kind of rotation point for these. Later I discovered that the Norwegian Research Council had started a large network project just around the time when the three committees were established. This project (called 'The New') should be led by one of the members of the new Power committee, Siri Meyer. To my surprise, I even discovered from a press release on Internet that this project had some similar traits with the one I had described in the application which had disappeared in a mysterious manner in the Research Council the year before - to the extent that I even started to wonder whether there could be any connection. It almost looked as if my project had disappeared on the one side of Christmas only to reappear on the other side in the form of Meyer's project. Unfortunately, it was impossible to get any closer discription of this new project, so I was left with the suspicion. (More recent investigations have shown that the project 'The New' has a considerable root in the program description for the Research Council's "Program for cultural studies", so it may well be the case that that is where the 'motivation' comes from, but this does not attenuate the impression of a parallel). In addition, there were a host of other indications that things were interconnected (to a larger extent than the normal feeling that things just coincide). I simply couldn't help thinking of these three committees as three components in a sign, such that they could swap somewhat like the swapping in the committee evaluation in my dissertation, in particular when the evaluation swaps in parallel with my book.

It is possible that this is a normal state when waiting for having a dissertation accepted, but I felt that it came to be too much coincidences. However conscious or subconscious these parallels have been made, they introduce social roles to the committee in such a way that it can relate to social/political matters when it takes a stance relative to the dissertation. By accepting or rejecting the dissertation, it performs a social/political act which in itself has nothing to do with the dissertation, but which receives a function by virtue of this net of parallel committees. There is, for example, rector elections ahead, which may perhaps be seen as standing in a certain relationship to these things.

This is the only rational reason which I can find for the committee's rejection of the dissertation. It has, of course, nothing to do with scientific reasons, but since humans are social beings and not machines, it may have had some effect nevertheless.

What seems to be the most peculiar aspect of this, is that this rational reason exclusively resides in the collective consciousness and is given expression there through institutionalized power. If this collective consciousness (the Big Brother) suddenly came to be described in such a manner that everybody could understand it, then there is a certain chance that political power would be worm-eaten right away. It is therefore no wonder that there can be some institutionalized power giving resistance to it when I come with my dissertation. Correspondingly, it is possible that the committee, when it is about to represent the academic institution, sees it as its task to provide proof for the truth of the statement that I commit a punishable act when I describe society as a puppet theatre, alternatively, that there are blasphemic traits in the dissertation when it claims that there exists a submorphemic signification. Just to remove all doubts, the proof is to be made by one of the country's most prominent mathematicians (I here suppose that it is Fenstad who has written sections 2, 3 and 4.2.2), such that there will be not a single shadow of doubt. The proof is, as one can see from the points 2, 3 and 4.2.2, extremely complicated, and only the most intelligent readers can follow it in its finest details. The paragraphs of the law thereafter ensures that it will be impossible to accept the dissertation, since one cannot throw Fenstad in jail just because he has carried out the task he was assigned - to make such a proof. Then the dissertation will be isolated and the power still be in good shape - had it not been for the poetic solution of moving point 4a up to point 1 in the Criminal Law § 249.

This is, as mentioned, the only reasonable background I can see for the fact that the dissertation has been rejected in the way it was, and to the strange formal discussions in the evaluation, which makes it look like a proof for the idea that I have stepped on the toes of the power or religion by suggesting that its domain be computable. It is possible that such social/political resistance against computability in the social space can be the reason for the strange handling in the committee for the lecture in the theory of science as well.

In reality, the reason for these two rejections will be that I have arrived at results that imply real progress in science. The alternative would be that I did not present results for any real progress at all. Then of course it would have been easy to accept the dissertation or the lecture.

The problem is, consequently, that a breakthrough in the description of the new computer episteme with its collective consciousness entails a disturbance of the power, since the power to a certain extent has to be given up in order that the new domain can be liberated for a scientific description of it. A breakthrough in the description of the collective consciousness will therefore be felt politically. I suppose that is the reason for the resistance.

In any case, I conclude that the doctorate is not a scientific academic degree if it lets itself be governed by power.