(SLIDE 0)
(De-)Socializing Historiography of Linguistics

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> "63. Deliberations about plausibility in support of historiographical reconstructions draw on the systems of arguments and experience of the historiographical, not the historical subject.
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> 78. Every historiographical subject turns into a historical one for coming generations; every historiographical text does the same.
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> 79. Within the conditions of historiographical work, the historiographical subject is free in relation to history. There is no objective reconstruction, just as there is no objective recollection.
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> 80. Reflecting on the schemata of historiography has the aim of defining the liberty of the historiographical subject vis-à-vis history"
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> (Werner Hüllen 2005: 17-19)

0. Introduction

Writing the historiography[1] of the language sciences is, like speaking, acting or teaching, a social act(ivity). This is as such not surprising. As Kristina Rolin writes:

> As philosophers of science know, to claim that 'science is social practice' is not yet to say anything philosophically interesting. The challenge is to distinguish those social dimensions which contribute either positively or negatively to the epistemic success of science from those which have *no* epistemic significance. (Rolin 2002: 5)

The 'social' can be interpreted as the existing relations between scientists or as scientific communities as units of epistemic appraisal (Rolin 2002: 5) leading to scientific successful results or to irrelevant research. The 'social' is characterised by a dynamics of communication providing accepted and rejected epistemic evidence. Thus social dynamics supports the historiographical as a scientific practice.

Certain rules and restrictions are part of the *scientific game* which historiographers as scientists like playing. The game of historiography, without referring to any specific domain of research in one's own history writing can do without a specific object - whether it is

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[1] Some years ago we have distinguished historiography from history of linguistics as two different levels of scientific practice (cf. Noordegraaf/Vonk 1996: 146). The historical approach seems to be the more common, however, without reflection on the justification of the historical selection, description and analysis history lacks any epistemological framework. Is it, for instance, possible to give an unbiased description of Wilhelm von Humboldt's (1767-1835) concept of language as energeia. But then again, what epistemological basics determine a justified description and interpretation of this notion (let alone the selection of a concept that seems to be a marginal one in his studies on language but is emphasized by Humboldt-experts for many reasons, one of them being the role of this notion in Chomskyan generative linguistics).

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writing the history of physics, football or linguistics -, because of its *linguistic* and *scientific* peculiarity. The impact of this will be dealt with in later paragraphs.

This is perhaps strange but it is not meant to let ourselves be disturbed by historical objects but by historiographical reflections, by the language I use, the concepts and theories, empirical evidence or disconnected documents. What I want to do today is to discuss 'writing the history of …' or historiography as based upon or in relation to some recent work in the "sociology of scientific knowledge", a philosophical approach starting off with the "daily practice" in science and the "social construction of facts" (cf. Radder 1995: 141). Facts are not the foundation of possible science. In general, this mainly "British" movement – although the main players nowadays act in American academia - says that "the contents of scientific knowledge itself, the evaluation and the acceptance or rejection of this knowledge should be completely explained in sociological terms" (Radder 1995: 141). Related to historiography, one could ask in particular, if and how discussions in the historiography of linguistics relate to social power, priorities, changes or developments as such an how these generate historical and/or historiographical knowledge.

First of all some preliminary remarks. In 'writing the historiography of ….' we are trained to collect and analyze data, develop a keen eye for what is more or less relevant, and use a certain method to secure the results of our research, analysis and interpretation.[2] By publishing the results our 'ways' are accepted by peers in the field, working in more or less the same domain of research. This acceptance is quite important for most historiographers, even if it is not always clear what, under certain circumstances, the accepted methodological ways are and the criteria for being accepted. Is it important to adjust your writing to the criteria accepted in certain journals, most of the time meant to improve the article or book. We can, of course, distinguish several methodological sound ways to unearth historical data, events or persons but the case at hand is concerned with the ways in which consensus, discussion or even conflict arise over applied methods in historiographical research. Most of the time the data are accepted as such, unless translations are questionable or the data are corrupt - but then again: who decides? The problems rise in case of analyses and interpretations. What is a sound historiographical analysis or interpretation? And what criteria do we have to determine this?

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[2] To become a historian or historiographer of linguistics is a complex process. In her last book Vivien Law devotes some pages to the historiography of linguistics: what is the history of linguistics ("the discipline which investigates what people thought about language long before we were born. […] a branch of intellectual history, for it deals with the history of *ideas* – ideas about language – and not directly with language itself", Law 2003, 2), the background knowledge you need ("a grasp of the main historical and cultural developments in the period under study; a basic knowledge of at least one model of linguistics; an a command of the relevant languages", Law 2003: 4), the practice of historians of linguistics ("start asking *why*", Law 2003: 4), why study the history of linguistics (a sense of perspective; becoming aware of the transitions in the past and the present and realizing that there are more options and tendencies than we realize today , cf. Law 2003: 7f.). At the end of her book Law explicitly deals with "Becoming a historian of linguistics" (Law 2003: 276-283) in which she lists some duties historians and historiographers have concerning their profession: reading texts, primary and secondary sources, learning about research techniques and methodology, dealing with expectations and assumptions in reading texts – these determined what we see and read which is as such not a bad thing but an awareness of this is necessary. Ask yourself questions in relation to the book at hand and try to reduce the assumptions you have, although, as we will see, it is hardly possible to become a neutral observer and interpreter of historical and present day texts. Another aspect of historiography is an ethical one: what is the impact of your work, which sources do you use and select and which ones not (cf. the discussions on using Wikipedia as a source in historiographical work, although, as we all know there are some good and rich lemma's we could use, including the references given), what about the cultural (Western) background and the judgments we have of historiographical work based upon other sources or methods? And how reliable are interviews given by students, specialists, etc.? If I use texts of others, how do I deal with them: quote or paraphrase? And how accurate are my paraphrases? The last issues are, as a matter of course, part of scientific work as such and not typical for historiography.

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Let us turn to some different histori(ographi)cal approaches outlined by Konrad Koerner in 1974 in the first *Historiographia Linguistica*. Here a sort of qualification of historiographical work is given being ahead of the developments of historiography during the last thirty years or so – one should reconsider this qualification based upon the different possibilities of (re-)constructing history and historiography:

1. a goal that has been reached in a particular science. There seems to be no longer "any need for a revision of the methodology or the approach to the subject matter under analysis" (Koerner 1974: 1). In linguistics the historiographical works by Theoder Benfey (1809-1881), Rudolf von Raumer (1815-1876) or Holger Pedersen (1867-1953);

2. a "campaign opposing previously cherished views and still prevailing doctrines" (Koerner 1974: 2). These are works by a new generation of "historians" like Berthold Delbrück (1942-1922), Hermann Paul (1846-1921) or Noam Chomsky (b. 1928), proposing new theories and replacing older ones;

Both 1. and 2. are "propagandistic", offering new or overall pictures of the history of the field of linguistics. There are, however, two more types of history writing:

3. a fairly personal motivation for writing this history, like the one offered by Hans Arens (1911-2003). The history of linguistics is based upon a personal choice, which might also mean that this choice is based upon personal interests and expertise, leading to histories of phonetics, psychology of language or morphology;

4. "the presentation of our linguistic past as an activity founded on well-defined principles which can rival those of 'normal science' (Kuhn) itself with regard to soundness of method and rigour of application" (Koerner 1974: 4)

Another question might be whether this abstraction of the bulk of historiographical work up to now, done by historiographers of the language sciences, adequately represents the historiographical intentions of these authors let alone the output, and the different qualifications which are possible to qualify the historiographer's work. It seems also a matter of discussion if the non-existent object and the existing subjects of historiographical research are relevant at all in classifying a historiographical work as such.

Linguistics or the language sciences as such are considered to be a social activity, even a social or sociological discipline because of its object of investigation, language, functioning as social glue and enabling social life as such. In fact all four types of history writing provide an acceptable historiographical focus of the many different 'language sciences' we know and work in today, because they explicitly reflect the specific *contexts*[3] in which the linguistic work has been written. In fact, this contextual approach constitutes the type of history writing that is considered to be acceptable from its own, contextual principles. If we look at contemporary textbooks in the historiography of linguistics they pretend to reflect the state of the art, the general results of historiographical research but they also reflect tendencies in historiography and a personal selection of more or less important historiographical ideas and personalities. It is not just an "or-or"-situation we nowadays perceive.

An interesting footnote here is the qualification of the historiographer him- or herself. According to Koerner, a historiographer of linguistics is qualified if he/she is "well versed in matters linguistic" (Koerner 1974: 5), "'ought to know a good deal about the intellectual

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[3] The notion of 'context' or 'the contextual approach' seems an easy way out of the immanent perplexities, paradoxes or dilemma's in 'the' historiography of the language sciences. On the other hand, concepts, theories, notions and other historiographer's constructs are not themselves part of the linguistic work the historiographer assumes to be the 'object' of his research. They are related to my instruments, imaginations, hypotheses etc. to be projected on, and thus contextualizing his work. To be aware of this is not the most problematic aspect of the historiographer's work, but to consciously use it and make it a structural part of his work in fact is, as can be seen in contemporary discussions on the 'object' and ' subject' of historiography.

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history (embedded within the matrix of general history)'" (Yakov Malkiel (1914-1998); in: Koerner 1974: 5) and furthermore needs to be aware of "the discussion and exploration of the epistemology of this highly complex discipline-to-be and the subsequent foundation of a sound methodology of investigation and presentation" (Koerner 1974: 5f.). This normative qualification even goes further in the sense that the historiographer of linguistics shows a broad scope, depth of learning, an almost encyclopaedic knowledge, given the interdisciplinary nature of the historiographical activity.

Of course it would be an advantage if the historiographer could fulfil these requirements but meeting them would already cost the historiographer a lifetime when looking at the amount of publications, texts and presentations that appear world wide. So these are not fair or even realistic criteria. On the other hand, if I would focus my attention on a specific historical event in linguistics it would be an advantage if I have an overview of the work done in relation to this specific event, for instance the development of specific linguistic ideas in the 16th and 17th centuries related to the rationalism-empiricism controversies (cf. Robins 1976: 22) or the changes in the linguistic outlook initiated by Ferdinand de Saussure (1857-1913) and his analysis of "la langue" as a social fact (cf. Normand 2004) on the one hand *and* a system of values on the other.

1. The language of historiography and the languages of the object of historiography

Every historiographical account, although it focuses upon the past, has an inherent incompleteness which would inductively contradict Koerner's professional requirements. These cannot be met in historiographical practices because of the inherent linguistic dilemma. Even integrity stops by the summarizing activity that characterizes the generalizations made by integer historiographers (cf. Shapin 1995: 24).[4] Because historiography encompasses the present time or context and contemporary standards of writing the history of linguistics, contemporary methods, translations and techniques also determine the historical object/subject, the analysis and interpretation and thus its *changing* over time – cf. the types of history-writing activities listed above.[5] As R.H. Robins concluded in his 1976 article on 'continuities' and 'discontinuities' in the history of linguistics:

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[4] And for which historiographers do we collect and analyze our data? For the specialists or for the sake of a thorough discussion on the social aspects of our profession? The cultural heterogeneity will, as I will show later, make it impossible to discuss the specialist's detailed and most of the time "endlessly complicated stories" (Shapin 1995: 24). In his "story of the scientific revolution", Steven Shapin explicitly writes that the mathematical and physical interpretation of this historical phenomenon is the accepted and traditional way to look at it but it ignores the impact of other sciences on "the" scientific revolution. The historical changes and its impact on knowledge and the ways to acquire this knowledge is a selective and interpretative procedure followed by the historiographer who is convinced that scientific knowledge, its acquisition and development is a social activity.

[5] In an earlier contribution to linguistic historiography the authors distinguished between 1. introductory, 2. historical-critical, 3. historical-philological and 4. discipline-oriented histories of linguistics, mainly focused upon the Dutch situation. These types of history-writing mark different functions of historiography, most of the time meaning to put the more systematic accounts of for instance classical philology, the notion of the 'metaphor' or grammar (cf. Noordegraaf/Vonk 1996: 138-146). One of the sub questions in my paper is related to the functional and dysfunctional aspect of historiographical work. What is the impact of emphasizing one of the minor linguists in Germany or the Netherlands or one of the schoolmasters and his impact on pedagogical grammars in relation to the mainframe linguists and philosophers belonging to the canon of the history of linguistics – take for instance the existing textbooks by R.H. Robins (1923-2000) or Vivien Law (1954-2002): which historical objects are selected and for what reason? And moreover: what is the added value of a recapitulation of existing ideas?

> Either those writers and views that most anticipate contemporary attitudes are highlighted and marked by approbatory comment as showing 'the main lines of development to the present day'; or viewpoints manifestly at variance with those now approved and presented as errors progressively removed from the path of the proper study of the subject. (Robins 1976: 14)

Robins then summarizes the possible changes in our current scientific outlook facing different aspects of historiographical practices: "1. the logical extension of existing theory and of practice sanctioned by that theory, 2. the genesis of new concepts and methods in partial conflict with existing theory, as the result either of reflection or of trying to cope with recalcitrant observed facts, and 3. the effects of new aims, applications, or external motivations" (Robins 1976: 18). If we could trace these three dimensions of scientific change, the historiography of linguistics could throw off its alleged subjectivistic stance, although initially it is always the decision of the historiographer or a group of historiographers to focus on the scientific stance in the historiography of linguistics. An example of this is what Peter Schmitter (1943-2006) maintains in his review article on the contributions to the 1992 Leeuwarden meeting of the Studienkreis "Quo vadis, ars historica" (cf. Schmitter 1992: 235-239). In this article he picks up two "not harmless" tendencies in current historiography of linguistics: First, a lack in historiography of theoretical and methodological reflections with "fatal consequences" for concrete historiographical work, lost in detailed reconstructions of past fragments and loosing out of sight the real problems and their meaning for historiographical research – a kind of neo-historicism in which all data are equally relevant. Second, we come up with possibly unknown or unnoticed materials as to uncover "new" areas in historiographical research. We thus leave out the main characters and movements in the history of linguistics leading to the side stages of historiography. This is not a negative development. But, as Schmitter concludes, it is important to relate the findings and evaluate them within the totality of the historical development (cf. Schmitter 1992: 239). Historical consciousness will be clouded because of the irrelevant importance of different historical texts, persons and events.[6]

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[6] An important aspect in the historiography of linguistics is, according to Schmitter (1996) also the position of the historiographer him- or herself. In critically reviewing Koerner's collection of historiographical studies *Professing Linguistic Historiography* he contrasts his own position, according to Koerner based on epistemological interests and less on practical and methodological considerations, with Koerner's approach. Koerner considers his position to be more "'positivistic'" (cf. Schmitter 1996: 156), meaning that his main objectives are: "the debunking of myths and the establishment of a framework that should help us avoid the pitfalls of historical inquiry which is often motivated by interest other than trying to get the record straight." (In: Schmitter 1996: 156). Schmitter defended himself by maintaining that he also studied the history of linguistics intensively – do epistemological interests exclude detailed studies in the history of linguistics is what Schmitter seems to maintain. Furthermore, and this is more of a fundamental issue in historiography: what does it mean to be "admittedly 'positivistic', guided more by an inclination to have the facts speak for themselves […] than a tendency to offer tantalizing speculations." (Schmitter 1996: 156)? What kind of concept of 'positivism' is used and did Koerner make his position in historiography clear by using that notion without clarifying its metahistoriographical connotations? The empirical foundation of historiographical statements seems to be a condition sine qua non, however, the suggestion that facts can be independent of interpretation leads to naïve realism which is fatal for its scientific status: "Aber eine solche Einsicht in die theoretische und historische Bedingtheit des eigenen historiographischen Handelns setzt vielleicht in der Tat einen 'strong background' in philosophy voraus." (Schmitter 1996: 157). More controversies and positioning concerning "theoretical and methodological aspects of the historiography of linguistics" can be found in Schmitter/Van der Wal 1998 on 'metahistoriography'.

I get the impression that 'historiography' is more of a continental phenomenon, whereas history is an Anglo-Saxon one. But of course this does not imply that what historians and historiographers do can be distinguished geographically, although in philosophy the differences between continental and Anglo-Saxon philosophy have become accepted.

It is therefore important to construct a historiographical framework which is an international, socially constructed necessity in which, however, also the critical remarks by Schmitter are part of the discussion. This discussion becomes obscure if we do not know why the historical issues, problems, characters and movements are important to our profession. A critical survey of historiographical work is thus a recurrent part of what we do but needs to be discussed more extensively to find out what constitutes historiographical research.

2. Historiography as a science

Before taking up the sociological aspect, the scientific character of historiography needs some further elaboration. What 'science' exactly is, what it means for the historiographer and historiography, has not been a recurrent topic in our profession. Science could be considered as "knowledge derived from empirical facts" (Chalmers 1976: 23). Empirical facts make science possible but what exactly these facts are is most of the time not part of the discussion. But what are these empirical facts: what do we *perceive* in historical and historiographical work? In his 1981 textbook on medieval philosophy the historian of medieval philosophy, Lambertus Marie De Rijk (*1924), discusses the problem of historical facts in a for our purposes revealing way: as a prelude to the sociological discussion of science. Historical facts can be qualified as physical entities and mental entities. We experience physical entities as a series of change processes called "events" or "states of affairs". As a historical fact interpreted as physical events or states of affairs these are past events or states of affairs. The historical fact as a mental entity is a product of the mind (cf. de Rijk 1981: 44). Now, it is de Rijk's firm belief that objective historical knowledge, based upon physical entities, opposed to the result of the adaptation of the past by the historian (the historiographer) is without any sense without a criterion or standard according to which the historical fact as a physical entity can be considered to be represented by the historian's narrative. However, this criterion cannot be given, because speaking about the physical entity already changes it into a mental entity, the product of the subjective activity of the historian (historiographer). A historical object without knowing it, being a mental entity, is cognitively impossible. Speaking about the past includes a contradiction, as de Rijk maintains. In speaking about the past this only says something about the speaker not about the past. But to say that one's own vision is the "right one", the only way out of this paradox, is not done in historiographical practice – which must again mean that we cannot stick to historiographical results as objective historical facts but as images of our or my contemporary historiographical point view. What we must conclude here is that the historiographer has an impossible task to select the relevant (for whom?) data as to make his historical account as objective or reliable as possible. The linguistic arguments, according to de Rijk, should suffice to convince the "historiographical realist". Compare the following statements:

     (a) The past itself does exist
     (b) The past itself did exist
     (c) The past itself did and does exist
     (d) The past itself did, does and will exist. (De Rijk 1981: 45)

If (b) is true then (a), (c) and (d) are false; then historiography has no object. But if (a) is true then the past as such occurs nowhere or somewhere. In the first case it is scientifically meaningless to speak about it, in the second case we meet a contradiction, because the past is about past monuments or documents as poly-interpretable entities which as such cannot be the past itself. This perplexity as such suffices to show how problematic historiographical accounts or utterances are and what their truth value is. Perhaps it is better not to talk about "truth values" in this case but about "social values". I will come back to this later

3. A concept of science

What makes historiography a scientific practice? Another description of a science is given by Richard P. Feynman. In "What is Science" (cf. Feynman 1999: 171-188) he describes science as follows:

> There was on this planet an evolution of life to the stage that there were evolved animals, which are intelligent. I don't mean just human beings, but animals which play and which can learn something from experience (like cats). But at this stage each animal would have to learn from its own experience. They gradually develop, until some animal could learn from experience more rapidly and could even learn from another's experience by watching, or one could show the other, or he saw what the other one did. So there came a possibility that all might learn it, but the transmission was inefficient and they would die, and maybe the one who learned it died, too, before he could pass it on to the others.
> The question is, is it possible to learn more rapidly what somebody learned from some accident than the rate at which the thing is being forgotten, either because of bad memory or because of the death of the learner or inventors? (Feynman 1999: 184)

Science is accumulated knowledge passable from one generation to another. But the issue is that "mistaken ideas" can be passed as well, not being necessarily profitable for "the race". The consequence of this is that contrary to a more positivistic look on science it is more about finding out what the "mistaken ideas" exactly are and how they became part of our transmitted knowledge over the generations. To Feynman it is more important to try to find out from experience what the case is, "rather than trusting the experience of the past in the form of which it is passed down" (Feynman 1999: 185). (SLIDE 10) But then again, what is the use of the accumulated knowledge handed over from generation to generation, written down in all sorts of text editions, extensive and detailed studies of the past? If science really is "the result of the discovery that it is worthwhile rechecking by new direct experience, and not necessarily trusting the race experience from the past" (Feynman 1999: 185), then our core value in historiography as a scientific enterprise is *to be critical on whatever has come down to us and even that*. And furthermore, what are the exact criteria to distinguish between a good, acceptable and unbiased piece of work as opposed to what is erroneous or insufficient or even false? And what social, political or even economic factors and values do play a decisive role in historiographical work

4. A sociology of historiography

Now, let's get back to the initial promise to focus upon a sociological approach towards the science of historiography. To see historiography as a science is perhaps somewhat problematic: What do we exactly mean by a scientific approach? Science is mainly related to giving a *true* account of reality, of natural laws and natural behaviour but historiography, even of the natural sciences, doesn't have this pretension to give a true account of a historical reality or event. This could imply, however, that the function of historical accounts changes over time. Although we encounter for instance problems of word order, case systems in different languages, similarities between different languages or influences between different linguists (consciously or unconsciously) this topic is determined by the interest, contemporary research shows for earlier lines of thought. Although an a-historical account would mean that we will definitely repeat earlier flaws and mistakes in linguistic work. Perhaps, then, we should not use the word 'progress' in this particular case but assume that science 'develops'

in line with social developments and demands concerning linguistic issues. It seems to be important for the self-awareness of the historiographer to take this into account, when starting his or her research: Why is this aspect of linguistics or language thought important or relevant to historiography of linguistics? And how will it contribute to the development of historical descriptions, analyses and evaluations? This focus already takes up the assumption that any historical reality (from our point of view) as such is in any case a (re-)construction. Scientific historiographical results are in principle falsifiable, part of a particular paradigm or research programme and they are testable. This intrinsic character is completed by an external view on science in which the social context of research is taken into account. But what does this external view add to the internal developmental characteristic of scientific research? To be more specific: Can I set my own standards without regarding the standards set by others which might be useful to follow to get my articles published or papers accepted for conferences? What would be an out-of-the-box presentation of Wilhelm von Humboldt or Karl Bühler (1879-1963) look like. What does my reading of his work, my publication of hitherto unpublished manuscripts, contribute to the historiography of linguistics? Do I aim at followers or that we can change current standards in historiography? Or is it wiser to adapt my research and research strategies to the common practice. It is sometimes hard to see your effort "sub specie historiographiae" but what makes linguists and philosophers of language like von Humboldt still rewarding as an object of historiographical research? Perhaps this sociological approach might help us out. It is important to notice in advance that the sociological approach fundamentally infiltrates all dimensions of science, as well as the external (organisational structures, political, social and economic influences on science) as the internal (conceptual, methodological or proofs) dimension. The sociological historian (historiographer) will "reproduce the acquiring and saving of knowledge as social processes" (Shapin 1996; 2005: 22). It means that an internal, conceptual or methodological approach as such doesn't suffice to avoid any relativist thinking in historiography.

5. Sociology and historiography: an example

Is there a link between historiography and sociological research? Historiography is writing the history of… and this discipline has several more or less accepted conventions (rules and restrictions). Now it is quite relevant to find out whether these conventions are externally or socially determined in all cases, as the strong programme of the sociology of scientific knowledge maintains.[7] Bloor (1976) and Shapin/Schaffer (1985) explicitly maintain that for instance early modern natural philosophers, like Robert Boyle (1627-1691) or Thomas Hobbes (1588-1679), in their work represent the epistemological assumption that experiments were subject to the social and political concerns in the English Civil War and the Restoration. The attribution of *value* to experiments was considered differently by Boyle and Hobbes, based upon a different perception of the status of experiments. Boyle wanted a mechanical defence of knowledge based upon a broad as possible support, whereas Hobbes saw experiments as artificial and unreliable not leading to a true insight into real nature at all. So, physical reality as such and creating broadly accepted knowledge are irrelevant in Hobbes's view on scientific knowledge but they are part of creating and maintaining a social order:

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[7] The weak version of the sociology of scientific knowledge programme maintains that only erroneous beliefs can be reduced to social explanations:

> The strong programme is a reaction against previous sociologies of science, which restricted the application of sociology to 'failed' or 'false' theories, such as phrenology. Failed theories would be explained by citing the researchers' biases, such as covert political or economic interest. Sociology would be only marginally relevant to successful theories, which succeeded because they had revealed a true fact of nature. ("Strong programme")

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> Because their decisions were made in a community of fellow practitioners, Boyle claimed that modest experimenters also avoided the philosophical and political dogmatism of one sole authority or Leviathan, much as the English Parliament in concert with King Charles II avoided absolutist government. Hobbes on the other hand "proposed that philosophers should have masters who enforced peace among them and laid down the principles of their activity." Hobbes viewed decision by committee, particularly on a subject as significant as natural philosophy, as dangerous, and likely to produce the same type of civil and religious strife that existed in the English Civil War. (Roos 2000)

The social embedding and the political interests accompanying scientific research are, from this point of view, decisive in understanding the positions of Boyle and Hobbes respectively. Philosophical knowledge, as Boyle maintained, was based upon sound experiments revealing true matters of fact Hobbes disputed this and defended the position that experiments could never yield the certainty to be expected in philosophical knowledge (cf. Shapin/Schaffer 1985: 22) and thus maintain the disorder in natural philosophy that existed by the time of Boyle's experiments in the 1660s and 1670s:

> First, Hobbes was concerned to defend his own standing as a major natural philosopher and to defend the natural philosophical schema he had constructed and refined in the 1640s and 1650s. Second, Hobbes had developed that system as uniquely suited to securing order and achieving the proper goals of philosophy. Any other project for natural philosophy endangered order. Third, there was the heightened sensibility to the practical problem of dissension that was displayed by all English intellectuals during the making of the Restoration settlement. (Shapin/Schaffer 1985: 81)

An interesting conclusion in this dissension then is the problem of reaching any consensus over natural order by two important players in the field of natural philosophy in the 17th century. The defence of one's own "garden" seems to be more important than the "matters of fact" as results of proper, experimental scientific research. In the sociological approach of historical events it is not about taking sides or find out who's right or wrong, whether the results of Boyle's experiments are true or false, but to describe impartially the socio-political and cultural background leading to debates like the one outlined above. Thus, Shapin & Schaffer come up with the notion of public and private space, where scientific practice gradually moves from the public debate towards the developing laboratories which are less accessible to a wider public, as are, most of the time, the results (publications):

> As a practical matter, Hobbes could hardly deny that the experimentalists [Boyle c.s. – fv] had established a community with some politically important characteristics: a community whose members endeavoured to avoid metaphysical talk and causal inquiry, and which displayed many of the attributes of internal peace. But this community was not a society of *philosophers*. In abandoning the philosophical quest, such a group was contributing to civil disorder. It was the philosopher's task to secure public peace. (Shapin/Schaffer 1985: 337)

One can see immediately, how dangerous it is to ignore the civil (social and political) impact of one's scientific work, especially outside the domain of the socio-political. We can find

many examples in the history of science. But let us go back to the historiographical profession and responsibilities.

7. Sociological aspects of historiography

The sociological or socio-cultural approach can be considered as a reductionist way to account for historical data – a point of criticism passed on many sociological approaches towards science (cf. Lokhivi 2003) – although historiographical knowledge is an outstanding example of scientific knowledge which is not considered to be "true and justified" but as a "natural phenomenon, […], whatever people take to be knowledge" (Bloor 1976: 5). This is not to say that knowledge is unimportant for sociologists of scientific knowledge but it has a different status: "In particular the sociologist will be concerned with beliefs which are taken for granted or institutionalized, or invested with authority by groups of people" (Bloor 1976: 5). In fact, this kind of knowledge is not about what I or any other historiographer think of as justified, settled or true beliefs but as institutionalized or collectively shared knowledge – for instance the kind of knowledge we find in many handbooks, but is still preliminary in the way it is codified. To find out where these shared or codified ideas come from, how they emerged or became part of a tradition or culture, is essential to this different approach towards the past and this constitutes another value of historiographical work. But it is not just the historical object, historical events, which are treated and discussed this way but also the self-awareness of historiographers of whatever discipline – thus de-disciplinizing "the" own profession and becoming aware of the divergence of historiographical work and thus creating the intersection of ideas, concepts and cultures in one's own "discipline" (cf. Johansson 2004). Meetings like these conferences serve as excellent opportunities to create intersections in our thinking, thus becoming aware of the importance of for instance sociological values motivating scientists to continue their work.

[In general one could say that groups of historiographers share knowledge of historical objects and events (states of affairs). This presupposes that there is a particular relationship between historiographers: they share an academic background (in whatever discipline) , access to text- and handbooks, periodicals, an institutional context in which they do their work – most of the time not historiographical -, stages to perform and develop new ideas, concepts and meet new colleagues. But their core business is historiographical *knowledge*, in our case considered as the subject of group processes. The issue now is that this kind of knowledge is always "mediated" by social circumstances: status, context, power or control, diversity, values, culture or exclusion (to name some basic notions in sociology). To deny this would mean that we deny the common practice in historiography that specific texts, data, events, persons or objects are criticized, accepted, rejected, modified for more or less acceptable reasons. When we make historiographical knowledge part of a sociological enterprise we methodologically have to take into account the four following principles in explaining historical data, selections, analyses and evaluations:
        (a) the causality of the explanation
        (b) the impartiality of the explanation
        (c) the symmetry of alternative views under investigation
        (d) the reflexivity of the principles. (Bloor 1976: 7)
These principles seem rather strange at first sight but they represent the core values of scientific work as such, seen from a sociological point of view. Thus causality has to do with the reasons or conditions that bring about a certain belief; whether scientific utterances are true or false is not an issue but how they are formed, under what social conditions; the style of explanations is the same for false and true beliefs; and explanations are s well applicable to the object of research as to the discipline, scientific historiography, as well. We need general

explanations to justify our beliefs and the four maxims so to say seem to justify the sociology of scientific knowledge that is "what is taken to be knowledge" (Bloor 1976: 5):

> "The sociology of knowledge focuses on the distribution of belief and the various factors which influence it. For example: how is knowledge transmitted; how stable is it; what processes go into its creation and maintenance; how is it organised and categorised into different disciplines or spheres?" (Bloor 1976: 5)

The principles above and the sociological approach have become part of a meta-scientific discussion on the all embracing influence of sociological factors in the development and distribution of scientific knowledge, whether this knowledge is part of the empirical sciences, based upon observations and experiments, or part of the social sciences and humanities.

Revealing in this methodological framework is that it could serve s a metahistoriographical theory, since it has suggested a way for "overcoming the notorious "Whig history" – a presentist methodology for studying the history of science which imposes modern evaluative standards upon past science" (Lokhivi 2003: 2). But how? And what is the added value of avoiding Whig history if we base our historiographical objectives upon sociological ground? First of all it is a descriptive approach instead of a normative one. Second, competing methodological approaches towards historical events, objects etc. have their own principles which help to cover historical problems in their own ways without taking sides:

> It does not mean that a sociologist comes to approve or disapprove the scientist's choices or decisions. Neither does it mean that a sociologist should necessarily favour a kind of truth relativism – rather a sociologist takes an indifferent position towards the truth-question and focuses on the social relations. (Lokhivi 2003: 3)

What then are the *sociological* conditions making a theory rationally acceptable? Important for Bloor in his particular sociological programme is the empirical underpinning of a scientific practice. Thus, from a logical point of view a historical event can only be an object of scientific knowledge if and only if it has empirical support – although in many sciences like mathematics or chemistry the methodological and theoretical principles do not show this empirical support and are not acceptable candidates for a SSK approach.[8]]

If we return to the historiography of linguistics or the language sciences what kind of empirical evidence or support is needed to yield scientific knowledge? Most of the time it is observational evidence which makes perceptions true in an epistemological way. It becomes true if I adequately describe and analyze this piece of evidence. But what can function as a warrant of this piece of evidence? A special training (like observing and analyzing X-Ray photographs) or instruments to find and formulate it as an empirical proof? And is it a training as a linguist (or of special branches) or as a historian or even as a historiographer or a combination of both? Or even a broader training encompassing philosophy, sociology and/or psychology and their respective histories? And are there specific qualifications needed to

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[8] Cf. Peter Meyer (1999) who discusses the problems of defining science in terms of "the collective activity". The lack of a "field of empirical phenomena" which would make mathematical hypothesis true or false. The validity of proofs does not explicitly presuppose the empirical proof, although the proofs in mathematical deductions and inductions might serve as necessary conditions to make hypotheses true or false and serve as an analogous empirical support: "When a proof (or a counter-example) is put forward it normally does not take long before other mathematicians to decide whether it is valid proof (or a true counter-example)". The social construction of validity thus seems to be even in mathematics an important scientific and methodological prerequisite.

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"practice" as a historiographer of linguistics? The qualifications mentioned by Koerner some 35 year ago? Or do these qualifications change over time? Koerner (1974: 5) already picked up these aspects and qualifications related to our profession but I doubt whether we really paid attention to these remarks and solved the problem of qualifying for a historiographer in linguistics? In relation to the periodical, *Historiographia Linguistica*, Koerner considered "the discussion and exploration of the epistemology of this highly complex discipline-to-be [different from the trials and errors before 1974 - fv] and the subsequent foundation of a sound methodology of investigation and interpretation," to be essential to the historiography of linguistics. This discussion and exploration of the epistemology or of the theoretical and methodological background of historiographical ambitions can be found "ansatzweise" in this and other periodicals but it is still unclear how historiography, mirroring the scientific discussions and wars over the past 30 years or so, has developed into a scientific practice and has constituted an interdisciplinary and epistemologically adequate way of unearthing the past in contemporary descriptions and analyses.

Thus historiography becomes a kind of Golem, a creation of our "art and craft" (Collins/Pinch 1993: 2) and generates within the social settings in which this creation, or perhaps more adequate: creature, operates as many controversies and mistakes as it pretends to generate EMETH, truth. From this point of view the Golem-approach is telling:

> To show what Golem science is, we are going to do something almost unheard of; we are going to display science, with as little reflection on scientific methods as we can muster. We are simply [!] going to describe episodes of science, some well known, and some not so well known. We are going to say what happened. […]. The results will be surprising. The shock comes because the idea of science is so enmeshed in philosophical analyses, in myths, in theories, in hagiography, in smugness, in heroism, in superstition, in fear, and, most important, in perfect hindsight, that what actually happens has never been told outside of a small circle. (Collins/Pinch 1993: 2)

This is perhaps the implicit idea of many historiographers: they want to tell their definitive story of what happened in the past, without realizing explicitly what is in fact always part of the narrative as an interpretation of historiographical utterances in a wider context of meaning (cf. De Wilde 1995: 373). This cultural or socio-political background is unavoidable if one wants to adequately link historical events, concepts, etc. to their context of meaning serving as a piece of evidence for the historiographer. Thus the idea behind 'Golem science' is an appealing one but in fact a result of the discrepancy between the historical events as such and the selection of, reflection upon and interpretation of these events. What the authors conclude is perhaps the startling conclusion of 'Golem science':

> […], our conclusion is that human 'error' goes right to the heart of science, because the heart is made of human activity [represented in Collins's and Pinch's 'episodes of science' - fv]. When things go wrong, it is not because human error could have been avoided but because things will always go wrong in human enterprise. One cannot ask of scientists and technologists that they be no longer human, yet only mythical automata – quite unlike the constituents of a golem – could deliver the sort of certainty that scientists have led us to expects from them. (Collins/Pinch 1995: 140)

However, this is not a 'new' idea: In the canonical writings in theories of science like Popper's or Kuhn's, think of falsification and anomalies, we find this 'error'-component as well as we saw in Feynman's concept of science. But in this particular case the 'individuality' of every single science, as well of the natural sciences as the cultural sciences, seems to be the

core element of the sociology of science movement. We cannot discover universal laws, working irrespective of the context of human activity or nature, but satisfy ourselves with preliminary results based upon error elimination, without knowing if there will be a final truth to be discovered.[9]

Thus, the SSK-movement is more interested in the workings of science and not in scientific knowledge as such, although this knowledge is part of the object of SSK. SSK stresses the "cultural basis of scientific knowledge" (Labinger/Collins (2001: 4), so an idea of the quality of scientific knowledge is part of it. Not realism, rationalism and objectivity but relativism, constructivism and subjectivism are key concepts in the sociological approach towards science: It focuses on: "the role of human factors in science and how scientific knowledge is *contingent* on and *constructed* by the operation of these factors – the social character of scientific institutions, the culture in which scientific investigation takes place, the language used to express scientific findings, etc." (Labinger/Collins 2001: 5). The natural world or the past in our case, its determination and codification is less important if not important at all. Although it is always relevant to ask, where do we take our empirical material from, does it exist and cannot be overlooked in historiography – imagine the original texts by authors, linguistic phenomena dealt with and their presentation in textbooks over the centuries, letters or social-political factors.

All in all, this means that we see a shifting away from the object or contents of the sciences towards its production and maintenance based upon different principles![10] In SSK

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[9] A good illustration of this is the case of the "Science wars", following the article "Transgressing the Boundaries: Towards a Transformative Hermeneutics of Quantum Gravity", published in 1996 in *Social Text* by Alan Sokal, a physicist from New York University:

> Sokal's piece appeared to be an analysis of certain aspects of physics and mathematics, conducted from the viewpoint of what has come to be known as postmodernism. It seemed to argue that language, politics and interests rather than objective reality, determine the nature of scientific knowledge, and it was written in the characteristic style of postmodernist discussions of science – pedantic bibliographic referencing, extravagant praise for an elite group of thinkers, and an elaborate language which some might call 'jargon'. (Labinger/Collins 2001: 1)[9]

Sokal immediately qualified the article as a hoax or parody but the serious aspect of this article was not its object or contents but if "the cultural studies community, and the journal's editors in particular, could tell the difference between serious scholarship and deliberate nonsense" (ibid.). In our profession, the historiography of the language sciences, it seems to be quite obvious that we need to be aware of the socially or community oriented methodological and metahistoriographical quarrels, differences in approach or the adequate interpretation of sources rather than focussing upon the object of our research – this is quite a statement but I have the impression that based upon many editorials in periodicals and textbooks this is not far beyond my relative truth.

[10] There is a correlation between the "empirical programme of relativism" (EPOR) and the "social constructions of facts and artefacts" (SCOT) as methodological premises of SSK (sociology of scientific knowledge). What they share is the so-called "interpretative flexibility" of scientific findings. In the case of technological artefacts these are culturally constructed and interpreted. Their design shows the "possible ways" in which artefacts can be constructed. The specific construction has been designed because of the cultural facility of just this construction. It fits in into the problem-solving activities of particular social groups who are responsible of the construction of this artefact. (cf. Bijker et al. 1987: 40)

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terms there is a shift in focus from the natural or historical world to the social world in which the interpretative flexibility is a condition for disclaiming any "truth" in scientific research – although stabilized findings often lead to a consensus or a "limitation of flexible interpretativity" (Bijker et al 1987: 27). Furthermore, it is relevant within the sociology of scientific knowledge to link the social mechanisms leading to such limitations or flexibilities to the social-cultural background. Science is a product of social life. Thus values like disagreeing with alternative views and attempts to monopolize one's own scientific findings are common traits of scientific practice. Another value, following from this outlook is the problem of what is "generally believed" in a scientific community (cf. Collins/Pinch 1993: 155): scientific knowledge is not a result but a process including errors and changes of perspective, based for instance upon disagreeing with one's scientific conclusions. Rejection and acceptance are important scientific and sociological factors in scientific development:

> The thought is rejected that 'the facts' could determine the foundation or the possibility of knowledge. Facts are the result of negotiations between scientists. The course of these social processes determines whether experimental data will or will not be rendered the status of a 'fact'. Therefore, facts are social constructions and nothing more than that. (Translation from Dutch FV - Radder 1995: 141)

Empirical and experimental studies are needed to support the theoretical claims, especially in the natural sciences but it is also important in cultural studies. The natural and the historical world, one might add, "'in no way constrains what is believed to be'" (in: Radder 1995: 143). The sociologically determined discourse (based upon interpretative flexibility) will lead to rules and criteria (arguments) for acknowledging epistemological claims.[11] Important here are the many case studies leading to a better understanding of what scientific practice in its broadest meaning contribute to its development.

8. A reaction on postmodern historiography

Contrary to the belief that sociology and therefore relativistic accounts of the past contribute more to historical knowledge and its possibilities than the traditional conviction that the past can be represented in a scientific true and adequate way, in 1999 Chris Lorenz published a defence of a more traditional objectivity- and truth-based historiography against what he calls "forms of postmodern scepticism" (Lorenz 1999: 564). Against the disclaimer of truth and objectivity in historiography postmodernists claim that because of the linguistic structure of historical accounts they can never be "absolute" or "universal". Historical reality is, according to historians like Hayden White (*1928) or Michel Foucault (1926-1984), not represented in language but consists in the ways in which historiographers represent their perception and interpretation of historical "knowledge". Thus postmodern relativism pops up, meaning that we cannot find epistemological objectivity in historiographical research because of its linguistic form, fictionalizing historical facts (cf. Lorenz 1999: 565). Lorenz now qualifies all language as metaphorical, fictionalizing reality, therefore it becomes an empty concept: "we are speaking metaphorically all the time" (Lorenz 1999: 568), meaning that the correspondence between language and the represented historical reality is not an issue in scientific work – and not a counter-argument against postmodern or "new science" approaches. Lorenz is right in claiming that relativism as such is an absolute pre-requisite in

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[11] A problem might be the under-determination of the epistemological claims of scientists propagating the sociology of scientific knowledge. The empirical foundations are irrelevant to these claims and thus might create scientific realm of myths an fantasies. On the other hand, epistemological beliefs never transcend the linguistic construction or structure that constitutes the ways in which knowledge is codified.

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sociological, psychological or postmodern approaches towards reality. But I guess that no representative of these approaches would deny that. We would accept "valid" counter-arguments. Then it is not so much the object of historiographical practices but the validity of relationships constituting the "ideal" discussion in the field. These endorse the validity and truth-claims of scientific work although they have a different meaning in the new science discourse. Contrary to what Lorenz maintains notions like "myth", "fantasy" or "fiction" are not "empty categories" but relate to another "objectivity" or "truth" determining the historiographical discourse – we could relate them to sociological categories like "meaning" or "acceptance" but also to decisions made by historiographers to opt for one interpretation rather than another based upon the evidence at hand. Thus creating internally (not externally) true linguistic representations of empirical evidence supporting my specific representations. It is hard to deny that postmodernism excludes any truth-claims in advance but these claims do not represent a historical past, present or future but they relate to the conceptual focus of the scientist causing socio-political decisions to publish, present or criticize.

The same problems Lorenz encounters in discussing the concept of "objectivity" in historiography. The cultural background of scientific conceptions and the socially and politically determined production of knowledge denounce the objectivist's claims on universal or absolute (historical) truths:[12]

> […] knowledge, and thus truth, can never claim to be universally valid, because both knowledge and truth are always connected to particular circumstances or interests of some sort, such as culture, class or gender. (Lorenz 1999: 570)

Universal truths cover particular interests and these are not sufficiently taken into account in objectivistic historiography: there is an objectively accessible past underlying historiographical statements. Social diversity, linguistic and cultural constructions on the one hand and systems of power, repression and exclusion on the other count as warrants of the relativistic or postmodern position in historiography. But how then, Lorenz argues, can we understand at all different cultures or transcend our relativistic position and what is the use of it? It doesn't only relate to contemporary scientific work but also to historical diversity which is perhaps even harder to empirically verify. We find several relevant observations in Lorentz's article indicating the problems of the sociology of scientific knowledge. First, it is the self-elected cultural prison with its own boundaries and the problem of transcending them. For scientific development this prison is hard to defend if one sees on the other hand no conceptual fixation in historical reality, even more when postmodernists maintain that the content of concepts are not fixed but changing in their contexts of use. What then are the sources for this change? Probably not the socio-cultural backgrounds. Perspectivism, partiality and revisibility are central notions in scientific research and they presuppose no limitations by a socio-cultural reduction. Second, if we presuppose that there is a struggle, based upon excluding systems of power as Michel Foucault defends, it is hard to construct any truth at all because every power system has its own and it is not excluded in advance that these systems change and thus have to revise its truth which seems to be not a conscious or rational decision but part of social and political changes. Power systems as such would not be able to adequately explain historical knowledge:

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[12] Cf. the problem of causal explanations in the human sciences. A causal reconstruction of human activities, especially those in the past, does not apply. They are hard to prove empirically and the activities and intentions are not logically separable which seem to be necessary conditions for causal explanations (cf. De Wilde 1995: 367).

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> If you remove freedom as the opposite of power, power simply ends up with no 'other'. If we cannot discriminate conceptually between making love and making war – and just state that all human relations are relations of power – then what can we say, except that in Foucault's night of power all cats are indiscriminately grey. (Lorenz 1999: 577)

Thus you would rule out the historiographer's decision to decide what is relevant and irrelevant in his or her historiographical account. On the other hand, the notions of power, exclusion, truth regime etc. can be used to distinguish for instance Foucault's approach from Lorentz's or other historians' methods. This is also the case regarding sociological notions constituting the framework for the analysis of the historiographer's behaviour and activities. These form as it were an objective basis for metahistoriographical analyses and thus for understanding historiographical work. There is a third observation in Lorenz's text related to this problem referring to the truth-claims of linguistic representations of historical events. Historiographers claim that there is "somehow" a correspondence between both. Every truth-claim that corresponds with "specific states of affairs" (Lorenz 1999: 578) exists as a reality. Therefore, these claims "always claim a universal validity" (ibid.). The simple reason for this is that these claims have an empirical truth-basis. The universalisation of the truth-claim is based upon the linguistic meaning of truth: it is not related to a truth-regime, a socio-cultural context but implicitly universalisable. This is not to say that historiographical knowledge is important politically and socially. But this is not part of the epistemological discussion of truth-claims. It means that past events serve (instrumentally) in social and political contexts, taking up historical events as important aspects of historical traditions or influences for instance. But this does not legitimize the historiographical truth-claims as such. Perhaps, the last position can be considered as a transcendental condition of historiographical research as such, however, it will not be independent of choices made by the historiographer to deal with his historical reality - take for instance the conscious choice to speak about development instead of progress in linguistics.

9. Some concluding remarks

It was my intention to reconsider the notions of truth and objectivity in today's historiographical discourse and to connect them to alternative epistemological and scientific strands in historiography. First of all to see what their added value in historiographical thinking could be, second, to reflect on the metahistoriographical dimensions of our profession, mainly as attempts to 're-construct' the historiography of the past based not upon 'individual decisions' but on a collective effort by a community of practicing historiographers who do not just collect, select and analyze historical evidence but collectively formulate the conditions and the social and rhetorical desires or values underlying their attempts to create a scientific field of research, dealing with as many errors as possible, because we are involved in human activities which cannot do without these errors and their validation.

Another outcome of my paper is the impact historiographical research has. Why do linguists (most of the time) decide to focus on the history of their expertise? What is the added value? Selecting some relevant historical data and events is not enough to explain the historiographical practice. Then, the impact and the scientific character of my work is not as much accepted is fundamental work in linguistics or one of its domains which seem to have some impact upon the communis opinio. We hear a lot about dialects, English as a lingua franca, social functions of language, language acquisition or foreign language learning and teaching but hardly any historiographer of linguistics makes his work accessible to wider audience. Another, not less important question: is this my responsibility as an expert in 16th-

century vernaculars or 18th-century grammars in the Netherlands? And a last one: am I responsible for a scientific justification of my historiographical work? Do I know what the scientific standards in the historiography of linguistics are and do I act accordingly?

A last remark concerns the values in "the social epistemology of scientific inquiry". In my paper I frequently used the term "value", based upon among others' Heinrich Rickert's study in concept formation in the natural and cultural sciences (cf. Zijderveld 2005: Chapter 4) and some recent publications of female scientists on "relevant factors" and values (cf. Rolin 2002; Koertge 2005) in scientific inquiry and their impact on society. Relevant for my research would be to find out why certain socially embedded values do apply to our scientific work as historiographers and are acknowledged as such and some values, leading to a different approach and methodology, are ruled out, even excluded in an era in which diversity for instance seems economically quite important. So, what is the impact of a feminist approach on historiography? What, if gender would be conceded a place in historiographical research? Will it contribute to a different practice in historiography? Or is historiography a gender neutral activity? What, if empirical evidence or trust, dynamics of communication or bias or non cognitive values become more or less important in historiographical work? Would that be of any importance to our research practice? Perhaps these questions are of a rhetorical kind, perhaps they are not.

References

Arens, Hans (1955): *Sprachwissenschaft. Der Gang ihrer Entwicklung von der Antike bis zur Gegenwart*. Freiburg, München: Karl Alber.

Bijker, Wiebe, Thomas P. Hughes, Trevor Pinch eds (1987): *The Social Construction of Technological Systems. New Directions in the Sociology and History of Technology*. Cambridge Mass, London: The MIT Press.

Bloor, David (1976; 1991[2]): *Knowledge and Social Imagery*. Chicago, London: The University of Chicago Press.

Chalmers, Alan (1976, 2003[11]): *Wat heet wetenschap?* Amsterdam: Boom.

Collins, Harry, Trevor Pinch (1993; 2005[8]): *The Golem: What You Should Know about Science*. Cambridge etc.: Cambridge University Press.

Feynman, Richard P. (1999): *The pleasure of finding things out*. London: Penguin.

Fitzi, Gregor (2008): Max Weber. Frankfurt/M., New York: Campus

Gergen, Kenneth J. (2001): "Psychological Science in a Postmodern Context". *The American Psychologist* 56, 803-813. Internet: <http://www.swarthmore.edu/SoSci/kgergen1/web/...>, consulted 14 July 2008

Hüllen, Werner (2005): „On the method of Linguistic Historiography. A Treatise." In: Nicola McLelland, Andrew R. Linn eds.: *Flores Grammaticae. Essays in Memory of Vivien Law*. Münster: Nodus Publikationen [in German: 1996: "Schemata der Historiographie. Ein Traktat". *BGS* 6.1, 113-125].

Johannsson, Frans (2004): *The Medici Effect. Breakthrough Insights at the Intersection of Ideas, Concepts & Cultures*. Boston: Harvard Business School Press.

Klukhuhn, André (1995): "Hoe postmodern valt een steen? Over wetenschappelijke zekerheid en het postmoderne relativisme". *Studium generale. Reeks UU*, 139-158.

Koerner, E.F.K. (1974): "Purpose and scope of *Historiographia Linguistica*". *HL* I/1, 1-10.

Koertge, Noretta (2005): "A Bouquet of Scientific Values". In: -- ed.: *Scientific Values and Civic Virtues*. Oxford etc.: Oxford Uniersity Press. Internet: <www.indina.edu/~koertge/rbouquet.pdf>, consulted 22 July 2008.

Kragh, Helge (1987): *An Introduction to the Historiography of Science*. Cambridge etc.: Cambridge University Press.

Labinger, Jay, Harry Collins eds (2001): *The One Culture? A Conversation about Science*. Chicago, London: The University of Chicago Press.

Law, Vivien (2003): *The History of Linguistics in Europe. From Plato to 1600*. Cambridge etc.: Cambridge University Press.

Lokhivi, Endla (2003): "On philosophers' criticism of the sociology of scientific knowledge". Internet: <http://www.helsinki.fi/filosofia/tutkijaseminaari/lokhivi.pdf>, consulted 14 July 2008.

Lorenz, Chris (1999): "'You got your history I got mine'. Some reflections on truth and objectivity in history". *ÖZG* 10, 563-584.

Meyer, Peter (1999): "An Essay in the Philosophy of Social Science". Internet: <http://www.hermetic.ch/compsci/pss1.htm>, consulted 23 July 2008.

Noordegraaf, Jan, Frank Vonk (1996): "'An Anatomy of Melancholy'. Writing the History of Linguistics in the Netherlands, 1900-1990". In: Herbert E. Brekle, Edeltraud Dobnig-Jülch, Helmut Weiß eds.: *A Science in the Making. The Regensburg Symposia on European Linguistic Historiography*. Münster: Nodus Publikationen, 137-163.

Norman, Claudine (2004): "System, arbitrariness, value". In: Carol sanders ed.: *The Cambridge Companion to Saussure*. Cambridge etc.: Cambridge University Press, 88-104.

Quine, Willard Van Orman (1960; 1964): *Word and Object*. Cambridge/Mass.: MIT Press.

Radder, H. (1995): "Het probleem van het wetenschappelijk realisme". In: Widdershoven-Heerding et al. Eds (1995: 130-153).

Rijk, Lambert Marie de (1981): *Middeleeuwse wijsbegeerte. Traditie en vernieuwing*. Assen: Van Gorcum.

Robins, Robert Henry (1976): "Some Continuities and Discontinuities in the History of Linguistics". In: Herman Parret ed.: *History of Linguistic Thought and Contempoary Linguistics*. Berlin: Walter de Gruyter, 13-31.

Rolin, Kristina (2002): "Why Gender is a Relevant factor in the Social Epistemology of Scientific Inquiry". Internet: <http://philsci-archive.pitt.edu/archive/00000837/00/Rolin.pdf>, consulted 23 July 2008.

Roos, Anna Marie (2000): Review of Shapin/Schaffer (1985). Internet: <http://www.h-net.org/reviews/showpdf.cgi?path=25967955484936>, consulted 18 July 2008. Reviewed by Anna Marie Roos, Department of History, University of Minnesota, Duluth. Published by H-Ideas (April, 2000)

Schmitter, Peter (1992): "Quo vadis, ars historica?". *BGS* 2.2-3, 235-239.

Schmitter, Peter (1996): "Profession und Bekenntnis – oder: Konrad Koerners historiographischer Nachlaß, III. Teil". *BGS* 6.1, 152-158.

Schmitter, Peter, Marijke van der Wal eds (1998): *Metahistoriography. Theoretical and Methodological Aspects of the Historiography of Linguistics*. Münster: Nodus Publikationen.

Schuster, John A. (1995): *The Scientific Revolution. An Introduction to the History and Philosophy of Science*. Sydney: School of History and Philosophy of Science [esp. Chapter 3: "The Problem of 'Whig History' in the History of Science"]. Internet: <http://hist-phil.arts.unsw.edu.au/staff/staff.php?first=John&last=Schuster>, consulted 15 July 2008.

"Strong programme". Internet: <http://en.wikipedia.org/wiki/Strong_programme>, consulted 16 July 2008.

Shapin, Steven (1995): *The Scientific revolution*. Chicago etc.: The University of Chicago Press [Dutch translation 2005: *De wetenschappelijke revolutie*. Amsterdam: Balans]

Shapin, Steven, Simon Schaffer (1985): *Leviathan and the Air-Pump. Hobbes, Boyle and the Experimental Life*. Princeton: Princeton University Press.

Yearley, Steven (2003): *Making Sense of Science. Understanding the Social Study of Science*. London etc.: Sage Publications.

_____

Widdersveld-Heerding, C. et al. Eds (1995; 2003[3]): *Wetenschapsleer*. Heerlen: Open Universiteit.

Wilde, Rein de (1995): "Narrativiteit in de mens- en cultuurwetenschappen". In: Widdersveld-Heerding et al., 358-379.

Zijderveld, Anton C. (2006)"Rickert's relevanvce. The Ontological Nature and Epistemological Functions of values. Leiden: Brill. Internet: http://repub.eur.nl/publications/law_cult_soc/index/822652110, consulted 14 July 2008.

Zilsel, Edgar (1942): "The Sociological Roots of Science". *American Journal of Sociology* 47.4, 544-562. Internet: <www.compilerpress.atfreeweb.com/Anno%20Zilsel%20Roots.htm>, consulted 14 July 2008.

Appendix 1: A case in the sociology of the historiography of linguistics / the language sciences

You read the announcement of ICHoLS XI, taking place by the end of August and beginning of September 2008 in Potsdam, Brandenburg (Germany). . You are an acknowledged expert in the field of the historiography of linguistics and you decide, a year in advance, to send in an abstract of approximately 300 words and then continue your daily work, which is most of the time not related to your expertise, meaning giving lectures in German grammar, applied linguistics, generative grammar, phonetics or even management skills.

      Now, there is something strange and at the same time intriguing about this intention to contribute to this conference. First of all you notice that there is no real topic to contribute to, except for some workshops in Micronesian and Philippine Linguistics before the Advent of Structuralism, , Linguistics and Rhetoric, the history of sociolinguistics, Indian Traditions of Language Studies or the Notion of person in Greek Grammar. So you carry on to pursue your historiographical work assuming that that it will fit into the goals of ICHoLS. Unfortunately, however, you are not really aware of the these goals, so more or less uncertain about them you visit www.ichols-xi.de and find a page where the history of ICHoLS can be found. The information is revealing: the conferences alternately take place in Europe and the Americas and they are "platforms for researchers working on the history of the language sciences", a still expanding field, it says. But is that enough to continue your efforts to write this abstract and a paper for next year? And afterwards, what will happen to your paper. Do you belong to the selected few who get only ten pages (including the references) to be published in the conference proceedings? Or do you have your own periodicals in which your paper will be published? And what about the accessibility of this publication? And what about the number of papers read during the conference? Over 200. What is the impact of your paper and research on other participants, especially when you realize that there six parallel sessions and some extra workshops. What can you do to draw the attention of the audience? Or do you prefer a smaller group of people discussing theses and preliminary results? Will your presentation be on the hand clear and precise enough and on the other hand controversial? Are there specialists in the audience who demand more than a general outline, who want more details or is it better to keep your paper as informative as possible for a broader audience? Do you want a discussion of your paper or do you prefer a list of data representing a historiographical issue? What about your fluency in one of the conference languages: Italian, Spanish and Portuguese, German, English, French and will this influence you visiting papers read by others? Furthermore, is this the right place to present and discuss my research findings? Or are there other "platforms" which are more suitable to present my research?

      These questions go through your mind if you decide to go to Potsdam. Even experienced readers do ask themselves these questions. And for me the most important metahistoriographical question would be: does my perception of historiographical work meet that of other historiographers? And do these differences, in advance, lead to different approaches and thus to fundamental discussions in historiography which are necessary to make clear what positions are at stake in our discussion? And what about changes in historiographical approaches during the last 30 years or so. Is historiography a science which serves topical, socio-political goals? And how did it develop? What about the metahistoriographical work taken up by the late Vivien Law, Robert H. Robins, Klaus D. Dutz and Peter Schmitter. Did the discussion stop? Or are there historiographers of linguistics who still feel the need to discuss the metahistoriographical concepts and theories? Are we satisfied with the presentation of a recently found medieval manuscript or grammatical peculiarities of Chinese or Bask, taken by Wilhelm von Humboldt? And what is the step forward we do, when analysing these documents? Does it add to what we already know about

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medieval or Humboldtian ideas. Isn't it fairly tragic that my expertise stays within a small group of experts and doesn't reach a broader audience – because it contributes to their cultural understanding of contemporary linguistic thinking?

The remaining questions are: will these questions influence my paper to be read in Potsdam? And more important, do I mind? The answers are, I hope in what I contributed to this conference in my paper.