

Specimen

Gerda Haßler (Hrsg.)

Nationale und transnationale Perspektiven der Geschichte der Sprachwissenschaft

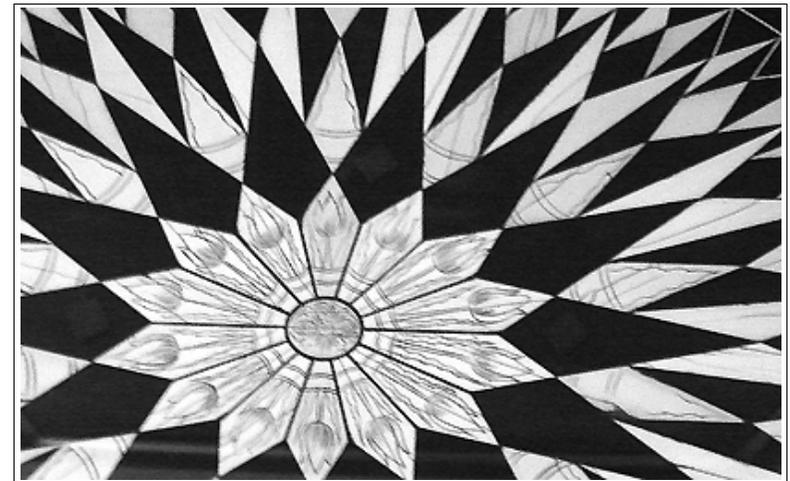
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(De-)Socializing Historiography of Linguistics

0. Introduction

Writing the historiography of the language sciences is, like speaking, acting, or teaching, a social act(ivity). This is as such not surprising:

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 practice.

In this article I will discuss ‘writing the history of ...’ or historiography in connection 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). In general, this mainly “British” sociological approach towards science maintains 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 the language sciences relate to social dimensions like power, priorities, networks, changes or developments.

1. The linguistic peculiarity of historiography

Every historiographical account, although it focuses upon the past, has an inherent incompleteness — the past itself is not the historiographical construction. Because historiography encompasses the present time or context and contemporary standards of writing the history of linguistics, contemporary methods, translations and techniques only these determine the historiographical constructions, the analysis and interpretation and thus its *changing* over time — cf. the types of history-writing activities listed above:

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 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).

It is therefore important to construct a historiographical framework which is an international, socially constructed necessity in which we take into account the question why the historical issues, problems, characters, concepts 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. The scientific character of historiography

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, discusses the problem of historical facts in a for our purposes revealing way: as a prelude to the sociological discussion of science.

The historical fact as a mental entity is a product of the mind (cf. de Rijk 1981: 44). It is de Rijk’s 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 meaningless without a criterion or standard according to which the historical fact as a physical entity can be considered as a representation of 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. In speaking about the past this only says something about the speaker and his selection(s) of data, not about the past. What we must conclude here is that the historiographer has an impossible task to select the relevant data as to make his historical account as objective or reliable as possible. The linguistic arguments 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, and historiography has no object. But if (a) is true (b) is obviously problematic as are (c) and (d). In the first case it is scientifically meaningless to speak about the past as such, 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 themselves. 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’ but about the ‘social values’ of historiographical research.

What makes historiography a scientific practice? In 1966 Richard P. Feynman (1918–1988), a theoretical physicist, gave the following description of science:

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). 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

3 Sociological aspects 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.

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, constituting another value of historiographical work. But not only historical constructions 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).

The ‘core’ business of historiographers is historiographical *knowledge*, in our case considered as the shared subject of group processes. A fundamental problem 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.

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 as 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 grounds? 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 uncover 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 sociological approach.

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 ‘practice’ as a historiographer of linguistics?

The discussion and exploration of the epistemology or of the theoretical and methodological backgrounds of historiographical ambitions can *ansatzweise* be found in textbooks, periodicals or during historiographical discussion when attending conferences, but it is still unclear how historiography, mirroring the scientific discussions 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.

4. Conclusion

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! In

the sociology of scientific knowledge 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/Hughes/Pinch 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 historiographical *cum* 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 in 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)

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