

## Readers and reading behavior in the past

The question is: What is the question?

Joost Kloek and José de Kruijf  
Utrecht University

When speaking of the empirical study of literature, our first association is with research into contemporary literature or reading behavior, and this is only natural. For studying the present the empirical method can be deployed to a high degree of perfection. For the past, possibilities are more limited. When suitable source material has come down to us, it is often fragmented and incomplete. Nevertheless, the psychological and sociological approach that became popular in the 1970s has also given new zest to historical literary research. Literary consumption and reading behavior, production and distribution, literary socialization and forms of organization became popular areas of research. In the last few years change has set in, however. There seems to be some disappointment about the results which these often labor intensive projects have yielded. It may well be time to recognize that the empirical method, in the final analysis, cannot mean all that much for historical literary research.

Matters have come to a suitable stage for taking stock, and that is what we wish to do here. In doing so, we are not concerned with an assessment of whether the reception studies, reconstructions of publishers' lists, analyses of inventories, explorations of bookshop administrations, and research into reading societies, which have been published over the past few years, have really been worth the trouble. The answer to that question depends, of course, on the expectations with which they were set up or, alternatively, their productivity for further research. It is these expectations and that productivity which we wish to discuss. It is our conviction that expectations at the time of the social turn in historical literary research were as unrealistically sanguine as they were insufficiently articulated. This could only lead to disappointment, whatever the results. It also made for research that was determined more by the availability of sources than by a research question — at least a relevant, well-reasoned and clearly defined research question — and this in turn led to a situation in which results of the different projects can hardly be compared. Naturally, this has not enhanced their productivity.

For an adequate understanding of the initial enthusiasm, and so of the recent disenchantment, we need to go back to the 'rediscovery' of the literary context, around 1970. Not only did this mean an enormous impulse for modern research into the psychology and sociology of literature, but it also gave new *momentum* to the historical study of literature. The preceding dominance of the work-immanent approach had made researchers of older literature feel uncomfortable, and also rather undervalued. The work-immanents or autonomists had as their creed that analysis of the unique organization of the individual text was the nuclear task of literary science; the arrangement of texts into genres and periods, and certainly the search for connections between literature and reality outside it, were at best regarded as derivative activities. That put researchers of older literatures in their place! The essence of much of their effort — research into genre and period, and the explanation of texts from their function and their place in prevailing social and cultural contexts — had thus been relegated to second-rate activities.

The rise of contextualising literary research in the form of reception research, sociology and psychology of literature, could be interpreted by historians of literature as a vindication of their point of view. At the same time they felt uncomfortable with the methodological claims, based on social science, made by the new movements. Results now had to be representative and verifiable, requirements that are often hard to meet in historical research. At the same time, however, in France in the 1970s the first fruits were harvested of a contextualising research project with a historical focus that was based on empirical sources. This so-called *histoire du livre* was the product of an alliance between book-historians and cultural historians. The link-up had been anything but obvious. Both fields had an impressive tradition, but the history of books had been mainly aimed at exhaustive bibliographical inventory and description, whereas cultural history had traditionally opted for broad synthesis and daring interpretations. In the *histoire du livre*, the bridge between the disciplines was built by historians who were not so much interested in the actual cultural products, as much as in their dissemination and how they were treated, and whose special focus was not on books as such, but on books as cultural media. The most monumental manifestation of the *histoire du livre* was certainly the four substantial volumes of the *Histoire de l'édition française* (Martin and Chartier 1989–1991) that aims to offer an integral history of books and book production, distribution and consumption. The project did not quite succeed in achieving its aims. In practice, the technical and commercial aspects are rather emphasized, at the cost of writers as well as readers. More inspiring, it would appear, than the *HEF*, as it is usually referred to, were studies by an American cultural historian involved in the *histoire du livre*: Robert Darnton. On the basis of extensive research of records in the best tradition of the history of books — and, it should be added, recorded in a great style schooled in crime journalism — he wrote fascinating studies about, for instance, the adventurous road which the *Encyclopédie* had to travel from publisher to reader,

or of the shady, picturesque world of eighteenth century hacks, or of Jean-Jacques Rousseau's fanmail (Darrton 1985, 1995). And in the best tradition of cultural history he succeeded in giving exemplarian significance to such case histories.

By its research into the way, and especially into the conditions in which books were written, produced, distributed, bought and received, the *histoire du livre* gave an unprecedented impact to the study of the history of literature. The fact that these explorations were based on large-scale research of sources, especially serial sources like publishers' administrations, registers of censors, and inventories, made clear that there were empirical possibilities for this kind of research which had been largely undervalued. The result was that literary historians ventured into fields and used methods of research which had hitherto been the reserve of book historians and social historians. The history of literature, it was proclaimed, should record the history of 'literary life'.

It is hard to put an otherwise than positive construction on this development. There is not much sense in speaking in terms of progress in the humanities, but when we value new impulses to scientific debate the contextualising turn scores highly. However, there is room for some reserve. The 'new' research often makes a rather haphazard, uncontrolled impression. The relevance and precise research questions are often couched in rather fuzzy language. This in itself is undesirable, certainly where empirical research is concerned. In addition, the results of the various projects can hardly be compared. This precludes the possibility of making real scientific progress.

The euphoria to which the 'discovery' of context sometimes led can best be illustrated with the aid of the research models which were launched in consequence. These were especially characterized by their holistic aspirations. Reinhold Grimm, for instance, in 1977 gave a blueprint of an empirically founded research programme for reception history, in which the variation of the reading public, the variation of literary consumption, and the variation in interpretations were to be traced and explained on the basis of the question: "Wer hat warum was warum wie gelesen?" (Grimm 1977). Very little can be done with this indeed, when there is not even a beginning of a formulation of the question as to what research concepts and definitions are to determine the analysis of 'wer', 'wie', 'was', and 'warum'.

There is a comparable problem with the model of communication used by Darrton (Darrton 1990:112). This is in fact an extended version of the well-known model of author-text-reader by Jakobson, including the institutions that play a role in the production and distribution of literature, as well as the quantities 'intellectual influence and publicity', 'economic and social climate', 'political and legal sanctions'. Judging by the frequency with which this model is reproduced, cited, and embroidered, many researchers assign great authority to it. This is even more true of Jacobson's little diagram. It is hard to imagine that in medical publications the model physician-treatment-patient would have comparable authority. That is strange.

In its comprehensiveness, and because of the lack of precision of its constituents and the nature of relations between them, it is exceedingly unsuited to the functions it pretends to fulfil: a diagrammatic representation of reality in order to make the latter accessible to research.

These examples are characteristic of the barely limited pretensions of empirical research in literary history, and, in addition, the concomitant lack of focus in research questions. In practice this has led to a situation where priority was given to the search for source material (preferably of a serial nature), and subsequently a suitable research question, with more or less holistic aspirations, would be thought up. A short survey of research practice in recent years may serve to illustrate this.

In the relevant studies two movements can be discerned. In the first place there are attempts at a reconstruction of 'literary culture' at microlevel; a case study serves to represent the macrolevel. Furthermore, there are projects using one specific source. In the case studies a publisher usually serves as point of departure. Selection of a publishing house is usually pragmatically motivated, on the grounds of the available archives: the greater the completeness of the records of a company, the greater the chance that the publisher and his firm will be the objects of scientific investigation. This immediately leads to a fundamental problem: what is the representative value that may be assigned to a single case determined by chance. But for another reason too, the relation between the microlevel of the case and the macrolevel of the research models is problematic in many of these studies. Usually, on the basis of author contracts, methods of production, channels of distribution, sales figures, and secondary sources, if any, in as much possible detail, and true to sources, the progress is described of books from a certain publishing house, from author to readers. But that already shows us the first pitfall of this kind of research. Exaggerated attention to detail and adhesion to sources can all too easily obscure the perspective on the larger context, and this puts pressure on precisely that which justifies the research. The same comments can be made on, in themselves broader, variations of this type of case study: research into the literary culture of a region or city in a certain era. Here too, choices have been made on the basis of quantity and quality of available materials, and again, a scrupulous reconstruction of the microlevel does not fulfil the holistic promise of the macrolevel.

The most important objection that can be made against this type of research, no matter how many interesting new facts are brought to light, is that results stand in isolation and are doomed to remain so. On the one hand, the broad, vague, holistic models based on Darrton lack viable possibilities for embedding, and it is no wonder that no more than lip service should be paid to them. On the other, a procedure that depends too heavily on sources presumably stands in the way of the possibility of drawing conclusions about the external validity of the observations. The source material was not chosen with the criterion of comparability with other research in mind. The approach of seeking, and listing and describing data in as

much detail as possible does yield substantial amounts of information, but offers no prospect of methodological or theoretical innovation.

Source materials in the latter current in empirical literary history research are basically selected for comparability. Studies are usually concerned with one specific type of source. This usually means that sources consist of serial data, like advertisements, auction catalogues, subscription lists, bookshop administrations, and estate inventories. In these cases the wider framework of the research is usually not formed by the Darntonian universe, but by an all-encompassing question in the spirit of Grimm: who read what where when?

This type of research has also failed to live up to the sanguine expectations that initially inspired it. As in the case studies, this can be blamed on the fact that those expectations were in fact inarticulate, and so could not be made operational in clearly formulated and motivated research questions. The disappointment that endless tallying and calculation involved in this type of research finally resulted in universally viable characterizations like 'modernization', 'commercialization', or 'process of bourgeois emancipation', was only to be expected. What serial research projects have in common with the case studies is that they stop at the limits of their source. Comparisons of different sources or covering various periods are few and far between. Some researchers have themselves shown their awareness of this limitation, and blamed it on their sources: too lacunae, or on the contrary, too unwieldy. These are strange excuses indeed: the social and economic sciences have long ago developed methods for tackling these kinds of problems.

It would appear, then, that the 'contextual turn' dating from around 1970, has come full circle. It is symptomatic that the interest in serial sources seems to have yielded to the pre-eminently individualist source: the ego document. Reading experiences noted down in letters and diaries, according to their researchers, could well be more revealing than entire archives or voluminous databases. For the moment the question remains: what precisely, and to what extent can meaning be extrapolated from the purely individual? An even more radical change of course has been made by the godfather of the *histoire du livre*, Roger Chartier, who seems to have lost all faith in empiricism. He wants to return to the book itself, there to uncover the implicit reader. Chartier does not limit his research to the text, but stresses the importance of physical factors like format, paper, font, illustrations, and so on (Chartier 1987 Chapters 3 and 7 and often repeated in later publications). We really seem to be back at the beginning: empirical research in literary history was originally based on the need of confronting the construction of the implicit reader with the historical reality of a colorful audience of real readers. The road taken by Chartier does not look in any way promising. This is all the more true since he does not make clear by what methodology the researcher is to demonstrate external validity of his construction of the implicit reader. How is one to determine what features of text and layout correlate with which reader characteristics?

What is this research really after? If the aim is an integral history of literature, including the conditions that play a role in its production, distribution and reception, any project is bound to disappoint. Is that the reason for turning away from empirically oriented studies, and to return to the more traditional avenues of literary history? That would certainly be a very unsound reaction. The issue is not that empirical research of recent years has not come up with any results. It has substantially enriched our insight in a number of areas, for much research has indeed yielded interesting new information about, for instance, buying and lending behavior, reading circles, popularity of genres, authors and titles, the size of the literary reading public, and much more. Only, these results were not in proportion to expectations, especially because, as we have mentioned, they were fragmentary. Nor is it the case that the empirical possibilities are nearing exhaustion. On the contrary, what is wrong is that they are usually deployed so inefficiently. And this has everything to do with our observation made earlier, that both theoretically, and in research practice, empirical literary historians keep going down the same well-worn paths. "All is quiet on the book history front", runs the ironic characterization made by Brouwer in his discussion of two volumes of reviews from abroad, in which inviting titles like *Histoires des livres*, *nouvelles orientations* certainly do not fulfill their promise (Brouwer 1996: 209).

If we see correctly, then, empirical research in literary history, as it has been conducted until now, has suffered from two shortcomings right from the beginning. The first is a lack of research questions that are focused, well-reasoned, and justified by the needs of the discipline. Situating literary consumption 'in the cultural context' is not a question that can drive concrete research, showing the same flaws as 'who read why what'. Meaningful empirical research requires the formulation of hypotheses; in the questions just formulated, it is hard to conceive what such a hypothesis would look like, let alone by which current intersubjective methods they would be tested. The most productive hypotheses for historical research are probably those in which situations are compared for a number of variables in either time or place. In this way historical developments or the meaning of local factors can be traced. Results will then not be restricted, as is often the case now, to a momentary snapshot of one city or company. Only after hypotheses, variables to be tested, and methods have been explicitly worked out, can a decision be taken on which sources are to be used. Of course sources will always show lacunae. Completeness, however, is not a guarantee of scientific validity. It is, then, a misunderstanding that research would be served *a priori* by as many and as complete source materials as possible. Certainly where processes are concerned, and not events, there is a range of methods available for achieving acceptable verification. First and foremost this is true of possibilities for statistical validation — far too little used in research of serial sources until now (De Kruijf 1999: Chapters 3 and 4).

In recent years empirical literary history has drastically demolished the boundaries that separated it from related disciplines like book science and cultural history. On the other hand, it has shown very little interest in disciplines which could have helped it along methodologically, like economy, sociology, psychology, social history, and certainly also empirical research aimed at modern literature (Groeben and Landwehr 1991). This abstinence has limited productivity. More especially, it could have profited by new methods made possible by increasing statistical knowledge and advanced computer technology, and which have proved their efficacy in other branches of historical research (for instance, sophisticated applications of database management, and modern statistical techniques like cluster analysis). The root cause must be found in the tradition of the discipline. Historical literary research lacks an empirical tradition. To be able to use samples from serial sources, a clear research question is essential. Beforehand one should be able to formulate precisely what it is one wants to know. This sort of precision does not come naturally to the literary historian. But when their research leads them into questions about the social dimensions of the history of literature, they will have to consult colleagues specialized in disciplines like (historical) sociology, economic science, and social history. They can furnish the tools needed for conducting adequate research of, first, the social context of literature, and second, historical developments in this field.

Apparently, the time is not yet right for abandoning empirical methods of research in this field. Nor is this the right moment for adjourning to new kinds of sources. Now is the time to return once again to results already available, armed with new questions. Such a question could be: to what extent are these phenomena constant in a national, or even international context, are they tied to time and place at all? Is the select group of large-scale consumers of books among earners of the highest incomes, which was found in research about book buying and book possession in the eighteenth and nineteenth centuries in Middelburg, Zwolle and The Hague, a timeless phenomenon? (Brouwer 1995; De Kruif 1999; Kloek and Mijnhardt 1993). And is it typical of the Dutch, or of city-dwellers? Is the severely limited acquisition of literary books by the general public a fact which is hardly, or not at all, subject to change by all sorts of processes of modernization? Is the gap between the canon of a certain period and the interests of its general reader historically constant? Answering these questions requires comparative historical research, in which the use of an intersubjective apparatus of concepts and methods should make effective comparisons a real possibility.

Some years ago Els Andringa published a review of the interpretations over time of Kafka's story "Vor dem Gesetz". Somewhat disconcertingly, she had to conclude that the authors in question hardly based themselves on earlier insights, or engaged in any discussion (Andringa 1994). Some dismay is indeed in order, but the question remains whether, if they had done so, results would have been

essentially different. That is probably characteristic of interpretational research of texts. What is certain is that the empirical researcher working in isolation is undervaluing his own research. In that light, the successes of empirical research into literary history booked over the past few years are just as many opportunities missed. So there is every hope for the future.

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