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Between Science, Art, and Forgery: Latin Textual Criticism as a Case Example

Abstract

The methodology and practice of Latin text editing is presented in the paper as an example of a humanities scholar's work that combines scientific effort with artistic activity, including forgery of works by old masters. Part one outlines the methodological situation in textual criticism in the 1960s and 1970s, when the author was a university student and novice researcher. Next, the author considers what caused changes in editing methodology in subsequent decades and, finally, he comments on present-day debates surrounding the role of the academic editor, with a special focus on the interdisciplinary aspects of this issue.

Keywords: Latin textual criticism, forgery, academic editing, intertext, science, art

1. Methodologies of my youth

My true initiation into Latin textual criticism was Prof. Kazimierz Kumaniecki's seminar at the University of Warsaw's Institute of Classical Philology, in which I took part as a student and then as an assistant lecturer in 1967–1972. The seminar was of the highest world standard, following the methodological principles and rules set down in the classic textbook by Paul Maas (first edition 1927[1]). Kumaniecki learned his research strategy directly from Maas during his seminars in Berlin even before it became

[1] Paul Maas first made his treatise *Textkritik* public in 1927 (Maas, 1927); a separate edition was published by B.G. Teubner in Leipzig in 1949, and another one in 1957 with an important addition by the author, "Retrospect". An English translation appeared in 1958 (Maas, 1958).

the canon of *ars critica* for the reconstruction and publication of ancient texts and continued as such for several decades. Maas' strategy derived from the "Lachmann method"[2] from the first half of the 19th century. Its advantage over earlier attempts to develop the method's theoretical basis lies primarily in applying an extreme version of principles taken from reasoning typical of science. Obviously in connection with the success of Darwin's theory, Maas proposed a rigorous approach to building the stemma, i.e. the genealogical tree, of an ancient text's tradition. Framed within rules as economical as a science textbook, Maas' lecture became a global success after its author, fleeing Nazi persecution, settled in Oxford. To editing in the English-speaking world, this version of the Lachmann tradition brought revolutionary results.

Elsewhere, relatively soon after Kumaniecki's passing, I tried to describe the essential experience of participating in his late seminars on textual criticism[3]. We worked on Cicero's *De Oratore*, which Kumaniecki published as a Teubner edition in 1969. There is no doubt that the methodological foundation of his teaching derived from Maas' stemmatic tradition, at the same time challenging it; this was never formulated in a theory, but was constantly practiced in his editing. Very early on, Kumaniecki took into account the possibility of a horizontal tradition that would weaken the authority of stemmatic patterns. This appreciation of how widespread the phenomenon of *contaminatio* could be was only starting to emerge in textbooks (Reynolds, Wilson, 1968, latest edition 2013; West, 1973).

[2] Karl Konrad Friedrich Wilhelm Lachmann (1793–1851). Lachmann, a theologian by education, gradually developed a keen interest in classical philology. A follower of Christian Gottlob Heyne and other scholars seeking new methods of comparative reading of the classics, he analyzed early German literature (*Song of the Nibelungs*) as well as Latin texts. His greatest achievement was the development of a new method of textual criticism called the "Lachmannsche Methode." He strove for scientific reconstruction of a text, aimed at producing a version as close to the original as possible. He considered the most important stage in arranging the source material to be drawing a stemma (genealogical tree) based on an analysis of a text's different witnesses to determine the inner relationships among the different variants. He was greatly successful in his work on *Song of the Nibelungs*. He opened a new era in research on Propertius, publishing the first truly critical edition of this poet in 1816.

[3] Cf. J. Axer (1989), presented on Dec. 15, 1987 at an academic session organized at the University of Warsaw's Institute of Classical Philology for the 10th anniversary of the deaths of professors Kazimierz Kumaniecki and Adam Krokiewicz.

Kumaniecki never wrote a textbook himself, and his interest in the theory of *ars critica* was slight. On the other hand, he spent many decades conducting an internal dialogue with great Roman prose writers and poets, in constant competition with eminent philologist editors from the older and his own generation. He was not tempted to revise or question Maas' main theoretical assumptions. I think this was because he believed that textual criticism is, above all, a practical art in which the researcher's personality and talent are crucial, while rules should only help at the early stage of practicing this art.

Once the stemma has been sketched, it is time for *examinatio* and *emendatio*. And these appear to the editor as an infinite set of individual cases, each in need of separate consideration after earlier and correct performance of operations organizing the material. My Master was interested mainly in those of the endless problems requiring solving that opened the way to speculation requiring in-depth understanding of the historical and cultural context of a given text and, above all, grasping a given author's language and artistic technique. This means his academic passion and creative temperament directed him toward actions that would reconstruct the original text in a way going beyond what could be unequivocally justified by the juxtaposition of preserved sources.

He had his greatest success with the textual criticism of Cicero's texts, thanks to phenomenal knowledge of his language and a deep understanding of the mentality of the so-called last generation of the Roman Republic. He moved beyond the boundary of scientifically verifiable knowledge; this gave students the impression that he could travel in time and intuitively establish contact with people of the Roman elite from the time of the Republic's disintegration. This is not the place to wonder what historical and personal experiences had produced this special gift. Suffice it to say that it was largely impossible to pass on in the form of rules, recipes, and instructions. This made the textual criticism we practiced during those seminars rather like artistic training.

It did not take me long to achieve sufficient proficiency in *ars critica* to base my postdoctoral degree (habilitation) on an edition of Cicero's oration *Pro Roscio Comoedo* (Axer, 1976) and to write studies on textual and structural criticism of this work (Axer, 1976, 1979). However, I was helped by a very happy

coincidence. Poggio Bracciolini's autograph, which had been lost since the 15th century, had just been found in the deepest recesses of the Vatican Library, and its finder Prof. Augusto Campana informed Kumaniecki of this fact. He in turn managed to get a photocopy of the autograph. Prof. Lidia Winniczuk had already generously transferred her contract with Bibliotheca Teubneriana for the publication of *Pro Roscio Comoedo* to me.

Therefore, I had a considerable advantage over the editors from the previous 400 years who had used many different copies originating from one source. I had chanced upon a work for which both *recensio* and *examinatio* were extremely simple and boiled down to careful reading of one manuscript; and, of course, there was no need to draw a stemma. Success depended on the ability to reconstruct the fragmentarily preserved text of Cicero's defense speech as reasoning and as a legal proceeding. This script then had to be superimposed on the very distinctive community of Roman theater actors and producers; the suspicion also existed that Cicero had used the language of that community to play a literary game, seeing as he was defending an actor and the accused was the owner of a theater school. Once I had arrived at opinions on all these matters, it was time to decide about the extent to which the preserved text required correcting (which in the language of textual criticism is known as applying conjecture and emendation) – so as to achieve the highest possible probability that the text proposed by the editor had come from the pen, or – rather – stylus, of Marcus Tullius Cicero. It was the dream opportunity for intuitive and artistic textual criticism[4]. The success went to my head, I signed seven contracts with the world's leading publishers for further editions of a number of Cicero's different orations; I had every year from 1977 to 1995 all planned.

I never finished any of those editions, even though I spent hundreds of hours working on the texts. Why? Because I lost faith.

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[4] Numerous, sometimes excessively positive reviews: Martin ven den Bruwaene, *L'Antiquite Classiqu*e, vol. XLVI, 1977, p. 640; Heikki Solin, *Arctos* XIII, 1979; Guglielmo Ballaira, *Giornale Italiano di Filologia* VII, 1977; Raphael Palmerini, *Latinitas*, 1977; Michael Winterbottom, *The Classical Review*, NS, Vol. XXVII No. 1, 1978; Elżbieta Olechowska, *Revue des Etudes Latines*, vol. LV; Fabio Cupaiuolo, *Bollettino di Studi Latini*, Anno VII-Fasc. III, 1978; Carl Joachim Classen, *Museum Africum*, Nigeria, 1978.

2. How did I lose faith?

Maas' textbook attempted to rework Lachmann's revolutionary discoveries from the early 19th century in such a way as to guarantee that textual criticism would, in terms of scientificity, stand up to the natural sciences[5]. Working in times of rapid development of knowledge about the Earth's past, especially paleontology and geology, Lachmann – a contemporary of Georges Cuvier and Jean-Baptiste de Lamarck – had designed procedures enabling the genealogical trees of early texts to be built (he started from the Bible, later moving on to Old German and Latin). This theory continued to be improved; at the same time, Darwinism turned out to be a very strong stimulus encouraging the propagation of stemmatic diagrams distinguishing manuscript families and subfamilies, just like animal genera and species are distinguished. At the height of this way of thinking's triumph, Paul Maas decided that true science requires short and simple rules; rules that are absolutely incontrovertible and universally binding. It was his ambition to strip textual criticism of subjectivity, the editor's influence on the object of research, and any speculations that represented risky hypotheses. Kumaniecki's late seminars, described earlier, provided proof that new narratological trends, theory of reception and, finally, the gradual decline of positivist authority, undermined the thus understood scientificity of textual criticism theory, even if, technically, Maas was the methodological patron of our Master's work in this field[6].

My loss of faith, therefore, was a natural realization of this, a gradually growing awareness that the status of everything I found fascinating and appealing about textual criticism was very ambiguous from a scientific point of view. Maas was no longer excuse enough. I came to suspect I was practicing an art, not a science, and perhaps even resorting to forgery.

What did I do? First and foremost, I tried to write a textbook that would serve as an "updated Maas." I wanted to

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[5] Of course the beginnings of Lachmann's stemmatic strategy can already be found in the work of great 15th- and 16th-century philologists. However, fundamental changes in the way textual criticism was practiced only took place thanks to the work and authority of Lachmann himself.

[6] Apart from myself, this seminar was also attended by Elżbieta Olechowska, an excellent editor of Cicero's orations *Pro CN. Plancio* and *Pro Rabirio Postumo*, B.G. Teubner, Stuttgart 1981.

restore harmony between scientific procedure and freedom to interpret and offer textual interpretation ideas with a resultant impact on a text's reconstruction; between scientific procedure in the analysis of sources and freedom to hypothesize. I did not publish any "new Maas," but tested my position by communicating it to classical philologists and researchers of antiquity representing different schools of textual criticism. I presented it in Germany (including Heidelberg, Tübingen, Munich), Italy (Rome, Naples, Turin), Spain (Madrid, Cadiz, Barcelona), the United States (Northwestern University, Harvard) as well as Centro Ciceroniano congresses. This manifesto was appreciated the least in Germany and the most in America. My argument went like this: The initial Lachmann-Maas procedure involves taking all the witnesses that document the history of a text and dividing them into groups, identifying similarities and differences between those groups. In order to be able to effectively distinguish "genera," Maas introduced the notion of the "indicative error," i.e. an error that is not accidental but indicative of a specific stream of tradition. However, the indicative error concept introduces a subjective element into the reasoning. This label is the result of the researcher's personal decision: It is the scholar who considers a given version to be erroneous, and the error to be indicative. It is hard not to see that with the introduction of the indicative error, the scientific model loses the purity of objective documentation, or even worse: The whole system is threatened even more by the issue of contamination.

Let us assume that we have managed to reconstruct families of manuscripts and each one is a certain ideal type. When it comes to working on the tradition of a specific text, though, things become much more complicated (I encountered such a situation when preparing Cicero's *Pro Milone* for Teubner at the turn of the 1970s and 1980s). An enormous part of the real transmission of ancient texts is impossible to organize in this way. Over several generations, the general belief was that if a researcher did not produce a stemma, it meant he did not know how. In fact, though, traditions often cannot be presented as a stemma of any practical usefulness to an editor because the text's history has not yielded any "genetically pure" models. The ideal model, according to which there primarily exists vertical transmission (younger sources are descendants of older ones and form generation sequences), is a great simplification. Manuscripts coexist

on a horizontal plane as well, and completely ignore chronology in those relationships. People carried codices around with them, copied them, and also sold and bought them without any concern for pure "breeding" of a specific tradition of transmission. Often – usually only along brief sections of a text's history – we can trace the direct descent of certain manuscripts from other, also identifiable codices. However, the world of manuscript sources expanded mainly in a process of constant contamination. A multitude of relationships resulted from the physical contact of individual volumes in circumstances that are impossible (or almost impossible) to reconstruct centuries later. Faced with this ocean of contaminated texts that cannot be grouped into proper families according to the stemmatic model, a classical philologist who believes in Maas is helpless.

Of course for someone whose initiation, like mine, had been based on a tradition as splendidly simplified as *Pro Roscio Comoedo*, the shock was double. In addition, all the traditionally educated classical philologists up to my generation had, since the early 19th century (i.e. from the time of the triumph of German neo-humanism), been schooled to be experts on antiquity (from the 10th century BCE to 400 CE); after that, they were allowed not to be interested in the Middle Ages, only to return as competent experts on Renaissance philology of the 14th and 15th centuries.

Meanwhile, everything that was important for manuscripts happened mainly in the Middle Ages. Manuscripts hid and were copied in isolated communities scattered all across the Christian world of the time. To trace their history, you need to know as much as possible about medieval people and local communities. In-depth knowledge of daily life between the 6th and 13th centuries turns out to be essential. Previously, the author was the editor's partner; however, it appears that now the partner should be the scribe, who traditionally was treated like a pest causing confusion in the sources (Reynolds, 1983).

I began to realize that we should accept this fundamental change of roles. Editors working on a critical edition are more closely related to the scribes whose mistakes and intellectual deficiencies they are used to mocking than to the text's author, M. T. Cicero, whom they worship and whose text they want to restore to its full glory. Thus, in the third year of working on the text of *Pro Milone* I acknowledged the prevalence of contaminated,

"mixed-breed" manuscripts as the main source of doubt regarding the scientific nature of Maas' concept[7].

Not feeling up to becoming a scribe's good partner (for reasons explained in footnote 7), I sought hope in a different area of a textual critic's activity. Because, even if we accept all the accusations against the stemmatic theory as a necessary evil, there emerges an area whose status is even more ambiguous. What does a scholar really do when they move on to *emendatio*? What do I do when I introduce conjecture – amending the text? Don't I make things up, by any chance? After all, I use my able mind to build hypothetical variants of the text that fit my interpretation, my understanding of a given fragment or a whole work. This means I am the one giving the text the shape and meaning that seems the most convincing at the moment of my own reading. Essentially, then, I am practicing literary criticism, and if I change the text in the process, I am involved in artistic and not scientific activity. It's no use pretending that *emendatio* is anything more than a hypothesis, practically impossible to disprove, or that devising it is a form of scientific activity, even just to the extent of *recensio* and *examinatio*.

And so I split my Maas into two parts, agreeing that stemmatics is scientific activity similar to the natural sciences, although tainted with our insufficient knowledge about the way of life and copying of texts in the past, and that it is followed by artistic activity which should be judged according to different rules. I finally wrote a commentary on Maas, which was translated into English, Spanish and Italian, and gave lectures on it in many places, until I realized it was addressed to no one. When artists heard it, e.g. actors and reciters, they liked it very much, but they do not work in editing. Classical philologists, on the other hand, said this division did not apply to them. They insisted that they do very professional work from start to finish, they never make anything up, they have evidence for everything[8].

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[7] I also started doubting my own skills and competence in textual criticism in this sense. Not only was I no expert on the Middle Ages and had learned paleography as an amateur (despite help from Aleksander Gieysztor); the worst thing was that by personality and disposition, I am a very poor scribe. I make mistakes when copying my own texts, and I do not see those mistakes.

[8] The evidence usually came in the form of arguments showing how a given correction or conjecture could have come from successive corruptions of the text at different stages of transmission, taking into account any hypothetical