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Handbook of Stemmatology : History, Methodology, Digital Approaches

Edited by: Roelli, Philipp

Abstract: Stemmatology studies aspects of textual criticism that use genealogical methods to analyse a set of copies of a text whose autograph has been lost. This handbook is the first to cover the entire field, encompassing both theoretical and practical aspects of traditional as well as modern digital methods and their history. As an art (ars), stemmatology's main goal is editing and thus presenting to the reader a historical text in the most satisfactory way. As a more abstract discipline (scientia), it is interested in the general principles of how texts change in the process of being copied. Thirty eight experts from all of the fields involved have joined forces to write this handbook, whose eight chapters cover material aspects of text traditions, the genesis and methods of traditional "Lachmannian" textual criticism and the objections raised against it, as well as modern digital methods used in the field. The two concluding chapters take a closer look at how this approach towards texts and textual criticism has developed in some disciplines of textual scholarship and compare methods used in other fields that deal with "descent with modification". The handbook thus serves as an introduction to this interdisciplinary field. – First systematic coverage of stemmatology as a field within textual criticism. – Written by 38 experts in fields from various philologies to biology and information theory. – Illustrations and many practical examples from a wide range of disciplines are provided to render the content more accessible.

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Handbook of Stemmatology

Handbook of Stemmatology



History, Methodology, Digital Approaches

Edited by
Philipp Roelli

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Cover: The graphic on the cover is a stylometric plot of the contributions in this book, illustrating the vocabulary and style used by its authors. The groupings hint at the extent to which the topics in the book are shared between the authors across their respective fields. The plot was generated by the editor of the book with the R package *stylo* (cf. *The R Journal* 2016, vol. 8:1) using the distribution of the 500 most common words in the book. The resulting tree (Cosine Delta distance) was subsequently retouched with FigTree and Inkscape, thereby assigning one colour per chapter.

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2.4 Neo-Lachmannism: A new synthesis?

Paolo Trovato

This section addresses some of the most relevant improvements and refinements of the genealogical method following Joseph Bédier's sharp criticisms (see 2.3), that is, from 1929 to the present day. This complex, steadily improved set of procedures, scarcely known in most Western countries, where the method is often associated with its unrefined applications in the nineteenth and early twentieth centuries, deserves to be considered by any scholarly editor because of its effectiveness in treating even very complicated textual traditions, such as that of the Old French *Chanson de Roland*.

2.4.1 From Lachmann's method to neo-Lachmannism

Let us begin this overview with two important, though obvious, remarks ("important" and "obvious" are not necessarily at odds) by Karl R. Popper and G. Thomas Tanselle:

The doctrine of [human] fallibility should not be regarded as part of a pessimistic epistemology. This doctrine implies that we may seek for truth, for objective truth, though more often than not we may miss it by a wide margin. And it implies that if we respect truth, we must search for it by persistently searching for our errors: by indefatigable rational criticism, and self-criticism. (Popper 1965, 16)

Every statement about editing [...] reflects, directly or indirectly, an attitude toward certain fundamental questions, and various families of editorial approaches have grown up over the centuries because these questions have been answered in different ways. (Tanselle 1995, 9)

As can be gleaned from the studies of Timpanaro (1961, rev. ed. 1981, repr. 2004, trans. Most 2005), Kenney (1974), and Fiesoli (2000), the so-called Lachmannian or genealogical method, or method of common errors, "was constructed over a rather long timespan (from the last decades of the eighteenth century to the early twentieth century) thanks to the contributions of many scholars, sometimes working in connection with one another, sometimes working autonomously" (Paolo Chiesa in 2.2.1 above). Key aspects that are today considered fundamental were precisely formulated only several decades after the death of Lachmann, such as the

criterion that only shared innovations can serve to prove the kinship between two or more witnesses (the oldest thorough formulation of this principle appears to be that of Lejay 1888; see Froger 1968, 41–42; Reeve 1998, 451–452 = 2011a, 57–58, with additions). Following the research of Reeve, one can also indicate a date for the birth of stemmatics based on shared errors that looks very reasonable, albeit conventional: 1872, that is, the year of the edition of the *Vie de Saint Alexis* published by Gaston Paris (1839–1903), the completion of which had been delayed by the Franco-Prussian War of 1870–1871. As Reeve puts it, no other editor “can challenge Gaston Paris for the title of the first scholar to have applied systematically the principle that only shared errors establish families of textual witnesses” (1998, 464 = 2011a, 68).

From then on, for nearly six decades, the method of shared errors spread and was applied – often in too naive and mechanical a way – in a variety of fields. Notwithstanding the appearance of the short but very dense treatise of Maas (1927), the party was ended rather brusquely in the second half of the 1920s when the Benedictine Henri Quentin (1922, 1926) and the most famous pupil of Paris, Joseph Bédier (1928), directed their critical blows against it (2.3 above). Even though many of their objections do not seem unassailable today, it happened, mostly thanks to the extraordinary argumentative skills of Bédier, that the relatively cohesive world of textual critics began to break up into several families. Classical philologists – who usually worked on texts from a millennium later than their lost originals, offering in many cases at least some easily identifiable common errors – remained in the Lachmannian orbit. North American Romance philologists did so too for several decades, and almost all Italian and several Spanish ones still do today (in Italy, the enormous prestige of Barbi, Pasquali, and Contini has been a decisive factor in this choice of method). In contrast, many biblical scholars (who have to work with overabundant traditions) were attracted by Quentin’s theories, and many Romance philologists (both French and other) who dealt primarily with mediaeval French texts quickly sided with the Bédierist camp. They often had to deal with works which lack a strong authorial mark and present hard-to-track errors, and besides, the new method allowed them to produce an edition much more quickly (Foulet and Speer 1979; Reeve 1986 = 2011a, 28–44; Speer 1995; Trovato 2017, 77–108, 289–297; see also the important C. Baker et al. 2018).

The most unfortunate consequence of Bédier’s “schism” was certainly the sometimes very heated conflict between the supporters of the different methodologies. Unfortunately, most members of many of these schools and traditions, which originated during the last century, are hardly even able to discuss their mutually incompatible methods with one another – even though textual scholars have become a much-reduced subset of that already small group that still does fundamental research in the humanities and they all basically address the same problems. Be that as it may, what is most important for this section is that Bédier’s criticism became a very efficient stimulus for supporters of the genealogical method to reflect on its flaws and review their own positions:

Sui principi di quello che fu chiamato lachmannismo [...] è seguitata a svolgersi nel secolo e mezzo successivo quell'opera di raffinamento, reazione e revisione per cui si può anche parlare di antilachmannismo (principalmente Joseph Bédier e dom Quentin), postlachmannismo (così Giorgio Pasquali e in certo modo Michele Barbi) e, perché no?, neolachmannismo (parte della romanistica italiana). (Contini 1977, 995 = 2007, 1:6)

[In the century and a half that followed [Lachmann's age], scholars were busy refining, reacting against, and revising [...] the principles of what was later called Lachmannism, so that we can also speak of anti-Lachmannism (first of all Joseph Bédier and Quentin), post-Lachmannism (Giorgio Pasquali and, to a certain degree, Michele Barbi) and – why not? – neo-Lachmannism (as part of the Italian school of Romance philology).]

As Paolo Chiesa puts this: “Discovering some limits of the genealogical method and discussing them has produced greater self-awareness, and has given scholars a more mature and refined method” (2.2.8 above). Even though, as far as I know, the term “neo-Lachmannism” (It. *neolachmannismo*) was coined by Gianfranco Contini in the 1970s in order to refer – by and large – to his own studies and to those of his pupils (cf. the above quotation), it seems nonetheless fully legitimate to employ the adjective “neo-Lachmannian” and the noun “neo-Lachmannism” in a wider sense to refer to all those who, from the 1930s onwards, have participated in the maturation and refinement of the method of common errors. Among others, Blecua (1995) did so in a substantial review of Spanish textual criticism, and Salemans – who occupies a unique position in what might be called the Dutch school – used the term in the very title of his noteworthy doctoral thesis (2000), “Building Stemmas with the Computer in a Cladistic, Neo-Lachmannian, Way: The Case of Fourteen Text Versions of *Lanseloet van Denemerken*”.

In this larger sense, it can be argued that the refinements of Lachmannism triggered by Bédier's corrosive criticism constitute a new synthesis of theoretical positions that came into conflict in the third decade of the last century, and perhaps we can look at this as a new paradigm in a Kuhnian sense (Kuhn 1962).

2.4.2 The diffusion of neo-Lachmannism in the twentieth century

In central and southern Europe, methodological reflections followed almost immediately after Bédier's criticism in 1928, at least among the more attentive scholars. In order to suggest a list – albeit certainly incomplete – of the most timely and significant reactions, we may quote the studies of Rajna (1929), Pasquali (1934), Maas (1937), Barbi (1938), Fourquet (1946, 1948–1949), Castellani (1957), and Timpanaro (1961). Many of these scholars are Romance philologists, but the importance of the questions under debate did not escape classical scholars. Besides Maas, Pasquali, and Timpanaro, it will suffice to cite here the names of Kenney and Reeve (a very rich collection of Reeve's methodological papers is Reeve 2011a). Nevertheless, it seems that these discussions have remained almost unnoticed by the rest of the world.

As the fairly recent bibliography on textual criticism by William Baker and Kenneth Womack (2000, 75–131) evidently conforms to its own “Monroe Doctrine” and ignores any work not written in English, I suggest a simple experiment to estimate the diffusion of the neo-Lachmannian turn in twentieth-century North American textual studies. Let us take the important and popular book by Greetham (1995) entitled *Scholarly Editing: A Guide to Research*, which dedicates twenty-four chapters to the same number of research fields (from “The Hebrew Bible” to “Eighteenth-Century English Literature”, and from “Russian Literature” to “Arabic Literature”), so that it may well be seen as a map of textual criticism at the end of the millennium from the American standpoint. On the basis of the invaluable “Name and Title Index” in Greetham’s book, the following table (table 2.1-1) displays in chronological order the presence or absence of references to Lachmann, Maas (readable since 1958 in the English translation by Flower), the equally but differently anti-Lachmannian Bédier and Quentin, and well-known neo-Lachmannian scholars (Pasquali, Kenney, Reeve, and Timpanaro – the latter was not yet readable in the 2005 English translation by Most when Greetham’s book was published, but had been translated into German by Irmer in 1971). The last column is for Tanselle, perhaps the leading American textual scholar of his generation. No references at all to these authors are found in the other twelve contributions in the collection, which are not listed here.

There is no need to emphasise both the knowledge mastered by Tanselle and his popularity among US textual scholars. On the other hand, we must note that, no differently from the Italianist Barbi or well-known neo-Lachmannian Romance scholars such as Avalle, Segre, Vârvaro, and Contini himself, whose output is also very rich from the point of view of methodology, Kenney and Reeve are quoted mostly or exclusively within their own discipline, that is, classical Latin. Robert Huygens and Giovanni Orlandi, that is, two eminent and original textual critics in the field of mediæval Latin, are never quoted. If we except Tanselle and Tarrant, the works of Bédier, Quentin, Maas, Pasquali, and Timpanaro are cited only by Romance scholars, and three out of four of those Romance scholars are Europeans or Latin Americans. Thus, the reader of this companion might be tempted to think that, in 1995, many North American textual scholars ignored or considered negligible not only the contribution of the Italians (for which a strong linguistic barrier may be to blame), but even that of two excellent British Latinists, Kenney and Reeve. Of course, this table might simply highlight the different relevance which the history of the discipline and discussions about methods have in North American textual criticism, but that does not substantially change its implications for methodological consciousness.

An analysis of the indexes of authors quoted in two important Dutch collections, both entitled *Studies in Stemmatology* (van Reenen and van Mulken 1996; van Reenen, den Hollander, and van Mulken 2004) would not yield all that different results. It is unnecessary to stress that a lack of interest in the history and methodological development of an academic discipline leads, very often, to the reinvention (sometimes in a wrong or imprecise way) of criteria and principles that have already been formulated perfectly well.

Tab. 2.4-1: Presence or absence of references to the lively European discussion on the genealogical method in the popular collection edited by Greetham (1995). A single plus sign (+) indicates 1 to 3 quotations, two signs (++) more than 3.

	Lachmann	Bédier	Quentin	Maas	Pasquali	Timpanaro	Kenney	Reeve	Tanselle
Tanselle: “The Varieties of Scholarly Editing”	++		+	+	+		+		+
Tarrant: “Classical Latin Literature”	+	+		++	++		++	++	
Edwards: “Middle English Literature”	+						+		
Speed Hill: “English Renaissance: Nondramatic Literature”	+			+					+
Howard-Hill: “English Renaissance: Non-Shakespearean Drama”	+								
Reiman: “Nineteenth-Century British Poetry and Prose”									++
Shillingsburg: “Nineteenth-Century British Fiction”									+
Myerson: “Colonial and Nineteenth-Century American Literature”									++
Speer: “Old French Literature”	+	++	+		+				
Campion: “Early Modern French Literature”		++							+
Cherchi: “Italian Literature”	++	++	+	+	+	+			
Bleuca: “Medieval Castilian Texts and Their Editions”			+						
Orduna: “Hispanic Textual Criticism and the Stemmatic Value of the History of the Text”	+	+	+	++	++	+			
Plachta: “German Literature”		+			+				
Rocher: “Sanskrit Literature”				+					

2.4.3 Some neo-Lachmannian contributions to the improvement of the method: A provisional list

In 2014, I proposed and commented on a small list of Bédier's contributions to the improvement of the genealogical method (Trovato 2014; see also Trovato 2017, 82–94). Here, I take up that list again, updating it and opening it up to neo-Lachmannism in the broader sense. Even if a thematic arrangement might perhaps be more convenient (and certainly less dangerous for the present writer), I prefer to follow the chronological order in which these updates and refinements were proposed, and I offer only a few, essential bibliographical references (of course, since I am far from all-knowing and this is, to my knowledge, the first list of this kind, it is very likely that some achievements had been made earlier and should actually be credited to different authors). Note that I do not try to identify the first time that a specific criterion was used, but the first time that the advantage of a procedure was highlighted from a general, methodological point of view. For a more detailed and systematic treatment of the most relevant terms and notions touched on here from a diachronic perspective, see Trovato (2017) and chapter 4 below, which addresses some of them from different points of view.

Notwithstanding their different focus and size, the conceptual distance between my list from 2014 and the present one is not as great as one might think. Indeed, even when Bédier is not explicitly mentioned, the shadow of his sharp criticisms almost always seems palpable. The first group of entries mostly concerns developments which we must credit to Pasquali (1932, 1934) and to Maas (1937). As for the latter, it should be remembered that Maas (1937) was added afterwards as an appendix to the subsequent editions of his 1927 *Textkritik* (Maas 1950, 1957, 1960) and included in all the translations of it. Thus, when we use any recent edition or translation of Maas's substantial booklet, including the English translation by Barbara Flower from which I usually quote in these pages (Maas 1958), we must consider that the text was written in 1927, but Maas (1937) was added as an appendix to the treatise of 1950 and a second appendix ("Retrospect 1956") was added to the 1957 version, almost without any changes to the main text in each case. In other words, these three parts (pp. 1–41, 42–49, 50–54 in the English translation) mirror different phases and focuses in the research of this great scholar.

1929: Recognition of limitations of the method (Rajna)

A few months after Bédier's famous attack against "la méthode de Lachmann", Pio Rajna (1847–1930), one of the oldest and more authoritative Italian Romance philologists, wrote the following words, which did not go unheeded: "Troppo poco si è badato alle cause perturbatrici, tali in moltissimi casi da rendere inapplicabile il sistema; e si è commesso il grave errore di procedere allo stesso modo in condizioni assai diverse" (Rajna 1929, 50) [We have paid too little attention to disturbing factors, such that could make the system inapplicable in a great number of cases, and

we have made the serious mistake of proceeding in the same manner under very different conditions]. A few examples of cases and kinds of texts for which the method is inapplicable or requires adaptation (works transmitted by one or a few manuscripts; works that are too short, such as sonnets; popular genres such as *chansons de geste* or *cantari*, or mediaeval Latin hagiographical texts) are listed in Trovato (2017, 155–161), but we could also add encyclopedias, chronicles, collections of short, practical texts, and so on. Perhaps, among the top-notch Italian textual scholars, it was the late Alberto Vârvaro (2004) who heeded Rajna’s concerns the most.

1932: The method must not be used with different authorial versions (Pasquali)

From Bédier to the present day, many scholars have warned that a flaw of neo-Lachmannism is the possible existence of authorial variants. But the distinction between the treatment of scribal and authorial variants has been clearly presented since at least Pasquali (1932). Of course, whenever it is possible to distinguish two or more different authorial versions, approved by the author at different times (e.g. Ariosto’s *Orlando Furioso* of 1516, 1521, and 1532; Chateaubriand’s three versions of his *Atala*; Whitman’s many versions of *Leaves of Grass*), scholars are not entitled to create a “texte unique et monstrueux” (Bédier 1913, xxxviii) by anti-historically merging variants belonging to the different versions. On the contrary, we must publish them separately or decide which version is more urgently to be made available to the community of readers. In fact, in the case of multiple authorial versions, it is not a matter of deciding which reading in every place of variation best represents the work of art, but of putting up for comparison two or more textual entities which, at different times, have reflected the author’s intention. This will allow critics to compare these different works and contrast their features and evolution.

After a few but very clear hints by Pasquali (1932, 1934), Contini’s seminal essay on Ariosto (1937), and the application of his “critica delle varianti” to Leopardi, Proust, and others, the treatment of authorial versions was addressed and refined by a scholar closely connected with Contini: Dante Isella. It must be emphasised that the Italian *critica delle varianti* is quite different from the French *critique génétique* (see Italia and Raboni 2010; Stussi 2015, chap. 5; 7.9 below).

1932–: Open vs closed recensions (Pasquali)

Nowadays, we frequently use the expressions “closed recension” and “open recension”. It is often forgotten that the author of this common distinction was none other than Giorgio Pasquali (the distinction is already found in Pasquali 1932):

Le riflessioni qui brevemente esposte [...] si applicano a ogni “recensione aperta”, se mi sia lecito introdurre qui un termine nuovo, che mi pare indispensabile, vale a dire si applicano ogniqualvolta la lezione dell’archetipo non si può fissare meccanicamente, mediante la consta-

tazione di coincidenze di lezione in certi apografi (“recensione chiusa”), ma si determina solo con il *iudicium*, scegliendo sul fondamento di criteri prevalentemente interni tra due (o più) lezioni nessuna delle quali è dimostrata secondaria dal criterio esterno, genealogico. (Pasquali 1934, 126)

[The reflections briefly outlined here [...] apply to every “open recension”, if I may introduce here a new term which seems to me indispensable, that is to say, they apply every time the reading of the archetype cannot be fixed mechanically through coincidences of readings in certain apographs (“closed recension”) but is determined only through *iudicium*, choosing on the basis of predominantly internal criteria between two (or more) readings, none of which is demonstrated secondary by the external, genealogical criterion.]

Of course, the main cause of open recensions is the distribution of the variants in the stemma: if in a certain place of variation no reading appears in the majority of primary branches, we must use *iudicium*.

1934—: Extension of the field of philological studies to areas which Maas 1927 had excluded (Pasquali, Frank, and others)

Pasquali’s crucial book (1934), which should be mandatory reading for everyone interested in textual criticism, was born as a review of Maas (1927). The best abstract of the book is found perhaps in Maas’s preface to the second German edition of *Textkritik*:

Die erste Auflage dieses Abrisses (erschienen 1927) wurde 1929 von G. Pasquali sehr eingehend und freundlich besprochen (*Gnomon* 5, 417 ff.). Die anschließenden selbständigen Forschungen Pasqualis (*Gnomon*, 5, 498 ff. und *Storia della tradizione e critica del testo* (1934)) bewegen sich vorwiegend auf nahverwandten, aber von meiner Darstellung ausgeschlossenen Gebieten, dem *der speziellen Überlieferungsgeschichte und dem der kontaminierten, also nicht methodisch entwirrbaren, Überlieferung*. (Maas 1950, 3; my emphasis)

[The first edition of this essay (published in 1927) was reviewed with great kindness and in great detail by G. Pasquali in 1929 (in *Gnomon*, V, 417 ff.), Pasquali’s own investigations (in *Gnomon*, V, 498 ff. and in his *Storia della tradizione e critica del testo*, 1934) deal in the main with topics closely related to but excluded from my presentation, i.e. with *the history of transmission of the individual texts and “contaminated” traditions, which of course cannot be systematically disentangled.*] (trans. Flower 1958, n.p.; my emphasis)

Among other things, Pasquali also addresses authorial variants and explains with a wealth of erudition why scholars cannot dismiss recent witnesses (*recentiores non deteriores*).

Frank pointed out that when we work with popular genres such as mediaeval *chansonniers*, the conditions of the transmission are quite different from the ideal conditions in which the genealogical method can work at its best:

La tradition que constituent les chansonniers lyriques du Moyen Âge apparaît, pour qui veut établir un stemma, comme grevée de tous les éléments de trouble: [1] original multiple (réel, virtuel ou possible), [2] variations et contaminations surgies de la transmission orale, [3] conta-

minations dues à l'utilisation par les copistes de plusieurs sources divergentes, [4] l'existence, enfin, dans les chansonniers mêmes, d'éditions résultant d'un travail réfléchi, usant de la conjecture. Que nous sommes loin de la transmission mécanique qui garantit l'efficacité du stemma! (Frank 1955, 472–473)

[The tradition constituted by the lyric *chansonniers* of the Middle Ages seems fraught with every sort of difficulty for the scholar who wishes to establish a stemma. (1) a multiple original (real, virtual, or possible); (2) variations and contaminations stemming from oral transmission; (3) contaminations due to the use of several divergent sources by the copyists; (4) and finally, the existence, in the *chansonniers* themselves, of editions resulting from thoughtful work, employing conjecture. How far we are from the mechanical transmission that guarantees the efficacy of the stemma!] (Frank 1976, 135)

In spite of this, some exemplary studies have shown that it is possible to approach the study of songbooks (*chansonniers*, *canzonieri*) in a rational way (e.g. Barbi 1915; Avalle 1985).

1934: Polygenetic vs monogenetic errors (Pasquali)

In order to single out significant tools for proving the relationship between copies (i.e. for inferring stemmata), it is important to distinguish between, on the one hand, unique innovations and, on the other hand, variants that copyists could produce independently of one another. The commonly used distinction between polygenetic and monogenetic innovations goes back to Pasquali: “Corruttele comuni a tutta la tradizione [...] possono essersi prodotte indipendentemente anche in mss. indipendenti, per ‘poligenesi’” (Pasquali 1934, 19) [Corruptions common to a whole tradition [...] may have occurred independently, even in independent manuscripts, by “polygenesis”]. Instead of “polygenesis”, other scholars use the more opaque terms “parallelism”, “coincident variation”, “convergent variation”, or “homoplasy”. See also the next point.

1937: Indicative errors and formulation of the basic rules for determining the relationship between two witnesses (Maas)

In his paper “Leitfehler und stemmatische Typen” [Indicative Errors and Stemmatic Types], Maas (1937) introduces two fundamental distinctions. The first opposition involves indicative vs non-indicative errors, the former being useful, the latter of no use, for inferring a genealogical relationship even if errors of both types belong to the set of substantial errors (for more on significant errors, see 4.3.1 below). By the way, Maas only discusses the “logical” requirements of the indicative error, which must be “so beschaffen [...], dass aller Wahrscheinlichkeit nach B und C nicht unabhängig voneinander in diesen Fehler verfallen sein können” (Maas 1950, 27) [of such a nature that it is highly improbable that B and C committed it independently of each other] (trans. Flower 1958, 43). The first requirement for an indicative error is, in Pasquali’s words, that of being monogenetic (see above). Second, the error must be really difficult to detect and correct.

The second opposition applies only to indicative errors and distinguishes between separative and conjunctive errors. The presence of a number of common errors (i.e. conjunctive errors) proves that two or more witnesses, *A B* ..., are part of the same group or family, while, say, indicative errors in witness *A* only (i.e. separative errors) show that *B*, which lacks them, cannot descend from *A*. Therefore, if two witnesses (say *A* and *B*) are connected by conjunctive errors, and both of them have separative errors, they must descend from a lost exemplar *y*. See Maas (1937) and subsequent editions, including Maas (trans. Flower 1958, 42–44).

As I have already noted, the wording of some sentences of Maas (e.g. the ones about conjunctive and separative errors) is very similar to that of Rajna (1907). Further research will show whether Maas (who indeed read a lot, even outside his own field of study, for example about Shakespeare or Bédier's theories) depends on Rajna or they both depend on a still unknown source. But all the "sources" of Maas's treatise, whether declared or implicit, deserve a specific study (Trovato 2017, 56). Conversely, the Maasian "rules" concerning the relationship among three witnesses (Maas, trans. Flower 1958, 44–49) do not hold water (Timpanaro 1981, 128–131 = 2004, 135–138 = trans. Most 2005, 162–166). See also "1937" and "2002" below.

1937: *Eliminatio codicum descriptorum* (Maas)

After the hints in Maas (1927, § 8a), a rigorous set of logical rules for the individuation of copies of preserved copies (*codices descripti*), that is, genealogically useless witnesses, is found in Maas (1937, later included in Maas 1950, 1957, 1960). Between any two witnesses *A* and *B*, only three kinds of relationship can exist – types 1, 2, and 3 in figure 2.4-1 below.

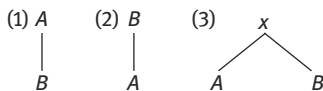


Fig. 2.4-1: The possible relationships between two witnesses *A* and *B*, according to Maas (1937).

There are two necessary conditions for type 1 (*B* derives from *A*) that must apply: *B* must contain all the significant errors of *A* and at least one that is not in *A* (obviously, a number of errors of the latter kind provide much more reliable proof than a single one). Likewise, for type 2 (*A* derives from *B*) to apply, *A* needs to contain all the errors of *B* plus at least one that is not in *B*. In both cases, we can distinguish between an exemplar, genealogically relevant, and a *codex descriptus*, genealogically of no use unless it preserves passages of the exemplar lost or destroyed after copying. See also "1958–" below.

1937–: First attempts to explain Bédier's so-called paradox (Maas and others)

The strongest argument against the genealogical method, known as Bédier's paradox, is the fact that, out of 110 stemmata of French manuscript traditions

Bédier examined, 105 were two-branched. According to Bédier, these figures were enough to prove that the method was inherently wrong. After Maas (1937) and Forquet (1946), other scholars also addressed the reasons why stemmata are so often bipartite in classical and Romance textual transmissions. Here, I mention at least Castellani (1957) and appendix C in Timpanaro (1961; appendix C is reprinted with updates in Timpanaro 1981; according to Reeve 1986, 69 = 2011a, 43, “there is no better warning against the pitfalls that may occur in classifying manuscripts”). More general and powerful explanations of the paradox were suggested later; see “1987–” below.

1938–: Recognition that, eventually, the method had been considerably updated and refined (Barbi)

Before presenting his brief criticism of Quentin’s and Bédier’s methods, Barbi notices:

Tutti sentono che il puro metodo lachmanniano è insufficiente e in certi casi inapplicabile [...]. Che si sia da molti e per molto tempo creduto di poter risolvere, ricorrendo al metodo del Lachmann, speditamente e con sicurezza di risultati ogni problema, può essere; ma ormai quel metodo s’è sviluppato, arricchito, adattato variamente ai diversi casi, e resta fondamentale nella critica del testo; e anche l’aggiunta di altri mezzi è subordinata spesso al principio degli errori comuni. Condannarlo dunque senz’altro perché non risponde sufficientemente, o non risponde affatto, a certe particolari necessità, è privarsi di un mezzo che in qualche caso risponde benissimo anche da solo o è il solo sicuramente applicabile, e in ogni indagine dà aiuti dei quali sarebbe dannosissimo fare a meno. (Barbi 1938, xvi)

[Everyone has the impression that the pure Lachmannian method is insufficient and in some cases inapplicable [...]. It is possible that many scholars for a long time believed they were able to solve quickly and with certain results every problem simply by applying Lachmann’s method; but by now that method has been developed, enriched, adapted variously to different cases, and remains fundamental to textual criticism. Even the addition of other means is often subordinated to the rationale of common errors. Therefore, to condemn it because it does not respond sufficiently, or does not fit at all certain particular needs, means to deprive ourselves of a means that in some cases responds very well even on its own, or is the only safely applicable one, and in every investigation offers aids which it would be harmful to do without.]

1946–: *Stemma codicum* vs “real tree” (Fourquet and others)

While the *stemma codicum* consists only of survivors, that is, the extant witnesses, Forquet noted in 1946 that textual scholars should also consider, despite it being a merely theoretical entity, the “real” or “complete tree” (Fr. *arbre réel*, *arbre complet*), that is, “l’albero genealogico di tutti i manoscritti di un dato testo che sono realmente esistiti” (Timpanaro 1981, 129 = 2004, 136) [the genealogical tree of all the manuscripts of a given text that really existed] (trans. Most 2005, 164), lost witnesses included. For a long time even excellent scholars did not distinguish between the two notions; after Fourquet (1946) and Timpanaro (1961, appendix C), see Guidi and Trovato (2004) and Trovato (2005; 2017, 65, 81–93, 138–146). I can add

now that, no differently from Bédier, Pasquali, and many others, Maas himself seems to confuse the real tree with the *stemma codicum*: “Ferner liegt es im Wesen der mittelalterlichen Überlieferung, dass bei wenig gelesenen Texten nur selten von demselben Archetypus drei Abschriften genommen wurden” (Maas 1937, 293 = 1950, 30) [It is in the very nature of the medieval tradition that in the case of little-read texts three copies were rarely taken from the same archetype] (trans. Flower 1958, 48). In reality, the fact that the stemma of a mediaeval work rarely has three or more branches does not mean that the shape of the real tree could not have had three or more branches stemming directly from the original.

1952: Recasting of the geographical criterion (Lachmann and others) as a criterion of peripheral areas (Pasquali)

Elaborating upon very recent developments in comparative linguistics (see 8.2) theorised by Matteo Giulio Bartoli, Pasquali formulated a criterion that can be particularly useful in the study of overabundant transmissions – see also the allusions in Pasquali (1932; 1934, 8, 160, 178 = 1952, 8, 160, 178):

Come nella linguistica è ormai pacifico che gli stadi più antichi si conservano più a lungo in zone periferiche, e che quindi coincidenza di due zone periferiche lontane l’una dall’altra in un fonema, una forma, un vocabolo, un costrutto garantisce la loro antichità, così anche coincidenza di lezione in codici scritti in zone lontane dal centro della cultura e lontane tra loro costituisce una presunzione per la genuinità di questa lezione. Spesso di testi molto letti sia nell’antichità, sia nel Medioevo, si è formata una vulgata che, come suole la moda, progrediva da un centro verso la periferia, ma non sempre la raggiungeva. (Pasquali 1952, xvii–xviii)

[Just as in linguistics it is universally agreed today that earlier stages are preserved for a longer time in peripheral areas, and that hence the occurrence of the same phoneme, form, term, or construction in two peripheral areas distant from one another guarantees their antiquity, so the agreement of codices written in areas far removed from the cultural centre and from one another constitutes an argument for the genuineness of a reading. Often texts that were much read, both in Antiquity and in the Middle Ages, form a vulgate text which spreads, as fashions are wont to do, from the centre towards the periphery, but does not always reach it.]

1955–: Diffraction (Contini)

The notion of *diffraction* is a refinement of the well-known criterion of *lectio difficilior* (see 6.2.3). It is a common experience that certain *places* of variation are surprisingly rich in readings. According to Contini, such richness often hinges on a particularly difficult or rare reading in the original, with which many scribes grappled and to which they reacted in different ways. The best criterion for addressing such richness is thus singling out the difficult variant that can best explain such a reaction by the copyists. This variant may still be preserved, but it may also no be longer present in the manuscript tradition. In Contini’s words: “C’è una progressione dalla diffrazione in presenza, dove un testimone [...] ha serbato la voce o forma relativamente rara, a quella in assenza, dove essa è rimasta documentariamente

stravolta” (1986, 102 = 2007, 2:989–890) [There is a progression from diffraction *in praesentia*, where a witness [...] has retained the relatively rare word or form, to diffraction *in absentia*, where this word or form has been lost in the transmission].

Nevertheless, diffraction “è sufficiente a legittimare una congettura *difficilior*” (Contini 1986, 102) [is sufficient to legitimate a *lectio difficilior* conjecture]. See also Contini (1955, 1977). Orduna (1995, 487) maintains that diffraction is “the most fertile development in the Lachmannian method since Pasquali”. Buzzoni and Burgio add that “diffraction pushes the philologist’s attention to focus less on the object (the identification/reconstruction of a ‘good reading’) than on the internal dynamism of the tradition, which is configured as a system of structures under tension” (2014, 174). A more detailed presentation of the concept of diffraction can be found in Trovato (2017, 117–124).

1961–: Research on contamination (Avalle, Segre, and Froger)

Maas’s treatise warns the reader that the stemma can settle “das Abhängigkeitsverhältnis der Zeugen für jede Stelle des Textes” [the relationship of witnesses for every passage in the text] under examination only “wenn jungfräuliche Überlieferung vorliegt. Gegen die Kontamination ist kein Kraut gewachsen” (1950, 31) [if we have a virgin [i.e. uncontaminated] tradition. No specific has yet been discovered against contamination] (trans. Flower 1958, 49), that is, against copies which show shared innovations with exemplars from two or more different families. But, when a text from the past was important and thus repeatedly reproduced and widely circulating, its tradition almost invariably became contaminated. Thus, textual scholars have no choice but to address contamination. Among the works on this subject, I mention only Avalle (1961) and Segre (1961; see 4.4 below).

A useful criterion for deciding which is the main source of a contaminated witness out of two possible exemplars was suggested later by Froger:

Le moyen de résoudre l’anomalie consiste à se fonder sur la fréquence relative [des accords] des groupes incompatibles, dont l’assemblage produit une irrégularité [...]. Pour choisir entre des groupes incompatibles, on accepte celui dont la fréquence est élevée, et l’on rejette celui dont la fréquence est faible; c’est-à-dire que l’on considère comme normal celui qui est engendré par une grosse collection de variantes et apparaît souvent, regardant comme anormal celui qui, engendré par une petite collection de variantes, n’apparaît que rarement. Ce faisant, on adopte l’interprétation la plus probable [...]. Étant donné par exemple les deux fréquences 15 et 1, on fait la somme $15 + 1 = 16$; la probabilité en faveur du groupe dont la fréquence est 15 sera $15/16 = 0,9375$, et celle du groupe dont la fréquence est un sera $1/16 = 0,0625$, soit en chiffres arrondis, 94 % et 6 % respectivement. (Froger 1968, 112–113)

[The way to solve this anomaly is to take as our foundation the relative frequency [of agreements] between the incompatible groups whose combination produces an irregularity [...]. To choose between incompatible groups, we accept the one whose frequency is high and reject the one whose frequency is low; that is to say, we regard as normal the one that is engendered by a large collection of variants and appears often, and regard as abnormal the one that is engendered by a small collection of variants and only appears rarely. By doing so, we are adopt-

ing the most probable interpretation [...]. Given, for example, the two frequencies 15 and 1, we sum them: $15 + 1 = 16$. The probability in favour of the group whose frequency is 15 will be $15/16 = 0.9375$, while that of the group whose frequency is 1 will be $1/16 = 0.0625$, that is, in round percentage figures, 94 % and 6 % respectively.]

Segre (1961) also introduced a useful distinction between contamination of readings and contamination of exemplars. The former “è conseguenza di una collazione eseguita sull’ascendente di un codice” (Segre 1961, 64 = 1998, 71) [is a consequence of collation [i.e. of at least one collation] performed on the ancestor of a codex]. In contrast, the latter occurs as follows:

quando un copista, o per integrare un esemplare incompleto, o perché imbattutosi in un esemplare più leggibile o autorevole, trascrive alternativamente da due esemplari, la sua copia appartiene, alternativamente, a uno solo dei gruppi di provenienza dei due esemplari. (Segre 1961, 63–64 = 1998, 71)

[when, in order to fill in gaps in an incomplete exemplar, or because he has chanced upon a more legible or authoritative one, a copyist alternately transcribes from two exemplars, his copy belongs, in turn, to only one of the groups that the two exemplars belong to.]

Dutch stemmatologists use the term “simultaneous contamination” for the contamination of readings and the term “successive contamination” for contamination by juxtaposition of exemplars; this terminology is also used below (4.4). The problem was studied more deeply by Vårvaro (2010). Tonello and Trovato (2011) showed that, in the case of a long and popular text such as Dante’s *Commedia*, the replacement of one exemplar with another manuscript is a feature present in more than 15 % of the manuscript transmission. A more detailed presentation of this issue is found in Trovato (2017, 128–134). It should be noted that, whenever we identify contaminated witnesses of this kind, it is convenient to use two or more slightly modified sigla in order to refer to the sections of the text that depend on different exemplars (e.g. *A'*, *A''*, *A'''*): in this way, ambiguity and useless complications both in studying the genealogy and drawing the stemma can be avoided.

1961–: Application of the genealogical method to *cantari* and other popular genres, to theatre, and to opera librettos (De Robertis and others)

The genealogical method can also be applied, with some adaptations, to common portions of texts belonging to popular literary genres such as *cantari*, *chansons de geste*, and the like, which are often transmitted in versions of different length and content. As De Robertis points out:

La filologia redazionale [...] non è, per intenderci, la filologia del codex optimus; e naturalmente non prescinde dall’esperienza e dagli strumenti della più affinata tecnica ricostruttiva [sc. quella neolachmanniana]. Solo che quell’esperienza va trasferita entro una nuova realtà [...], questi strumenti hanno bisogno di essere riadattati ai nuovi oggetti e alle nuove esigenze. (De Robertis 1961, 124–125)

[Redactional philology [...] is not, to put it in plain terms, the philology of the *codex optimus*, and of course cannot do without the experience and tools of the more sophisticated reconstructive technique [i.e. the neo-Lachmannian method]. It is just that this experience needs to be transferred into a new reality [...], and these tools need to be readapted to new objects and new needs.]

For an example, consider the analysis of the transmission of Pucci's *Reina d'oriente* by Bettarini Bruni and Trovato (2009), summarised in Trovato (2017, 200–207). The method can be applied, with analogous adaptations, also to theatre plays (see Tissoni Benvenuti 1986; Riccò 1996). In order to allow a thorough study of the reception of *Il Turco in Italia* by Romani (libretto) and Rossini, the method was successfully applied also to around thirty librettos of the opera printed between 1814 and 1830 (Nicolodi and Trovato 2003).

1963: Extra-stemmatic (or extra-archetypal) contamination (Timpanaro)

There are cases where conjunctive errors indicate that a witness belongs to a clearly identifiable subset of manuscripts. However, some of its readings, although they cannot be found in the other manuscripts of the subset, or in any other area of the known tradition, have to be considered authentic – even after a careful examination. As Timpanaro observes:

Vi sono lezioni giuste che nessun copista filologo medievale (in certi casi nemmeno il miglior filologo moderno) può raggiungere per congettura. Un pericolo più grave consiste nell'eventualità che un copista, per es., del ramo α [...] abbia risanato errori non per congettura e nemmeno attingendo a uno degli altri testimoni a noi giunti, ma collazionando un codice di un ramo σ di una tradizione del tutto diversa, andato poi perduto. Casi in cui bisogna ricorrere a questa ipotesi sono citati in buon numero nel libro di Pasquali [...]. Nell'articolo in "Maia", XVII (1965), che ho già avuto occasione di citare, ho proposto (p. 397) di usare per questo fenomeno il termine di "contaminazione extrastemmatica" (cioè derivante da manoscritti che non fanno parte della tradizione a noi giunta più o meno integralmente). (Timpanaro 1981, 152–153)

[There are correct readings at which no mediaeval copyist-philologist (in certain cases not even the best modern philologist) could arrive conjecturally. A more serious danger consists in the possibility that a copyist, for example, of the α branch [...] might have healed errors or filled lacunas not by conjecture and not even by checking one of the other witnesses that have survived to our day, but by collating a manuscript of a completely different branch or tradition which was later lost. In his book Pasquali cites many cases in which one must have recourse to this hypothesis [...]. At Timpanaro 1965: 397 I suggested designating this phenomenon by the term *extra-stemmatic contamination* (that is, contamination deriving from manuscripts that do not form part of the tradition that has survived more or less completely).] (trans. Most 2005, 179)

Other scholars have alternatively suggested designating this phenomenon as *extra-archetypal contamination*. See Trovato (2017, 134–138). Figure 4.1-6 in section 4.1 provides an example of the phenomenon.

1963–: Partial obscuring of a hyparchetype or the archetype (Timpanaro)

In some cases, a thorough review of the distribution of errors in the various families shows that some copies which offer some genuine or at least good readings belong to subfamilies full of innovations. In such cases, the good readings cannot go back to the archetype through the ancestors of a family: we are facing the obscuring of a hyparchetype or the archetype. This means that these copies owe their good or genuine reading(s) to conjectural emendation or to contamination with a lost witness (see “1963” above), and that our reconstruction of the relationships between the most important branches of the stemma, as well as the textual reconstruction, could become gravely biased unless we do not collate a very rich set of *loci*. While correct deductions about this bias can already be found in the seminal edition of Paris (1872), a recent presentation of the problem is found in Timpanaro (1981, 143–147 = 2004, 153–157 = trans. Most 2005, 179–184). The problem is also analysed in Trovato (2017, 147–154).

1970: Active manuscript traditions vs quiescent manuscript traditions (Vàrvaro)

In 1970, Vårvaro published a brilliant article in which, starting from Fränkel (1964), he compared from many perspectives the practices of classical scholars and Romance philologists. Among other things, he distinguished between two different scribal attitudes, the one more respectful of the text and the other more prone to adaptations and modernisations:

Quella di opere latine e greche è in genere una tradizione libraria poco folta nel settore fra archetipo e copie umanistiche [...]; è una tradizione di ambienti limitati, di professionisti (copisti o a volte studiosi) tendenzialmente rispettosi del testo trådito: una tradizione che chiamerei *quiescente*. Le tradizioni di testi romanzi sono già a prima vista assai diverse per la minima distanza che intercorre tra autografo e archetipo (se pur questo esiste) e per quella assai ridotta fra questo e i testimoni conservati [...], la posizione del copista rispetto al testo è infine assai meno rispettosa: un tipo di tradizione che chiamerei *attiva*. (Vårvaro 1970, 86 = 2004, 580; emphasis in original)

[The manuscript tradition of Latin and Greek works is generally not very plentiful in the space between archetype and humanistic copies [...]; it is a tradition of very specific milieus, of professionals (copyists or sometimes scholars) that tend to be respectful of the written text: this is a kind of tradition that I would call *quiescent*. The traditions of Romance works are very different at first sight in the minimum distance between autograph and archetype (if one exists) and in the very small distance between this and the preserved witnesses [...]; the position of the copyist towards the text is, finally, much less respectful: it is a type of tradition that I would call *active*.]

Of course, it is obvious that, in the above contrast, “Romance works” can be substituted with “works written in vernacular languages in general”. Nevertheless, as Vårvaro adds further on, this is simply a polarised framework that does not exclude, in different times and environments, intermediary forms of both types, even in Latin or Greek.

1970–: The concept of confirmatory readings (Vårvaro, Divizia, and others)

Indicative errors in the Maasian sense can be very difficult to find. Thus, some editors erroneously use lists of variants, which can by no means replace errors. Nonetheless, they can be useful for confirming textual relationships. In 1970, Vårvaro noted:

L'errore debolmente congiuntivo è intrinsecamente poligenetico, sicché in teoria sia la serie breve che quella ampia [di errori debolmente congiuntivi] potrebbero essere casuali, ma è evidente che ciò è tanto meno probabile quanto più la serie è lunga [...]. Questa labilità dell'errore, a sua volta, non è che una conseguenza dello stato "attivo" della tradizione [dei testi romanzati], che non tollera a lungo guasti senza tentare di ripararli in qualche modo, col risultato, spesso, di confondere la situazione testuale. (Vårvaro 1970, 95 = 2004, 589–590).

[The weakly conjunctive error is intrinsically polygenetic, so that in theory both the short and the long series [of weakly conjunctive errors] could be random, but it is evident that, the longer the series, the less likely this is [...]. This unreliability of the error, in its turn, is nothing but a consequence of the "active" state of the tradition [of Romance texts], which does not tolerate flaws for long without trying to repair them somehow, often with the result of confusing the textual situation.]

As pointed out above, it would be easy to object, with Maas or Luciano Canfora, that, if we do not have indicative errors, we cannot reconstruct a sound genealogy. But the question is quite different if at least a few indicative errors do exist. Paolo Divizia remarks:

quanto più ci si allontana dai [...] punti in cui si riscontrano gli errori guida, tanto minore è la probabilità che i rapporti tra i testimoni rimangano gli stessi. Per questa ragione una serie identica di innovazioni poligenetiche di poco peso distribuite su tutta l'opera dà maggiori garanzie, nella costruzione di uno stemma codicum, rispetto a pochi errori monogenetici evidenti concentrati in una sola parte del testo. (Divizia 2009, 46–47)

[the further one moves away from the [...] places where indicative errors are found, the lower the probability that the relationships between the witnesses remain the same. For this reason, an identical series of polygenetic innovations of little weight distributed over the whole work gives greater guarantees in the construction of a *stemma codicum*, than a few evident monogenetic errors concentrated in a single part of the text.]

Other scholars choose to connect these useful observations even more strictly to Maasian orthodoxy, maintaining that a series of weakly conjunctive errors added to a few indicative errors allows us to confirm that the relationship between the copies is the same in any passage of the work:

Especially in areas [of the copies] that have few or no significant errors, it is best to supplement them with an adequate number of confirmatory readings as a control [...], which will serve the purpose of orienting judgment in the case of dense contamination. (Trovato 2017, 117)

See also Divizia (2011, esp. 63–71).

1976: The concept of diasystems (Segre)

Segre (1976) proposed applying to textual criticism the linguistic concept of the dia system, which had been introduced by Weinreich in his classic work *Languages in Contact* to indicate a linguistic system which is a compromise between two systems that are in contact. As Segre underlines:

Se è vero [...] che i concetti di variante, errore, lezione equipollente rientrano nei due insiemi complementari di lezioni conservate e lezioni innovate, l'individuazione del sistema stilistico proprio di ogni copista fornisce il filologo di un nuovo strumento di analisi. Non gli errori soltanto, infatti, permetteranno di cogliere l'affinità genetica tra due o più manoscritti, ma anche l'appartenenza di questi manoscritti a uno stesso sistema stilistico diverso da quello realizzato nell'opera [...]. Questo criterio diventa particolarmente fruttuoso se applicato a testi nei quali si incontrino, piuttosto che errori, vere e proprie rielaborazioni, come le *chansons de geste*. (Segre 1976, 283 = 1979, 59)

[If it is true [...] that the concepts of variant, error, equally acceptable reading fall into the two complementary sets of preserved readings and innovations, the identification of the stylistic system of each copyist provides the philologist with a new analytical tool. Not only errors, in fact, will allow us to grasp the genetic affinity between two or more manuscripts, but also the belonging of these manuscripts to the same stylistic system different from that realised in the work [...]. This criterion becomes particularly fruitful if applied to texts like the *chansons de geste*, in which re-elaborations are encountered more frequently than errors.]

Perhaps we can recall here the lucid remark of Maas, who noted: “Den Kern fast jedes textkritischen Problems bildet eben ein stilistisches, und die Kategorien der Stilistik sind noch viel ungeklärter als die der Textkritik” (Maas 1950, 24–25) [The core of practically every problem in textual criticism is a problem of style, and the categories of stylistics are still far less settled than those of textual criticism] (trans. Flower 1958, 40–41). Nevertheless, the current growing availability of digitised texts and rich textual databases greatly facilitates stylistic analysis.

On a more general level, Paolo Divizia has kindly pointed out to me a paper by Segre (1978) which underlines, among other things, that neo-Lachmannism (though not mentioned as such) owes a lot of its refinements to key concepts of structural linguistics, such as the notions of the system and of paradigmatic and syntagmatic relations.

1981–: *Emendatio ex fontibus* (Orlandi, Brambilla Ageno, Maggioni, and Del Popolo)

Within some fields of research (mediaeval Latin, Old French, Nordic and Germanic philology), singling out indicative errors can be quite difficult due to a high degree of loss and fragmentation in the manuscript material, and the compilatory nature and anonymity of many works, that is to say, the lack of strong authorial marks more common in some other fields. In these very fields, the *Quellenforschung* of our positivist grandfathers can play an important role, offering precious clues for distinguishing between preserved readings and innovations:

Anche quando è possibile dimostrare obiettivamente, per via di varianti, che una famiglia sia migliore di un'altra, ossia contenga meno corrotte, ciò non ha pratica utilità a risolvere i problemi posti dal singolo passo in cui esse divergono. Qui possono valere soltanto i noti criteri interni; campo nel quale, quanto più dall'antichità ci si addentra nel medioevo, si ha a disposizione uno strumento di verifica che in generale il filologo classico non ha: le fonti dell'autore. Il caso più ovvio è rappresentato dalle traduzioni. In innumerevoli passi l'editore ha potuto decidere a favore dell'una o dell'altra classe di mss. della versione latina di Giuseppe Flavio [...] tenendo d'occhio l'originale greco [...]. *A fortiori* il criterio è valido per l'emendatio [...]. La scoperta di una fonte – specialmente di un modello d'imitazione letteraria, come ci ha insegnato in analoghi casi il Mariotti – serve proprio a correggere il testo. Ciò è stato fatto da Dag Norberg per la *Vita ritmica* di s. Zeno, emendata appunto sulla base dell'opera prosastica di cui pare rielaborazione [...] e per talune poesie di Paolino di Aquileia, mediante il confronto con passi di poeti cristiani antichi cui il carolingio si era rifatto [...]. La ricerca delle fonti ha quindi fondamentale importanza anche per la critica testuale, e per tanti autori resta ancora in buona parte da fare. Talora, anzi, la lezione della fonte può funzionare come “terzo ramo” di uno stemma altrimenti bifido e “chiudere” una *recensio* per sé aperta. (Orlandi 1981, 336 = 2008, 7–8)

[Even when it is possible to demonstrate objectively, by means of readings, that one family is better than another, that is to say, contains fewer errors, this is not of practical use for solving the problems posed by a given passage in which they diverge. Only known internal criteria can apply here; this is a field in which, as one moves from Antiquity and enters the Middle Ages, one acquires a verification tool that, in general, the classical philologist does not have: the sources of the author. The most obvious case is represented by translations. In countless places, the editor was able to decide in favour of one or the other family of manuscripts of the Latin version of Josephus [...] by keeping an eye on the original Greek [...]. *A fortiori* the criterion is valid for *emendatio* [...]. The discovery of a source – especially of a model of literary imitation, as Mariotti has taught us in similar cases – serves precisely to correct the text. This has been done by Dag Norberg for the rhythmic life of St Zeno, corrected precisely on the basis of the prose work which seems to have been reworked [...], and for certain poems by Paulinus of Aquileia corrected by means of comparison with passages from ancient Christian poets whom the Carolingian poet had reworked [...]. The study of sources is therefore of fundamental importance also for textual criticism, and for many authors still remains to be done. Sometimes, the reading of the source can function also as the “third branch” of an otherwise two-branched stemma and “close” a *recensio* that appeared per se to be “open”.]

See also Brambilla Ageno (1986), Maggioni (1994), Orlandi (1995, 4 = 2008, 100), and Del Popolo (2001). This method has been fruitfully applied to the edition of Old Norse sagas by Bullitta (2017), who systematically compared the witnesses of *Niðrstigningar saga* using the Latin source text underlying the work.

1982–: Deepening of the concept of the archetype (Weitzman, Reeve, Guidi and Trovato)

The genealogical concept of the archetype introduced and used by Lachmann and some of his contemporaries was, and still can be, rather difficult to handle because the new technical meaning of the term (a lost manuscript on which the extant transmission depends) overlaps with the classical and humanistic meaning of the word (an “official text” checked by the author and intended to be published

afterwards in further copies), thus causing ambiguity or misconceptions. Reeve observes: “Since the Renaissance, when scholars at work on the text of Greek and Latin authors took it [*archetypus*] up, the classical term [...] has been used in so many senses that no-one today can safely use it without defining it” (1985, 193 = 2011a, 107).

Thus, even highly experienced scholars appear not to have broken completely free of the classical meaning of the term “archetype”, failing to view the archetype as a manuscript whose existence is “by chance” detected by philologists within the stemma, and treating it instead as an especially authoritative exemplar or as the result of a sudden and inexplicable bottleneck in the ancient and mediaeval tradition whereby only one copy survived. To provide just one example of this line of reasoning, even the very competent Pasquali notes: “A chi ben consideri deve sembrare inverosimile che ogni volta di ciascun’opera tuttora superstita si fosse salvato nel Medioevo (occidentale e bizantino) un solo esemplare, mentre tutti gli altri erano andati a fondo con la caduta della civiltà antica” (Pasquali 1934, 15) [On careful consideration, it must appear unlikely that, each time, only one exemplar of each surviving work had been saved in the Middle Ages, whether Western or Byzantine, while all the others had perished with the fall of ancient civilisation].

The studies on two-branched stemmata and the loss of manuscripts by Weitzman (1982, 1987) and Guidi and Trovato (2004) allow us to explain the concept in a different and very simple way. As I have already pointed out,

tracing a tradition back to an archetype dating, say, from the fourth century, does not at all mean that “in antiquity” (or in the Middle Ages, or in the early modern period) a single witness of our text was preserved, or a single copy that was authoritative for one reason or another. What it means is that the witnesses available today do not allow modern philologists to trace their way any further back than a given manuscript (usually lost), often far removed from the original, and sometimes datable with fairly reasonable approximation. [...] In Latin and Greek classics, the archetype is often from the age of Charlemagne, so what has disappeared is not just the first four or five generations of witnesses, but – with very rare exceptions – the whole manuscript tradition preceding the ninth or tenth century AD. (Trovato 2017, 66)

The following diagrams from Weitzman (1982) can help to explain how a copy can become the archetype, for they depict different phases of a simulated manuscript tradition. In Weitzman’s own words:

Omega represents the lost original. All manuscripts alive at the stated time are shown, without any ring, except that four codices descriptive in the final population (“sons” of 61 and 95, another “son” of 95 and its own “son”) are omitted. Manuscripts fully ringed are dead; many other dead manuscripts are omitted. A dotted ring indicates a dying manuscript. (Weitzman 1982, 59)

As noted above, the four diagrams in figure 2.4-2 depict four different phases of a manuscript tradition. It is easy to understand that, if today we could work with the witnesses extant in 1287, our knowledge of the archetype, that is “13”, would be more sound than that obtained by working with the witnesses found in the final

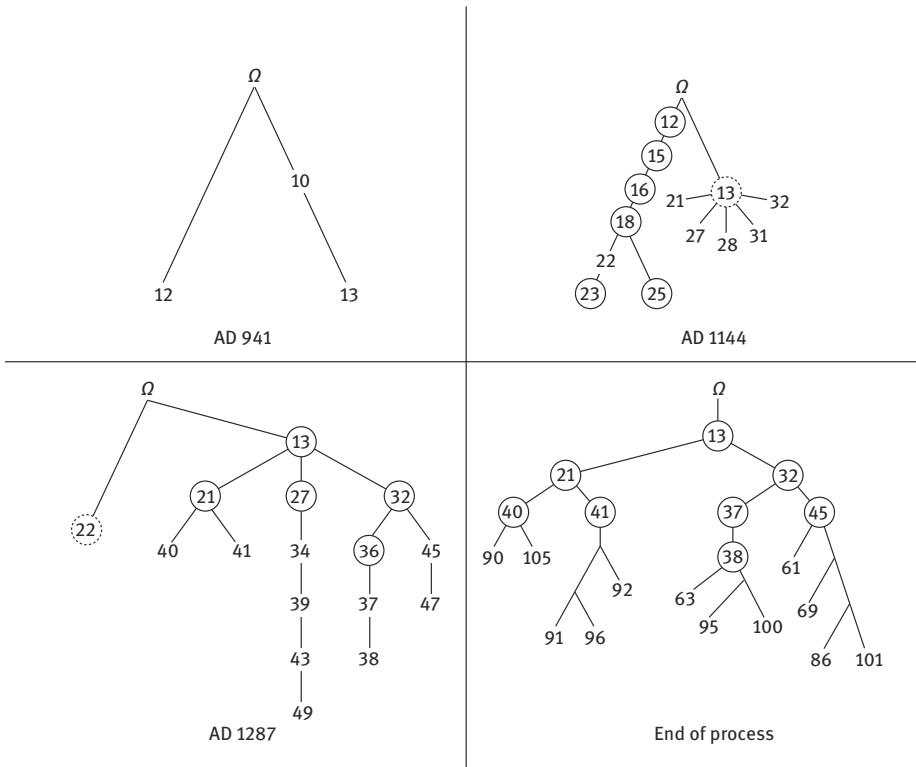


Fig. 2.4-2: Diagrams from Weitzman (1982, 59). The diagrams represent four phases of one of the artificial transmissions produced by means of software. I have corrected the last figure (“End of process”) as per Weitzman (1987, 289).

diagram because we would still have three lines of descent from it. At the end of the process, in 1500, one of these lines is completely extinct, so we would be in trouble whenever the two surviving branches differ. What if we imagine that, both in 1287 and at the end of the process, due to a severe loss rate, only some copies that depend on “32” had survived? It is readily apparent that in this case the archetype that textual scholars could reconstruct would be “32” and not “13”. Therefore,

the archetype of the stemma has nothing to do with the history of the tradition (official copies, if any; copies commissioned for circulation by the author himself, etc.), but only with the ensemble of manuscripts that happen to be available today, used by the philologist in the stage of *recensio*. Textual critics should only use the word *archetype* to designate the point in the stemma beyond which the surviving tradition does not allow them to reach. (Trovato 2017, 66)

The concept of the archetype is further discussed in sections 4.1 and 4.2 below. In order to analyse in depth this recent process of clarification of the notion, the reader can consult Weitzman (1982), Reeve (1986), Weitzman (1987), Guidi and Trovato (2004), and Trovato (2005).

1985–: Additional criteria for *eliminatio codicum descriptorum* (Timpanaro, Reeve, and others)

In the wake of Maas (1927, § 8a) and Pasquali (1932; 1934, 30–34), Timpanaro, Reeve, and others maintain that physical evidence is an important clue for proving that a manuscript is a *codex descriptus*, and not simply a relative of another witness to which it is very close but in relation to which it cannot be definitively positioned. In the words of Reeve’s most important work on this topic:

Physical evidence is any peculiarity of a witness other than its reading that accounts for an innovation in another witness. The most familiar examples are physical changes, especially damage or misbinding: a tear in a Beneventan manuscript of Apuleius accounts for gaps in many later manuscripts, and Politian demonstrated in two traditions, those of Cicero’s *Ad familiares* and Valerius Flaccus, that transpositions in the majority of manuscripts had their origin in extant manuscripts where leaves were misplaced. Perhaps the most familiar example of all is a physical accretion, the speck of straw in L of Euripides that the scribe of P reproduced as punctuation before it came away in 1960 under the heat of Zuntz’s lamp and the finger of a librarian [...]. A different form of physical evidence, not created by later accidents [...], is peculiarities of layout. (Reeve 1989, 10–11, 13 = 2011a, 152, 155)

See also Timpanaro (1981) and Orlandi (1995 = 2008, 63–94).

1987–: New attempts to explain Bédier’s so-called paradox (Weitzman and others)

In the wake of Weitzman (1987; see “1982–” above), Guidi and I addressed the problem of the modifications that a real, or complete, tree (see “1946–” above) may exhibit after more or less severe loss of witnesses. We used the stemma of a relatively rich printed tradition from the sixteenth century (Sannazaro’s *Arcadia*) as the model for a real tree (see fig. 2.4-3).

Then, we decided to decimate this model tree between 10 % and 90 %. I quote here from my handbook the summary of the results of this experiment:

Assuming a not too slender three-branched real tree, including some thirty witnesses, and – as is very often the case with the stemmata of the most diverse works – more or less markedly asymmetrical, low rates of decimation (from 10 to 30 %) do not result in very significant modifications. High decimation rates (70, 80, 90 %), however, result in:

- a’) a clear-cut increase in the probability (varying from case to case, but not inferior to 60 % in the traditions Guidi and I studied) that the tree will lose some of its flimsier branches, turning into a two-branched stemma;
- b’) a high probability (varying from case to case) that this two-branched stemma will be drawn up from what are actually descendants of a single branch (the more luxuriant one) of a multipartite real tree.

The prevalence of two-branched stemmata thus depends on the intensity of decimation which in its turn depends on T, that is, as I said above, the time that elapsed between the early transmission of a given text and the genealogical classification of its surviving copies. (Trovato 2017, 92)

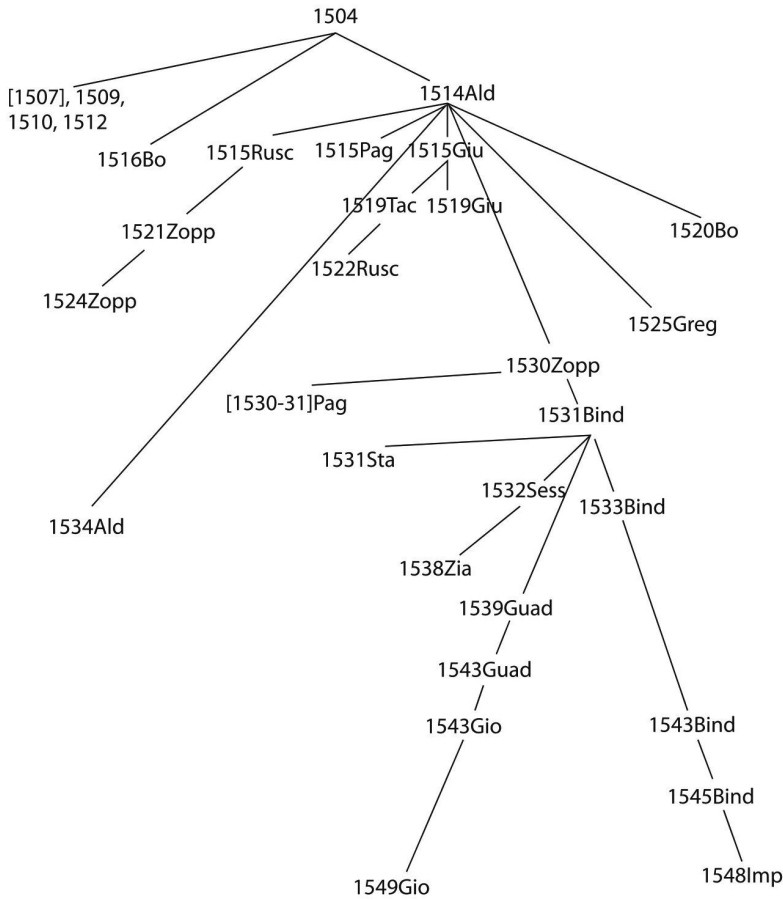


Fig. 2.4-3: The genealogical tree of a printed text (Sannazaro's *Arcadia*, second redaction), which is used as a model for a real tree and subjected to increasing decimation rates by Guidi and Trovato (2004, 23).

Several scholars (e.g. Divizia 2009, 42; L. Leonardi 2015) objected to our conclusions that a printed transmission is quite different from a manuscript tradition in many features. In my opinion, this point deserves close attention. Scientific modelling means generating a physical, conceptual, or mathematical representation of a real phenomenon that is difficult to observe directly. A theoretical model does not need to reproduce all the features of the phenomenon it intends to analyse. On the contrary: “Il problema fondamentale [nella scienza sperimentale] è stato quello di individuare per ogni fenomeno pochi dati giudicati rilevanti, decidendo di trascurare tutti gli altri” (Dalla Chiara and Toraldo di Francia 1999, 4) [The fundamental problem [in experimental science] has been to identify for each phenomenon the few data deemed relevant, deciding to neglect all the others]. For further details, I refer

to Weitzman (1987) and Guidi and Trovato (2004), where the relationship between the loss of witnesses and the morphology of the stemma is studied in greater depth.

1995: *Interpretatio vs iudicium*: On the correct interpretation of Lachmann’s “recensere sine interpretatione et possumus et debemus” and of the term *iudicium* (Orlandi)

Many scholars have criticised Lachmann’s “recensere sine interpretatione et possumus et debemus” (1842–1850, 1:v) [we can and must undertake *recensio* without interpreting], often without even having read the context of the statement. In a lucid essay, Orlandi re-evaluated a good deal of Lachmann’s bequest and analysed this well-known sentence in depth. After a close reading of Lachmann’s words, he remarked:

Dovrebbe essere chiaro [...] che il mantenersi al di qua dell’*interpretatio* non significa limitarsi a un lavoro meramente meccanico (quasi che la *recensio* e la conseguente *constitutio textus* non richiedessero scelte coscienti), bensì bandire, per quanto possibile, gli apprezzamenti personali sul pensiero dell’autore (o sulla sua poesia) per attenersi rigorosamente a ragioni oggettive: corrottele certe e indubitabili, lacune del testo, trasposizioni e così via. L’alternativa all’*interpretatio* è il *iudicium*: occorre rifuggire dalle scelte testuali fondate su preferenze individuali, e “giudicare” solo con l’ausilio dei fatti [...]. Perciò la polemica anti-lachmanniana, tante volte ripresa, a favore dell’interpretazione che tutto pervade, dipende in ultimo da un equivoco terminologico. (Orlandi 1995, 13 = 2008, 106)

[It should be clear [...] that abstaining from *interpretatio* does not mean limiting oneself to a merely mechanical task (as if the *recensio* and the consequent *constitutio textus* did not require conscious choices), but banning, as far as possible, personal appreciation of the thought of the author (or his poetry) in order to strictly comply with objective reasons: certain and undoubted corruptions, lacunae of the text, transpositions, and so on. The alternative to *interpretatio* is *iudicium*: we must avoid textual choices based on individual preferences, and “judge” only with the help of the facts [...]. Therefore, the anti-Lachmannian controversy, claiming that interpretation is always present (so often repeated), is ultimately based on a terminological misunderstanding.]

2002–: Indicative errors again (Chiesa and Divizia)

Returning to Maas’s contrast between separative and conjunctive errors, Chiesa noted:

In pratica le innovazioni che effettivamente servono alla ricostruzione dello stemma sono quelle che identificano i gruppi in modo univoco. Quelle che possono essere poligenetiche non realizzano questa condizione perché la stessa innovazione può riferirsi a più di un gruppo; quelle che possono essere reversibili non la realizzano perché anche testimoni che non le riportano possono far parte del gruppo. (P. Chiesa 2002, 68)

[In practice, the innovations that are actually needed to reconstruct the stemma are those that identify the groups in a unequivocal way: those that could be polygenetic do not satisfy this condition because the same innovation may refer to more than one group; those that could be

reversible do not satisfy it because even witnesses that do not contain them could be part of the group.]

Elaborating on the categories introduced by Chiesa, Divizia observes (the table in question is translated as table 2.4-2 below):

I concetti di congiunzione e separazione stanno dunque su piani diversi e non contrastanti, così come le categorie di monogeneticità/poligeneticità e irreversibilità/reversibilità da cui dipendono, che possiamo rappresentare – secondo il loro combinarsi – nella seguente tabella: (Divizia 2011, 58–59)

[The concepts of conjunction and separation are therefore on different and non-contrasting levels, as are the categories of monogeneticity/polygeneticity and irreversibility/reversibility on which they depend, which we can represent – according to their combination – as displayed in the following table.]

Tab. 2.4-2: A table recording the extreme poles of a continuum of the different kinds of innovation, translated from Divizia (2011, 59).

	irreversible innovations ("separative errors")	reversible innovations
monogenetic innovations ("conjunctive errors")	monogenetic irreversible (MI)	monogenetic reversible (MR)
polygenetic innovations	polygenetic irreversible (PI)	polygenetic reversible (PR)

Divizia also adds:

Da quanto si è detto, si può vedere che le opposizioni monogeneticità/poligeneticità, irreversibilità/reversibilità e evidenza/adiaforia, non sono opposizioni booleane, ma rappresentano piuttosto i poli estremi di un *continuum* che prevede una svariata gamma di sfumature intermedie. (Divizia 2011, 59–60; emphasis in original)

[From what has been said, we can see that the oppositions between monogenetic and polygenetic innovations, irreversible and reversible innovations, unacceptable and equally acceptable innovations are not Boolean contraries, but rather represent the extreme poles of a *continuum* that provides a wide range of intermediate shades.]

2004: On the loss rate of mediaeval traditions (Guidi and Trovato)

“What fraction of the total number of manuscripts at one time in existence is represented by those that survive: is it 50 %, or 20 %, or less?” This quotation is from Reynolds (2000, 3). In 2004, I tried to address, at least partially, this crucial question by looking outside mediaeval manuscript traditions. I worked on the few printed books of the fifteenth and first half of the sixteenth century for which I could find the numbers of the initial prints, excepting booklets of a few folios (too thin to be easily preserved).

Loss ranges from 73 % for the parchment copies of the *Decretales* by Gregory IX and 76.9 % for Poggio's *Historia* to 100 % for some especially popular chivalric poems. Unsurprisingly, every edition has its own distinctive history. The only conclusion that can be drawn from these percentages is that, although the books in question were printed only a few decades before book-collecting spread amongst the European aristocracy and high bourgeoisie [...] and although an unknown but certainly significant number of early editions are still in private collections (but this is also true of manuscripts), natural calamities (including mice and bookworms), various ways of recycling parchment and paper, fires (including those started intentionally for ideological reasons, from Savonarola to the Inquisition, the Nazis, and Serbian nationalists), plundering, bombings, and mere use seem to have done away with the majority of early European printed production in the brief space of 500 years. I do not see valid reasons to imagine that the manuscripts of classical or medieval authors, which were exposed to the same agents for even longer periods, stood higher chances of survival. On the contrary, the fact that every printed book is produced in n copies, while manuscripts are unica, suggests that, with the increase of T [that is Time, Temporal distance], losses among handwritten texts were even more dramatic. (Trovato 2017, 108, based on Guidi and Trovato 2004, 27–29)

Loss rates are smaller in subsequent centuries. Thus:

The probability that one of the many (and converging) factors in the possible destruction of a book, whether handwritten or printed (fires, floods, war, mold, use ...), will cause its loss increases proportionally to the temporal distance (T) between us and the early copies. (Trovato 2017, 135)

As a consequence of this very high loss rate, there is a high probability that surviving mediaeval textual traditions stem solely from witnesses belonging to some vulgate tradition (see Guidi and Trovato 2004; “1987–” above).

2009: Multi-text codices and cluster philology (Divizia)

The genealogical method, as is well known, does not work with very short texts (lyric poems, letters, and the like), which as a rule do not contain enough indicative errors to reconstruct a stemma. Elaborating upon studies by Barbi, De Robertis, Reeve, and others, Paolo Divizia theorised that not-obvious clusters of texts collated as a unique relatively long text could permit the determination of filiation. See Divizia (2009, 2017).

2019: Methods for studying scribal behaviour and scribal habits (Marchetti)

One of the aims of the so-called New Philology is the assessment of scribal behaviour, but the studies published so far do not provide relevant information. The Colwell method, named after Ernest C. Colwell (1901–1974), proposes, once a manuscript is chosen, attributing to the copyist all its *lectiones singulares* without attempting to make any distinction between group and individual innovations. Elaborating upon tenets of genealogical textual criticism such as *eliminatio codicum descriptorum* and combining the rationales of common errors and codicological evidence (see “1937” and “1985–” above), Marchetti studied in his PhD thesis five pairs

of an exemplar and its copy (*exemplar* and *descriptus*) of Dante's *Commedia* belonging to different areas, years, and graphic models. The outcome is that, while innovations in the accidental readings are around 90 % of all variant readings, professional copyists, at least in fourteenth- and fifteenth-century Italy, have a dramatically low rate of significant innovations. Both if *exemplar* and *descriptus* are in a "bad" position in the genealogy (i.e. they share a number of common errors) and if they are in a prominent position (i.e. they exhibit only a few common errors), these copyists introduce new significant errors only once every eight hundred lines. There is no need to emphasise that the research of Marchetti – who is planning to verify his findings in other manuscript traditions – seems useful not only as a model for rigorously evaluating scribal behaviour but also as a diagnostic tool: whenever scholars face two or more copies which present (i) a high number of common errors and (ii) a dramatically low rate of *errores singulares*, they can suspect (even if codicological evidence is missing) that those copies could be in an *exemplar–descriptus* relation, or very close to this kind of relation.

My list, which is certainly, up to a point, personal and subjective and even partial, ends here. It can easily be enriched by readers, especially if they work in other research fields. Classical scholars will note, for example, the absence of references to the contributions of a master such as Jean Irigoien, but I preferred to work only on issues which I could master, at least to some extent. There is no need to underline that many of the additions and updates reviewed here are closely bound up with the starting assumptions of the founding fathers of the method. As Popper would put it, the theories of textual transmission on which the genealogical method was based were "passed on not as dogmas, but rather with the challenge to discuss them and improve upon them" (1965, 50). Therefore, they have become richer and more comprehensive. On the one hand, they are able to explain in a simple way intuitions of Lachmann's contemporaries which had not been adequately clarified (e.g. the very notion of "archetype"). On the other hand, they allow both predictions and diagnoses (e.g. that a two-branched stemma indicates, as a rule, that the witnesses suffered a high loss rate).

2.4.4 Neo-Lachmannism in the third millennium

As is naturally to be expected, as soon as cladistics and other forms of computer-assisted philology (the greatest novelty of the end of the last century) reached a certain maturity, their promises of amazing advances and their polemical stances against the method of common errors has diminished. The latter has more and more become recognised as a method "there is no need to defend" and whose "main elements [...] are simply self-evident" (I quote from an email by Odd Einar Haugen to the authors of this chapter). Even a champion of the "new digital frontiers" of textual criticism such as Peter Robinson could declare in 2013:

There has been a great deal of rhetoric, some of it from myself, in the last decades about how scholarly editions and editing have been fundamentally changed by the digital turn. So let me say it plainly. *I don't think there has been any such change. A scholarly edition is still, as it has been for centuries, an argument about a text. The fundamental players in this argument are still documents, works, and the editor's interpretation of them.* The editor is the editor, and not a “facilitator”. There are still many more readers than editors, and most readers do not want to be editors. (Robinson 2013b; my emphasis)

In this different context, in a situation of greater mutual respect between philologists who favour the new methods and “traditional” textual scholars, there are signals suggesting that a renewed interest in a refined kind of genealogical textual criticism is spreading even in fields and cultural traditions that have in the past shown little interest in what happens outside the confines of their own horizons. As many of the various traditions are studied in depth in chapter 7, I restrict myself to briefly commenting on a few examples.

In France, a country where scholars traditionally follow in the wake of the Bédierist (or maybe better neo-Bédierist) tradition, the few textual critics open to the genealogical method – who were until recently confined to the *Ecole des chartes* and the Institut de recherche et d'histoire des textes (IRHT) – are especially productive. I mention here only the non-Bédierist manual by Bourgain and Vielliard (2002), the interesting collection edited by Frédéric Duval (2006), and the latter's lexicon *Les Mots de l'édition de textes* (Duval 2015).

It becomes clear even from the terminology that Alberto Blecua uses in his survey – “Wagner confused polygenetic errors and modernizations with authentic common errors” (1995, 470), “the ten manuscripts [...] offer not varying versions, but, rather, scribal variants that had to be organized into a stemma” (472), “the criteria of *lectio difficilior* and diffraction” (473), and so on – that he has been spreading a very solid and up-to-date neo-Lachmannism in Spanish-speaking countries. He did the same also in his excellent manual (Blecua 1983) and his works of textual criticism (collected in Blecua 2012).

As has been noted, Russia

did not develop its own tradition of stemmatics and the introduction of the printing press led to the search for a standard of uniformity, largely based on the ideological choices of church clerics, rather than to attention to text history. As a consequence Bédier's anti-stemmatic approach [...] was easily adopted and widely accepted during the Soviet period. (Bausi et al. 2015, 322)

Thus, and even though the Italian Angiolo Danti had tried to disseminate for many years the practices of neo-Lachmannism among Slavonic scholars (cf. the posthumously published collection Danti 1993), I consider it a very significant fact that both a partial Polish and a complete Russian translation of Maas (1960) have appeared since 1994 (trans. Sybilska 1994; trans. Toršilov 2011). For a reconsideration of Maas's fortunes as well as his importance, see now the introduction by Ziffer in Maas (2017).

In biblical studies, which for decades were quite isolated compared to other philologies, a desire to make contact with colleagues from other fields of textual criticism that seemed unimaginable a few years ago has developed. The discovery of and the research on the Qumran manuscripts certainly brought new life to the field. For instance, there is a very popular blog on Greek New Testament textual criticism, *Evangelical Textual Criticism*, with more than 4,700,000 page views as of September 22, 2018 (evangelicaltextualcriticism.blogspot.com), which publishes very interesting short contributions almost on a daily basis (I mention here only the one entitled “Top Ten Essential Works in New Testament Textual Criticism”, which led to thirty-nine posts). The recent, very interesting book by Hendel (2016), called *Steps to a New Edition of the Hebrew Bible*, contains long quotations in English from works written in Italian by Pasquali, Contini, Segre, Chiesa, and the present writer.

In Sanskrit studies, besides Western textual scholars, there is a long tradition of Indian scholars educated abroad and knowledgeable about Western philological approaches (e.g. Sukthankar, Katre, De). In 1954, Katre published a revised edition of his *Introduction to Indian Textual Criticism*, in which, among other things, he offered “a glossary of some important terms used in textual criticism” and stated: “Textual criticism has come to India to stay” (quoted in Rocher 1995, 587). From the point of view of the present overview, it is worthwhile underlining, again, the interest in and comparison with European neo-Lachmannism evident in recent Indian publications (e.g. quotations from Barbi, Contini, Leonardi, Pasquali, Reeve, and Trovato in Adluri and Bagchee 2018).

A survey of Ethiopic editions can be found in Bausi 2016a (and 7.5 below). Moreover, we must remember the companion by Bausi et al. (2015), where the third chapter is dedicated to “textual criticism and text editing”. This long chapter, authored by Caroline Macé et al. (pages 321–462), has a number of very detailed sections (e.g. “Textual Criticism and Oriental Languages”, “Steps towards an Edition”, “Heuristics of Manuscripts and Witnesses”, “Witness Classification and History of the Text”, “Apparatuses”, “Philological Introduction, Translation, Commentary, Indexes and Appendices”), and offers a wealth of case studies. The chapter’s introduction begins by referring to Lachmann’s method, which

can be very roughly summarized as follows: complete survey of all the direct and indirect witnesses of the work to be edited (manuscripts, printed editions, quotations, allusions, translations, etc.); defining mutual relationships between the witnesses; reconstruction of an archetypal text. Since the critical edition is a scientific hypothesis, it can be disputed and new hypotheses can be proposed or new evidence can be found, which is why some mediaeval texts are edited more than once. (Bausi et al. 2015, 321)

And further:

In recent times, the opponents of the genealogical method of textual criticism and of the reconstructive method of text editing often associated with it are mustered under the flag of “new philology”, a trend in scholarship which came about in the 1990s especially in the United

States (see Gleßgen – Lebsanft 1997), following the publication of Cerquiglini (1989), claiming that mediaeval literature being by nature variable, mediaeval works should not be reduced to an edited text, but all mediaeval manuscripts should be considered equally valuable [...]. *However attractive the “new philology” approach may be in the field of literary studies, it is nevertheless almost completely irrelevant for the purpose of this chapter, as it does not provide any method to edit texts with a more complex manuscript tradition.* (Bausi et al. 2015, 321; my emphasis)

To come to a close, one cannot but be impressed by the distance between these formulations and the attempt by Carter to justify objectively the impossibility of applying criteria of Western textual criticism to Arabic texts:

Both filiation and copy-text present themselves as different from the corresponding topics that have attracted so much attention in Western editing [...]. Knowledge is, after all, the common property of the community, administered and distributed by people of probity and recognized competence – for this reason there is probably a much lower proportion of truly anonymous works in Arabic than one finds in medieval European literature. *It is therefore unlikely that the indigenous Arabic manuscript tradition will reflect the principles and objectives of modern editing.* (Carter 1995, 556–557; my emphasis)

All in all, one gets the impression that this beginning of a new millennium may herald a vaster diffusion and a more conscious application of those most conspicuous refinements of the method of common errors which, not without hesitation, we have proposed calling the neo-Lachmannian method.

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