

Storie e linguaggi.

written with the non-Italian reader in mind, addresses a central problem of criticism, and one that it is currently fashionable to regard as insoluble: how to reconstruct a text of the past so that it is *as close as possible* to the original, starting from a number of copies more or less full of mistakes. I was writing this book—which I left to age, as one does with wine and—first occurred to me in 2006-2007, when I had the privilege of being a professor at the Hebrew University of Jerusalem. As the students felt the pain to me: ‘Nobody had ever talked to us about these things.’ For very few, if any, Biblical, Germanic and Slavonic philologists, or French Roman editors of Anglo-American or Medieval Latin texts, have been “many of the things this book is about” (from the author’s preface).

ay on editorial methodology concerned the number of branches in which my latest concerned editing with the aid of computer programmes topics and many another, Paolo Trovato’s combative and richly erok leaves me far behind, and it is a privilege to have the opportunity ofing it” (from M.D. Reeve’s foreword).

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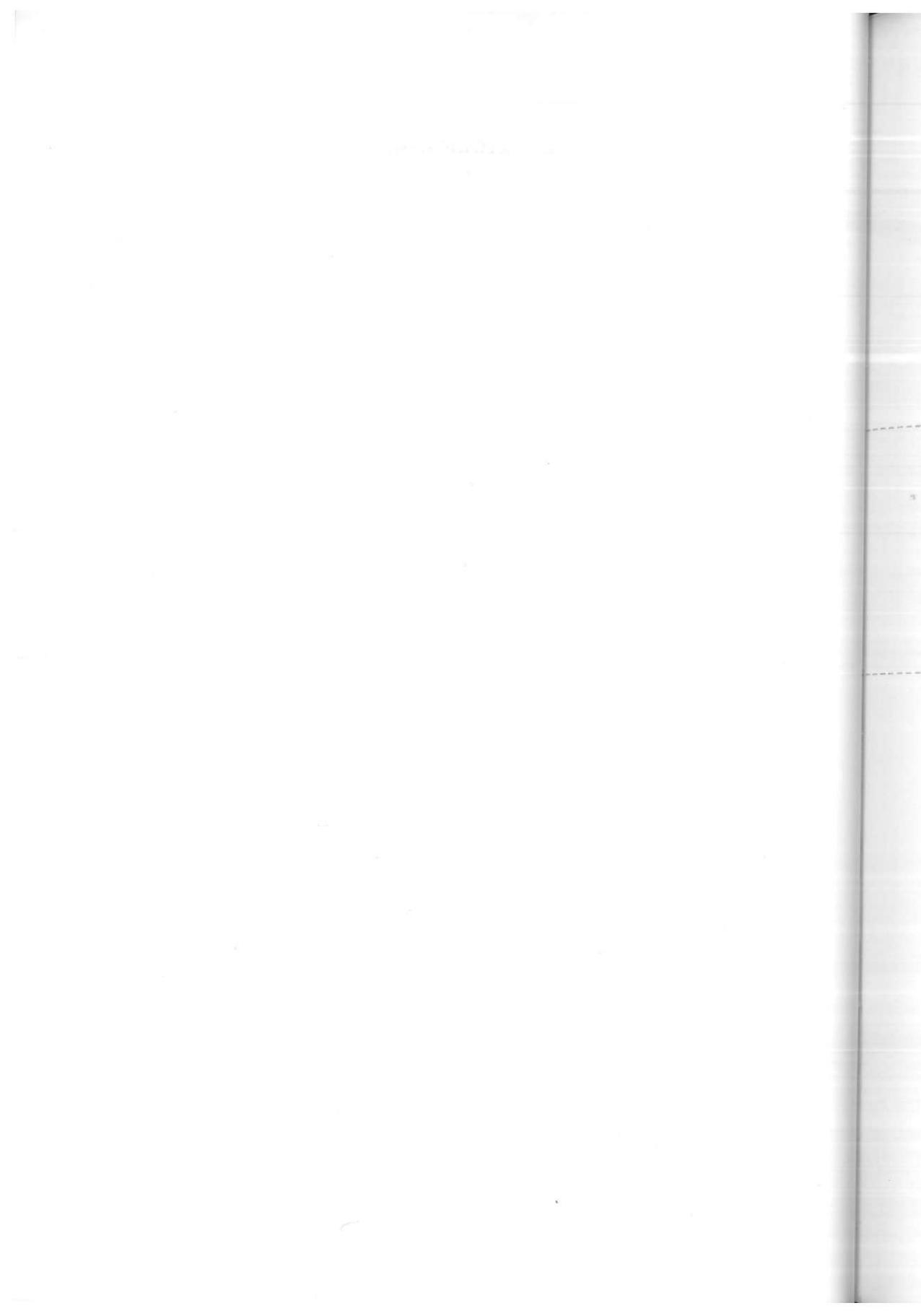
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Paolo Trovato

**EVERYTHING YOU ALWAYS
WANTED TO KNOW ABOUT
LACHMANN'S METHOD
A NON-STANDARD HANDBOOK OF
GENEALOGICAL TEXTUAL CRITICISM
IN THE AGE OF POST-STRUCTURALISM,
CLADISTICS, AND COPY-TEXT**

Foreword by Michael D. Reeve



STORIE E LINGUAGGI

Collana diretta da Franco Cardini e Paolo Trovato

*Everything You Always Wanted
to Know about Lachmann's Method*

A Non-Standard Handbook of Genealogical
Textual Criticism in the Age of Post-Structuralism,
Cladistics, and Copy-Text

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FOREWORD

Pythagoras, says Jerome, preached that after God truth should be worshipped, because nothing else sets mortals next to God, *post Deum veritatem colendam, quae sola homines Deo proximos faciat*. So we read in manuscripts as old as the 9th century. The passage often appears in medieval anthologies, but one such, a 15th-century manuscript at Tarragona, drops the explanation and offers *post Deum varietatem esse tollendam*, ‘after God variety should be abolished’. So much for Bernard Cerquiglini’s *Éloge de la variante*, to say nothing of God.

Without variants, commentators would survive, but editors would be reduced to transcribing and assembling texts, because copies not written out by the author would amount at worst to photocopies at various removes from the original. There would be no debate about which of two or more variant readings the author actually wrote, or whether the author left behind more than one version, or which of the variants are worth communicating, and why, to other readers of the text.

Since the last third of the 19th century, editors cut off from the original of a work by a maze of variants have tried to assess their relative value not by totting up the copies that present each of them, or by seeking out the oldest copies, but by placing all the copies in a genealogical relationship. Obviously earlier copies cannot descend from later ones, but the main principle adopted has been that when copies share an innovation absent from the rest they are related (more closely, that is, than by being copies of the same work); if none of those that share the innovation can plausibly be regarded as the one where it originated, it must have origi-

nated in a lost ancestor common to them all. With luck, the extant copies and their postulated ancestors can be arranged in a family tree, which in any passage will show whether a variant could go right back to the original. If more than one variant survives the test, the editor will have to choose on other grounds; and if none survives that looks fit to have been written by the author, the editor will have to come up with a bright idea, grandly known as a conjecture or emendation.

With luck. How can I tell, though, whether a reading is an innovation? Granted that ‘variety’ and ‘abolished’ are innovations, must any other manuscript where they occur be related to the one at Tarragona, or could they have come about more than once, *varietatem* through resemblance in shape and sound to *veritatem, tollendam* through the resemblance between *c* and *t* in the commonest medieval scripts and the equivalence of single and double consonants in the practice of some scribes? If a manuscript has *veritatem*, can I be sure that it was not copied by a reflective scribe from one that had *varietatem*, or that none of its ancestors had *varietatem* corrected to *veritatem* by someone who checked another copy? Is it neither the mind of Pythagoras nor Jerome as translator that matters to me but perhaps the use made of the passage in medieval sermons? Were the compilers of medieval anthologies not authors quite as much as Jerome?

Not just these objections have been levelled at genealogical methods but two others of very different kinds. If a work survives in hundreds of copies, who has time to compare their texts at every point? and why, in almost all the family trees that editors have sketched, do two branches, not more, sprout from the original or from the reconstructed copy nearest to the original? Famously, the second question was posed a century ago by the Romance philologist Joseph Bédier, after whom it is often called Bédier’s paradox. It led him to recommend just picking a good manuscript and sticking to it, a policy also comforting to anyone daunted by the first question.

It was the second question that brought me in the 1980s, by way of Sebastiano Timpanaro’s monograph *La genesi del metodo del Lachmann*, to the study of editorial methods, and through it I made the acquaintance of Paolo Trovato, who kindly sent me in 2005 an offprint of an article in which he too addressed it. In 2012 I had the pleasure of meeting him in Bologna, and in 2013 another offprint arrived, in which he struck at the roots of the various family trees that Bédier drew up in desperation one after another for the *Lai de l’ombre*. Shortly afterwards, he offered

me the chance to read a draft of the present book, and I seized it. A classicist myself (I stuck my neck out when I edited Geoffrey of Monmouth), I have often felt that the sharpest controversies over editorial aims and methods have engaged medievalists, who in consequence have been at the forefront of developments.

Especially in France and North America, the heritage of Bédier has come to dominate, but Italy maintains a strong tradition of defending and refining genealogical methods. As Italian is seldom the foreign language that speakers of English learn first even when they do learn it (or any), Trovato sets out to acquaint English-speaking scholars with the work of such figures as Michele Barbi, Gianfranco Contini, Cesare Segre, Alfredo Stussi, and Alberto Varvaro, as well as those better known in classical circles such as Giorgio Pasquali and Sebastiano Timpanaro; and he sternly warns against the inaccurate and outdated accounts of genealogical methods often given by scholars impatient with them. As the text at stake for him was often written by authors as great as Dante, Petrarch, and Machiavelli, he deserves the gratitude of everyone for his trouble.

To his trenchant and vigorous arguments he adds a wealth of convincing examples, in other languages as well as Italian; two of the most striking, for instance, are taken from the Spanish *Celestina*. The rewards of perusal include entertainment. At the outset I mentioned Cerquiglini, and not far into the book, in connexion with the misprint *corte esplosiva*, readers will find a witty rejoinder to one of his more portentous assertions; so I like to think that the *corte esplosiva* was mined against Cerquiglini. What accounts for it, though? I once read in the *Times* that a Member of Parliament had accused another of 'looking like a cat that had got at the Queen'; the usual expression is 'got at the cream', doubtless misheard, but perhaps there was also interference from a nursery rhyme ('Pussycat, pussycat, where have you been?' / 'I've been to London to see the Queen'). Readers of the Watergate transcripts may have encountered the strange comment 'That's verbal evil'; I think it was Leonard Boyle, during his tenure at that great storehouse of variants the Vatican Library, who told me that Nixon actually said 'That's very believable'. From things misread we have moved to things misheard, but in scribal culture, as Alphonse Dain pointed out, there was something in between reading and listening: *dictation interne*, the habit of reading words and saying them silently to oneself before copying them out.

Enough. I have mentioned that my first essay on editorial methodology concerned the number of branches in family trees, and my latest concerned editing with the aid of computer programmes. On these topics and many another, Paolo Trovato's combative and richly instructive book leaves me far behind, and it is a privilege to have the opportunity of commending it.

Michael D. Reeve

PREFACE

Every practising critic, for the humility of his soul, ought to study the transmission of some appropriate text [....]. Many a literary critic has investigated the past ownership and mechanical condition of his second-hand automobile [...] more thoroughly than he has looked into the qualifications of the text on which his theories rest.

Fredson Bowers, *Textual and Literary Criticism*, Cambridge, Cambridge UP, 1966, 5

Without memory, there is no culture. Without memory, there would be no civilization, no society, no future.

Elie Wiesel

1.

This book, written mainly with the non-Italian reader in mind, addresses a central problem in textual criticism, and one that it is currently fashionable to regard as insoluble or anachronistic—although the traditional method has yet to be proved inadequate—; namely, how to try to correctly reconstruct a text of the past so that, even if not identical, it is *as close as possible* to the lost original, starting from a number of copies more or less full of mistakes; that is to say, how to preserve part of the memory of our past. In Western literatures, this problem mainly concerns Greek or Latin classical texts, and medieval and Renaissance texts in any language, including Latin and Greek. However, this kind of difficulty also

occurs, to different degrees, for the texts of great authors of our recent past, from Melville to Whitman, and from Kafka to Eliot.

I am well aware that in various parts of the world, and especially in the United States, most varieties of contemporary critical thought deny the legitimacy of any conjectural attempt at reconstruction, indeed, of any editorial technique. As early as 1987, Lee Patterson addressed this issue in depth in a very acute and to some degree prophetic essay. However, as Patterson himself warns, “the refusal to edit—which is a part of the larger refusal to interpret”—ultimately threatens to “undermine both the rigor and the inclusiveness of the interpretative activity that is at the heart of textual criticism” (PATTERSON, “The Logic of Textual Criticism”, 112).

After all, as Peter L. Shillingsburg observes,

we don't want editions to do what [...] old editions did. For example, we don't want an edition that represents the aesthetic tastes of editors in the 1890s; we don't want the clear reading text representing some 1960s editor's notion of what the author's final intentions might have been; we don't want a clear reading text of a historical edition from which the influences of the production process have been purged. Well, how long do we suppose that the current fashion of disdaining these achievements will last? And how long will it be before we start hearing that scholars do not want multiple texts, historical or otherwise, for the works they wish to interpret? (SHILLINGSBURG, *From Gutenberg to Google*, 154).

¶ Throughout the book, references to works regarded as especially useful from the perspective of the book are given in an abbreviated form; the full citation can be found in the “General bibliography”. Works equally important, but with a more specialized approach and language, are cited in full in the text, mainly in the “Bibliographical notes” at the end of each section

2.

Actuellement, il n'existe qu'une seule filière “Editionswissenschaft” en Allemagne, à la Freie Universität de Berlin [...]. La politique de l'éducation et de l'université ne s'intéresse guère au travail des philologues et éditeurs. Leur travail est considéré comme improductif, peu profitable et peu rentable (Thomas Bein, “L'édition de textes médiévaux allemands en Allemagne: l'exemple de Walther von der Vogelweide”, in DUVAL, *Pratiques philologiques en Europe*, 30).

Whatever the reason, it is obvious that the mood in literature departments, at least in the United States, is certainly less philological (if not antiphilological) than, let us say, in the 1960s (Peter F. Dembowsky, review of CARAPEZZA, *Ecdotica, Romance Philology* 62, 2008, 175-184: 180).

As the above quotations suggest, today we find ourselves in a historico-cultural context indifferent, if not hostile, to textual criticism. Scholars who still produce editions of texts transmitted in more than one copy—an essential activity for our knowledge and understanding of our past—can be divided into three groups, reflecting three different approaches or editorial philosophies:

A) those who believe that the original—i.e., the text written by the author—is completely beyond our reach, and that we should be content with a real text whose historical existence is beyond question, a scribal version, that is, a text in the form in which it has been handed down to us, the work of this or that other copyist, in a broad sense (that is, including typists and such);

B) those who argue that the editor cannot trace the original of a text beyond the so-called “archetype” (i.e., the manuscript from which the surviving tradition derives, which is by definition lost, but can be reconstructed by comparing all surviving copies), that is, the most correct text one can reconstruct by comparing the readings extant in available manuscripts and editions;

C) staunch “reconstructionists”, who believe that the task of a scientific edition is not merely to transcribe a manuscript, or to reconstruct the archetype of surviving manuscripts, but to use the archetype as a point of departure, using all available means—linguistic, stylistic or metrical information, historical data, etc.—to try to come as close as possible to the lost original, detecting and correcting, as far as possible, but always as rationally and transparently as possible, the errors shared by surviving copies.

I believe an up-to-date illustration of approach C, that is, the genealogical-reconstructive method, also known as the common-error or (Neo-) Lachmannian method—which happens to be mine—may also be useful, at least as a stimulus, to scholars who favor approaches A or B. This is because approach C, which has been applied for more than a century in the fields of classical philology, medieval Latin and modern languages, can boast a more plentiful and varied range of experiences, from the reconstruction of an ancient text through the collation of all its surviving copies to the investigation of several authorial versions, whether autograph or not. Approach C is also the only one of the three that has known a centuries-long and very lively debate about the pros and cons of its different procedures. This book may therefore be useful, even by confirming them in their positions, to all (poststructuralists, postmodern critics) who believe that the only possible solution is not “to revise our editorial techniques, but to abandon them entirely” (I am quoting again from PATTERSON, “The Logic of Textual Criticism”, 111).

3.

I will try to explain more clearly, to myself first of all, why I have written this book. I am a professor of the history of the Italian language. For many years I studied a subject that was later to become fashionable, namely, “the forms of texts that emerged from the social process leading to public distribution”, that is, in my case, how sixteenth-century editors published, or rather “rewrote”, Italian literary texts. I could never have addressed this theme without having recourse to the basic criteria of genealogical textual criticism.

¶ A synthesis of my research can be found in my book “*Con ogni diligenza corretto.*” *La stampa e le revisioni editoriali dei testi letterari italiani, 1470-1570*, Bologna, il Mulino, 1991, repr. Ferrara, UnifePress, 2009; to be complemented with Brian Richardson’s excellent book *Print Culture in Renaissance Italy. The Editor and the Vernacular Text*, Cambridge, Cambridge UP, 1994.

More generally, for about thirty years I have regularly worked in the field of textual criticism. For about fifteen years, I have been teaching a course entitled “Textual criticism” at my university in Ferrara, where I discuss with students whatever textual problems I happen to be working on at a given time. For about ten years I have codirected a journal of textual criticism, *Filologia italiana*. I have never thought of writing a manual for the Italian public. Many are available, possibly too many, and some are excellent. Significantly, the idea of writing this book—which I largely wrote off the cuff in the summer of 2011, and then left to age, as one does with wine and cured meats—first occurred to me in 2006-2007, when I had the privilege of being a visiting professor for a semester at the Hebrew University of Jerusalem. Of the three courses I taught, the most selective, reserved for graduate students, was called “Textual criticism”, and was attended by four “students” of uncommon competence: a Romance philologist who was planning a critical anthology of early French poetry, two medieval history students working on an edition of Latin texts from the Kingdom of Jerusalem, and a student who wished to publish an edition of a remarkable sixteenth-century Yiddish text, and was also a capable translator into Italian of twentieth-century Hebrew narrative. From the first session, we agreed that, after I had briefly introduced them to the basic rules of textual criticism, we would collectively work on their respective editorial projects. On that occasion, more than in any other course provided in Italy, where students in faculties of letters usually have at least a smattering of knowledge on the subject, I felt these

students' gratitude to me for having given them the keys to a kingdom that had been unknown to them, but whose existence they had suspected or caught glimpses of. As each of the four, at different times, felt the need to explain to me: "Nobody had ever talked to us about these things". For several decades, very few, if any, Biblical, Germanic and Slavonic philologists, or French Romanists, or German editors of Anglo-American, German or Medieval Latin texts, have been talking about many of the things this book is about.

4.

A second reason, which may be branded as chauvinist, for the genesis of this book and the readership it is addressed to coincides with the one facetiously expressed by the Romance philologist Alberto Varvaro:

I am well aware that, as an Italian, I can be suspected of maintaining a preconceived position. In fact there is no doubt that at least since 1945 we Italians have made a reputation for ourselves as unshakable believers in neo-Lachmannism. Due to this reputation, the majority of our foreign colleagues consider us incurable madmen insofar as we follow an abstract and irrational form of fundamentalism; however, sometimes those same scholars also give us a benefit comparable to that enjoyed by the followers of an alien religion, namely: the suspicion that we might well be correct (Varvaro, "The New Philology from an Italian Perspective", in *id., Identità*, 613-622: 613-614, originally published in Italian by GLESS-GEN-LEBSANFT, *Alte und neue Philologie*, 35-42).

Later on in his article, Varvaro rightly makes a connection between Italians' apparent conservatism and the teachings of an extraordinary trio of scholars who carried our textual criticism beyond the naïve practices of genealogical reconstruction brilliantly exposed by Joseph Bédier in 1928, but also beyond the paralyzing skepticism of Bédier himself. These three scholars were the classicist Giorgio Pasquali, the Dante scholar Michele Barbi, and the Romance philologist Gianfranco Contini.

For almost a century, the repute of the method ascribed to Lachmann had been sinking lower and lower. Criticism of its real or presumed excesses and limits had overshadowed the fact that even for deconstructing a text, or studying the different layers deposited on a text by successive waves of scribal culture, the primary condition is having a reliable text, as close as possible to that produced by its author—regardless of whether we are dealing with Dante, Shakespeare, or an anonymous French compiler of Arthurian ro-

mances—, or at least being able to distinguish, even if only roughly, between different textual layers more or less far removed from the (lost) original. In the same period, Barbi, Pasquali, Contini and their best students contributed decisively to refounding post-Bédier textual criticism on rigorous premises, that is, to founding what can be appropriately called Neo-Lachmannism. On the other hand, for reasons whose discussion lies outside the scope of the present work, in spite of fashion, Ferrari, Brunello di Montalcino and spaghetti, Italian is no longer an international language. More than a hundred years of methodological refinements, including some very significant ones, to “Lachmann’s method” have been published predominantly in Italian—by the likes of Barbi, Pasquali, Contini, Avalle, Folena and Segre, to mention only a few names—and have thus remained practically inaccessible to most scholars in the rest of the world. Even Tanselle’s solid and very well informed review “Textual Criticism at the Millennium” contains almost no citations of Italian scholars, and the newly published *Cambridge Companion to Textual Scholarship* devotes less than 20 pages out of a total of 300 to “Continental editorial theory”. (The exceptions that prove the rule are Paolo Cherchi’s incisive review of Italian textual criticism, in *Scholarly Editing. A Guide to Research*, edited by D.C. Greetham, New York, The Modern Language Association of America, 1995, 438-456, and the English translation published in 2005, forty years after it first came out, of Sebastiano Timpanaro’s splendid book on the genesis of Lachmann’s method).

5.

Now, I do not want to convey the idea that the genealogical method is something narrowly Italian, like pizza or mandolins. In spite of recurrent attempts to celebrate the funeral of the reconstructive, or common-error, method, by Bédier converts first, by New Philologists next, and most recently by adepts of cladistics, in spite of the deep-seated mistrust that the philological schools of whole nations harbour towards harmless technical terms like *author*, *archetype*, *conjecture*, and *error*, dozens of scholars in Austria, France, Great Britain, North America, Sweden, etc. have unabashedly and successfully continued to employ the method, perfecting it and finding ever wider fields of application for it. The following quotations, chosen from the many that I might have used, bear this out:

When adherence to the conservative principles of Bédier induces an editor to refrain from changing MS readings in those cases where he merely happens to be able to think of “better” ones, the result is no doubt salutary. But when an editor [...] takes too literally the remark that after all a

thirteenth-century scribe had a better chance of knowing the language of the thirteenth century than a twentieth-century critic [...], then the wisdom of a very conservative treatment of the text is more than questionable. Every one who has worked with medieval MSS knows that scribes, in spite of their knowledge of the language of their own time, were often guilty of inattention and carelessness; and that clear, intelligible alternatives may occur in parallel copies, which were made by men with no less contemporary knowledge, but whose work has not happened to win the honor of being chosen [...] as a “manuscrit de base” (*The Continuation of the Old French Perceval of Chrétien de Troyes, II. The First Continuation. Redaction of MSS. EMQU*, ed. by W. Roach and R.H. Ivy, Philadelphia, University of Pennsylvania Press, 1950, viii-ix).

I dare say Professor Vinaver's necrologue on *recensio*, *stemmata* and composite texts [i.e. E. Vinaver, *Principles of Textual Emendation*, in *Studies in French Language and Medieval Literature Presented to Prof. Mildred K. Pope*, Manchester, 1939, 351-369] is in its generalization somewhat ahead of the event. At a later stage the author himself has recourse to the principle that common errors must have a common source [...], and even goes as far as to base on such common errors a *stemma codicum* (BIELER [1962], 32).

Bédier's own explanation of the preponderance of two-branch stemmata implied no radical defect in the method but assumed that it was almost universally misapplied (WHITEHEAD-PICKFORD, “Introduction to the *Lai de l'Ombre*”, 149).

Parmi les *stemmata* qu'on nous a présentés beaucoup sont bifides, personne ne s'en est inquiété [...]. Est-ce que vous pensez, comme j'ai été enclin à le faire, que logiquement, si la méthode qui aboutit à une majorité de *stemmata* bifides est valable, il n'y a qu'à s'incliner devant le fait et en chercher l'explication dans les conditions de transmission des textes et non pas à inculper la méthode? (R. Marichal, “Conclusions du colloque”, in PdO, 287).

Much of our evidence about how they [i.e. medieval scholars and scribes] acquired their exemplars, what kind of text their exemplars presented, what resources besides their exemplars they employed in making their copies [...]—much of this evidence we owe to stemmatic method (M.D. Reeve, “Stemmatic Method: ‘qualcosa che non funziona?’” (1986), now in ID., *Manuscripts and Methods*, 38).

The stemmatic method [...] is clearly superior to the traditional one when the tradition is bad, i.e. when there are many errors in the MSS. If the MSS. are good, with few errors, it does not matter very much which of the methods is used (EKLUND, 18).

Voici enfin la version française de la première édition critique depuis 1900 de *La Chanson de Roland* [...]. L'ensemble est mené avec une rigueur absolue. L'intérêt de ce travail est d'offrir des perspectives neuves alors que la valeur heuristique de la méthode de Bédier, fertile à l'origine, avait fait place à une certaine sclérose: l'objectif de chaque chercheur était d'être plus bédérien que ses prédécesseurs (G. Roques, review of *Chanson de Roland* S, *Revue de linguistique romane* 54, 1990, 631).

L'effort "traditionnel" pour classer les témoins et reconstituer le texte de l'archéotype n'étouffe nullement la conscience de leur variance originelle: bien plus [...], la remontée stemmatique vers l'origine du texte apparaît le meilleur moyen pour comprendre et retracer la genèse d'une œuvre et les remaniements successifs qu'elle a subis de l'auteur ou d'autres après lui (D. Poirel, "L'édition des textes médiolatins", in DUVAL, *Pratiques philologiques en Europe*, 151-173: 157).

6.

I must add that the event that led me to write the present manual—whose impact on myself I would liken to the pistol shot fired in Sarajevo that, according to old history textbooks, provoked World War I—was my need to prepare rapidly a long-distance course in textual criticism for a private university I collaborated with for a few years. In e-teaching, the professor cannot decide, or even update, the contents of his course lesson by lesson, as I, at least, do when teaching face-to-face courses. A long-distance course must be uploaded to the platform before the beginning of the university year to allow students to start following it at any time. While I was writing my lessons, I could not help thinking of those other students, equally interested in the subject and no less lacking in information about it, a sample of whom I had met in Jerusalem: non-Italian textual critics who were not classical philologists. (Classical philology is a discipline in which relations between the best Italian tradition and the best non-Italian scholars were never cut off—we only need to think of Ludwig Bieler's admiration for Pasquali, or the dense exchanges of Edward J. Kenney and Michael D. Reeve with Timpanaro).

A commonplace of mature textual criticism is that every national philology—indeed, every single edition—addresses a different problem. (This is a sort of *locus modestiae* that possibly also serves as an alibi for being poorly informed about other scholars' research, or to alleviate scholars' guilt when their proposed methods malfunction). Now, to limit myself to a pair of obvious considerations, it is evident that:

a) The existence in the classical world and in the Italian Middle Ages of major authors with a clear-cut style and ideology, such as Cicero and Virgil, or Dante and Petrarch, has of necessity made the respective philological schools more sensitive to the issue of authoriality (and more diffluent of the latest or next-to-latest fashion) than critics of medieval texts in French or German, which are very often anonymous, or even if they are not nevertheless lack a “strong authorial mark” (A. Varvaro).

¶ Please note that the words “philologist”, “philology” and “philological” are always used throughout the book in the narrow German and Italian meanings of, respectively, ‘textual scholar or editor’, ‘textual scholarship’, and ‘pertaining to textual criticism’.

b) The strong differences between Western countries in the organization of university learning—for example, the separation between linguists and literary scholars in French universities—and the diversity of the textual traditions, whether manuscript or not—for example, the prevalence of single-witness texts in medieval German philology—strongly influence editors’ choices of tools and their decisions.

I, like Giorgio Pasquali, however, still believe that the original of, say, a Chinese or Bantu text cannot be reconstructed from surviving copies unless one follows (a small number of) general philological rules, albeit adapted to the great variability and specific requirements of particular texts (and contexts). That is why, although my knowledge of medieval Latin and early varieties of Spanish and French is decidedly inferior to my knowledge of the subject I teach, viz., the history of the Italian language, I have occasionally studied textual problems regarding literary texts in those languages, and shall occasionally be referring to these problems in the present book.

7.

In spite of my attempt to provide an honest and clear presentation of most of the procedures and problems of the Neo-Lachmannian reconstructive philological technique, the present manual is little more than an appetizer, especially for readers who have fasted for so long. I trust that the specific bibliography I provide at the end of most sections, I hope with sufficient generosity, will help readers to find not only their bearings, but also enough nourishment.

To present honestly does not mean to present neutrally. Having read with interest several dozens of textual criticism studies written over the

last thirty years by scholars of various nationalities—North American, Dutch, etc.—who adhere to the New Philology, study “socially produced texts” or are involved in computer-assisted philology (or New Stemmatatics), I was struck by four recurrent features:

a) A little familiarity with the genealogical or common-error method. These scholars only cite a few late-nineteenth or early-twentieth century works, or elementary and at least unwittingly tendentious generalizations by earlier New Philologists, while never citing recent authoritative applications, from which much is to be learned even as far as methodology is concerned.

¶ Just by way of example, an outstanding work in this regard is the important collection *Texts and Transmission. A Survey of the Latin Classics*, ed. by L. Reynolds, Oxford, Clarendon Press, 1983; for medieval Latin, one could cite *La trasmissione dei testi latini del Medioevo—Medieval Latin Texts and their Transmission*, ed. by P. Chiesa and L. Castaldi, the first 5 volumes of which have come out (Firenze, Sismel, 2004-2013); for early French, Cesare Segre’s 1971 edition of the *Chanson de Roland*, and his revised edition of 1989 (*Chanson de Roland S*, to be taken together with his book *La tradizione della “Chanson de Roland”*, Milano-Napoli, Ricciardi, 1974); for Italian, Tiziano Zanato’s edition of Lorenzo il Magnifico’s *Rime* and *Comento*, published in 1990 by Olschki (to be read jointly with his commentary, Lorenzo de’ Medici, *Opere*, Torino, Einaudi, 1992), Antonia Tisconi Benvenuti and Cristina Montagnani’s 1999 edition of Boiardo’s *Inamoramento de Orlando* (Milano-Napoli, Ricciardi), or Lucia Bertolini’s recently published edition of Leon Battista Alberti’s *De pictura* (Firenze, Polistampa, 2011).

b) The consequent attempt (typical of new disciplines, in search of proselytes and funding) to discredit the genealogical method by caricaturing it and blaming it for shortcomings that are non-existent, or have been overcome or have lost importance in a long and undeniable history of successes.

¶ I fully agree with TANSELLE, “Textual Criticism at the Millennium”, 71-74, who writes, among other things: “Those who have taken an either/or position, suggesting that an interest in authorial intention is futile, unproductive, and outmoded, have shown by their manner of proceeding that they are more concerned with promoting a particular point of view than with welcoming all approaches that can contribute to fuller understanding [...]. Understanding rather than victory: this is the motto for civilian scholarship”.

c) “Alternative” philologists’ renaming of key terms of genealogical theory such as *error* (→ 4.4) or *stemma* to make them acceptable to their co-religionaries (e.g., “It is best described not as a ‘stemma’, but as a ‘table

of relationships": Peter Robinson, → 4.3), while a non-ambiguous terminology is actually indispensable when working with more than one witness.

d) A reluctance to make explicit and discuss all the limitations and data-manipulation usually involved in the use of alternative methods, such as the choice of a single manuscript (the *bon manuscrit*) or digital philology.

For these reasons, after illustrating the basic rules of the genealogical method inaccurately attributed to Karl Lachmann, I will try to explain its strengths, its advances (in some cases ascribable to what, from afar, may appear as the Italian "school"), and the limits of its application, some of which Bédier had already acutely pointed out.

The first part of this manual deals with "Theories". After a brief illustration of the basic assumptions of the genealogical-reconstructive method, or common-error method (often called Lachmann's method: Chap. 1), I shall be dwelling at length, in Chap. 2, on the objections of the great Joseph Bédier (1913, and especially 1928-29) and on the consequences, at the global level, of his impassioned argumentation, from the spread of the *bon manuscrit* criterion to Quentin and the dawn of digital philology.

In Chap. 3, I will examine in more detail some essential notions, such as those of archetype, stemma, and *vulgata*.

In Chap. 4, I will discuss the theory and practice of computer-assisted stemmatics, which for at least twenty years has been meeting with great success in North America, Great Britain, and the Netherlands, and is beginning to take hold in Italy, too.

After discussing methods of reconstructing textual *substance*—establishing whether at a certain point in the text it is more correct to choose the variant *cat* or the variant *rhinoceros*—in Chap. 5 I will touch on issues of reconstruction of the *form* of a text (Engl. *love* or *loove*? French *amot* or *amoit*? It. *spazio* or *spatio*?).

Chap. 6 addresses the *emendatio* (weeding out errors, improvement) of a text, i.e., that part of textual criticism that is as creative and difficult as it is indispensable. I subscribe to the opinion of many scholars that philology is a technique rather than a science, that is, a set of procedures to be followed and decisions to be made that are more easily learned by editing an actual text than by reading a manual. I therefore decided that the most effective way to "teach" this way of working on texts would be to collect and comment on some universally accepted emendations, or some especially significant ones, first and foremost in Italian texts, but also in some Latin, French and Spanish ones.

As I mentioned at the beginning, for reasons of parsimony—an important criterion, in textual criticism as in any other field—I will touch only marginally on issues concerning other historical sectors of textual criticism, such as textual bibliography (It. *critica dei testi a stampa*) or a vital ancestor of the French “*critique génétique*”, *critica delle varianti* [criticism of variants], also known as *filologia d'autore* [author philology]. Excellent studies on this subject exist, which I will be referring to as needed.

Since my approach has been to draw my readers gradually to the discussion of real problems, the second part of the book (“Practical applications”) is devoted to a brief but I hope not superficial analysis of three manuscript traditions of different degrees of difficulty: the relatively simple one of a short Latin treatise produced in Palestine in the time of the Crusades (Chap. 7); that of the *Lai de l'ombre*, the early French poem that inspired Bédier's methodological schism, but which in my opinion is relatively easy to rationalize in terms of standard Neo-Lachmannian rules (Chap. 8); and that of Dante's *Comedy*, that is, the most difficult textual problem in any modern European literature (Chap. 9).

I dedicate this manual to my students in Aix-en-Provence, Ferrara, Jerusalem, Leiden, Novedrate, Salerno, and Venice, from whom I have learned more than I have taught them, and to my future students, in the hope of learning from them, too.

P.T.

Capo Palinuro-Cariati-Ferrara, summer 2011-summer 2014.

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It goes without saying that any errors that remain are my own.

For anyone who has teaching duties and is not single, writing a sizable book inevitably means reducing, for a more or less long period, the time devoted to “normal” life (taking care of the kids, going to the movies, etc.). Last but not least, therefore, I thank Beatrice, Gregorio and Margherita for allowing me to “steal” some of the time that was rightfully theirs.

HOW TO USE THIS BOOK

Unlike other Western countries such as Germany, England, France, the United States of America and others, which have great classical, medieval, Humanist, Romance and Germanic philological traditions, but are all in the process of parting ways with these very same traditions, in Italy textual criticism is still practised with great intensity. The vitality of the discipline in its various fields of application—from classical philology to modern literature, with a special focus on the Middle Ages and the Renaissance—is reflected in an abundance of university manuals, often of a very high level, and in updates to them. I will cite a number of them, obviously with no claim to exhaustiveness:

- A. Del Monte, *Elementi di edcotica*, Milano, Cisalpino-Goliardica, [1975];
A. Roncaglia, *Principi e applicazioni di critica testuale*, Roma, Bulzoni, 1975; G. Contini, entry “Filologia”, in *Enciclopedia del Novecento*, 1977 (subsequently republished with updates several times); d.S. Avalle, *Principî di critica testuale*, Padova, Antenore, 1978² (I ed. 1972); F. Brambilla Ageno, *L'edizione critica dei testi volgari*, Padova, Antenore, 1984² (I ed. 1975); A. Balduino, *Manuale di filologia italiana*, Firenze, Sansoni, 1989³ (I ed. 1979); A. Stussi, *Introduzione agli studi di filologia italiana*, Bologna, il Mulino, 2011⁴ (I ed., entitled *Avviamento agli studi di filologia italiana*, 1983); G. Inglese, *Come si legge un'edizione critica. Elementi di filologia italiana*, Roma, Carocci, 2006² (I ed. 1999); B. Bentivogli, P. Vecchi Galli, *Filologia italiana*, Milano, Bruno Mondadori, 2002; A. D'Agostino, *Capi-toli di filologia testuale: testi italiani e romanzi*, 2. ed. corretta e accresciuta, Milano, CUEM, 2006 (I ed. 2005); P. Mari, *L'armario del filologo*, Roma,

Istituto storico italiano per il Medio Evo, 2005; P. Stoppelli, *Filologia della letteratura italiana*, Roma, Carocci, 2008; P.G. Beltrami, *A che serve un'edizione critica? Leggere i testi della letteratura romanza medioevale*, Bologna, il Mulino, 2010; P. Chiesa, *Elementi di critica testuale*, Bologna, Pàtron, 2012² (I ed. 2002); A. Varvaro, *Prima lezione di filologia*, Roma-Bari, Laterza, 2012.

In spite of notable exceptions, in other countries textual criticism is less and less practiced, and not regarded as an autonomous discipline, in the conviction that

a man who possesses common sense and the use of reason must not expect to learn from treatises or lectures on textual criticism anything that he could not, with leisure and industry, find out for himself. (A.E. Housman, cited in KENNEY, *Textual criticism*).

Here Housman is obviously overlooking the fact that someone endowed with these faculties—common sense, the use of reason, leisure and industry—could reinvent many things, from the wheel up, or rewrite many chapters in modern medicine manuals, but this would require a huge expense of time and labor.

Moreover, I find him overoptimistic when he argues that “what the lectures and treatises can do for him is to save him time and trouble by presenting to him immediately *considerations which would in any case occur to him sooner or later*” (my emphasis).

That is why this manual, as I said above, is mainly addressed to non-Italian scholars and students. The following pages do not constitute an organic course of textual criticism, from A to Z: I would end up doing nothing but repeating what has already been written, often in an impeccable manner, by some of the authors just cited. My intention, instead, is to elucidate aspects and questions of the discipline that I find important or interesting, but still not sufficiently known.

I would therefore invite non-specialist readers to first familiarize themselves with one of the above-listed manuals, or with manuals written in other languages.

All quotations from books and articles written in Italian have been translated into English. I have been less systematic in all such cases where I could assume among non-Italian textual scholars a wider knowledge of the language in question, or where the context suggested paraphrasing as a more appropriate solution.

I will often be giving essential definitions of technical terms as I introduce them. I also refer the reader to the Index at the end of this manual.

The symbol → means that the subject is addressed or further discussed in other chapters and paragraphs in the book (for example, → 2.7 refers to Chapter 2, Section 7).

The symbol Ø before a sentence in a smaller font size means that this sentence contains bibliographical or terminological remarks, that can be skipped without compromising the reader's understanding of the text.

The symbol [] (square brackets, sometimes with a space between them) means that some words or the words in the brackets are lacking (Lat. *lacuna*) and need to be restored.

The following abbreviations are used:

Engl. = English.

f., ff. = folio, folios.

Germ. = German.

It. = Italian.

l., ll. = line, lines.

Lat. = Latin.

ms., mss. = manuscript, manuscripts.

s.v., s. vv. = *sub voce*, *sub vocibus*.

Bibliographical notes. For those who read Italian, I would recommend STUSSI's manual for its clarity and orderly progression. For those who read Spanish, a good choice would be BLECUA (other Spanish manuals are discussed in H.O. Bizzarri's overview "Veinte años de reflexión sobre crítica textual (1983-2003)", *Revue Critique de Philologie Romane* 4-5, 2003-2004, 296-321). Among manuals in French, I recommend BOURGAIN-VIELLARD's. For English readers, useful points of departure include WEST or FOULET-SPEER, or KENNEY's fine summation "Textual criticism", or WEGNER, *A Student's Guide*.

Actually, almost all existing manuals of textual criticism are useful, because they reflect the experiences of different scholars—that is, the characteristics of the texts they studied—and, apart from the simplest cases, different texts usually pose partially different problems.



GENERAL BIBLIOGRAPHY

Below I list a few dozen works or repertoires that are used especially frequently, which I will henceforth cite in an abbreviated form (usually with the name of the author in small capitals). As I said before, works that, while no less important, are less crucial for what we could call the xenophile perspective of the present manual will be cited in full at the end of the corresponding section (in such cases, the name of the author will be in Roman type).

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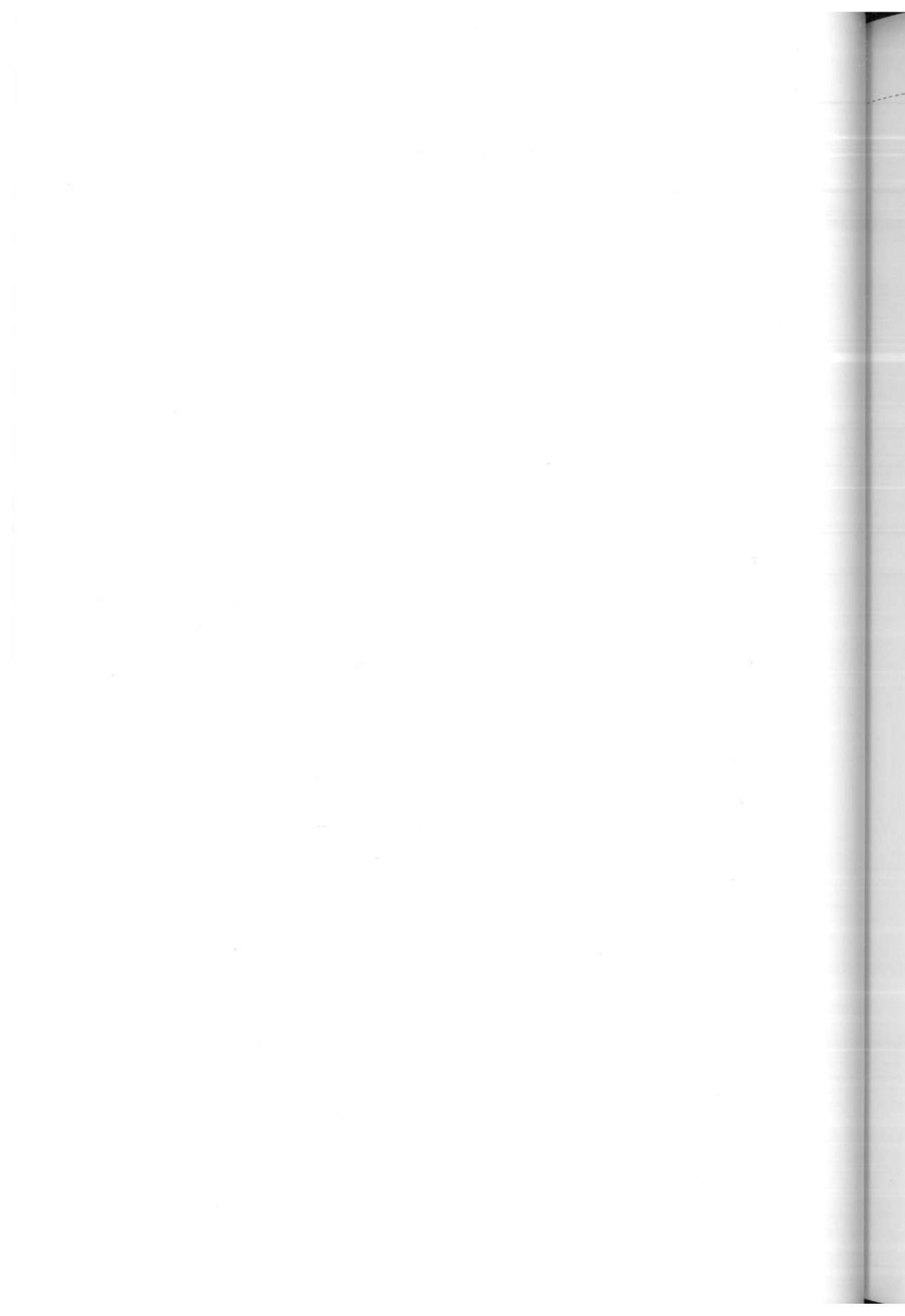
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INTRODUCTION

1. Philology or textual criticism

The term “philology”, which however has several meanings, and its less ambiguous equivalents “textual criticism” (French *critique textuelle*, Germ. *Textkritik*, It. *critica testuale*) or “ecdotics” (French *ecdotique*, It. *ecdotica*), designates a set of techniques or operations whose purpose is to reconstruct texts whose complete genuineness is open to doubt; which is to say practically all texts, in any ancient or modern language, that have not come down to us in a faultless autograph or in a copy that the author—sometimes a poor copyist of his own works—carefully revised, and especially texts transmitted in a series of copies that are more or less far removed from the original, that is to say, deformed by a number of errors.

Bibliographical notes. On the real, albeit circumscribed, possibility of authorial errors, see most recently REEVE, *Manuscripts and Methods*, 3-23.

2. We are all philologists

Every day, when we read the papers—where spelling mistakes have become increasingly common since computer operators have replaced typesetters and proofreaders—or when we proofread our writings—where, especially when quoting other authors, we often act as mediocre medieval copyists—we are forced to mentally correct texts that are larded with errors; that is, we are forced to turn into textual scholars. Some correc-

tions could not be simpler. For example, any reader with a fair knowledge of English would be able to correct the following errors (in italics) in the first draft of the present book:

- (1) *Id* did not seem possible to use a stemma with so many lines of descent.
- (2) Much of our evidence about how they acquired *they* exemplars.
- (3) Bédier suddenly turns *to* the current prevalence of two-branched stemmata to the actual historical situation of medieval writers.

Going from simplest to (relatively) complex: in (1), *Id* is a misspelling of *It*, possibly favored by some kind of acoustic or graphical assimilation; in (2), the second *they* (echoing the correct *they* two words before) should be corrected to *their*; in (3), the first *to* should be emended to *from*.

An insidious, but not irremediable, polar error (*unlikely* for *likely*), corrected indeed in the *Times* of October 19, 1987, had altered the meaning of an article published the previous Friday about Soviet missiles in Cuba:

- (4) It seemed *unlikely* that some of the missiles might be ready, either for intimidation or for use, within a few days.

It is more difficult, in the absence of the original or of a better copy than ours for comparison, to remedy certain less frequent incidents, most notably: the insertion of a word in the wrong place, as in (5), from the proofs of this book; the replacing of a word with a more or less similar one, as in (6) from the *Times* of April 25, 1987; or the omission of one or more words (Lat. *lacuna*), which, as I explained above, I indicate with a space between square brackets [], as in (7), also from the *Times* (January 16, 1987):

- (5) These editions seek an ‘authentic’ text, one which is made up from all extant manuscripts *reconstruction*. The reconstruction of a hybrid text was no longer in fashion when Joseph Bédier published his edition of 1913.
- (6) Fr *Oswald Lewry*, OP, who died on April 23, aged 57, had a special place among mediaeval scholars in Oxford.
- (7) They create the impression that the City is nothing better than a jungle where predators [] are disregarded.

In (5), the first *reconstruction* anticipates the beginning of the next sentence. It is an anticipation error (It. *errore d’anticipo*). In (6), “due

to an error in transmission, the Christian name of Fr Osmund Lewry, OP, appeared as Oswald in his obituary of April 25" (*Times*, 30 April 1987). In (7), where the [] denotes a lacuna, the text does not make any sense, and was corrected in the *Times* of January 17, 1987: "The City is nothing better than a jungle where predators *and speculators* thrive while the interests of the small investor are disregarded" (I put the dropped words in italics). The misprint was caused by the similar ending of the words *predators* and *investor* (Engl. omission by *homoio-teleuton*, French *saut du même au même*, It. *lacuna per omeoteleuto*). A mere knowledge of spelling or grammar is not sufficient to remedy a lacuna. One needs, at the very least, specific information about the context of the corrupted passage. Such information is not easily arrived at by conjecture.

In the March 13, 1987 issue of the *Times Literary Supplement*, a reader points out a particularly insidious error, referring to a copy closer to the original:

(8) Sir,—James Serpell, in his review of *The Collins Encyclopedia of Animal Evolution* [...] finds such pleasure in the discovery of modern bacteria with Precambrian affinities "in wine-soaked soil beneath a wall of Harlech castle" that I feel churlish in pointing out a typographical error. In the original report it was "urine-soaked soil", which makes both the men of Harlech and the primordial slime far less interesting.

The next example, in Italian, is drawn from an advertisement for an apartment:

(9) Zona centro. Bilocale in contesto prestigioso. Corte *esplosiva*. [Town center. Two-room apartment in posh surroundings. *Explosive* courtyard].

In this case, too, the solution is not automatic. The meaning is unacceptable, and yet there are no spelling or grammar mistakes, or suspicious repetitions, and apparently no word is missing. To solve our problem we would need to have read many advertisements of this kind (which is why philologists working on a corrupted text carefully read many texts from the same period and of the same literary genre, and study their language). Usually, in Italian housing ads, a courtyard or yard is either *comune* [shared] or *esclusiva* [private]. The appropriate conjecture is thus:

esclusiva. Our correcting action can be graphically described as follows: *es[c]l[u]siva*.

At this point, we could take a closer look at how such errors are produced (replacement, insertion or moving of a letter or word, etc.), but we will do this later. Any way, the subject is treated in many manuals, such as BLECUA, 19-30; STUSSI, 96-97; BOURGAIN-VIELLIARD, 35-37; WEGNER, A *Student's Guide*, 44-57). Now we have a more urgent question to address.

Bibliographical notes. In the first draft of this section, all the examples were from Italian newspapers. I owe those from the *Times* to the kindness of Michael D. Reeve.

3. Why do we need textual criticism?

Philological practices, that is, practices aiming at emending as many flaws (corruptions, errors) as possible in texts, are found in all cultural traditions that grant importance to one or more *books*: for example, the *biblia*—the Greek word for “books”—regarded as indispensable in the various Bible traditions (although the very fact that the Bible is *also* a sacred text was for long an obstacle to a frank philological discussion of the possible errors in the officially adopted text, the *textus receptus*); the texts of Virgil, Ovid, etc. in the Carolingian age; Homer, the tragedians, and Plato in the Byzantine Greek period; and, in the Renaissance, a number of rediscovered classics, which brought about an extension of the canon of required readings for scholars.

Philological expertise does not just help to improve the quality of texts. In 1440, the Humanist Lorenzo Valla, then in the service of the king of Naples, Alfonso of Aragon, proved on linguistic and historical grounds that the *Donatio* (or *Constitutum*) *Constantini* could not date from the time of the emperor Constantine (c. 280-337), but was a fake compiled in the Middle Ages (according to contemporary scholars, sometime between the seventh and ninth century), and that the temporal claims of popes were hence unfounded. In 1894 in France, the Romance philologist Paul Meyer applied his extraordinary paleographical abilities to some nineteenth-century hands, and thus gave the lie to the false accusations of betrayal leveled against the Jewish-Alsatian French officer Alfred Dreyfus in a trial that received widespread publicity and galvanized the interest of liberals as well as anti-Semites.

Less dramatically, philological skills can help art historians faced with problems of attribution, or historians *tout court* faced with problems of

authenticity or dating. (A painting cannot be ascribed to a major Florentine painter if words painted coevally in a cartouche in the painting reveal a northern origin. A treatise cannot be from the mid-thirteenth century if all its known copies refer to events that occurred in the last year of that century. And so on).

Bibliographical notes. An up-to-date introduction to biblical philology is offered by WEGNER, *A Student's Guide*.

On the difficult relationship between the sacrality of the text and biblical scholarship, I will only cite B. Chiesa, “La filologia della bibbia ebraica. Passato, presente e futuro”, in NOF, 59-84; G.L. Prato, “Gli scritti biblici tra utopia del canone fisso e fluidità del testo storico”, *Ecdotica* 7, 2010, 19-34.

The literature on Valla's *oratio* is also vast. We owe notable recent contributions on this subject to Riccardo Fubini and Carlo Ginzburg, among others.

On Paul Meyer, see A. Limentani, “Paléographie, épopée et affaire Dreyfus. Quelques remarques sur le thème: Paul Meyer et les Chansons de Geste”, in *Au carrefour des routes d'Europe: la chanson de geste. X^e Congrès International de la Société Rencesvals pour l'étude des épées romanes* (Strasbourg, 1985), Aix-en-Provence, Cuerma, 1987, II, 815-842 (It. transl.: id., “Meyer, l'epopea e l'affaire Dreyfus”, in *Alle origini della filologia romanza*, a cura di M. Mancini, Parma, Pratiche, 1991, 123-144).

4. Who's afraid of philology?

In several parts of the world and in many intellectual milieus it is fashionable to discredit textual criticism, and especially the genealogical-reconstructive method that constitutes the main theme of the present book. Adversaries of philological investigations have included, for example, the great philosopher Benedetto Croce, but also, in more recent years, so-called “deconstructionism”, disseminated by disciples of the philosopher Jacques Derrida especially in the United States, or certain somewhat naïve devotees of Hans Robert Jauss' aesthetics of reception who are much concerned with the reader's point of view, but not very interested in the author's, and accordingly incapable of reconstructing his or her text.

The French scholar Bernard Cerquiglini, in spite of having authored only one critical edition, and of a not especially difficult text at that, has become, possibly involuntarily, the champion of the so-called New Philology and, more generally, cultural relativism applied to textual criticism. Cerquiglini argues that nineteenth- and twentieth-century philology tended to erect “monuments” without regard for the variability of copies, and that it is a reactionary technique in the service of the ruling class:

Philology is a bourgeois, paternalist, and hygienist system of thought about the family; it cherishes filiation, tracks down adulterers, and is afraid of contamination. It is thought based on what is wrong (the variant being a form of deviant behavior), and it is the basis for a positive methodology (Cerquiglini, *In Praise...*, 49).

In the same vein, he argues that to try to reconstruct the original form of a text implies “an authoritarian theory of the subject”, etc.

But the truth seems to lie elsewhere. I am sure that, if he were looking for a new apartment, even Prof. Cerquiglini would be able to mentally correct “*Explosive* courtyard” to “*Exclusive* courtyard” (→ 1.2), and that this would not make him think of himself as an upholder of a “bourgeois, paternalist, and hygienist system of thought about the family”. To correct ancient texts, maybe in a different language from one’s own, is much harder and more laborious work than correcting a slip in a newspaper, but equally necessary.

The mental attitude of the conscientious philologist, who mistrusts the deformations and falsifications texts have undergone in the course of their transmission, and tries in every way to trace his or her way back from copies contaminated by errors—however worthy of study as specimens of “scribal culture”—to a text that is *as close as possible* to the lost original, is a powerful agent of demystification, much feared by both right- and left-wing totalitarian regimes. Continuing in the noble tradition of Valla and Meyer, two Italian philologists, Giorgio Colli and Mazzino Montanari, unmasked “the strategies that National Socialism employed to appropriate Friedrich Nietzsche—for example the falsification and recontextualization of his fragments to construct *Wille zur Macht*, the key text of Nazi propagandistic manipulation” (C. Urchueguía, “‘Kritisches Edieren’...”, 118).

And, in any case, understanding the most common mechanisms whereby errors are generated is an important resource even in everyday life in a world that, from the eighteenth century on, has become increasingly populated by textual messages.

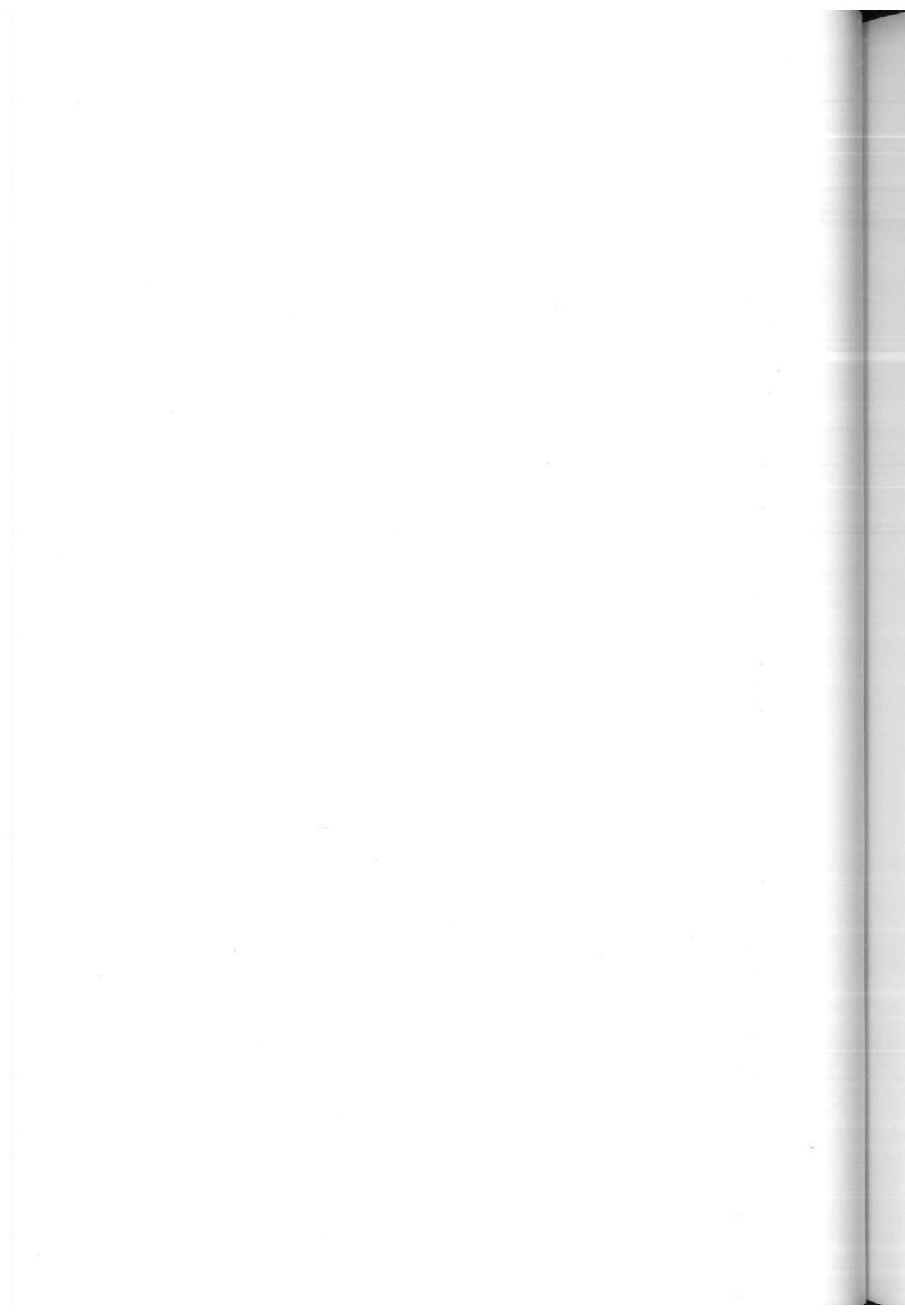
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86; P.F. Dembowski, "Is there a New Textual Philology in Old French? Perennial problems, provisional solutions", in *The Future of the Middle Ages*, 87-112; GLESSGEN-LEBSANFT, *Alte und neue Philologie*; A. Varvaro, "The New Philology from an Italian Perspective", in ID., *Identità*, 613-622; G. Orlandi, "Perché non possiamo non dirci lachmanniani", in ID., *Scritti*, 95-130; L. Formisano, "Gaston Paris e i 'nouveaux philologues'. Riflessioni su un libro recente", *Ecdotica* 2, 2005, 5-22: 5-9.

On New Philology, or rather its relationship with computer-assisted philology, → 4.

On the situation of textual criticism in Nazi Germany and the DDR: C. Urchueguía, "Kritisches Edieren". L'edizione critica in Germania oggi", *Ecdotica* 1, 2004, 116-156.

Some interesting reflections on the difficult relationship between Biblical philologists and the various Christian churches, albeit lapsing into some ideological excesses, can be found in L. Canfora, *Filologia e verità*, Milano, Mondadori, 2008.



PART 1
THEORIES



1. “LACHMANN’S METHOD”

1.1. A brief history

Ancient writings have not reached us as they left their authors’ hand. We have only copies, each at an unknown number of removes [...]. As every act of copying introduces fresh errors, the extant manuscripts differ among themselves and all differ from the lost original. Modern editors can often identify errors in manuscripts, as offences against grammar, poetic metre, consistency or good sense. Where two or more manuscripts share errors (or rather, readings that can safely be said not to have stood in the original) not attributable to coincidence, editors infer common descent. *These historical inferences*, which ideally add up to a stemma (i.e. family-tree), later *form a basis for choice between the rival readings in passages where intrinsic criteria fail*.

WEITZMAN, *The Evolution*, 287 (my emphasis)

Since the second half of the 18th century first biblical and then classical scholars and others have preached that part of the thinking [i.e., the judicious selection of variant readings] can be conducted less arbitrarily if all copies of a work are surveyed and their relationship established. It may be possible to portray these in a family tree, a stemma, which whenever the copies diverge will show at a glance which have preserved and which have abandoned their inheritance; *and one will be able to see beyond them, if not to the author’s text, at least to a purer text than any single one of them presents [...]*. Shared mistakes may show that what appeared at first to be 27 manuscripts of equal authority are tantamount to four or five, of which some have more authority than others; and in any passage

the distribution of variant readings among these four or five may leave no doubt about the reading of the archetype.

Michael D. Reeve, "Stemmatic Method: 'qualcosa che non funziona?'" (1986), now in ID., *Manuscripts and Methods*, 27-28 (my emphasis)

Although philological procedures are found in the Greek and Latin tradition, the Latin and Byzantine Middle Ages, Humanism, etc., the rise, or rather the fixation, of a relatively rigorous and consistent editing technique occurred, actually as a collective effort, in late eighteenth and early nineteenth-century Germany. The best known name among the scholars involved is that of the great Latinist Karl Lachmann (Braunschweig, 1793-Berlin, 1851). This technique was refined, as well as adapted to the peculiarities of medieval and modern texts, by Romance philologists, and especially the patriarch of French Romance studies, Gaston Paris (Avenay, 1839-Cannes, 1903).

¶ A detailed methodological analysis of all of Lachmann's philological works can be found in FIESOLI, *La genesi*, who reaches surprising conclusions (→ 2.2).

Among the works of Paris (on whom Ursula Bähler recently wrote a monograph), the most memorable is his edition of the *Vie de saint Alexis* (1872), the text with which, in the opinion of some great experts, "Romance textual philology begins" (CONTINI, 83 [= ID., *Frammenti*, II, 971]; see also REEVE, *Manuscripts and Methods*, 61ff.).

Paris' studies of assonance and rhyme in the various copies of the *Saint Alexis* are obviously inspired by Karl Bartsch's work on the *Nibelungenlied*, as we gather from an early review of Bartsch by Paris himself (*Revue critique d'histoire et de littérature* I/38, 22 septembre 1866, 183-189: 185), and as Paul Meyer confirms in his obituary of Gröber (*Romania* 40, 1911, 631-633: 632, note).

One of the most significant distinctions introduced by Paris in comparison with the reconstructive techniques of classicists is that between *critique des leçons* [analysis of readings], used to reconstruct the substance of a text, and *critique des formes* [analysis of forms], used to reconstruct the linguistic aspects of a text. The second problem is of negligible importance for classical literary works, written in "dead" languages that went through centuries of standardization, but ubiquitous and very delicate for works written in modern languages. These texts for a long time lacked a grammatical tradition, and copyists from different areas more or less easily adapted them to their own vernacular. Thus, along with the substance—determining whether the Saracen knight brandished a *sword*

and rode a *horse*, as in ms. A, or brandished a *lance* and rode an *elephant*, as in ms. B—the philologists studying medieval or Renaissance literature in modern languages such as Dutch, English or Italian, must, or should, also determine on various grounds (geographical origin and date of the author, provenance and date of the copies, etc.) whether to keep in their reconstruction of the text the forms *love* or *loove*, *lancia* or *lanza*, *ick* or *ic* or *ich* (→ 5).

¶ PARIS's distinction between *leçons* and *formes* (1872) is probably the origin of the common opposition in Anglo-American studies between *substantive readings* and *accidental readings*, introduced, as far as I know, by Greg in 1935.

After a first phase when we observe a huge technical gap, especially in the field of Romance philology, between philologically advanced nations and the rest (the former including France, with the above-mentioned edition of the *Vie de saint Alexis* and the journal *Romania*, founded by Paris himself and by Paul Meyer in 1872, and Germany, with its *Zeitschrift für Romanische Philologie*, founded by Gustav Gröber in 1877), in Italy, too, the genealogical method was applied in an exemplary way to works of great importance. Among the first exemplary editions of medieval texts produced in Italy, I will mention those of Dante's *De vulgari eloquentia* (1897) and *Vita Nuova* (1907 and, with minor changes, 1932), respectively edited by the energetic Pio Rajna (Sondrio, 1847-Florence, 1930) and the very lucid Michele Barbi (Taviano di Sambuca Pistoiese, 1867-Florence, 1941).

Bibliographical notes. The history of philology in the ancient world (and beyond) is very well outlined by REYNOLDS-WILSON, *Scribes and Scholars*. That of methodological advances from Humanism to the eighteenth century, and until Lachmann and beyond, has been masterfully reconstructed in its crucial stages by Sebastiano Timpanaro, and fleshed out in various aspects by Edward J. Kenney and Giovanni Fiesoli: TIMPANARO (MOST); KENNEY; FIESOLI, *La genesi*. See also, on the remarkable philological skills of the mathematician John Wallis (1616-1703), REEVE, *Manuscripts and Methods*, 297-313.

On Paris, see U. Bähler, *Gaston Paris et la philologie romane*, Genève, Droz, 2004.

On Italian studies of philology and linguistics after the unification, see A. Stussi, *Filologia e linguistica dell'Italia unita*, Bologna, il Mulino, 2014.

The following are pioneering editions, but already exemplary in their method: PARIS; Dante Alighieri, *La Vita Nuova*, per cura di M. Barbi, Firenze, Società Dantesca Italiana, 1907, or *La Vita Nuova di Dante Alighieri*, edizione critica per cura di M. Barbi, Firenze, Bemporad, 1932 (a slightly updated version of the former).

1.2. Classification of witnesses. Variants, polygenetic errors, and significant errors

You have to be painstakingly accurate—and, incidentally, do not expect to be praised for this because, in the words of A.E. Housman, accuracy is a duty and not a virtue.

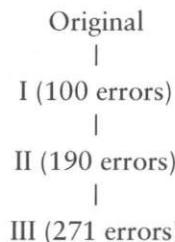
HUYGENS, 10

Sloppy and thus unreliable collations are bound to result in unreliable editions.

HUYGENS, 29

If, even before we tackled the manuscripts, we had access to a genealogical tree of the tradition of Dante's *Commedia*—which has been handed down in hundreds of copies—we could disregard all of the remotest descendants (great-great-great-grandchildren) and focus on ancestors, that is, the manuscripts closest to the lost original of the *Commedia* (the progenitor of the clan).

As we know, in the typical working conditions of copyists in the classical world and the Western Middle Ages, every act of copying introduces a certain number of important errors into the text. For simplicity's sake, let us assume an average rate of one error per page, and let us conservatively assume that the copyists are capable of correcting, on average, only about 10% of the errors in the exemplar they are copying from. Thus, if the original of text A takes up about 100 A4 pages, the first copy (I) should have 100 errors, a copy of I (= II) 100 + 90 errors, and so on, according to the following pattern:



The aim of the common error method is to determine the genealogical relationship between the surviving manuscripts to allow the philologist to discard the lower-ranking manuscripts—those containing more errors—and work on the remaining ones.

The most commonly adopted procedure for singling out these errors—often inaccurately described by computer-assisted editors—is the

following. We begin by setting as our term of comparison an apparently complete manuscript, one that is traditionally regarded as authoritative, or a randomly chosen one for want of a better alternative. We give it a label, such as A, and proceed to compare the first verse or, in the case of a prose text, the first line of A with the corresponding text portion of B, the second verse or line of A with the second of B, and so on, accurately recording any substantial differences. Once we have gone through all the differences between A and B, we go on to compare A with C, D, etc., until we have examined all the witnesses (manuscripts or printed editions, thus called because they bear witness to the content of the original text). The technical terms used for the action of comparing and the manuscript used for the comparison are respectively to collate (French *collationer*, It. *collazionare*, from the Latin *collatio* [comparison]) and collation text (It. *esemplare di collazione*). All the differences thus observed can be called, in a broad sense, readings or variants.

¶ For obvious reasons, the copy we are comparing the others with should never be changed during collation. This does not mean, however, that when, at a subsequent stage in reconstruction, we need to pick a basic text for our edition, the results of our genealogical classification and linguistic comparison of copies will allow us to use our collation exemplar for this purpose, as this could turn out to be low-ranking or from a linguistic area different than the author's.

Below is an example of a collation of the first verse of Guittone d'Arezzo's *Canzone XXII*, transmitted in four manuscripts: P (Palatino 418, now Banco Rari 217 in the Biblioteca Nazionale Centrale in Florence), L (Laurenziano Redi 9), V (Vaticano Lat. 3793), R (Riccardiano 2533). The example, where the collation also extends to graphic aspects—which can be ignored, as we shall soon see, during the classification stage—comes from Stussi's manual. Please note that when a reading in mss. L V R coincides with the reading in the collation text, P, the space in the corresponding column is left empty.

P	O	kari	frati	mei	ke	malamente
L		cari		mei	con	malame(n)/te
V	Oj	chari			che	
R		chari		mei	con	

In Stussi's own words:

I have not taken account of differences in word division (*mala mente* in V R), but for the rest have reproduced even purely graphic variants such as *kari*, *cari*, *chari*, *ke* and *che*. In L, I have indicated line breaks with a slash and spelled out in full the *titulus* for *n* between round brackets. In passing, it should be remarked that stains from damp make R difficult to read, and the use of an ultraviolet lamp to illuminate the text is hence in order. As to the interpretation, on which the following verses also shed light (*bendata hane la mente / nostro peccato e tolto hane ragione! [...]*), *con* is a truncated form of *come*, and hence an almost synonymous variant of *che*, *ke* (STUSSI, 118).

Thus, in the first instance collation provides the philologist with a simple list of variants (not yet of errors) in all the witnesses as compared with the collation text.

The fundamental principle of the genealogical-reconstructive method was formulated with particular clarity only after Lachmann's death, by the classical philologist Paul Lejay. It was already clear, however, to several of Lachmann's contemporaries. This principle is that the agreement of two or more witnesses in a good reading—that is, a correct one, one that reproduces the original, a primary one—is irrelevant: only shared errors—innovations, secondary readings—can provide certain proof that two or more copies are related (FROGER, 42, FIESOLI, *La genesi*, 32, 220, 263, 293, 393, and Michael D. Reeve, "Shared innovations...").

¶ Of course, as scholars have repeatedly remarked, some readings which, in the absence of alternatives, appear to be genuine ("good" readings), could actually be clever but nevertheless arbitrary corrections of old mistakes. However, to think that this possibility is statistically frequent, or even normal, goes against the principle of parsimony.

Among the first to prefer the term *innovation* to the traditional *error*—which I will use here, with not too many qualms, in the technical sense of non-original reading—was Paul Lejay in his famous 1888 review of Sabbadini ("une bonne leçon contre une faute ou plutôt une *innovation*"); the quotation is from REEVE, *Manuscripts and methods*, 58). Another remarkable example is in M. Roques, "Comptes rendus", *Romania* 69, 1946-1947, 117 ("Il faut avec un soin rigoureux tenter de déterminer ces fautes communes ou plutôt, pour ne pas s'en tenir à la notion incertaine de "faute", déterminer les *innovations* communes"). Paris himself already spoke of "certaines fautes ou certaines modifications" (PARIS, 24).

At least since the time of Pio Rajna (1907), but presumably from as early as the end of the nineteenth century, scholars have been pointing out that errors are not all equally useful. The only ones that really count are those that do not have an intrinsically high probability of occurring independently of the exemplar—that is, errors that are not polygenetic.

In other words, if in the whole corpus of surviving witnesses to Dante's *Commedia* two or more mss. have the standard version of the first verse, "Nel mezzo del cammin di nostra vita" [In the middle of the journey of our life], even though in different forms (*meçço* or *megio* instead of *mezzo*, *camin* or *chammin* instead of *cammin*), this will not help us in the least to produce a genealogical tree of the tradition of the text. If, instead, some mss. were to read "Nel mezzo del festin di nonna Rita" [In the midst of Grandma Rita's party], we would be entitled to believe that all these mss. were related.

Of course, in the real world innovations are neither so patent nor so obviously parodistic. If the copyist of our ms. writes *meçço* for *mezzo*, this is not an error, but a simple graphical variant. He might write *nossa* instead of *nostra*, but this is a mere phonemorphological variant, common in western Tuscany in the Middle Ages. If a scribe writes *iusto* [fair, right], but for the sentence to make sense this needs to be restored as *uisto*, that is, *visto* [seen], or vice versa, this is an error, but presumably a polygenetic one: an error, that is, that several copyists could have made independently of one another, given the difficulty of distinguishing between the *ui* and *iu* sequence in the scripts used between the twelfth and fifteenth century.

Until well into the sixteenth century the grapheme *u* was used in Europe both for the vowel *u* and the consonant *v*; and even in the fourteenth century it was still hard to distinguish between *m*, *in*, *ni*: tiny strokes and dotted i's were invented precisely to address this problem.

Variants and polygenetic errors—which together account for the majority of divergences between witnesses—are not useful for genealogically classifying witnesses, and can hence be ignored (of course, only for the purpose of genealogical classification). It is a striking paradox that the principal tool used by genealogical-reconstructive philology to purge texts of less conspicuous errors (latent errors, as Eklund calls them) is major errors, such as a lacuna not caused by a *saut du même au même* or other flaws that are undeniable, although it takes a watchful eye to detect them (as in the above-mentioned case of "explosive courtyard").

Only errors of this kind—errors that copyists can reproduce, but that as a rule cannot be made independently by several scribes (monogenetic errors)—should be used for the classification of witnesses.

To sum up: only important monogenetic errors should be used as indicative errors (Germ. *Leitfehler*, It. *errori direttivi* or *errori guida*) to reconstruct a genealogy of the copies known to us.

Bibliographical notes, MAAS (FLOWER), 42 ff. (although Maas' statement that "if we have three witnesses at our disposal, then we have twenty-two possible types of stemma" is unfounded). "Indicative error": the term itself was first introduced by Paul Maas in his 1937 article *Indicative Errors and Stemmatic Types*, later in ID. [FLOWER], 42ff. Among recent attempts to define the main types of presumably polygenetic errors, I will cite SALEMANS, *Building Stemmas*, 94-98 and *passim*; and C. Brandoli, "Due canoni a confronto: i luoghi di Barbi e lo scrutinio di Petrocchi", in NP, 99-214 (→ 3.1).

Examples of erroneous or imprecise descriptions of the common-error method include: DEARING, 5; VAN MULKEN, *The Manuscript Tradition of the Perceval*, 13ff.; ROBINSON, "Computer-Assisted Stemmatic Analysis", 72-73, who manages in a single breath to mistake the collation text with both the basic text (the one selected as the basis for the fixation of the text) and the reconstructed text (the final result of the editing process), and writes, among other things: "Classical stemmatic theory demands that all agreements by witnesses against the base be agreement in introduced reading. The base must represent at all points the 'original'; only when we have this base can we identify what witnesses have introduced unoriginal readings and so establish a stemma. E. Talbot Donaldson [...] has elegantly pointed out the illogic of this procedure" etc.

Michael D. Reeve's article "Shared Innovations, Dichotomies, and Evolution" (1998) is now in ID., *Manuscripts and Methods*, 55-103.

RAJNA's illustration of the basic principles of textual criticism comes across as surprisingly modern and at times almost Maesian in his "Testi critici", published in 1907, where he states that the method he is summarizing in his survey was "mainly fashioned by practitioners of the classical disciplines". On p. 213, for example, we read: "In B there are errors that do not appear in C, and are of such nature (the *lacunas* being especially significant) that the transcriber could not correct them by conjecture". On p. 215: "It would be a wonderful thing if water ran separately through canals and reservoirs all the way to the bottom, branching out, yes, but without mixing again". Although the watercourse metaphor—which incidentally is also used by Maas, Pasquali and Folena—goes all the way back to St. Jerome (FROGER, 268), a systematic investigation of the classical philology manuals used in the late nineteenth century would undoubtedly yield interesting results.

The assumption that in every new copy the copyist introduces new errors is one of the bases for the reconstructive method. It was confirmed experimentally by Kantorowicz, Pasquali and Peri (Pflaum), and is very easy to test by giving out a text for copying to a group of students or friends. As far as I know, the only scholar who has questioned this principle is James Grier, "Lachmann, Bédier and the Bipartite Stemma: towards a Responsible Application of the Common-Error Method", *Revue d'Histoire des Textes*, 18, 1988, 263-278: "It is true that the scribe has not been born who does not err, yet it is most assuredly not certain, no matter how probable it is, that he did make errors, or firm detectable errors, in every text he copied. If moreover this fatal assumption is accepted, the entire fabric of the common-error method collapses". Now, if what Grier is saying is that one copyist out of 5,000 or 10,000 is so accurate that he or she never makes significant mistakes, his statement is almost irrelevant, both in theoretical and in operative terms. Of course, modern experiments should reproduce at least one distinguishing characteristic of the professional production of copies in the ancient and medieval worlds, namely the basic economic need to proceed at a relatively rapid pace. A reasonable rule for an experiment would be to read one passage (Greek *pericopè*, It. *pericope*) at a time, for prose, and one distich or tercet at a time, for poetry, and then transcribe from memory what one has just read, without double-checking the text.

1.3. Genealogical criticism, formal logic, and probability calculus

General phenomena [...] require general explanations [...]. A mathematical model, as Kleinlogel and others urge, is not the same as the intricate processes of history. It can, however, establish a reasoned presumption, in the place of sheer conjecture.

WEITZMAN, "The Evolution", 288, 303

Some of the fundamental criteria of the common-error method are based on formal logic and the calculus of probability. The ABC of any genealogical classification, however complex the relations between the surviving witnesses, can be summarized as follows. Between any two mss. A and B, only 3 possible relationships can exist, Types 1, 2, and 3:



There are two necessary conditions for Type 1 (B derives from A) to 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 provides much more reliable proof than a single one).

Likewise, for Type 2 to apply (A derives from B), A needs to contain all the errors of B and at least one that is not in B.

For Type 3 to apply (A and B derive independently from a lost witness x), three conditions are required: A should include errors that are not in B (so far we could still be within Type 2, as this means that B cannot be copied from A); B should contain errors that are not in A (so A cannot be copied from B); A and B should share at least one error (or better, a certain number of errors), what is known as a conjunctive error (Germ. *Bindefehler*, It. *errore congiuntivo*).

The two primary conditions, that is, the existence of at least one separative error (Germ. *Trennfehler*, It. *errore separativo*) of A versus B, and one of B versus A, make Types 1 and 2 impossible. By exclusion, their relationship must be of Type 3.

Bibliographical notes. The opposition between *conjunctive errors* and *separative errors* was allegedly first formulated by P. Maas, "Indicative errors and stemmatic types" (1937), in ID. (FLOWER), 42-43.

Maas rightly specifies that, to be valid, a separative error of A versus B should be "so constituted that our knowledge of the state of conjectural criticism in the period between A and B enables us to feel confident that it cannot have been removed by conjecture during that period". Not all surviving traditions lend themselves to the application of Maas's prescriptions. Nevertheless the existence of long series of significant errors that are not separative in the strict sense affords the critic a rather easy way around the rule, since even the most gifted conjecturer could hardly have repaired *all* the damage in his or her exemplar.

I have not managed to gain access to A. Gercke, *Formale Philologie*, in A. Gercke, E. Norden, *Einleitung in die Altertumswissenschaft*, I, Leipzig und Berlin, Teubner, 1910, which L. Canfora ("Origine della 'stemmatica' di Paul Maas", *Rivista di filologia e di istruzione classica*, 110, 1982, 362-379) indicates as a certain precursor of Maas. Less rigorous formulations are found in HALL's nevertheless remarkable manual, published in 1913. On the contrary, at least from 1907 Rajna correctly enunciates the conditions for the occurrence of Types 1, 2, and 3, in terms close to those employed by Maas (RAJNA, "Testi critici", 212-213), and must hence be drawing, as I noted above, on late nineteenth-century manuals of classical philology.

1.4. The *stemma codicum*, the elimination of copies of preserved witnesses (*eliminatio codicum descriptorum*), and the majority principle

Stemmata are an objective and mechanical tool, invented to sort out, in the first instance, the quarrel between variants that are in themselves equally acceptable: [...] variants that are pronounced erroneous serve to brand as erroneous those that have remained equally acceptable.

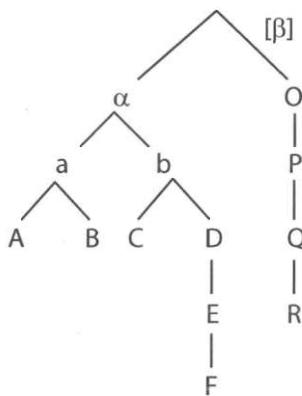
CONTINI, 72 (= ID., *Frammenti*, II, 961)

A very serious criticism of the stemmatic method is that it is a single, gigantic vicious circle [...]. This objection may seem well founded but it may, nevertheless, be rejected [...]. As I have just pointed out, the stemma is built up by means of the *manifest errors* but then it is not these same manifest errors which are corrected by means of the stemma [...]. No, the stemma is used to enable a choice to be made as soon as *latent errors* are encountered.

EKLUND, 12 (my emphasis)

As I hinted above, by using only a small part of the totality of the variants, that is, only significant errors that can legitimately be assumed to be monogenetic, we can put together a *stemma codicum*, i.e., a diagram clearly indicating genealogical relationships between surviving witnesses.

Let us suppose, for example, that the significant errors we have found by collating the witnesses of a given work lead to the following stemma:



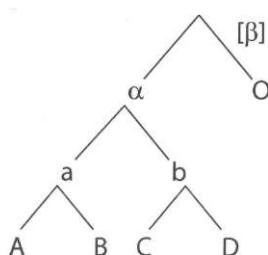
For the vertex of the diagram—which since Lachmann's time it is customarily called the “archetype”, and which will need some additional

explanation—I refer the reader to the following section (→ 1.5). The archetype aside, what we are looking at is a two-branched stemma, i.e., one that has two main branches. (This is the statistically more frequent type of stemma). I should mention right away that it is common practice to label surviving witnesses with the capital letters A, B, C etc., while using lower-case Greek or Latin letters for copies which, although lost, can logically be assumed to have existed. The left branch, α , in its turn, is divided into two subfamilies, a and b . Subfamily b is much more numerous than subfamily a . The right branch, β , is constituted only by O and its direct descendants.

Both in β and in α , many codices descend from copies that are still extant. These codices we designated as *descripti* [copied], one of many Latin words introduced by classicists into philological terminology. Considering that it is a well-proven fact that every new copy usually introduces new errors, it is evident that none of the descendants of ms. D is really necessary for improving our knowledge of the original text. We can, and indeed should, discard all *descripti*.

¶ Among classicists, the first stemma was introduced by Carl G. Zumpt in his edition of Cicero's Verrine orations (1831), immediately imitated by Friedrich W. Ritschl (1832) and Johan N. Madvig (1833). Allegedly the first textual critic to use a stemma was Carl Johan Schlyter, "in the first volume of an edition of ancient Swedish legal texts published in 1827" (TIMPANARO [MOST], 92 [= TIMPANARO, 61-62]).

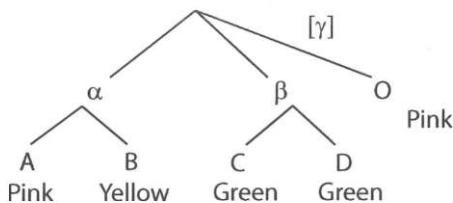
For clarity's sake, I will now show what cannot be eliminated from the stemma after this ideal cleanup, an operation technically known as *eliminatio codicorum descriptorum* [elimination of copies]—again, only those of witnesses that are still available.



Thus, to reconstruct (with some approximation) the vertex of the diagram we only need 5 mss., instead of 10. Let us note that A and B, which descend from *a*, and C and D, which descend from *b*, are not *codices descripti*, since copies *a* and *b*—which I have designated with lower-case letters—are not preserved, and thus can only be reconstructed through a comparison of their closest descendants.

Once we have weeded out *codices descripti* from our stemma, it is easier to see how it functions as a means of reconstructing the text by allowing us to choose among equally acceptable readings (It. *varianti adiafore* or *neutre* or *indifferenti*). It is hardly worth pointing out that the large majority of significant variants are of this nature.

If the heroine of the story to be reconstructed wears, in a certain passage of the text—a “variation place”—a sweater that is *pink* in some copies, *yellow* in others, and *green* in still others, to choose rationally between the variants *pink*, *yellow* and *purple* it is sufficient to apply the majority principle (It. *legge della maggioranza*). Let us assume a tripartite stemma with the following distribution:



Branch γ —which is worth 33% of the stemma, although it only comprises a single witness, O—has *pink*. *Pink*, however, also occurs in one of the two copies of branch α , namely A, which is worth half of 33%, that is, 16.5%. The variants *green* and *yellow* are worth, respectively, 33% and 16.5%. On the strength of the majority principle, *pink*, which is worth $33 + 16.5 = 49.5\%$, is the variant most likely to derive from the vertex, i.e. the archetype, and it is legitimate to assume that the two remaining variants were intentionally or unintentionally introduced, respectively, in β (*green*) and B (*yellow*).

As we have seen, by “majority” here what is meant is not the majority of all the mss., or all the branches, but a qualified majority, so to speak.

Only primary branches count, that is, those that branch out from the vertex. If we follow this method, a relatively low number of significant errors—as long as they are sufficient for building a *stemma codicum*—will allow us to rationally choose the most likely alternative among innumerable equally acceptable variants in the text (*yellow sweater* or *pink sweater*? or, in Dante, *tinto aspetto* or *tristo aspetto*? *durerà quanto il mondo* or *durerà quanto il moto*?).

* * *

So far, the discussion in this section has been based, for didactic reasons, on an imaginary stemma, and one not founded on significant errors. In real life, the stemma is only arrived at after long preparatory work, that is, after the longest possible lists of conjunctive and separative errors have been compiled to justify each split in the stemma. (As I observed above, the “at-least-one-error” rule, that is, the rule found in manuals, is sufficient to meet the minimum requirement, but it is a rather fragile base, especially in a fairly long text). The essential rules for the construction of a stemma, even an extremely complicated one, are nevertheless limited to what I said above concerning the three possible types of genealogical relationship between 2 mss. (→ 1.3).

Bibliographical notes. Conceptually, it is advisable to distinguish between variant readings, or variants (French *variants*, It. *varianti*), and the places in the text where they occur, i.e. variation places or points of variation (French. *lieux variants*, It. *luoghi di variazione*). From my personal experience, I would argue that variation places containing significant errors are usually found in less than 10% of the verses or lines of a text. Especially in texts with an abundant tradition, many substantial variants can be found in a single point of variation. Indeed, the number of variants seems to be directly proportional to the number of surviving witnesses, but their increase tends to stabilize, following a saturation curve, once most of the witnesses have been collated.

Even the length of variants can be a problem. Those who use computer programs tend to single out very short ones, of one or two words. Some indications in this regard can be found in Jean Duplacy, “Préalables philologiques à la classification automatique des états d'un texte”, in PdO, 23-33. Excessive breaking down of verses or prose, however, makes it harder to assess the “logic”, if any, of variants, and the sequence of complex variants, whereas such an assessment is necessary when applying the criteria of *lectio difficilior* and diffraction (→ 3.2). Given that “some variants comprise more than one verse, interpolations and omissions most often concern verse pairs, while changes in

narrative perspective can entail a large number of verses”, I am in full agreement with the criterion proposed by Van Mulken: “The length of a variant is determined by whether or not it can be considered to be one single independent genealogically transmittable item. If a copyist of the *Perceval* decides to leave the complete passage of the damsel with the short sleeves out of his transcription of the whole text, then this is to be considered one variant” (VAN MULKEN, “The Manuscript Tradition of the *Perceval*”, 17).

Following in the wake of other New Testament scholars, Eldon J. Epp (“Towards the clarification of the term ‘textual variant,’” in *Studies in New Testament language and text. Essays in honour of George D. Kilpatrick on the occasion of his sixty-fifth birthday*. Ed. by J.K. Elliott. Leiden, Brill, 1976, 153–173: 173) proposes to distinguish between variant and variation-unit (“the shortest grammatically-related segment of text that will still encompass all the variants from across the manuscript tradition that present themselves at that point”), but after one has recalled that a variant can consist of a single word or a whole passage, and that it occurs in a specific variation place, the distinction no longer appears to be especially advantageous.

As we shall see in greater detail further on, in the second half of the twentieth century there was a spread of what we may call “partial” uses of the stemma. These often fail to apply one of the most valuable tools of the reconstructive method, namely, the majority principle (FOULET-SPEER, 49–50; DUVAL, *Pratiques philologiques en Europe*, 29 and *passim*; → 3.4).

1.5. The archetype

Since the Renaissance, when scholars at work on the text of Greek and Latin authors took it up, the classical term ἀρχέτυπον has been used in so many senses that no-one today can safely use it without defining it.

Michael D. Reeve, “Archetypes” (1986), now in ID., *Manuscripts and Methods*, 107

I have devoted a special section, this one, to the discussion of the vertex of a stemma, which—when its existence can be demonstrated—is called “archetype”. *Archetype* is truly a multi-layered and ambiguous word, which has created some problems even for highly competent scholars, from Pasquali to Timpanaro, West and Contini. In Timpanaro’s words:

Scholars used to think that the Humanists (like the ancients before them: cf. Cicero, *Ad Att.* 16.3.1) meant by the term *archetypum* or *codex archetypus* only the “official text” checked by the author and intended to be published afterward in further copies. A wider and deeper examination (Rizzo 1973: 308–317) has made it clear that alongside that meaning

(perhaps the prevailing one) the term also has many other usages in the Humanist age (TIMPANARO [MOST], 49-50 [=TIMPANARO, 20]).

Henceforth I shall use *archetype*₁ in the first, i.e., the most widespread, of these ancient and Humanist meanings, that is, in the sense of "official text, prepared by its author for publication".

However, in the usage of some fifteenth- and sixteenth-century Humanists (Merula, Poliziano, Erasmus), *archetype* can also mean "ancestor of a tradition" (Rizzo). Elaborating on this meaning, the great textual critics of the early nineteenth century, and especially Lachmann, introduced a decidedly different one into the philological lexicon,

namely that of *a manuscript*—even if it is later than the author by many centuries, even if it has been preserved by chance and is devoid of any 'official' quality or authority, even if it is *disfigured by errors or lacunas—from which all the others are derived* (TIMPANARO [MOST], 50 [=TIMPANARO, 20]; my emphasis).

Thus, by further refinements, scholars evolved increasingly rigorous formulations, such as "a lost copy marred by at least one error of the conjunctive type, from which the whole tradition derives".

In this second meaning, the archetype (*archetype*₂) can be reconstructed logically—with varying degrees of accuracy—on the basis of the number of conjunctive errors that are common to all its descendants; that is, by subtracting from the total number of errors attested in the tradition all separative errors that are exclusive to each family of witnesses, and those characterizing individual witnesses. All practitioners of the genealogical method, from Pasquali to Irigoin, and from Blecua to Kenney and Reeve, essentially employ the term in this technical sense, although not without deviations, even substantial, or laudable attempts at revision.

However, as I noted above, even highly experienced and sophisticated scholars appear—at least in some cases—not to have broken completely free of sense 1 of the term *archetype*. These scholars do not view the archetype as a manuscript whose existence is "by chance" detected by philologists within the stemma, but as an especially authoritative exemplar, or as the result of a sudden and inexplicable bottleneck in the ancient and medieval tradition whereby only one copy survived. Kantorowicz, for example, observes that

it is possible, and often even necessary [*sc.* in the study of classical traditions], to distinguish from the original an “archetype”, which a “textual history”, often centuries long, separated from the original, and *which was saved by chance, or because it carried an erudite version*, until it came down, alone or with only a few companions, to a time closer to our own (H. Kantorowicz, *Introduzione...*, 13; my emphasis).

Pasquali writes:

Lachmann founded his method on the assumption that the tradition of every author always and in every case harked back to a single exemplar that was already disfigured by errors and *lacunas*, which he called the archetype. No one doubts that this is mostly the case [...]. On careful consideration, *it must appear unlikely that in every case 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 civilization (PASQUALI, 15; my emphasis).

Contini underlines:

Classical philologists take care to specify that reconstruction leads back, not to the original, but to the *ancient copy of the most authoritative edition of the text* (CONTINI, 76 [= ID., *Frammenti*, II, 965]; my emphasis).

Timpanaro, in his turn, observes:

Some years ago [*sc.* in 1981], Courtney wrote a curious essay where he postulates a fourth-century archetype for all the Virgilian codices that have come down to us [...]. He *appears to be aware of the unlikelihood that a single copy of the Virgilian text was preserved at a certain point in antiquity* (Timpanaro, *Per la storia della filologia virgiliana...*, 181; my emphasis).

Observations of this kind end up confusing the meaning that the word *archetype* often had in the classical world (*archetype₁*) with that used by philologists from Lachmann on (*archetype₂*). To phrase it as Pasquali would, the result is a confusion of two conceptual spheres that we need to keep distinct: the *history of the tradition*, with its unattainable real trees, that is, the often very rich ensemble of all mss. that historically existed, including those that disappeared without leaving traces, and *textual criticism*, with its very tangible although perfectible *stemmata codicum*, based on the few mss. that have come down to us.

Recent examples of this confusion are found in MONTANARI, 424-425 (§ 132.6.1), 426-427 (§ 132.7.3), and in Michelangelo Zaccarello's fine essay "Metodo stemmatico ed ecdotica volgare italiana. Brevi considerazioni su alcuni recenti contributi metodologici", *Textual Cultures* 4/1, 2009, 55-71: 64.

Actually, 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 the case of Dante's *Commedia*, for example, it is hard to go much further back than 1330-1331 (the age of lost manuscript *a*, the earliest that can be certainly dated). The first generations of manuscripts, copied between Dante's death in 1321 and 1330—inevitably less numerous than those produced serially between 1330 and 1360, or in the fifteenth century—have all disappeared. 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.

In other words, 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.

In the light of the above considerations, the most complete and rigorous definitions of *archetype* offered by the recent manuals I am familiar with are those of Blecuá and Stussi. These authors also explain the logico-formal conditions proving the existence of an archetype:

Un códice o impreso perdido, X, o conservado, A, B, C, etc., que transmite errores comunes a todos los testimonios (BLECUÁ, 71).

A lost copy marred by at least one error of the conjunctive type, from which the whole tradition derives (STUSSI, 121).

However, although it may seem superfluous (both definitions occur in chapters with eloquent titles: “Constitutio stemmatis”, “Articolazioni dello stemma”), they should possibly be reformulated as follows:

Un códice o impreso, perdido [...], que transmita errores comunes a todos los testimonios *conservados*.

A lost copy marred by at least one error of the conjunctive type, from which the whole *surviving* tradition derives.

Bibliographical notes, J. Irigoin, “Quelques réflexions sur le concept d’archéotype”, *Revue d’Histoire des Textes*, 7, 1977, 235-245; M.D. Reeve, “Archetypes”, *Sileno*, 11, 1986 (= *Studi in onore di A. Barigazzi*), 193-201, now in ID., *Manuscripts and Methods*, 107-117; P. Trovato, “Archetipo, stemma codicum e albero reale”, *Filologia italiana*, 2, 2005, 9-18); → 3.5-7.

I quote Kantorowicz from the recent Italian edition with commentary: H. Kantorowicz, *Introduzione alla critica del testo. Esposizione dei principi della critica del testo per filologi e giuristi*, a cura di L. Atzeri e P. Mari, Roma, Istituto storico italiano per il Medio Evo, 2007 (original ed.: Leipzig, Dieterich’sche Verlagsbuchhandlung, 1921).

Blecua and Reeve distinguish between *stemmata* with a reconstructed archetype and *stemmata* with a preserved archetype; however, like Maas, Contini, Stussi, Montanari and others, I believe a different term should be used in the second case.

The important studies by Silvia Rizzo and Sebastiano Timpanaro quoted above are, respectively, *Il lessico filologico degli umanisti*, Roma, Edizioni di Storia e Letteratura, 1973 (reprint 1984) and *Per la storia della filologia virgiliana antica*, Salerno ed., 1986.

1.6. Two complementary approaches to the genealogical-reconstructive method: Paul Maas and Giorgio Pasquali

The contrast between the mathematical mentality of Maas (who was interested above all in the rigorousness of his formulations, without their always being rigorous in fact), and the lively sense of the uniqueness of each manuscript tradition that animates Pasquali’s exposition, leaps to the eyes of every reader.

TIMPANARO (MOST), 130 (=TIMPANARO, 101-102)

Nonetheless, although in more recent works the separation between history of the tradition and textual criticism has now taken place and been codified, in Pasquali’s book the two disciplines are still conjoined. Pasquali’s interest in the vicissitudes of Classical texts in the medieval and Humanist period, lively as it was, never makes him forget his job as textual critic and interpreter. It is in this combination of a broad perspective

in cultural history with an acute philological intelligence directed to the individual passage of an ancient author that the unmistakable character of Pasquali's work resides.

TIMPANARO (MOST), 138 (= TIMPANARO, 20)

Paul Maas (Frankfurt, 1880-Oxford, 1964) was a prominent German classical philologist and Byzantinist. A pupil of Ulrich von Wilamowitz-Moellendorff in Berlin, he later moved to Munich, where he studied Byzantine literature with Karl Krumbacher. He then became a professor in his turn, first at the University of Berlin, then in Königsberg. He edited works by Byzantine poets, Apollonius Dyscolus, and Romanos the Melodist. Several of his textual emendations (especially of Pindar and Bacchylides) aim at metrical regularization. His solid skills as a metricist and textual critic are reflected in his manuals *Griechische Metrik* (1923) and *Textkritik* (1927; 4th ed., enhanced, 1960).

In 1934, the Nazis removed him from his university chair. He lived in seclusion until 1939, when he migrated to Oxford, living off prestigious but badly paid collaborations (he was the author, among other things, of the supplements to *Liddell and Scott's Lexicon*). According to TIMPANARO (MOST), 187, note * (= TIMPANARO, 164), "his best work [...] is to be found in the notes on individual passages of ancient authors now collected in Maas 1973 and in the contributions he made, with equal acumen and modesty, to many editions of the *Bibliotheca Oxoniensis*, of which he was an invaluable proofreader".

His extremely popular manual of textual criticism, translated into English with the title *Textual Criticism* in 1958 (Oxford, Clarendon Press), provides an at once rigorous and concise illustration (or, if one prefers, a compendium in the form of rules) of the logical and statistical foundations of genealogical-reconstructive criticism. An expert like Michael Reeve regards him as "the first philologist to construct a theoretical system of genealogical classification for a textual tradition" (REEVE, *Manuscripts and Methods*, 46-47). The latest Italian edition of Maas's manual, published in 1990, was reprinted in 2003 with a 500-page continuous commentary by the classical philologist Elio Montanari, under the title *La critica del testo secondo Paul Maas: testo e commento* [Textual Criticism according to Paul Maas. Text and Commentary] (= MONTANARI).

One of the most striking characteristics of Maas's *Textkritik*—besides the terseness of its formulations and its organization along the lines of a hard-science manual (see, by way of example, MAAS [FLOWER], 2-3,

reproduced in Figures 1 and 2)—is its attempt to define the limits of applicability of the genealogical method. It is not applicable, Maas argues, to contaminated texts, and is hence ineffective when dealing with late traditions, dating from philologically mature ages, such as the Byzantine Middle Ages or Humanism.

2	RECENSIO	B
B. RECENSIO		
(cf. § 25)		
3. The tradition rests either on a single witness (<i>codex unicus</i>) or on several.		
In the former case <i>recensio</i> consists in describing and deciphering as accurately as possible the single witness; in the latter it is often a very complicated business.		
4. Each witness depends either on a surviving or on a lost <i>exemplar</i> . If it depends on a lost exemplar, this lost exemplar either can or cannot be reconstructed. If it can be reconstructed, this may be done either without the aid of the witness or only with its help.		
It will now be obvious that a witness is worthless (worthless, that is, <i>qua</i> witness) when it depends exclusively on a surviving exemplar or on an exemplar which can be reconstructed without its help. A witness thus shown to be worthless (cf. § 8) must be <i>eliminated</i> (<i>eliminatio codicum descriptorum</i>).		
5. If there still remain several witnesses after the <i>eliminandi</i> have been excluded (§ 4), then we have a <i>split</i> in the tradition. This can only arise if two or more copies were made from a single exemplar; the ‘branches’ of the tradition arising in this way appear in the surviving witnesses, with or without further splits (<i>intermediate splits</i>).		
The exemplar from which the first split originated we call the <i>archetype</i> . The text of this archetype is free from all errors arising after the split and is therefore closer to the original than the text of any of the witnesses. If we succeed		

Figure 1. MAAS (FLOWER), 2.

then in establishing the text of this, the *constitutio* (reconstruction of the original) is considerably advanced.

The special importance of the exemplar which I have termed the archetype is not contested, and there is no other name available. For this reason we should be careful not to use the term archetype of other connecting links between the original and the surviving witnesses, however important they may be at times. [This reminder has again become very necessary at the present time. 1956.]

6. In what follows it is assumed (1) that the copies made since the primary split in the tradition each reproduce one exemplar only, i.e. that no scribe has combined several exemplars (*contaminatio*), (2) that each scribe consciously or unconsciously deviates from his exemplar, i.e. makes 'peculiar errors'.

On the consequences of a different set of assumptions see §§ 9, 10, 11.

7. On these assumptions it becomes possible in general (a) to demonstrate uncontestedly the interrelationship of all surviving witnesses, as well as the number and position of all intermediate splits in the tradition, (b) where the primary split is into at least *three* branches, to reconstruct with certainty the text of the archetype at all places (with a few exceptions to be accounted for separately), (c) if the primary split is into *two* branches, to restore the text of the archetype to a point where (again with exceptions to be separately accounted for) we have at no place more than two readings (*variants*) from which to choose.

8. A typical instance (see Diagram). Given are the witnesses A to J (not K), all differing in date and in kind (manuscripts, printed copies, epitomes, excerpts, paraphrases,

Figure 2. MAAS (FLOWER), 3.

* * *

As the title of this section suggests, Maas's approach was complemented by that of his brilliant contemporary Giorgio Pasquali (Rome, 1885-Belluno, 1952). Pasquali studied with Nicola Festa at the University of Rome. His later education in Göttingen in 1908 and 1909

was equally important. He was a temporary professor of Greek and Latin Grammar in Messina in 1911-1912 and subsequently in Göttingen from 1912 to 1915, and a temporary professor of Greek Literature in Florence from 1915 to 1920. He became a full professor of Greek Literature in 1924, and later took over the chair of Greek and Latin Literature. In 1925, he was one of the signatories of the *Manifesto degli intellettuali antifascisti* [Manifesto of Anti-Fascist Intellectuals], written by Benedetto Croce. In the 1930s he supplemented his teaching in Florence with a professorship in Classical Philology at the Scuola Normale Superiore in Pisa, where he trained generations of top-notch scholars, including Lanfranco Caretti, Gianfranco Folena, Scevola Mariotti, Aurelio Roncaglia, and Carlo Ferdinando Russo. Among other things, he studied Neo-Platonic texts, Eusebius's *Life of Constantine*, Pausanias, Callimachus, and the poetry of Horace and its relationship with its Greek models.

His masterpiece, *Storia della tradizione e critica del testo* [History of textual tradition and textual criticism], was written under the stimulus of the publication of Maas's *Textkritik*, which Pasquali reviewed "with great kindness and in great detail" (in Maas's own words). Pasquali, however, also introduced into textual criticism, or recovered for it, some themes and approaches that Maas, whom I again quote, had "excluded from [his] presentation" (MAAS [FLOWER], page not numbered).

What these themes and approaches were can be gathered, at least in part, merely by skimming the index of Pasquali's book—which abound in classical Greek and Latin examples, but also includes, albeit less frequently, examples from Italian texts written in Latin or *volgare* [vernacular]. After the Preface and Chapter I (ten pages entitled *Lachmann's Method*) we find the following chapters, whose length I also specify, as it varies greatly:

II. *Ci fu sempre un archetipo?* [Was there always an archetype?]: 7 pages.

III. *Eliminatio codicum descriptorum* [The elimination of *codices scripti*]: 16 pages.

IV. *Recentiores non deteriores. Collazioni umanistiche ed editiones principes* [More recent, not inferior. Humanist collations and *editiones principes*]: 66 pages.

V. *Tradizione meccanica e varianti medievali* [Mechanical tradition and medieval variants]: 73 pages.

VI. *Varianti antiche e antiche edizioni* [Ancient variants and ancient editions]: 205 pages, with sections on Euripides, Homer, Plato, Terence, Horace, Ovid, etc.

VII. *Edizioni originali e varianti d'autore* [Original editions and authorial variants]: 67 pages.

While we cannot help being impressed by the farsightedness, or rather the contemporaneity, of chapters such as II and VII, it should be said from the start that, far from placing himself outside or at the fringes of the genealogical method, Pasquali actually improved its effectiveness thanks to his knowledge of dozens of case studies, his concreteness, and his sense of history. It is sufficient to remember that, distancing himself from Lachmann and many other classical philologists' mistrust, even as late as the twentieth century, of late copies (the *itali*, that is, Humanist mss. produced in Italy, which were suspected of arbitrarily emending errors in texts), Pasquali argues for full collation of all the witnesses. Notably, he recalls the many examples of "valuable Greek codices that were still accessible and transcribed in the sixteenth century, and then disappeared", and warns that "the same thing occurred, possibly in even greater measure, with Latin codices" (PASQUALI, 49).

The twelve-item resumptive "decalogue" contained in the Preface is also important. I will only quote numbers 2, 3 and 8, which draw on what, at the time, was the new-fangled discipline of geolinguistics or spatial linguistics to provide a scientific foundation for some intuitions of Lachmann as a New Testament scholar:

- 2) The Byzantine age in the East, and the Carolingian age and Renaissance in the West, possessed many more manuscripts of still-preserved classics than is usually supposed. As late as the sixteenth and seventeenth centuries, French and Flemish scholars or printers ruined, lost or destroyed some very valuable manuscripts [...].
- 3) For the same reasons, in the tradition of Latin authors it is always likely *a priori* that late witnesses descend, totally or partially, from ancestors other than those the earlier witnesses descend from. A *recentior* is therefore not necessarily a *deterior*. The authority of a witness is independent of its antiquity.
[...]
- 8) 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 occur-

rence 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 center and from one another constitute 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 center towards the periphery, but do not always reach the periphery (PASQUALI, XVI-XVII).

Many terminological oppositions coined or redefined by Pasquali are still valid today, such as those between *vertical* and *horizontal* (that is, contaminated) traditions, or between *textual criticism* and the no less important *history of textual tradition*, or between closed *recensio*, to which the majority principle applies, and open *recensio*, where determining the correct reading remains problematic (Timpanaro, however, prefers the more analytic term of “non-mechanical” *recensio*).

I think it is legitimate to credit Pasquali, at least until proven otherwise, with the handy distinction between *polygenetic* and *monogenetic* errors, which is especially common in Italian studies. See for example his statement:

Corruptions common to a whole tradition, so obvious that they may have occurred independently even in independent mss., by ‘polygenesis,’ do not require it [an archetype; viz., they are not sufficient proof of common origin from an archetype; the reference is to the tradition of Tertullian’s *Apology*]” (PASQUALI, 19).

Here the quotation marks suggest a personal use of a term that has not yet become established.

¶ In lieu of *polygenesis*, *polygenetic*, French philologists have been using, at least since the time of Louis Havet (1911), the more opaque term **parallelism*. Anthonij Dees, in 1976, speaks of “rencontres fortuites”. Dutch stemmatologists use *parallelism* or the synonyms *homoplasy*, *coincident variation*, *convergent variation*, *accidental variation*: Ben Salemans, “Cladistics or the Resurrection of the Method of Lachmann...”, 10 with note. On the other hand, Michael D. Reeve often uses the adjective *polygenetic*, I think in the wake of Pasquali and Timpanaro.

In an interesting essay on the Persian tradition of a tenth-century Arab chronicle, Andrew Peacock mistakenly attributes the paternity of the concepts of “vertical and horizontal transmission” to REYNOLDS-WILSON, *Scribes and scholars* (“The Mediaeval Manuscript Tradition of Bal’amī’s Version of al-

Tabarī's History", in PFEIFFER-KROPP, *Theoretical Approaches*, 93-105: 102 note 30).

In a no less interesting review of the philology of Arab texts in the Middle Ages, the codicologist Jan Just Witkam mistakenly attributes the term "open recension" to WEST, 37-38 ("Establishing the stemma: fact or fiction?", *Manuscripts of the Middle East* 3, 1988, 88-101: 99 note 37). The opposition between "recension fermée ou tradition fermée" and "recension ouverte ou tradition ouverte" is mentioned in the *Petit lexique* by BOURGAIN-VIELLIARD, 213, s.v. *recension*. I assume that the expression "tradition resserrée", found in DUVAL, *L'édition*, 149, is modelled on Pasquali's "recensione chiusa" [closed recension]. See also MONTANARI, 437-446.

Pasquali's *Storia della tradizione* should be compulsory reading, right after Maas' *Textual Criticism*, for all would-be philologists, whatever their specialty. As Pasquali himself wrote in his presentation of the Italian translation of Maas's manual:

I, at least, cannot imagine that the original, say, of a Chinese or Bantu text could be reconstructed from copies or any other testimony, in sum, from its tradition, otherwise than on the basis of Maas' considerations and the rules he laid down (in MAAS [MART.], V).

Likewise, it is hard to imagine that a Chinese or Bantu text could be handed down independently of real historical conditions, and for dealing with these Pasquali's great book is an indispensable roadmap.

Bibliographical notes. MAAS (FLOWER); G. Pasquali, review of MAAS, *Gnomon*, 5, 1929, 417-435, 498-521 (later in id., *Scritti filologici. II: Letteratura latina, cultura contemporanea, recensioni*, edited by F. Bornmann, G. Pascucci, S. Timpanaro, Firenze, Olschki, 1986, II, 867-914); M.D. Reeve, "Da Madvig a Maas, con deviazioni" (2005), now in ID., *Manuscripts and Methods*, 44-49. Maas' volume should be read now with MONTANARI's commentary, which was discussed in its turn by P. Chiesa, "La critica del testo secondo Paul Maas", *Ecdotica*, 1, 2004, 65-77.

Information about Pasquali's teaching assignments in Pisa can be found in A. Stussi, "La filologia italiana di Aurelio Roncaglia", in *Aurelio Roncaglia e la filologia romanza*. Convegno Internazionale (Roma, 8 marzo 2012), Roma, Scienze e lettere, 2013 ("Atti dei convegni Lincei", 273), 61-73: 62 and footnote 10.

Storia della tradizione e critica del testo (= PASQUALI; first ed. 1934) was reprinted, with Preface, appendices and useful analytical indexes, in 1952, 1974 and 1988.

One of the most remarkable reviews of the first edition of Pasquali's book is by a very young Contini. It was published in 1935 and elaborates on some of Pasquali's observations, extending the discussion to Bédier and dom Quentin's coeval methodological proposals: CONTINI, *Frammenti*, I, 99-112.



2. BÉDIER'S SCHISM

2.1. Bédier's objections to Gaston Paris's method

La tradition d'un manuscrit représente du moins quelque chose de réel.
Joseph Bédier, in Thomas, *Le roman de Tristan*, par J. B., II, Paris, Didot, 1905, 35

Some scholars have gone so far as to forgo any attempt at a classification of manuscripts; they prefer to single out one manuscript that is on the whole more satisfactory than the rest and follow this *codex optimus* except where it is defective or unintelligible; there, and there only, they would have recourse to other manuscripts or to emendation at their own risk. This practice has no less a champion than Professor Bédier [...]. To be sure, it does away with a great deal of trouble—but is it really safer or more objective? Unless an extant manuscript is demonstrably the original or at least the archetype (in which event it would be treated as unique no matter what method the editor adopts), the choice for a basic text of one manuscript rather than another rests on the same subjective considerations as does the old-fashioned *stemma codicum*; and if this *textus unius codicis* is safe from the fallacies of *recensio*, it has admittedly no determinable relation to the original—*in other words, it is deliberately uncritical*. The inevitable improvements upon the basic manuscript must be arbitrary, even if the editor has tact and taste; where these indefinable qualities are lacking, we may be prepared for the worst [...]. *In the last analysis this is the abdication of reason in a sphere where it was least expected.*

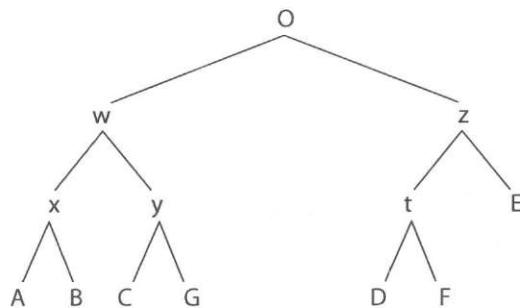
BIELER, 18-19 (my emphasis, except for Latin words)

Between the late 1920s and early 1930s, genealogical criticism seemed to be faring very well. Maas (1927) had reformulated with remarkable effectiveness most of the ground rules of the method in a set of brief and clear principles. Pasquali (1934) had boldly expanded the field of philological inquiry to areas Maas had excluded, but which some Italianists investigated in depth, such as authorial variants (French scholars were to "reinvent" *critique génétique* in the last quarter of the last century). However, as early as 1913 and more effectively in 1928, one of the most renowned French Romance philologists, Joseph Bédier (Paris, 1864–Le Grand-Serre, 1938), expressed a number of often radical perplexities regarding the genealogical-reconstructive method developed by German scholars; which, incidentally, was the method used by his mentor Gaston Paris, as well as by dozens of editors who followed in Paris's wake, albeit sometimes in a naïve and excessively mechanical way.

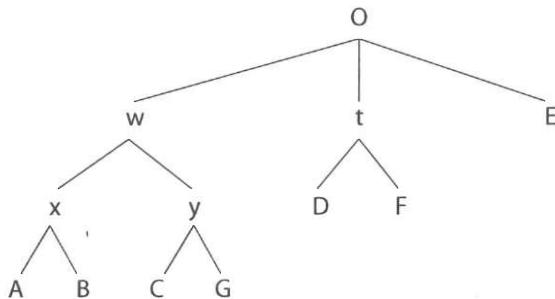
Although today, a century later, we can prove that Bédier's principal objections were unfounded, the prestige of the great scholar and his extraordinary gift for argumentation brought about an irremediable schism in the relatively peaceful world of textual scholars. While classicists remained essentially unaffected, almost all francophone Romance philologists and many Biblical philologists rejected the common-error method. Advocates of Factor Analysis, like John G. Griffith, followed in their wake in the 1970s, as have many Anglo-Saxon or Dutch propounders of Computer-Assisted Philology who over the last decades have been striving to adapt to philological research software programs that were originally developed by biologists for studying evolution. Many contributors to this trend have not read a single important genealogical philology study published in the last seventy years (→ 4).

Nevertheless, the questions raised by Bédier, which are intimately connected to the methodological refinements introduced from 1928 to the present day to refute his criticism, remain of the highest interest.

In 1890, Bédier published, in the manner of Gaston Paris, a short poem by Jean Renard, the *Lai de l'ombre*. The two-branched stemma he proposed:



was immediately rejected by Paris in an overall very laudatory review (*Romania*, 19, 1890, 609-615), where he proposed, however, a three-branched stemma:



The fact that two competent scholars employing the same method—although sometimes in ways we would today call naïve—ended up reconstructing two different stemmata, with all the implications that the shape of a stemma has for the reconstruction of a text, led Bédier to radically rethink his approach. In the words of STUSSI, 275:

The history of this crisis and its final outcome are told by its protagonist himself in the article that brought this vexed question to a close in 1928: the mechanical nature of the Lachmannian method is illusory [viz., according to Bédier] because, since most stemmata are two-branched, the decisive choices depend exclusively on the will of the philologist, who ends up putting together readings of different origins, creating a new text that never existed in reality. It is thus best to choose a good manuscript, the best, if possible, and limit ourselves to reproducing it, introducing only obvious and indispensable corrections. Only thus will we be sure we are reading something that had a historical existence, and not the subjective product of the combinatory taste of a modern scholar.

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. In his own words:

Tous sont pareils, ou du moins 105 sur 110 sont pareils. D'où une loi, qui peut s'exprimer ainsi: dans la flore philologique, il n'y a d'arbres que d'une seule essence: toujours le tronc s'en divise en deux branches maîtresses, et en deux seulement [...]. Tout philologue qui publie un texte après étude des copies diversement altérées que nous en avons, arrive fatalement à se persuader que ces copies, si nombreuses qu'elles puissent être, ont dérivé de l'original par l'intermédiaire de deux copies perdues, *w* et *z*, et de ces deux-là seulement [...]. Un arbre bifide n'a rien d'étrange, mais un bosquet d'arbres bifides, un bois, une forêt? *Silva portentosa*" (BÉDIER, 11-12),

While staying with his exquisite botanical metaphor ("flore philologique", "arbres... d'une seule essence" etc.), Bédier suddenly turns from the current prevalence of two-branched stemmata to the actual situation of medieval writers, who sought remuneration "en argent ou en gloire" from their activity:

Soit donc le *Roman de Troie*, que trente-neuf manuscrits nous ont conservé. Ce qu'il faut se représenter au point d'origine et de départ, c'est un pullulement presque immédiat de copies plus ou moins fidèles, tirées dans l'entourage même de l'auteur, et qui toutes eurent des chances égales d'être recopiées [...]; en sorte que les trente-neuf manuscrits venus jusqu'à nous peuvent représenter cinq, dix, vingt, et pourquoi pas?, trente-neuf lignées distinctes, indépendantes entre elles. Pourtant, à en croire l'éditeur, Léopold Constans, elles n'en représenteraient que deux, issues l'une de *w* et l'autre de *z* (BÉDIER, 11-12).

In other words, making a serious logical error, first remarked by Fourquet in 1946, Bédier strangely mistakes the *stemma codicum* (the graphic representation of the relationships between surviving witnesses, the only ones known to philologists) with that for which Fourquet himself was to propose the term of *arbre réel* or *complet* [real or complete tree], that is, the unknowable, but certainly much larger and more complex ensemble of all copies that ever existed (→ 2.3).

Now, a conservative estimate of the decimation rate of thirteenth-, fourteenth- and fifteenth-century mss., based, by analogy, on the low number of surviving copies of fifteenth- and sixteenth-century printed editions whose run is known (a datum admittedly not available in Bédier's time;

→ 2. Appendix), suggests that surviving thirteenth- to fifteenth-century manuscripts (that is, the manuscripts that are present in the stemmata of our editions) constitute, at best, between 10 and 15% of those that actually existed (the real or complete tree of each tradition). As a consequence, only rarely does the *stemma codicum* actually inform us about the original branching points of the real tree. Above all, the *stemma* will almost invariably ignore the more slender primary transmission lines, those that did not find one of those editorial outlets we call a *vulgata* [vulgate text], which owed their success to diverse factors, such as their commentary or the prestige of the copyists, or the especially inexpensive arrangement in two columns instead of one per page etc. (→ III.5).

At any rate, Bédier's conclusion was that those trees were not originally two-branched, but, as we shall see more clearly below, had been reduced to that condition, albeit unconsciously, by philologists themselves ("Nos arbres bifides n'ont pas tous poussé tels quels; ce sont, pour la plupart, des arbres ébranchés...": BÉDIER, 12-13).

Bibliographical notes. BÉDIER; also interesting, although much less read, is one of Bédier's last essay, "De l'édition princeps de la *Chanson de Roland* aux éditions les plus récentes. Nouvelles remarques sur l'art d'établir les anciens textes", *Romania* 63, 1937, 433-469; 64, 1938, 145-244 and 489-521, where he applies his 1928 approach to the *Chanson de Roland*.

Studies on Bédier include Gianfranco Contini's fine portrait "Ricordo di Joseph Bédier", in *Esercizi di lettura*, Torino, Einaudi, 1974, 358-371, Alain Corbellari's monograph, *Joseph Bédier. Écrivain et philologue*, Genève, Droz, 1997, Bédier's own correspondence with Paris (G. Paris-J. Bédier, *Correspondance*, éditée par U. Bähler et A. Corbellari, Firenze, Edizioni del Galluzzo per la Fondazione Ezio Franceschini, 2009) and the collection J. Bédier, *Philologie et humanisme. Articles et préfaces inédits en volume, choix, notices et avant-propos d'A. Corbellari*, Paris, Classiques Garnier, 2010. Bédier's strong nationalist, and especially anti-German, sentiments are evocatively reconstructed by M.R. Warren, *Creole Medievalism: Colonial France and Joseph Bédier's Middle Ages*, Minneapolis, University of Minnesota Press, 2011, and mentioned in ead., "The politics of textual scholarship", in *Cambridge Companion*, 119-133).

A conference organized by Craig Baker, Marcello Barbato, Mattia Cavagna and Yan Greub was held in Brussels in November 2013, titled *L'ombre de Bédier. Cent ans après*, whose proceedings are currently being prepared for publication.

Various examples of adhesion to Bédier's principles are cited by FOULET-SPEER, 19-39; J.G. Griffith, "Non-Stemmatic Classification of Manuscripts by Computer Methods", in PdO, 73-86: 73-74; FIESOLI, *La genesi*, 418

and note 196, 422-425. Another advocate of Bédier's method is I. Frank, "De l'art d'éditer les textes lyriques", in *Recueil de Travaux offert à M. Clovis Brunel*, Paris, Société de l'École des Chartes, 1955, I, 463-475.

As regards later French Romance studies, which are largely "bédieristes" or "néo-bédieristes", Duval acutely notes, following in the wake of Gilles Roques, that Bédier's manifesto appeared when the German Romance school was prevailing over the French: "Plus rapide et plus simple à mettre en œuvre que la méthode des fautes communes, la méthode de Bédier permettait aux Français de refaire leur retard" (DUVAL, "La philologie française", 116). As to the present, "le refus d'une réflexion sur les dimensions théoriques de telle ou telle option d'édition a conduit beaucoup de jeunes éditeurs français à être bédieristes sans le savoir" (p. 119).

For a confutation of Bédier's objections, → 2.2-3.

2.2. Bédier's contribution to perfecting the genealogical method

As a reaction against purely mechanical rules for recovering the original of a text from revised and re-revised manuscripts his [i.e. Bédier's] protest was wholesome: no one today would wish or dare to revive the system of Wendelin Foerster in editing the works of Chrétien de Troyes. But to find in this a justification for neglecting intensive comparative study of manuscripts, and for uniformly renouncing efforts to arrive closer than one or another of those manuscripts to the text of the original author, is another matter. Bédier has not, as some may have thought, hewn down at the root the 'manuscript tree'; he did, however, effectively prune from it a number of diseased offshoots

Armstrong, Ford, Nitze, "Joseph Bédier", 412

In the century and a half that followed, 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 (principally Joseph Bédier and dom Quentin), Post-Lachmannism (Giorgio Pasquali and, to a certain degree, Michele Barbi) and—why not?—Neo-Lachmannism (part of the Italian Romance school).

CONTINI, 7 (= ID., *Frammenti*, I, 6)

One of Bédier's most cutting objections to Gaston Paris's method is the above-mentioned accusation of, so to speak, therapeutic or rather philosophical excess. Philologists, Bédier argued, kept hunting for hypothetical conjunctive errors until they obtained a two-branched tree. This allowed them to bring back in through the window the subjectivity and freedom to choose between rival readings that the iron rule of majority (→ 1.4) had driven out by the door. In sum, Bédier sees the prevalence of two-branched stemmata as a mainly ideological or even psychological problem.

Actually, Bédier's own brilliant essay of 1928 lends itself to what can be called (in the broad sense) a psychoanalytical interpretation. It is indeed an out-and-out act of rebellion against his academic father, Gaston Paris, with the usual attendant self-censorship and denial. One only needs to consider that Bédier constantly refers to Paris's method as "la méthode de Lachmann" [Lachmann's method]—a designation that was to become immensely popular in the twentieth century and is still found in many textual criticism manuals. Now—as Sebastiano Timpanaro guessed in the 1960s and a young but already accomplished Giovanni Fiesoli proved in 2000—Lachmann *never* employed the common-error method, in any of the fields of study he worked in, whether in his essays on classical philology, on Biblical philology, or on Germanistics (TIMPANARO [MOST]; FIESOLI, *La genesi*). But let us return to Bédier's contribution to perfecting the genealogical method.

A good starting point is an observation by Gianfranco Contini (Domodossola, 1912-1990), one of the greatest disciples of the French master and one of the major exponents of so-called Neo-Lachmannian philology (which could be roughly characterized as a method that remains faithful to the common-error method, but after taking Bédier's objections into account). In an essay of 1970, "La vita francese di Sant'Alessio e l'arte di pubblicare i testi antichi" ([The French life of St. Alexis and the art of editing ancient texts], whose title is already an obvious homage to Bédier, Contini remarks that "to be Lachmannian today, it is indispensable to have gone through an Anti-Lachmannian apprenticeship (that is, Bédier) and a Post-Lachmannian experience (that is, at least in classical philology, Pasquali)" (CONTINI, 68 [= ID., *Frammenti*, II, 958]).

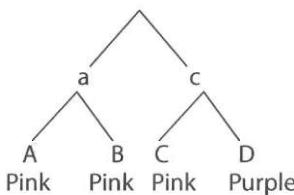
Shortly thereafter, Contini acknowledges "the incomparable contribution of Bédier's objections to the new Lachmannism" (CONTINI, 74 [= ID., *Frammenti*, II, 963]).

In Chap. 5 I will briefly discuss the beneficial effect of Bédier's critique of the early Romance philologists' reconstructive excesses as regards the language of texts (such as, for example, Paris's own attempt, in his *Extraits de la Chanson de Roland*, to translate into Francien the Oxford *Roland*, which is in Anglo-Norman). As regards textual substance, Rajna's position in an essay of the same year, 1929, is noteworthy. After carefully considering Bédier's objections, Rajna reasserts his trust in the reconstructive method ("I still find the contested method to be good"), but frankly admits that the method is of uncertain effectiveness when applied to *mixed*, that is, contaminated traditions:

We have paid too little attention to perturbing factors, such as to 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 [...]. In a mixed transmission [...], even when genetic relationships exist between several individuals of a lineage, these relationships become so uncertain that we should give up the notion of identifying them, and the confidence that we can use them to reconstruct the text with procedures *pour ainsi dire mathématiques*, as Paris thought he could (Rajna, "Un nuovo testo...", 50).

In fact, after Bédier's objections to the practice of arbitrarily reconstructing texts, all the most scrupulous Neo-Lachmannian philologists, stemmatic conditions being equal, have retained the readings of the base manuscript (It. *manoscritto base*) adopted for the language of the text, after the example of Provençalists. They thereby reduce recourse to the other branch(es) of the stemma to a minimum, that is, only to cases of errors in the witness adopted as the base manuscript.

Another innovation introduced, or rather made explicit, under the spur of Bédier's objections is that for the majority principle to be applicable—and for having what, ever since Pasquali coined the expression, we call a “closed recension”—a three-branched stemma is not necessary. Given a two-branched stemma, it is not at all inevitable for each equally acceptable variant to be found in 50% of the surviving tradition, and thus to be equally probable. On the contrary, in a significant number of cases a majority is obtained, albeit a fractional one. In the following case:



the majority in favor of the reading *pink* (versus *purple*) is overwhelming, even in the absence of three branches: 75% against 25%.

© As Jean Fourquet points out (“Fautes communes ou innovations communes?”, *Romania* 70, 1948-1949, 85-95: 89), this observation is already found in P. Collomp, *La Critique des Textes*, Paris, Les Belles Lettres, 1931, 68: “Si deux familles s’opposent, on a le droit de choisir, mais si une famille s’accorde avec una partie de l’autre famille contre l’autre partie, le calcul de probabilité impose la leçon donnée par cet accord”.

Contini replied to two very momentous objections by Bédier, viz., that the prevalence of two-branched stemmata reveals an unconscious desire for freedom of choice, and that the discovery of new witnesses can alter the stemma and thus deeply modify the text. Every critical edition, Contini observed, is simply a “working hypothesis”, and the quality of results inevitably depends on the quality of the documents available to the editor, which varies from one case to the another, but progressive approximation as increasingly adequate solutions are found, sometimes through the discovery of new witnesses, is a typical scientific approach (CONTINI, 32-33, 73-74 [= ID., *Frammenti*, I, 29-30; II, 963]).

Bibliographical notes. E.C. Armstrong, J.D.M. Ford, W.A. Nitze, “Joseph Bédier”, *Speculum* 14, 1939, 411-412; FOULET-SPEER, 19-28; CONTINI, 67-97 [= ID., *Frammenti*, II, 957-985]; L. Leonardi, “L’art d’éditer les anciens textes (1872-1928). Les stratégies d’un débat aux origines de la philologie romane”, *Romania* 127, 2009: 273-302. I drew my quotations of Pio Rajna’s admirable considerations (“Un nuovo testo parziale del Saint Alexis primitivo”, *Archivum Romanicum* 13, 1929, 1-86) from FIESOLI, *La genesi*, 426.

Almost all of Contini’s philological studies are collected in CONTINI, *Frammenti*, with an important essay by Giancarlo Breschi. See also P. Italia, “Contini, Gianfranco” (2013), in *Dizionario biografico degli italiani*. [http://www.treccani.it/enciclopedia/gianfranco-contini_\(Dizionario-Biografico\)/](http://www.treccani.it/enciclopedia/gianfranco-contini_(Dizionario-Biografico)/)

In the light of GUIDI-TROVATO’s work—which I discuss immediately below—we could add to Contini’s reply that new witnesses found after the completion of a critical edition belong, as a rule, to the most common vulgate texts, while the discovery of “new” upper-level manuscripts, such as to modify significantly the shape of the stemma, is unfortunately a rather uncommon event.

2.3. One paradox less: the question of two-branched stemmata

Stemmata of this type [*i.e.* two-branched ones] are just what, in the great majority of cases, we should expect to find.

W.W. Greg, “Recent Theories...”, 404

Other advances we can credit Neo-Lachmannian philologists with are the result of their attempts to explain Bédier’s paradox, that is, the overwhelming prevalence of two-branched stemmata in classical and Romance philology. Sebastiano Timpanaro acutely provides a number of possible partial explanations for this phenomenon, including contamination and extra-stemmatic contamination (→ 3.4), in Appendix C of his book on Lachmann’s method (TIMPANARO, 129-160 [= ID. (MOST), 157-

187]): “Stemmi bipartiti e perturbazioni della tradizione manoscritta” [Bipartite Stemmas and Disturbances of the Manuscript Tradition].

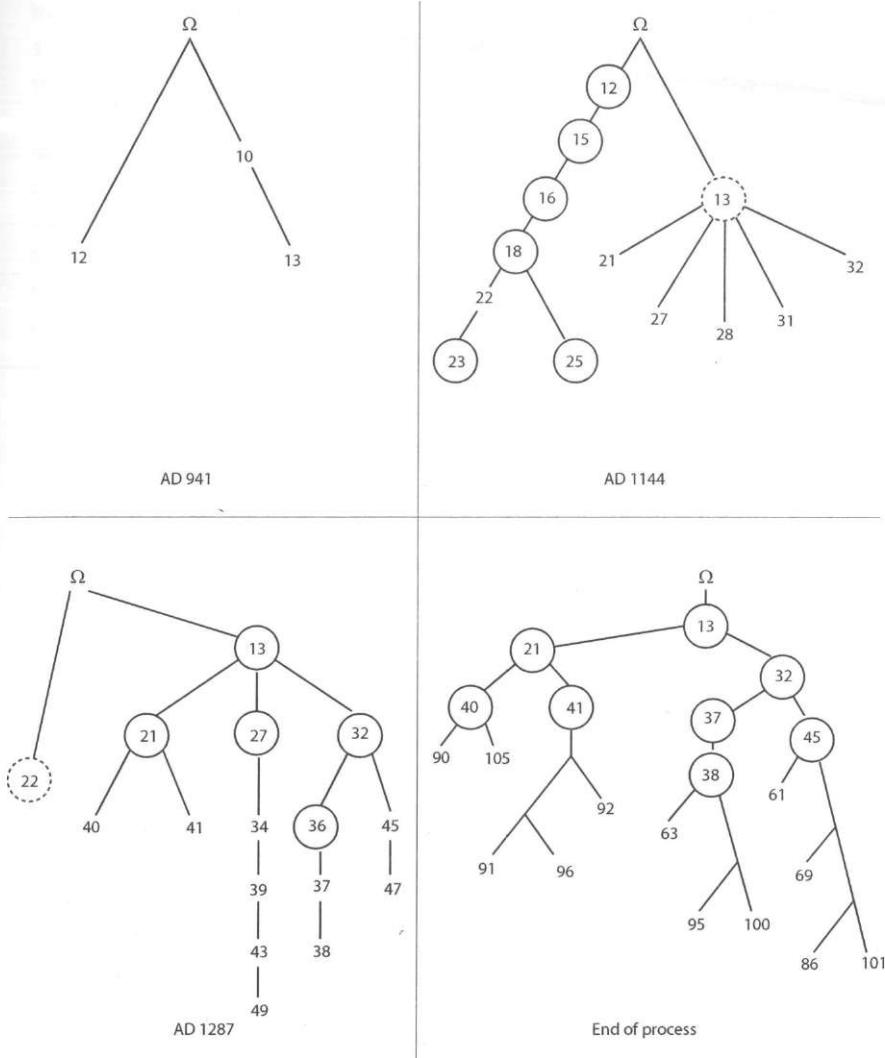
As to the issue of how the decimation of witnesses affects the real tree over time, significant light has been shed on it in two articles by the Orientalist Michael Weitzman, who also adopted an experimental approach to address other crucial problems of textual criticism, such as that of “open” traditions. In the first essay, of 1982, Weitzman adapts a “birth-and-death process” statistical model to virtual manuscript traditions of classical texts. The instructions he gave a computer to automatically generate genealogical trees were based on the hypothesis that texts composed in AD 500 and copied until 1500 could either disappear or spawn descendants. At the beginning of each manuscript tradition (or “population”) texts could only be copied, whereas at the end of the thousand-year-period in question (following the spread of printed books) they could only “die”. Furthermore: 1) the average size of a surviving population—by analogy with various ancient Greek literary works—was set at 40 copies; 2) the average “date of birth” of exemplars had to be 1400, that is, the golden century of Humanism (as is the case for so many *recentiores* of classical literary works); 3) the rate of extinction was set at about 90%. In 46 experiments, the computer generated 31 populations that became extinct early on, and 15 surviving populations, of various sizes and complexity. Two of these were composed, respectively, of only one and only two copies. The remaining 13, in Weitzman’s own words, had the following characteristics:

In all thirteen other experiments, all the manuscripts derived from a lost archetype, i.e. their latest common ancestor (now lost) was distinct from the original. In ten experiments, the tree split thence into two branches; in the other three, it had three branches. At stages later than the archetype, rather more three-way and occasional four-way splits occurred, though most splits were still into two branches only (Weitzman, “Computer simulation...”, 56).

The author observes, very reasonably in my opinion, that the high rate of lost archetypes and two-branched stemmata is explained by the high (90%) rate of extinction of individual copies.

The ability of Weitzman’s software program to monitor variations in a stemma over time by successive “photographic” frames, confirms that the fundamental reason for the prevalence of two-branched stemmata and the failure of the archetype to be preserved in his stemmata is the high

mortality rate of witnesses (entrusted, in the real world, to fragile media, such as papyrus, parchment, and paper). Notably, Weitzman shows four genealogical trees of a single experiment, which photograph as many different stages in transmission between the year AD 941 and the end of the process:



In Weitzman's own words ("Computer simulation...", 59): " Ω represents the lost original. All manuscripts alive at the stated time are shown, without any ring, except that four *codices descripti* 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". I corrected the last figure ("End of process") as per WEITZMAN's own indications in "The Evolution", 289.

I will now briefly comment on the four trees. Year 941: in spite of the disappearance of witnesses 1-9 and 11, a small two-branched tree lives on (witness 12 on one side, witnesses 10 and 13 on the other). Year 1144: the branch of 12—which had generated 15, 16, 18, 23, etc.—is almost wholly extinct, while the other branch (descended from 13) continues to thrive and reproduce. Year 1287: the first of the two branches of 941 (presumably, but not certainly, original, since transmission began in AD 500) consists of a single, moribund copy (witness 22, a remote descendant of 12), while the other branch is still prospering, although 13 has by now become extinct. The two-branched stemma we find at the end of the process, with two subfamilies per branch, is thus the result of an almost unbroken chain of transformations, including: 1) the extinction of one of the two primary branches in AD 941; 2) the (gradual) shrinking of the most fortunate of the two initial families from 5 branches in 1144 to 3 in 1287 and 2 in 1500.

Differently from what Weitzman suggests at this point, this is a result not so much of low productivity at the upper levels (the real tree, which numbered 101 witnesses, was a lot larger!), but rather of decimation, as he himself has noted above. We remark the disappearance, among other things: a) of the first 9 copies; b) of a whole branch of the 941 tree; c) of several witnesses on the other branch, including witness 13 (the manuscript all the surviving end-of-process witnesses descend from, and hence, if we were to trace their stemma, their archetype: → 1.5).

* * *

Weitzman's long essay of 1987 elaborates on his 1982 study. Recalling other scholars' attempts to explain "historically" the trend to two-branched stemma or the almost invariable loss of the archetype (for example, in Greek, with the transition from the upper-case to the lower-case alphabet), Weitzman rightly observes that the prevalence of archetypes and two-branched stemmata in literary traditions that are so diverse "must be due to a limited number of factors which are common to manuscript traditions in general and which one can hope to incorporate in the mathematical model" (WEITZMAN, "The Evolution", 289).

Several parameters adopted in his new experiments are similar to those of 1982 (for example, he assumes 1400 as the average date of manuscripts and a probability of extinction of 90%), but he raises the average population to 55 exemplars, and other parameters differ from Greek to Latin literature. For example, for Greek texts the period under consideration goes from 450 BC to 1450, for Latin from 50 BC to 1450. Furthermore, thanks to the counsel of two authoritative classicists (Nigel Wilson and Robert Ireland), Weitzman now appears to be more aware of the variables that can come into play in the course of transmission (oral tradition, author's variants, contamination, differing popularity of texts, the influence of the time factor, as well as the influence of catastrophes, such as the destruction of the library at Alexandria and the sack of Constantinople in 1204 and 1453, or of favorable periods such as the Carolingian Renaissance, etc.), and manages to draw from his model clues or experimental solutions for other momentous problems (e.g., in § 6, the probable chronological distribution of *descripti*, with peaks in the later stages; in § 7, the high probability—between 76 and 79%—that, after a 90% loss, the whole transmission will descend not from the original, but from an archetype; and the probability that an “archetype” will be preserved, which is inversely proportional to its age).

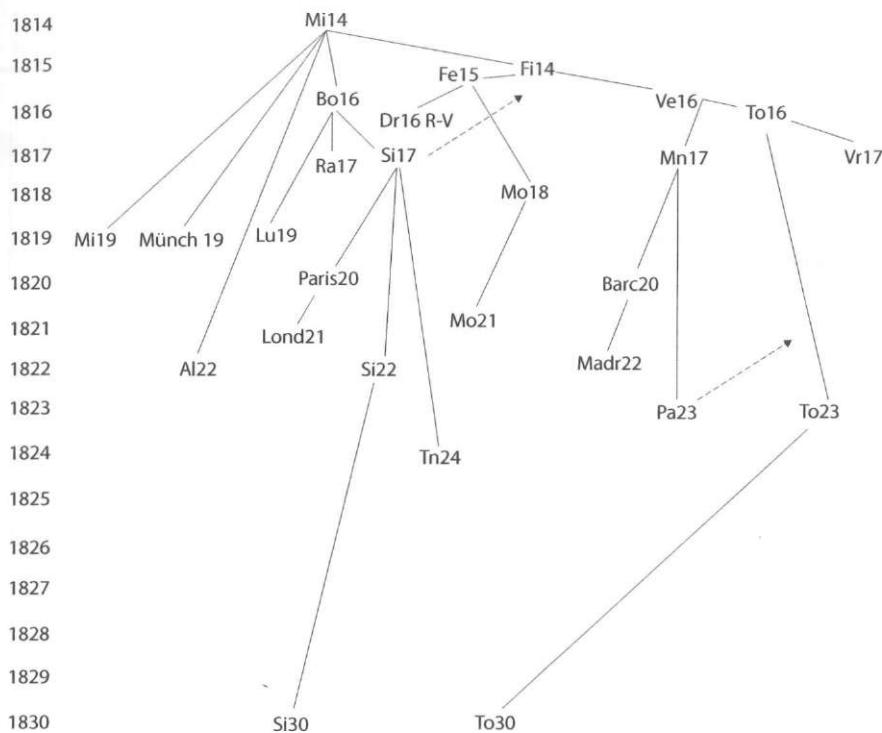
As regards the trend to two-branched stemmata, Weitzmann points out that earlier attempts to neutralize Bédier's paradox through probability calculus were regarded as unsatisfactory by the proponents themselves, whereas his own mathematical model indicates a 77% probability for two-branched trees for Greek texts, and 71% for Latin texts. Weitzman concludes:

Here [...] the phenomena are held to follow naturally from features common to most traditions—the chronological spread of extinctions from the ever present risk of manuscript “death”, and the prevalence of archetypes and two-branched stemmata from the high extinction probability for the population arising from any manuscript [...]. A mathematical model, as Kleinlogel and others urge, is not the same as the intricate processes of history. It can, however, establish a reasoned presumption, in the place of sheer conjecture; the present model, for example, overturns Bédier's assertion that the majority of stemmata cannot be two-branched (WEITZMAN, “The Evolution”, 303).

Vincenzo Guidi and I have recently attempted to reexamine Bédier's paradox and explain it in terms of probability calculus, in a study entitled *Sugli stemmi bipartiti. Decimazione, asimmetria e calcolo delle probabilità* [On Two-Branched Stemmata. Decimation, Asymmetry, and Probability Calculus].

To begin with, after collecting the not too numerous stemmata of fifteenth and sixteenth-century printed editions known to us (about fifteen), we noticed that almost half of them were three-branched. So we asked ourselves in what way these printed books were different from manuscripts. The obvious answer is that, since every printed edition is printed in n copies, each edition has not one but n chances of surviving. This led us to hypothesize that the prevalence of two branches in the stemmata of classical, medieval and Renaissance manuscript traditions results in the greatest measure on the effects of decimation over time, which are more devastating for manuscripts than for printed editions.

This empirical observation has been confirmed countless times. Valentina Gritti, the most recent editor of Carlo Goldoni's *Puntigli domestici* (15 printed editions from 1754 to 1792), reconstructed a four-branched stemma for the printed tradition. The earliest printed tradition for the famous opera libretto *Il Turco in Italia* (Romani-Rossini), studied by Fiamma Nicolodi and Paolo Trovato (27 editions preserved between 1814 and 1830), has a five-branched stemma, reproduced below:



We could add that, since decimation is directly proportional to the time (=T) that has elapsed between the creation of the witnesses and the moment when philologists try to reconstruct their text, scholars who study ms. traditions of the last four or five centuries are more likely than most classicists, or than Bédier (who worked on thirteenth- and fourteenth-century traditions), to run into or obtain stemmata with more than two branches. The classicist Giovan Battista Alberti remarked that “among the few manuscript traditions [sc. of classics] that remain multipartite even after his strict examination, almost all ‘are represented by rather recent manuscripts’” (TIMPANARO [MOST], 185 [= TIMPANARO, 159]). Reeve has brought attention to multi-branched stemmata of Latin classics with a relatively recent tradition, such as the “Ciceronian speeches discovered by Poggio in 1417” (REEVE, *Manuscripts and Methods*, 31). Peter Robinson, who has studied the transmission of two Old Norse poems preserved, under the common title *Svipdagsmál*, in 42 mss. dated

between the seventeenth and nineteenth century, has reconstructed a stemma with three or possibly more branches (though the positions of witnesses P, J, 1609 etc. are not all that certain: → 4.4).

¶ Michael D. Reeve kindly reminds me that he subsequently published editions of two texts that seem to him to have a three-branched stemma: Cicero's oration *Pro Quinctio* (1992) and Vegetius's *Epitoma rei militaris* (2004). He admits that Vincenzo Ortoleva has a different opinion on Vegetius, *Philologus* 148, 2004, 162–163 (see also REEVE, *Manuscripts and Methods*, 32 note).

Our subsequent step was to use the stemma of some apparently complete printed traditions—with no witnesses marked with lower-case Greek or Latin letters, that is to say, lost and only logically assumed to have existed—as a possible *model* for a real tree, that is, the ensemble of all manuscript copies that ever existed of a given text. We then decided to decimate one of these model trees more or less severely, between 10 and 90%, and then calculated:

- a) the probability of a reduction of originally multi-branched real trees to two or single-branched stemmata;
- b) the probability—since philologists draw up their stemmata blindly, with whatever witnesses happen to have survived decimation—of assigning manuscripts belonging to the same (albeit luxuriant) branch of the real tree to different primary branches of the stemma.

¶ This part, of course, was done by Vincenzo Guidi, a nuclear physicist and hence more experienced than I am in fairly complex calculations. The results of our work are very different from those obtained much earlier by Arrigo Castellani, who had imagined real trees with three relatively symmetrical primary splits, populated roughly by the same number of copies. Here I will provide a decidedly brief summary. I will be drawing further on this study in the Appendix to the present chapter and in later sections, and refer interested readers to GUIDI-TROVATO.

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 (→ 3.3), modest decimation rates (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.

Bibliographical notes. W. W. Greg, “Recent Theories of Textual Criticism”, *Modern Philology* 28, 1931, 401-404; J. Fourquet, “Le paradoxe de Bédier”, in *Mélanges 1945, II. Etudes littéraires*, Paris, Les Belles Lettres, 1946 (“Publications de la Faculté des Lettres de l’Université de Strasbourg”, 105), 1-16; A. Castellani, “Bédier avait-il raison? La méthode de Lachmann dans les éditions de textes du Moyen Age” (1957), now in id., *Saggi di linguistica italiana e romanza* (1946-1976), Roma, Salerno Ed., 1980, III, 161-200; H. Peri (Pflaum), “Une méthode expérimentale de critique des textes”, in *VIII Congresso Internazionale di Studi Romanzi* (Firenze, 3-8 aprile 1956). *Atti*, Sansoni, Firenze, 1959-1960, II, 719-747; M. Weitzman, “Computer Simulation of the Development of Manuscript Traditions”, *Bulletin of the Association for Literary and Linguistic Computing* 10, 1982, 55-59; WEITZMAN, “The Evolution”; A. Castellani, “Joseph Bédier et l’édition critique de textes médiévaux”, in *Menschen und Werke. Hundert Jahre wissenschaftliche Forschung an der Universität Freiburg Schweiz*, Freiburg, Universitätsverlag, 1991, 119-131 (the lamented Castellani was kind enough to give me a photocopy where some slips and omissions in the book had been corrected); GUIDI-TROVATO (these are actually two distinct, albeit closely connected essays, the first addressing the philological implications of the new data, the second the methods used for the probability calculus: P. Trovato, “Dagli alberi reali agli stemmi” [From real trees to stemmata]; V. Guidi, “Manuscript Traditions and Stemmata: a Probabilistic Approach”).

The stemma of *Il Turco in Italia* is taken from F. Nicolodi, P. Trovato, “La tradizione primo ottocentesca dei libretti (1814-1830)”, in Gioachino Rossini, *Il Turco in Italia*, a cura di Fiamma Nicolodi, Pesaro, Fondazione Rossini, 2003 (“I libretti di Rossini”, 9), LXI-CI.

Michael D. Reeve read TIMPANARO’s important article, 129-160 [=TIMPANARO (MOST), 157-187] as a declaration of skepticism toward the genealogical method (REEVE, *Manuscripts and Methods*, 27ff.), but I find that Timpanaro

was actually as much a practitioner of this method as Reeve is. This is especially evident in an unfinished reply to Reeve written by Timpanaro and published by Glenn W. Most in a welcome appendix to his fine English translation of Timpanaro's masterpiece (TIMPANARO [MOST], 207-215). A letter by Timpanaro in a similar vein was published by Paolo Mari in 2011, and is now in id., *Timpanariana e altri saggi di metodo filologico*, Roma, Istituto storico italiano per il Medio Evo, 2013, 24-31.

2.4. The classification of the *Lai de l'ombre* and Bédierist editions

While the Bédierist critique successfully dealt with the excesses of restorative and interventionist practice in textual criticism—or, more specifically, in the establishment of the “critical text”—it also diminished the role of editing in general. In stressing the role of the base manuscript, this critique often ignored the rest of the manuscript tradition. The great successes of the CFMA series [i.e. the “Classiques Français du Moyen Âge”, Paris, Champion] in practice confirmed, so to speak, the validity of the one-manuscript method and made any emendation, except that of the most obvious errors, somehow illegitimate.

DEMBOWSKI, “The ‘French’ Tradition”, 531

In editorial practice, especially in France, the hegemony of the base manuscript has been growing under the impulse of an increasingly empirical conception: the manuscript one preaches loyalty to is exactly that, the “base”, to which corrections and modifications are added, often on no other criterion than the presumed evidence of their necessity, and without trying to account for the development of the tradition.

LEONARDI, “Il testo come ipotesi”, 11

While many philologists have overhastily espoused Bédier’s positions, putting a “virtual ban on stemmatic studies” (Dembowski), a number of scholars, from Greg to Maas, from the American Romanists of the “Chicago School” to Fourquet, Castellani, Timpanaro, Segre, Peri (Pflaum), Reid, Roncaglia, Blecua, Reeve, Montanari, Guidi and Trovato, have exposed the limits of Bédier’s anti-Lachmannian arguments.

As Segre observed regarding Bédier’s editions of the *Chanson de Roland*:

L'esprit de système ne pouvait fermer à la réalité les yeux d'un philologue averti comme l'était Bédier: il est absolument impossible qu'un copiste ne commette pas un certain nombre d'erreurs; et puisqu'il y a au moins un manuscrit interposé entre l'Archétype et O [viz., the famous Oxford

manuscript], deux séries d'erreurs au moins doivent s'être superposées dans notre manuscrit [...]. Bédier 1938 reconnaît qu'il doit bien se trouver en O 142 *lapsus calami* et une dizaine d'erreurs (p.161), puis il accepte les corrections d'autres éditeurs, ici deux (p.179), ici douze (p. 189), là cinq (pp. 190-191), là quatre (pp. 231-232), et ainsi de suite, pour un total de 25 au moins, 35 au plus (p. 520) [...]. Que ces concessions de Bédier soient réduites au minimum (leur nombre pourtant est déjà considérable) importe moins que le fait qu'elles ouvrent irrémédiablement une brèche dans le mur des positions de principe. Les copistes se trompent; il faut corriger les textes; la critique textuelle nous donne la méthode pour les corriger, souvent avec la plus grande probabilité d'atteindre l'original au plus près. Les concessions de Bédier impliquent tout cela. Et dès lors l'opposition manichéenne entre "interventionnistes" et "conservateurs" doit faire place à une discussion tranquille, cas par cas, sur la réalité effective de l'erreur (*Chanson de Roland* S, 11-12 note; my emphasis)

However, the thesis that it is not possible to produce a satisfactory classification of the *Lai de l'ombre* has passed scrutiny. In the context of growing adhesion to Bédier's conservatism, all the twentieth-century editions of the *Lai* limited themselves to reproducing, with slight changes, one or another of Bédier's editions, sometimes stressing the higher degree of "scientificity" of the French master's editing method. Adrian Tudor, for example, observes:

The text was edited twice in the nineteenth century, by Francisque Michel and Achille Jubinal [...]. These editions seek an 'authentic' text, one which is made up from all extant manuscripts. The reconstruction of a hybrid text was no longer in fashion when Joseph Bédier published his edition of 1913. He attempted to conserve as much and correct as little as possible, a principle generally adopted by scholars today (Jehan Renart, *Le lai de l'ombre*..., 7).

Apart from the fact that Tudor appears to be poorly informed about the editorial history of the *Lai de l'ombre* (the Michel editions of 1836 and the Jubinal edition of 1846, both earlier than Gaston Paris's methodological revolution, are respectively based on mss. A and F, and thus, one could say, Bédierist *ante litteram*; Bédier's "hybrid" edition of 1890 is strangely forgotten), his conclusion that Bédier's attempt "to conserve as much and correct as little as possible" is "a principle generally adopted by scholars today" is hardly disputable.

Actually, editors of various nationalities—French, British, etc.—have shared the perception that editions *à la manière de Bédier*, which are

often reticent about the reasons for the choice of the base ms., were extremely respectful of the historical reality of the text, in spite of Alberto Varvaro's *caveat* ("Is it really true that the Bédierist Bédier was a conservative editor? My impression is that he was not, and that there is often a confusion between the theory of *recensio* and the practice of *emendatio*": VARVARO, *Identità*, 645) and in spite of the even more explicit warnings of a scholar who can hardly be suspected of anti-Bédierism, viz., Contini. Obviously alluding to the more recent Bédierist edition of the *Lai*, as well as that of the *Roland*, Contini observes:

As to the radical freedom [of philologists], we can rest assured that no one will ever be able to destroy it. Bédier's skepticism of textual paleontology [i.e., nineteenth and early twentieth-century editions based on the common error method] led him to radically restrict its freedom by confining it to the edition of a single manuscript. However, since it was neither photographic nor diplomatic, but still interpretative, *within that same boundary he had confined it in he made it perform unheard-of orgies* (CONTINI, 78 [= ID., *Frammenti*, II, 967]; my emphasis).

Contini then asks himself:

Indeed, what does purging the "obvious" errors in a manuscript mean? What is more disputable than obviousness, and the limits of the obvious? Not to mention the wealth of ingenuity he [Bédier] lavishes on us to demonstrate the rationality of readings which a more parsimonious approach would have qualified as irrational (CONTINI, 78 [= ID., *Frammenti*, II, 967]).

* * *

In his encyclopedia entry "Filologia" (1977), Contini takes a more explicit stance against the Bédierism of Bédier's followers, who, unlike the Master, lacked the necessary competence to choose the best manuscript in a tradition:

The single-manuscript remedy proposed by Bédier—but it is worth stressing that he only proposed it for medieval literary texts in the vernaculars transmitted with the utmost freedom—is not [...] immune from flagrant shortcomings [...]. The mere correction of "obvious" slips introduces a subjective criterion with shifting boundaries (as borne out *a posteriori* by editions of a text such as the *Roland* of Oxford, a victim of the Bédierist method reduced from a repository of angst to a lazy fashion). Above all, the choice of the manuscript [i.e., the best manuscript] is fraught with difficulty, given the impracticability of having as many editions as there are

manuscripts. Bédier is the first to realize that the “best” is not necessarily the earliest [...], nor the most correct, which could owe its polish to a scribe with an eye to meaning and willing to bear the cost of emendation. An objective definition, developed in the Neo-Lachmannian milieu, of the best manuscript as the one which has been so resistent to banalization as to offer the highest percentage of *lectiones singulares* to be preserved presupposes a Lachmannian edition. Indeed, only a Lachmannian editor, such as Bédier was for quite a long time, can point out the best manuscript, or even merely a good one (ID., 37-38 [= ID., *Frammenti*, I, 34]).

¶ *Lectiones singulares* are readings found in a unique ms. (Engl. *unique, individual*, or *singular readings*).

Contini’s most important objection is that Bédiérian editors are defenseless when their base manuscript confronts them with a reading that is not manifestly wrong, but which a comparison with other witnesses, and especially the detection of so-called diffraction, in presence or absence (→ 3.2), would reveal to be very probably not original, that is, a latent error:

The decisive objection to the myth of the unique manuscript is the following: besides easily emendable erroneous innovations, besides trivializations (*lectiones faciliores* in the case of several witnesses) that are corrigible [...] within the tradition [i.e., adopting the reading of another witness], there are also equally acceptable ones that are only detectable by collating the other witnesses, as these all show equally acceptable variants [...]. A multiple innovation at the same variation place does not preclude reasoning: why have all the manuscripts [...] innovated, and in a colorless manner to boot? Was this not because there was an objective obstacle in the original? (ID., 140 [= ID., *Frammenti*, I, 67]).

In the late-twentieth-century practice of so-called Bédierist editing, things do not seem to have improved. According to Dembowski,

many editions of important Old French texts do not offer any appreciable quantity of variants and are not, in fact, “critical” in any sense [...]. Unfortunately, many literary scholars do not realize that an acquaintance not only with a good manuscript but with the rest of the manuscript tradition is no outlandish “philological” requirement. This can be vital to the understanding of literary sense [...]. The scarcity of variants [*viz.* in Roques’s edition of Chrétien de Troyes] does present serious problems not only for text-minded philologists but also for the literary scholars who thereby remain unaware that they are studying the practices of the

good but doubtless interventionist scribe Guiot and not the unmediated production of the poet Chrétien (DEMBOWSKI, "The 'French' Tradition", 525-526).

As Lino Leonardi observes, today the prevalent philological practice is to transcribe a manuscript, merely throwing in essentially haphazard corrections:

Editions conceived and realized according to purely stemmatic logic, or at the other extreme purely conservative editions, are increasingly rare. As early as 1974, at the memorable round table of the congress of Romanists held in Naples, supporters of Neo-Lachmannism (Segre) and Neo-Bédierism (Lecoy) reached not too distant conclusions, summarizable as the desirability that the critical text should not to stray too far from the text of one of the manuscripts (except in *loci* that were admitted of certainty, according to Segre), whereas the critical apparatus should be the place for comparison of readings and reconstructive hypotheses. However, editions aspiring to absolute respect for the reality of a single manuscript, after an exhaustive analysis of the *varia lectio*, and that are rigorously consistent with this principle, are decidedly in the minority. The authentic Bédierist inheritance has in fact exhausted its momentum, after reaching its peak with Mario Roques's editions of Chrétien and Félix Lecoy's of the *Roman de la Rose* between the 1950s and 1970s, both of which were intended to outclass Wendelin Förster and Ernest Langlois' renowned editions (LEONARDI, "Il testo come ipotesi", 8).

Leonardi adds:

We could say that today the majority of editions of Romance texts consist in the transcription of one of the manuscripts carrying the work, with corrections in variable measure [...]. This return to a pre-scientific practice involves two kinds of problems, concealed by apparent editorial efficacy, but unacceptable for those who understand editing in the terms I was referring to above [viz., scientificity, an eye for diachrony, and legibility].

1. On the ontological plane, the text thus produced is not defined even roughly on the diachronic axis running from the original to the witness. The material attractiveness of the base ms. leads to a conservative edition, but the possibility of correcting it more or less freely produces a text that stands in an ambiguous and heterogeneous position, in an indistinct hinterland of the base ms., outside the two hypothetical alternatives, the two "truths" outlined above [viz., that of philology oriented to the text and that of philology oriented to the witness].

2. On the phenomenological plane, too, the fact that the choice of the base ms. and the control mss. and the procedures for correcting the base ms. are not illuminated by *recensio* often leads to contradictory solutions, and even misleading ones as regards comprehension of the base ms. itself, which does not find its proper place in the tradition (ID., "Il testo come ipotesi", 9, 12).

¶ Outside the editions of texts of medieval French literature, the base-manuscript method has prevailed in several other fields. The recent and important edition of Bartholomeus Anglicus, *De proprietatibus rerum*, by Baudouin van den Abeele, Heinz Meyer et al., Turnhout, Brepols, 2007, vols. I and VI, that is, one of the most popular Latin "encyclopedias" of the Middle Ages (about 200 manuscripts), was put together, after extensive sampling of 10 witnesses, from only 5 mss., A B C D E. By checking the apparatus of variants, Rosa Casapullo ("Sull'edizione...") managed to reconstruct the reciprocal relations among the 5 witnesses. Remarkably, B, the ms. chosen as the base text by the editors, is genealogically the lowest and textually the least correct ms., although it shows, by contamination, a small number of apparently genuine corrections (B2). Keeping in mind that there exist several non-authorial versions of this encyclopedia, the reader should be aware that these corrections, mostly by professional hands different from the main one, go back to an unrecognized source, and not necessarily one from the same chronological and textual layer as the selected witnesses.

To expose the less than impeccable logic of many editions founded on a single ms. regarded as the best, one need only point out that both Bédier's 1913 and 1928 editions of the *Lai de l'ombre*, and those derived from it, by Orr, Limentani, Lecoy and others, draw on several different witnesses to fill in presumed lacunas in the *meilleur manuscrit*. In the absence of a general genealogical classification, however, it is impossible to know if these are truly lacunas or, on the contrary, interpolations. Sometimes it is even impossible for the reader to understand whether the text he or she is reading, which is in fact a "reconstructed" one, is actually in the real historical manuscript chosen as the base witness. In particular, in his 1913 edition Bédier, following A, makes 34 corrections to the base text, including the filling in of what are presumed to be extensive lacunas, and in his 1929 edition he corrects E in 26 cases and suspends judgment in another 10 (BÉDIER, 98; BOURGAIN-VIELLIARD, 17).

Reusing Leonardi's observations on recent editions of Arthurian prose romances, we could argue that the text offered by the editors of the *Lai de l'ombre*, including Bédier, "stands in an ambiguous and heterogeneous position, in an indistinct hinterland of the base ms." and "ends up os-

cillating between conservation of the manuscript and reconstruction of its model, without making up its mind between these two alternatives” (LEONARDI, “Il testo come ipotesi”, 17). Still in Leonardi’s words, we could argue that “the editorial formula of adopting the base ms. unless there are manifest errors *actually leads to a reconstructive edition, but without the application to this reconstruction of a method capable of dealing with the dynamics of variants and accounting for them in the edition*” (ID., “Il testo come ipotesi”, 26; my emphasis).

The impression, however, is that even in French Romance studies—which are Bédierist by tradition, sometimes without even realizing it (as Frédéric Duval has noted)—the wind is changing. It is especially interesting that one of the most authoritative exponents of Bédierism, Félix Lecoy, has taken a clear-cut position against the editions of the *Lai* based on ms. E (that is, Bédierist editions guilty of not having chosen a good enough witness as a base manuscript). In 1974, at an international conference held in Naples, Italy, Lecoy had again affirmed his trust in the method, or rather the “pratique”, of Bédier (“ni un théoricien ni un dogmatique [...]. Un pragmatiste”). In his own words:

L'on s'efforcera de réduire au minimum les interventions que l'on fera subir au témoin que l'on a choisi de faire connaître. Ce témoin, on le présentera dans toute sa franchise, dans toute son ingénuité et dans toute sa vérité, car il est porteur, comme nous l'avons noté tout à l'heure, d'une certaine vérité que ce serait une erreur certaine de faire disparaître ou d'obnubiler. Ce faisant, l'éditeur saura qu'il ne donne pas un texte prétendûment authentique, mains du moins il le saura (ainsi que son lecteur), alors que l'édition dite critique ne peut pas ne pas donner l'illusion d'une fallacieuse conformité à l'original (LECOY, “L'édition critique”, 506).

Lecoy’s loyalty to Bédierist “pratique” is even more evident in a lively discussion published in the proceedings of the Naples conference. When Roncaglia proposes to adopt in Marcabru’s *pastorela* the *difficilior* technical term *calmissa*, occurring in troubadoric mss. from Italy (instead of the commonplace *planissa*), Lecoy objects, formulating a not very parsimonious hypothesis: “Rien ne le preuve! Rien n’empêche que votre copiste italien pouvait avoir à côté de lui un provençal, qui lui a glissé ce mot” (p. 510). And in his edition of the *Lai* according to ms. A (1979), Lecoy maintains what we might call a possibilistic Bédierist aplomb (“L'éditeur se contentera d'établir quelle est, parmi les copies de l'oeuvre qui ont survécu au naufrage des temps, celle qui lui paraît avoir

conservé la meilleure version de son texte, et il reproduira cette copie en la retouchant le moins possible": p. VI). Accordingly, he limits himself to retouching the text of ms. A about thirty times.

In 1982, however, discussing a new edition of the *Lai* based on E, Lecoy points out, apparently with a certain annoyance, a number of readings he finds erroneous—or in any case unacceptable—in E, which Bédier himself (BÉDIER, 97-100, followed by Orr in 1948 and by Hindley and Levy in 1977) had identified as a different authorial version of the *Lai*.

¶ A useful presentation of the main terms "used to refer to the works of art and to the sources of textual evidence" is offered by SHILLINGSBURG, *Scholarly Editing*, 41-47 (notably, Shillingsburg defines "version" as "one specific form of the work—the one the author intended at some particular moment in time". Wherever necessary, I will also use "version" to indicate texts produced by editor-copyists (called *redazioni* in Italian), but not for mere copies, however full of errors.

Notably, Lecoy, whose essay is based on an analysis of *all* the variants in the tradition of the *Lai*, proves, against BÉDIER, that:

- 1) The few acceptable variants of E cannot be ascribed to authorial initiative. They are transmission variants, common to the DEF group.
- 2) Most unique readings (*lectiones singulares*) in E are inferior to the competing readings of A, which are confirmed by the rest of the tradition.

The scholar thus concludes:

Je ne crois pas que le texte de E puisse soutenir la comparaison avec celui de A. Le rédacteur de E était un homme très habile et la version du *Lai* qu'il a publiée est parfaitement lisible dans l'ensemble. Mais elle me paraît dans le détail plus remaniée que celle de A (LECOY, "Variations", 467).

More generally, according to Lecoy:

Aucune copie, sauf exceptions notables (car il en existe tout de même de fort bonnes), n'est exempte de fautes, parfois grossières, mais aussi souvent plus subtiles. C'est là que peut, que doit s'exercer la sagacité de l'éditeur, qui est pris entre la nécessité de respecter son modèle et son désir de fournir au lecteur un texte lisible et cohérent. La marge de manœuvre est étroite, mouvante, et l'on peut toujours redouter d'en avoir outrepassé les limites, car la tentation de "corriger" est là, qui nous guette et cherche à nous séduire (ID., "Variations", 469; my emphasis).

In other words, beyond the “ideological” stances of non-editors like Cerquiglini, even an expert champion of Neo-Bédierism like Lecoy sees the *Lai* not as a collection or repository of chaotic *variance*, but as a text characterized by numerous variants whose reciprocal relationships can be rationalized by scholars, as long as they are adequately trained.

An interest in editing methods alternative to Bédierism, and especially in a “lachmannisme modéré”, can be found in some recent French manuals or companions such as BOURGAIN-VIELLIARD, 14-22, 40ff., and Duval, who goes so far as to argue that

la malaise tient à l’analyse des principes exposés dans les introductions. Repris de génération en génération, ils n’ont pas suivi l’évolution des pratiques, souvent moins nettement bédieriste que ce qui est affirmé [....]. L’insuffisance de la réflexion méthodologique conduit à revendiquer un pragmatisme qui n’est souvent que de façade. En effet, quelle que soit la configuration de la tradition textuelle, les éditeurs français ont tendance à suivre des règles identiques, alors qu’il pourraient se situer davantage par rapport à l’archétype en cas de tradition resserrée (DUVAL, “La philologie française”, 149).

It is barely worth adding, quoting Segre, that

dans ce climat différent les exhortations de Bédier à chercher tous les moyens de justifier une leçon douteuse avant de la déclarer erronée gardent toute leur valeur: à condition que le doute méthodique ne devienne pas de l’agnosticisme ou, pis, une sorte de dogmatisme négatif (*Chanson de Roland* S, 12 note).

* * *

Let us briefly return to the *Lai de l'ombre*. In my opinion, the classification of the witnesses of this work, of which I will be giving a basic overview below (→ 8), does not pose the insurmountable problems lamented by Bédier, and taken for granted by his followers. For my part, one aspect of the problem I find most instructive is that, in spite of the profusion of alternative stemmata found in Bédier’s 1928 study, the stemma in my opinion most likely to be correct (or, as Contini would put it, the most parsimonious working hypothesis about the surviving tradition) is radically different from all those that have been proposed so far, which are mostly abstract and more or less baroque elaborations of the stemmata

drawn up by Bédier and Paris in 1890 (→ 2.1), and are not founded on a real re-examination of the tradition.

Bibliographical notes. On post-Bédier “French” editions of medieval texts, see DEMBOWSKI, “The ‘French’ Tradition”; DUVAL, “La philologie française”; LEONARDI, “Il testo come ipotesi”. Important insights on Bédierist editions and studies of *Roland* are found in Cesare Segre’s introduction to the *Chanson de Roland* S, 9ff. Alberto Varvaro’s study of Bédier’s 1902-1905 edition of the fragments of the *Roman de Tristan* by Thomas (VARVARO, *Identità*, 636-645) is also interesting.

The 2007 edition of *Bartholomaeus Anglicus* was reviewed by R. Casapullo, “Sull’edizione di un testo mediolatino a tradizione sovrabbondante: il ‘De proprietatibus rerum’ di Bartolomeo Anglico”, *Filologia italiana* 9, 2012, 9-25.

After Bédier’s editions of 1890, 1913 and 1928, several editions of the *Lai de l’ombre* have seen the light, all declaredly Bédierist: Jehan Renart, *Le lai de l’ombre*, Edited by J. Orr, Edinburgh, Edinburgh University Press, 1948 (“Edinburgh University Publications. Language and Literature. Texts”, 1) on ms. E; Jean Renart, *L’immagine riflessa*, a cura di A. Limentani, Torino, Einaudi, 1970 and Parma, Pratiche, 1994 (“Biblioteca medievale”, 34), on ms. A; Jehan Renart, *Le lay de l’ombre*, edited from Ms B. N. Nouvelles Acquisitions 1104, text and glossary established by B.J. Levy, A. Hindley; notes by F.W. Langley; introduction by C.E. Pickford, [Hull], University of Hull, Department of French, 1977 (repr. 1985), on E; Jean Renart, *Le lai de l’ombre*, publié par F. Lecoy, Paris, Champion, 1979 (“Les classiques français du Moyen Âge”, 104), on A; Jean Renart, *Le lai de l’ombre*, edited from Ms E [B. N. *Nouv. Acq. Fr. 1104*] by M.E. Winters, Birmingham, Alabama, Summa Publications Inc., 1986; *El lai de la sombra. El lai de Aristóteles. La Castellana de Vergi*, introducción, traducción y edición por F. Carmona, Barcelona, PPU, 1986 (“Textos medievales”, 3), on “las ediciones de Bédier y Lecoy”; *Nouvelles courtoises*, éd. et trad. S. Méjean-Thiolier et M.-F. Notz-Grob, Paris, Librairie générale française, 1997 (“Le livre de poche”, 4548. “Lettres gothiques”), on A, with the variants of E; Jehan Renart, *Le lai de l’ombre*, Translation and Introduction by A. Tudor, Text Edited by A. Hindley and B.J. Levy, Liverpool, University of Liverpool, Department of French, 2004 (“Liverpool Online Series: Critical Editions of French Texts”, 8), on E.

There has been no dearth of studies on the issue of the classification of the witnesses of the *Lai* (e.g., A. Dees, “Considérations théoriques sur la tradition manuscrite du *Lai de l’Ombre*”, *Neophilologus* 60, 1976, 481-504; P. Galloway, “Manuscript filiation and cluster analysis. The *Lai de l’Ombre* case”, in PdO, 87-95, reproduced—with only a few retouches and the title “Filiation, Classement, Cluster Analysis: *Lai de l’Ombre*”—in *Le Médiéviste et l’ordinateur* 7, 1982, 10-14; ead., “Clustering variants in the *Lai de l’Ombre* manuscripts. Techniques and principles”, Association for Literature

and *Language Computing Journal* 3, 1982, 1-8). Remarkably, none of these studies has ever spawned an edition.

The present writer has proposed a new classification of the mss. of *Lai* in “La tradizione manoscritta del *Lai de l'ombre*. Riflessioni sulle tecniche d'edizione primonovecentesche”, *Romania* 131, 2013, 338-380. Carlo Beretta, Gian Paolo Renello and others are planning an online edition of the *Lai*.

Appendix. How many lost medieval mss. are there?

The number of surviving manuscripts largely depends of course on the popularity of the work in the Middle Ages and Renaissance [...]. But chance must also have played a large part [...]. What I would really like to know, and never will, is 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?

Leighton D. Reynolds, “Experiences of an Editor of Classical Latin Texts”, *Revue d'Histoire des Textes* 30, 2000, 1-15: 3

In a textual criticism workshop held in Leuwen in 2004, the deceptive analogy between the data of biologists and those of philologists inspired the following observations, which have a rather abstract and uninformed ring to them:

Stemmatological methods often suppose missing witnesses which never really existed. This is probably the weakest aspect of stemmatic methods, and maybe a side-effect of their ‘obsession’ with the root [sc. of textual transmission]. This fixed idea of the philologists is not shared by people working with phylogenetic methods (BARET-MACÉ-ROBINSON, “Testing Methods”, 279).

Actually, the certitude that a large part of ancient, medieval and modern books have disappeared without leaving any traces is hardly an *idée fixe* (would that it were!). It is instead regularly confirmed by historical data, and merely depends on the fact that they were written on extremely perishable material (papyrus, parchment, paper).

¶ Not unlike the major libraries of antiquity, or the libraries destroyed in wartime in the last few decades (for example, in Dresden in 1945, in Sarajevo in 1992, in Timbuktu in 2013, or in Tripoli, Lebanon, in 2014), only a few years ago, in peacetime, the library of Weimar went up in flames, although fortunately not all of it. In any case, 50,000 books were lost, including autographs by Goethe, manuscripts, and old and rare printed books.

With many very reasonable distinctions (format, number of pages, language, "literary genre", etc.), historians of the printed book estimate that fifteenth-century printed books were several times more numerous than those that have survived down to the present day. They were printed in hundreds of copies, whereas often only one, two or three survive. In the light of the above, by what number should we multiply surviving manuscripts in any Romance language to obtain a plausible total for medieval manuscript production? Or, to reverse the question: how far-reaching must the historical process of "decimation" of handwritten copies have been for what were presumably luxuriant real trees to yield, almost invariably, such skeletal stemmata?

On this point, too, with all due caution and differentiations, fifteenth and sixteenth-century printed books provide some interesting clues. Discussing the number of lost fifteenth-century editions, bibliographers have gone from the optimism of nineteenth- and early twentieth-century scholars to the recent, problematic awareness of huge losses. In particular, in attempts seeking to reconstruct the number of lost editions from the number of preserved ones of which only 1, 2 or 3 copies survive, "the curve points to a number of lost editions that is, in the mathematical sense, limitless" (Green-McIntyre-Needham, "The Shape of Incunable Survival...", 175). Let us shift our attention from the number of editions—which depends on a great number of variables—to the number of surviving copies per edition, which can be likened, in a certain measure, to what survives of different manuscript traditions. Basing myself on indications found in the works of Neil Harris, Jonathan Green, and other scholars cited below, I will not take into account small-format books made up of only a few pages, which are extremely perishable even when printed in many copies (like our daily newspapers today). I will only cite data relative to folio or quarto editions of at least 100 pages and with runs of 200 copies or more. In some cases, I will limit myself to providing "raw" data; these will be either underestimated (for example, when the repertoires I draw on indicate the number of libraries the copies appear in, but not the actual number of preserved copies) or overestimated (as when I use certainly known data about copies in Italy to estimate the number of copies preserved abroad). Furthermore, I will also include incomplete or even fragmentary copies among the survivors. I find, however, that the resulting numbers do not appear to be unrealistic.

The lost 1495 edition of Boiardo's *Inamoramento de Orlando* (Scaniano, Pellegrino de Pasquali) was printed in 1250 copies. As in the case of the *editio princeps* of 1482-83 (whose place of publication and number of copies we do not know), no specimen has survived. Their decimation is thus 100%.

We know the runs of several Florentine incunabula, or incunabula financed by Florentines. We know, among other things, that in 1476 Jacques La Rouge printed in Venice, in about 600 copies, the *Historia fiorentina* by Bruni, and the work by Poggio with the same title (IGI 7940). We also know that Jenson printed one thousand copies of Landino's translation of Pliny (IGI 7893). In 1479, the *Vite dei pontefici e imperatori* by pseudo-Petrarch (IGI 7563) was printed in 650 copies, the *editio princeps* of Dante's *Commedia* with Landino's commentary (IGI 360), in 1200 copies, Plato's *Dialogues* (IGI 7860) and the *Morali di San Gregorio Magno volgarizzati* (IGI 4447), in 1025 copies.

I will list these titles again in survival rate order. *Historia* by Poggio. Loss: 76.9% (ISTC data). St. Gregory's *Moralia*. Loss: 88% (ISTC). Plato's *Dialogues*. Loss: 88.5% (ISTC). Landino's *Commedia*. Loss: 89.2% (ISTC). Pliny. Loss: 89.4% (ISTC). *Vite dei pontefici*. Loss: 90% (ISTC).

From the fifteenth century on, the spread of the printed book and the change of reading habits from intense use of a few texts to the consultation of a large number undoubtedly facilitated the preservation of the more recent texts. But we should never forget to take "genre" differences into account. So, for example, the first and third versions of Ludovico Ariosto's very popular *Orlando Furioso* (respectively A and C), both in-quarto, were printed in runs of 1300 and at least 2650 copies, respectively. As far as we know, however, only 12 copies of A survive, and Conor Fahy was able to trace only 24 copies of C. In both cases, decimation was thus higher than 99%.

For texts in-folio, and less avidly consumed, things have gone somewhat better. The *princeps* of the *Cortegiano* (Venice, 1528) was printed in 2000 copies. According to the database in progress of Italian editions of the sixteenth century, Edit16, copies of this work are found in 66 Italian libraries (one more than in a printed census of 1993). This means that at least 66 copies exist. More precisely, 44 libraries have the *editio princeps* and 22 the 1545 issue, that is, the unsold copies of the first edition with a modified title page and colophon. As this book is an Aldine edition, an object of desire for collectors worldwide, I would assume that at least

another 134 copies are preserved, including possible doublets in Italian libraries as well as copies outside Italy. The presumed total is thus 200 (66 + 134). This yields a loss of 90%. If twice as many copies were actually preserved, loss would still be very high, around 80%.

Roughly the same number of Italian libraries (63, according to Edit16 data) have at least one copy of the folio *princeps* of the *Ricchezze della lingua volgare sopra il Boccaccio* by Alunno (Venezia, case d'Aldo, 1543), which, as the author himself states, was printed in 2000 copies. In this case, too, I would assume, subject to verification, the existence of a significant number of doublets in the same library and copies abroad, and hence a loss between 80 and 90%.

Format and runs being equal, books in Latin or Greek (employed in libraries or other scholarly contexts) are more easily preserved than books in modern languages. But a table in a recent book by Uwe Nedermeyer (*Von der Handschrift zum gedruckten Buch*) listing known runs of printed books earlier than the nineteenth century does not encourage optimism. The list does not usually include data on format, but up to the year 1500 it also gives statistics about the numbers of surviving books, which I double-checked with the more up-to-date data of the ISTC. Out of a run of 360 paper copies of the *Decretales* by Gregory IX (Mainz, Schöffer, 1473: GW 11451), only 69 survive (ISTC). If we resign ourselves to disregarding what private libraries own, loss is thus 80.8%. (Not surprisingly, the 40 parchment copies of the same work were significantly more durable: 11 survive, bringing loss down to 73%.) Of the *Institutiones Iustiniani*, printed in 200 copies by Belfort in Ferrara in the same year 1473 (GW 7585), only 2 copies are preserved (loss 99%). The same percentage is observed for the Venetian edition of 1493 (*Codex Iustiniani*, Battista de Torti, Venezia, 1493), printed in 1300 copies of which 13, and for Cicero's *Familiares*, printed in Bologna (1477) by Domenico de Lapis and Sigismondo a Libris in 500 copies (GW 6821), of which 5 survive. The in-folio Latin *Bible* printed by Wild in 930 copies (Venice, 1478: GW 4233) has fared better, having come down to us, according to the ISTC, in 81 copies, and thus with a loss close to 90%.

As we can see, 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 later the American élite as well), 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 , losses among handwritten texts were even more dramatic.

Bibliographical notes. N. Harris, "Marin Sanudo, Forerunner of Melzi", *La Bibliofilia* 95, 1-37, 101-145; 96, 15-42; id., "La sopravvivenza del libro, ossia appunti per una lista della lavandaia", *Ecdotica* 4, 2007, 24-65; J. Green, F. McIntyre, P. Needham, "The Shape of Incunable Survival and Statistical Estimation of Lost Editions", *The Papers of the Bibliographical Society of America* 105, 2011, 141-175; N. Harris, "The Italian Renaissance book: Catalogues, censuses and survival", in *The book triumphant. Print in transition in the Sixteenth and Seventeenth centuries*, edited by M. Walsby, G. Kemp, Leiden-Boston, Brill, 2011, 26-56: 50-56.

The book by Uwe Neddermeyer cited above is *Von der Handschrift zum gedruckten Buch. Schriftlichkeit und Leseinteresse im Mittelalter und in der frühen Neuzeit. Quantitative und qualitative Aspekte*, Wiesbaden, Harrassowitz, 1998, 2 vols.

The text of the present Appendix is reproduced, with a few minor changes, from GUIDI-TROVATO, 27-29, where it is supplied with bibliographical notes.

3. A MORE IN-DEPTH LOOK AT SOME ESSENTIAL CONCEPTS

3.1. Again on the notions of variant and significant error

The stemma is necessarily based on errors shared by two or more MSS, yet it is not always clear which of the competing readings are errors.

John G.Griffith, “Non-Stemmatic Classification of Manuscripts...”, 75

The term “error” is used here with a wide meaning: it stands not only for a real blunder, but for any variant inferior to its alternative. It is implied that the same mistake will rarely be made several times independently [*sc.* by different scribes] in the same place. We are thus assured only of a certain probability [...]. Probability will vary also with the nature of the mistake: an extravagant blunder in which several manuscripts agree proves more for their relation than does an omission by *homoioteleuton*.

BIELER, 13

It can happen that a variant reading is a so-called *accidental reading* or *accidental*. Then, the variant is not determined by the layer [i.e. the exemplar], but by writing peculiarities of the copyists. For instance, copyists very often have their own ways of using capital and small letters, orthographical or spelling systems, language (dialects!), etc. An accidental reading is so heavily copyist bounded, that agreement in accidental readings must be considered as coincidental. Accidentals do not contribute to the revelation of relationships between the text versions. To distinguish accidental readings from non-accidental readings, we call the ‘non-accidentals’ *relevant or text-genealogical readings*

SALEMANS, *Building Stemmas*, 16

As we read in a manual by a pioneer of computer applications to textual criticism, published by the prestigious University of California Press in 1974:

Lachmann's rule for textual genealogy is that states [sc. copies] of a text having a common error have a common ancestor from which they have derived the error; that when there are no common errors, states agreeing in a striking way have a common ancestor; and that when there are no striking agreements, states often agreeing have a common ancestor from which they have derived these agreements (DEARING, 5).

Besides being somewhat silly, this "rule"—so full of partial repetitions that it would be useful to have students copy it several times to understand how lacunas by *homoioteleuton* occur—is false. Not even vaguely similar formulations are found in any of Lachmann's many philological writings, or in any post-Maesian genealogical criticism manuals known to me. To make things worse, only the first of Dearing's three formulations could be, under certain conditions, correct, and acceptable for a Neo-Lachmannian critic. I will reformulate it more adequately:

Witnesses of a text having at least one *non-polygenetic significant* common error and not being copies of each other have a common ancestor from which they have derived this error.

As I remarked above, PARIS's edition of *Saint Alexis* (1872) marked a decisive step forward in our ability to distinguish between variants and significant errors in the construction of a stemma. Since then, the distinction between variants (which are very numerous, polygenetic, and irrelevant for genealogy) and significant errors (which, as a rule, are few, may derive from earlier copies and are thus useful for the construction of the stemma) is made in just about *all* manuals based on the common-error method, and has also been adopted by some exponents of computer-assisted philology. As a sagacious Dutch practitioner of computer-assisted editing, Ben Salemans, wrote in 1999:

Indeed every variant is an objective fact. It must be doubted, however, that every variant contains or includes information that can be used to develop a chain or a stemma. In the case of *Lanseloet van Denemerken*, for instance, three text versions were printed in Cologne. The first pro-

noun “I” is in the Cologne, Ripuarian, language “ich”, while it is “ic” / “ick” in the Dutch dialects. The occurrence of the variant “ich” versus “ic” / “ick” [...] will be caused by the language of the copyist/printer and their public, and not by the historical relationship of the texts [...]. Therefore, the opposition “ich” versus “ic” / “ick” must not be used to develop the chain and stemma of *Lanseloet van Denemerken* [...]. *I am convinced that only very few variants can be building tools. Perhaps one out of a hundred variants* (B. Salemans, “The old text-genealogical method of Lachmann...”, 117-118; my emphasis).

In the same article, Salemans gives a number of useful (often negative) instructions for developing a software for the automatic classification of texts. I will quote some of them:

Characteristic 6 (6a, 6b, 6c). Text-genealogical variants [known in traditional terminology as indicative errors] are not accidentals or small spelling differences [...].

Characteristic 7 (7a, 7b, 7c, 7d). Text-genealogical variants are not potential regional, idiolectic, diachronic or other parallelisms [these are what are called polygenetic readings in traditional terminology]. [...]

Characteristic 8. Text genealogical variations in word order.

A difference in a syntactically adequate *word-order* has to be considered a genealogical variation as long as the difference in word-order does not merely concern different placement of an adverb in a verse or sentence.

Characteristic 9. In verses, rhyming conventions must have been obeyed. [...].

Characteristic 10. Inversion of verses.

The inversion of (rhyming words of) verses is genealogically informative when these verses fit well.

Characteristic 11. Addition and omission of words and verses.

11a. The addition (or ‘interpolation’) and omission of words is genealogically informative when these words fit well or offer non-crucial information.

11b. The addition (or ‘interpolation’) and omission of complete verses is genealogically informative when these *verses* fit well or offer non-crucial information.

We have to beware that the absence or addition of small, highly frequent words (like ‘so’) can be parallelistic (Salemans, “The old text-genealogical method of Lachmann...”, 124; the same criteria are described in more detail in ID., *Building Stemmas*).

Other stemmatologists also list types of variants to be disregarded for the purposes of classification. In her studies on the transmission of the *Cligès* by Chretien de Troyes, Margot van Mulken dismisses the following types as non-significant:

- Les changements dans l'ordre des mots [...].
- Les différences métriques.
- Les variants concernant les pronoms, les prépositions ou les particules.
- Les variants concernant les préfixes.
- Les variants concernant l'emploi de temps, de mode ou de nombre [...].
- Les variantes qui s'expliquent par une préférence pour la rime léonine.

According to this scholar, all these variants, which she evidently regards as polygenetic, would only increase “noise”, that is, the inaccuracy of the classification, and “le philologue doit se décider sur le pourcentage de bruit qu'il admettra” (van Mulken, “Les changements de parenté...”, 105).

A not too different grid, incorporating converging indications by several scholars, both traditional and non-traditional (Havet, Dain, Fränkel, Young, Brambilla Ageno, Fromkin), was drawn up by Caterina Brandoli in 2007 to compare two different lists of variation places (*Lat. loci critici*) in Dante's work, respectively drawn up by Michele Barbi, in 1891, and by Giorgio Petrocchi in his collations, which began in the 1950s and culminated in his edition of 1966-1967.

The grid is organized in three major categories. Its ambition is to list the most frequent types of commonly agreed-upon variants that are polygenetic and, as such, disregardable for the purposes of Neo-Lachmannian classification and stemma construction.

*Presumably polygenetic variants according to Brandoli***1. Variants of paleographical origin**

a) insertion, omission, substitution, paleographical change of a single grapheme or of an abbreviation mark.

Examples: insertion: *Inferno* (= If) XXIV 129 ch'io il vidi omo di sangue e di *crucci*] corucci;

omission: If XXIV 69 ma chi *parlava ad ira* parea mosso] parlava d'ira; *Purgatorio* (= Pg) XXIII 36 e quel *d'una aqua* (= acqua) non sapendo como] dunqua;

substitution: Pg II 44 tal che *parìa* beato pur descripto] farà; Pg XXVIII 141 forse in Parnaso esto loco *sognaro*] segnaro;

paleographical change: Pg III 50 romita] rimota; If XII 49 ria] ira.

b) erroneous resolution of an abbreviation (e.g., a copyist may spell out what is actually not a *titulus*, not spell out an actual *titulus*, or mistakenly spell out a *titulus*).

Examples: Pg XXVI 72 alti] altri; Pg XXVIII 53 a terra e *intra* sé donna che balli] e in terra.; *Paradiso* (= Pd) I 38 la lucerna del mondo *ma da quella*] manda; Pd XXIX 100 e mente] e mentre;

c) erroneous analysis of sequences of graphemes (the apostrophe only begins to be used consistently in modern languages from the sixteenth century on).

Examples: If XXXII 34 lividi] li vid'io; Pg XXVIII 102 e libero n'è] e liberonne; Pd XXII 151 *l'aiuola* che ci fa tanto feroci] la mola.

2. Variants relating to "empty" words and changes in word order that do not violate meter and prosody

a) insertion, omission, replacement of empty words (articles, conjunctions, prepositions, modal verbs, etc.), especially monosyllabic and bisyllabic ones.

Examples: insertion: Pd XXVI 74 sì nescia è *la sùbita vigilia*] la sua; omission (or univerbation): Pg XI 98 la gloria de la lingua e *forse* è nato] forse;

substitution: If II 57 con angelica voce *in sua favella*] in la, in lor; If III 7 dinanzi a *me* non fuòr cose create] noi;

b) insertion or omission of prefixes that do not significantly modify the sense of a verse.

Examples: insertion: If V 73 cominzai (-ciai)] incominciai; Pg XXV 89 la vertù *formativa* raggia intorno] informativa; Pg XXX 72 riserva] serva; omission: If XVII 74 distorse] storse; Pg IX 39 dipartiro] partiro;

c) small word order changes that do not modify the meaning of a verse.

Examples: Pg XXV 82 tutte quasi] quasi tutte; Par. XXX 125 che si *digrada e dilata e redole*] dilata e dignada.

3. Other formal variants

a) graphic or phonemorphological variants:

Examples: If V 96 mentre che 'l vento, come fa, *ci* tace] si; If XXV 117 due] duo, duoi; Pd VI 75 Mutina] Modena, Modona, Modina;

b) graphic, phonemorphological or syntactical banalizations or localisms:

Example: If IV 146 *lungo* tema] lunga;

c) slight divergences in inflection, with the exception of cases falling under the more general heading 1:

Examples: If III 51 non *ragion[i]am* di lor ma guarda e passa] ragionar; If XXX 44 *falsificare* in sé Buoso Donati] falsificando.

I will also quote Brandoli's comments:

1. the grid is based on the criterion according to which we deem an innovation to be polygenetic if it displays *only one* polygenetic phenomenon;
2. as regards the paleographical phenomenon of grapheme inversion (Category 1), unlike earlier scholars who speak in general terms of "anagrammatism, or disordering of the letters of a word" (Young), I regard as polygenetic only the inversion of two (not necessarily contiguous) graphemes (e.g., *ria* / *ira*);
3. while most of the above-mentioned scholars speak in general terms of the omission of empty words, I prefer, for Category 2, a broader range of types, since these not very significant elements of a sentence can also be *replaced* or *inserted* by mistake;
4. still regarding Category 2: observations on empty words, in my opinion, can extend from monosyllables (which are certainly more easily omitted, erroneously inserted, or inverted) to bisyllables (words like *benché*, *ormai*, *però*) or short syntagms such as *in parte*, *così che*, of not more than two words (in all cases, the "weight" of the elements in question is less felt in concomitance with abbreviations).

Conventionally assuming that all that is not polygenetic is monogenetic, and that, when in a verse there are two or more polygenetic variants, their association, too, should be regarded as a monogenetic error, Brandoli has succeeded in "measuring" differences in reliability in Barbi and Petrocchi's lists of variation places in the *Commedia*. While in Barbi's list only 30 out of 396 loci (7.5%) are polygenetic in the above-specified conventional sense, in Petrocchi's this is true of 195 loci out of 477 (40.8%). Furthermore, 132 points of variation out of the 477 indicated by Petrocchi are already in Barbi's list; as a consequence, if we filter out of the second list material that is already in the first, polygenetic (that is, unreliable) variants end up comprising the absolute majority of Petrocchi's list, or 55.7% (→ 9).

Obviously, none of the grids discussed in the present section exhausts the problem of how to be sure of spotting significant errors. For this, I refer the reader to the considerations and examples scattered over the following sections and chapters. However, singling out the innumerable

non-significant variants (what van Mulken calls “noise”) and deciding to disregard them in manuscript classification are two indispensable steps for the construction of a reliable stemma. Given any text, the variation places that survive this first selection will already constitute a sharply reduced textual subgroup, and thus one that is not paralyzing for the philologist. This subgroup may need to undergo further selection in the search for certain proofs of a genealogical relationship (indicative errors, in Neo-Lachmannian terminology); in any case, we will rest assured that the material discarded in the first instance could not have been of any use.

I would add that, in general, the difficulty of spotting significant errors depends on the fact that every literary genre has a different degree of tolerance for certain types of error. The textual critic should thus distinguish noise from significant errors case by case. While in major poets such as Virgil or Dante, the mere omission of a disyllable, being prosodically unacceptable, is usually a significant error, in thirteenth-century copies of prose Latin guides to the Holy Land neither the addition nor the omission of an epithet, or the abbreviation or extension of a scriptural quotation, is genealogically significant. Variants such as those in italics:

ubi decollatus est Iohannes baptista / ibi fuit decollatus *beatus* Iohannes baptista

locus ubi *beata* Helena invenit veram crucem / locus ubi sancta crux fuit inventa ab Helena

porta dicitur paradisi et est fons paradisi de qua locutus propheta dicens
Vidi aquam egredientem de templo a latere dextro / dicitur porta paradisi et est fons paradisi, de qua loquitur propheta *Vidi aquam egr.* / dicitur porta paradisi et est fons paradisi de quo dicit propheta *Vidi aquam de templo*

Gloria in excelsis *Deo* / Gloria in excelsis

et factus est sudor eius *sicut gutte sanguinis recurrentis in terram* / et factus est sudor eius *sicut gutte sanguinis* / et factus est sudor eius *etc.*

depend exclusively on the culture and mental attitude of the copyists, and on the time they had at their disposal. Even if the phrasing of these variants is very different, in all these cases we are still in the realm of noise (equally

acceptable readings), not yet in that of indicative errors, which are, in Salemans' terms, the only "building stones of chains and stemmas".

Bibliographical notes.

The first epigraph is from J.G. Griffith, "Non-Stemmatic Classification of Manuscripts by Computer Methods", in PdO, 73-86

The passage by Vinton A. Dearing quoted above is already found, phrased almost identically, in his *A Manual of Textual Analysis*, Berkeley and Los Angeles, University of California Press, 1959, vii, where it is followed by a disconcerting comment by the author: "This principle [...], self-evident, [...] will serve in the analysis of only the simplest textual relationships". An analysis of Lachmann's true methodological positions can be found in FIESOLI, *La genesi*, 3-358.

The essays quoted above by van Mulken and Salemans ("Les changements de parenté dans le *Cligès de Chrétien de Troyes*" and "The old text-genealogical method of Lachmann updated with the help of cladistics and the computer") are published in NOF, 103-114 e 115-125; that by Caterina Brandoli ("Due canoni a confronto: i luoghi di Barbi e lo scrutinio di Petrocchi") in NP, 99-214.

The works used by Brandoli are: L. Havet, *Manuel de critique verbale appliquée aux textes latins*, Paris, Hachette, 1911, 2 vols. (anastatic reprint: Roma, L'Erma di Bretschneider, 1967); A. Dain, *Les manuscrits*, Paris, Les Belles Lettres, 1964; H. Fränkel, *Einleitung zur kritischen Ausgabe der Argonautika des Apollonios*, Göttingen, Vandenhoeck & Ruprecht, 1964 (partly translated into Italian in *Testo critico e critica del testo*, a cura di C.F. Russo, trad. it. di L. Canfora, Firenze, Le Monnier, 1983²); D. Young, "Some types of error in Manuscripts of Aeschylus' *Oresteia*", *Greek Roman and Byzantine Studies* [Duke University, Durkam, North Carolina] 5/2, 1964, 85-89; id., "Some types of scribal errors in Manuscripts of Pindar", *Greek Roman and Byzantine Studies* [Duke University, Durkam, North Carolina] 6/4, 1965, 247-273; F. Brambilla Ageno, "Osservazioni sugli errori significativi", *Lettere Italiane* 19, 1967, 457-459; V. A. Fromkin, *Errors in linguistic performance. Slips of the tongue, ear, pen and hand*, New York, Academic Press, 1980.

The fact that different literary genres have, accordingly, a different tolerance for errors is briefly discussed in P. Trovato, "What if Bédier was mistaken? Reflections of an unrepentant neo-Lachmannian", forthcoming.

Even though the best manuals agree that only significant and non-polygenetic errors can be used as indicative errors, once a preliminary stemma has been sketched on the basis of indicative errors, lists of equally acceptable readings matching the distribution of indicative errors can provide a valuable control in complicated traditions. In Italian studies, these readings are often called *lezioni caratteristiche* [characteristic readings], i.e., readings which characterize, besides indicative errors, a group of witnesses; in this manual, I have opted for the non-literal but less ambiguous translation "confirmatory readings". Some indications in this regard can be found in P. Trovato, "Con-

ogni diligenza corretto.” *La stampa e le revisioni editoriali dei testi letterari italiani, 1470-1570*, Bologna, il Mulino, 1991 (repr. Ferrara, UnifePress, 2009), 94-95 (“in the lower levels of the tradition [of printed books], broad constellations of innovations (abstractly: equally acceptable readings) and even series of linguistic corrections can confirm, though not replace, the orientation provided by patent errors, which by their nature are exposed to progressive decimation”); P.G. Beltrami, *A che serve un’edizione critica? Leggere i testi della letteratura romanza medioevale*, Bologna, il Mulino, 2010, § 54; Trovato, “Nuovi dati sulla famiglia p”, in NP2, 188 (“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”).

See also J. Duplacy, “Préalables philologiques à la classification automatique des états d’un texte”, in PdO, 23-33; SALEMANS, *Building Stemmas*; F. Rico, *En torno a l’error. Copistas, tipógrafos, filologías*, Madrid, Centro para la Edición de los Clásicos Españoles, 2004; G. Orlandi, “Errore, corruttela, innovazione”, in ID., *Scritti*, 233-247. E.J. Epp (“Towards the clarification of the term ‘textual variant,’” in *Studies in New Testament language and text. Essays in honour of George D. Kilpatrick on the occasion of his sixty-fifth birthday*. Ed. by J.K. Elliott. Leiden, Brill, 1976, 153-173), proposes, among other things, a distinction between “reading” and “variant”, where the former exclusively designates significant readings; however, it does not seem expedient to oppose two terms that have been used as synonyms for centuries.

3.2. From *lectio difficilior* to diffraction

Easy and difficult are not absolute terms, and what is difficult, that is, unusual, for us may have been easy for people in other ages.

PASQUALI, 122

Lectio difficilior is [...] a criterion that needs to be employed with great care

CONTINI, 111 [= ID., *Frammenti*, II, 998]

To my knowledge (my “source” is again TIMPANARO, 38 with note [= ID. (MOST), 68 with note]), the concept of *lectio difficilior* [literally: ‘the more difficult variant’] was already correctly theorized in Jean Le Clerc’s *Ars critica* in 1696 (“Si una ex iis [sc. lectionibus] obscurior sit, ceterae clariores, tum vero credibile est obscuriore esse veram, alias glossemata”). I will attempt an up-to-date translation of this passage (Timpanaro had already objected to the reduction of all competing readings to glosses, that is, explanations):

If (weight in the stemma being equal) in a variation place one reading is more difficult and the others easier, it is more likely that the *lectio difficilior* is the original one, and any attempts to make the passage more easily understandable, whether intentional (glosses) or not (unconscious banalizations), are secondary readings.

This theory needs to be clarified with a couple of examples. The *difficulty* can be of various kinds, e.g., lexical, syntactical, or conceptual. As late as the fifteenth century, some Italian writers still refer to lovers by the learned compound word *filocapti*, from the Greek *philos* [friend] and the Latin *capere* [to capture]. The word was frequently used in medieval Latin (Latin examples can be found in Ducange's dictionary or online s.vv. *philocaptus* or *filocaptus*; some Italian examples are listed in GDLI and LIZ, s.v. *filocatto*). If in the course of the transmission of the work a copyist were to introduce the phrase *preso d'amore* [captured by love] or such, usually this would be assumed to be a typical lexical banalization, and the Greek-Latin compound would be the *lectio difficilior*. (This solution would be correct, unless one could prove—but this is an excessive hypothesis that I only submit here to discourage overly mechanical applications of the criterion—that all the mss. preserving the Latinism *filocatto* actually descend from a text partially rewritten in an archaizing vein, ascribable, for example, to a Franciscan friar, whereas the rest of the transmission is culturally more “modern”; in fact, well into the Humanist period the training of the personnel of the major thirteenth-century orders, the Franciscans and the Dominicans, was based on an outdated, essentially scholastic curriculum).

I will now give an example of syntactical *lectio difficilior*. In a variation place in Dante's *Commedia* already singled out by the young Barbi in 1891, *Paradiso* I 25-26 (→ 9.1), the tradition of the poem essentially offers three main variants:

- 1) *vedrami venire al tuo diletto legno | venire e coronarmi delle foglie*
(*b La₁*);
- 2) *venir vedra'mi al tuo diletto legno / e coronarmi allor di quelle*
foglie (Can. 104 Franc. Gamb. D II 41 l Pal. XIII.G.1 + α [- *b La₁*]);
- 3) *vedrami al pie del tuo diletto legno | venire e coronarmi delle foglie*
(β + *g* [- Est. it. 196 Franc. Gamb. D II 41 l Pal. XIII.G.1] + *p* + Ph).

Without letting his or her attention be overly distracted by this crowding of symbols (the Greek letter β stands for one of the hyparchetypes of my preliminary stemma of 2007: → 9.4; the lower-case Latin letters, *b g p*, for various subfamilies; the other abbreviations, commonly employed in

Dante scholarship, designate individual mss.), the reader will note that in variant 3 the verb of the main clause (*vedra'mi*) and the two infinitives (*venire* and *coronarmi*) are pushed far apart by a strong hyperbaton, which reflects Dante's stylistic and rhetoric models, and his intention to model himself on the great Latin poets, first and foremost Virgil.

¶ For a useful overview of Dante's hyperbata, see F. Tateo, "Iperbato", in ED, III, 1971, 506-507.

Variants 1 and 2, the former almost prosaic (with a clumsy anticipation of *venire*), the latter prosodically more carefully formulated, are clearly explainable as syntactical simplifications made by copyists during self-dictation of the tercet (French *dictation interne*, It. *dettatura interiore*). And there are indeed good reasons to believe that the length of the passages most frequently memorized by copyists of the *Comedy* coincided with the tercet.

Variant 2 (common in the Florentine *vulgata*, whose spread began in the 1330s) is not necessarily an adaptation of 1 (which is attested in a few early witnesses of this vulgate text). While major poets are often tempted by *ordo artificialis*, the simplification of syntax by copyists can often be polygenetic. It is unquestionable, however, that 3 is the *lectio difficilior*, that is, the one that is most likely to be the original one. It is also the only reading that explains the double occurrence of *venire* in 1, in l. 26, which has the original reading, and l. 25, which appears to be a presumably unconscious attempt at syntactical banalization.

An important refinement of the criterion of *lectio difficilior*—which draws on certain elements of the eighteenth-century criterion of *lectio media*—was theorized by Gianfranco Contini in an introductory lecture of 1953 (only published in 1970) and again in works of 1955, 1968 and 1977. This improved criterion is that of so-called *diffraction* (It. *diffrazione*)

¶ On the *lectio media*, besides Maas, quoted below, see PASQUALI, 11; TIM-PANARO, 40 note 34 [= ID. (MOST), 70 note 34]; J. Lindon, *Notes on the British Contribution...*, 239 note 42.

To my knowledge Contini's metaphor, drawn from the semantic field of optics, was introduced into English-speaking scholarship by FOULET-SPEER, 83.

But let us read Contini's 1968 essay, "Scavi alessiani":

In a review of Gaston Paris's *Saint Alexis*, published in the *Göttingische Gelehrte Anzeigen* in the same year 1872, Tobler proposes an emendation

that is faultless in its elegance and certainty [...]. The speech of the bride's mother ends with the following words (31d-e = 154-155 in the *editio major*)

Plainons ensemble le dol de nostre ami,

Tu del seinor, jo l' ferai por mon fil.

Different manuscripts variously render this last verse as [...]

L tu de tun *seinur*, jo.l friai pur mun filz

A tu pur tun *sire* e je pur mun chier filz

P tu por tun *seignor*, je.l ferai por mun fiz (P2 tu t. seigneur...)

S (452) l'une son fil et l'autre son *ami* [Paris *mari*].

The correction *per* put forward by Tobler appeared undisputable to Paris himself, who in his *editio minor* [...] therefore has:

Tu por ton *per*, jol feraid por mon fil.

The brilliance of the solution thus needs no further praise. It is important, however, to define Tobler's intervention, for heuristic purposes as well as other reasons. It is important to make explicit the latent rationality of his intuition, and possibly even find a label for it allowing it to be recognized *a priori* [...]. What Tobler proposes is [...] a *lectio difficilior*, and an all the more praiseworthy one since the text otherwise has *per* only in the sense of "socially equal", not in that of "spouse" or "loving comrade"; this rarity, it should be noted, only concerns the male *per*, not the female one, which in this acceptation is [...] extremely common and formulaic. However, this *lectio difficilior* is peculiar in nature, since usually a *lectio difficilior* occurs in one textual tradition and stands in opposition to another [...] or even several other readings [...]. What Tobler puts forward instead is a conjectural *lectio difficilior*, one absent in the tradition. He thereby answers the question: What synonym of *seignor*, when in an oblique case, is an oxytone monosyllable or a paroxytone disyllable? Exploration beyond the transmitted evidence [that is, the conjecture *per*] is justified by a particular configuration [...] of the tradition: not just the corruption in every witness, but their divergence. To use a more openly metaphorical expression I would like to call it diffraction, more precisely, diffraction in absence (of the acceptable reading), because in the case of normal *lectio difficilior* [...] diffraction is *in presentia* (of the good reading).

The legitimacy of *difficilior* conjecture (not of mere conjectural correction, because the latter is required by the presence of the error) is connected to diffraction, and thus to multiple transmission, which therefore reveals itself to be manifestly superior to single or simply binary transmission (CONTINI, 99-101 [= ID., *Frammenti*, II, 987-989]).

Contini goes on:

In editorial phenomenology there is a progression from diffraction *in praesentia*, where a witness (usually a single one) has retained the relatively rare word or form, to diffraction *in absentia*, where this word or form has

been documentarily swept away [...]. Taking the matter a step further, I propose to demonstrate that simple diffraction *in absentia* [...] is sufficient to legitimate a *difficilior* conjecture [...]. If we reverse Tobler's procedure, turning it from an answer into a question, his "having found" will become a way to "seek" (CONTINI, 101-102 [= ID., *Frammenti*, II, 989-890]).

¶ Independently of Tobler's praxis, Maas too appears to have approached a solution close to the one Contini was later to theorize: "When the tradition splits into two branches the process of *recensio* [...] often leads us to two variants [...]. [Sometimes] both variants may be understood as errors stemming from the same reading in the archetype. This reading of the archetype, discovered by *divinatio* (*combinatio*), hereupon becomes the basis of further *examinatio*" (MAAS [FLOWER], 17-18, § 19).

The rest of Contini's essay contains a number of examples of the application of the new criterion (in its dual guises of diffraction in absence and diffraction in presence of the good reading) to the old French tradition of *Saint Alexis*. Not all cases discussed there, which require a good knowledge of historical grammar of early French (Bartsch's law, etc.), are equally comprehensible to non-specialists. I will therefore offer a couple of examples which I find to be didactically more effective, as they require less linguistic and metrical competence.

I will begin with a fine example of diffraction *in praesentia*, drawn from a recent edition by Valentina Gritti and Cristina Montagnani. In the surviving mss. of the anonymous poem *Spagna in rima*, which are all from the fifteenth century, but partly northern Italian and partly Tuscan, the rhymes of a proemial octave (VIII 1 or X 1, depending on the witnesses) change radically according to the area where and time when the individual witnesses were produced. Aside from minor variants, which I will only partially reproduce, the tradition is divided into three fundamental types. On the one hand, there is the text preserved by the most authoritative of the northern witnesses, F (VIII 1):

- O vero Idio che formasti i cieli,
 il fuocco e l'acqua, la terra con l'âre
 [...],
 5 Misericordia a Te nesun si cielli.
 Umiltà ver noi sì dona, Pare.
 Donami tanta gratia, nobel sire,
 ch'io possa far d'esta storia el finire.

Several other witnesses (B, C, G, P') retain the rhymes with *are* (or also *mare*): *andare: pare* (*padre* G, with an imperfect rhyme). However, the southern ms. P' has ll. 5-6 in a different form compared to F (l. 6 reads “darimeti gratia che yo pocza cantare”), and the incunable B also has a different version of ll. 4-6 (l. 6 reads “cum pena amara haranno sempre a stare”).

In ms. L (X 1), ll. 1-6 are completely different, including the respective rhymes: 1-5 *nazareno: meno: almeno*; 2-6 *aria: isvaria: contraria* (ll. 1-2 read, for example: “O vero iddio giasu nazareno / che faciesti fuocho et terra aqua e aria”, l. 6: “umilta de superbia chontraria”). The rhymes *aria: varia: contraria* return, with only minor adjustments, in M (X 1) and R (X 1). In N (which usually agrees with R and M) the octave is missing.

In ms. P (X 1), too, the whole octave is different, except for ll. 7-8. For example, l. 2 has a rhyme with *foco* instead of *are*. Consequently, ll. 4-6 read: “volesti esser tenuto a scherno et gioco / e poi volesti che giudei crudeli / di vita te privassero a poco a poco”.

As in every case of diffraction, we must ask ourselves, to quote Contini again, which is “the relatively rare term or form” that causes difficulties and reactions in the transmission. Here the answer is particularly easy. On the one hand, F shows a more than unusual apocope complicated by a hyperbaton (“Misericordia... nesun” almost certainly stands for “nessuna misericordia” [no mercy]). On the other hand, the form *pare* (instead of *padre* < PATREM), whose genuineness is guaranteed by the above-mentioned system of perfect rhymes in *-are*, is common in northern Italy but unacceptable in Tuscany or southern Italy as well as in those northern printing houses that were more under the influence of the Tuscanizing fashion. Thus, the copyist of G introduced the imperfect rhyme *padre*, and more interventionist scribes radically altered the rhyming system precisely to avoid the rhyme-word *pare*, so conspicuously contrary to Tuscan phonology. (In its turn, the erroneous *mare* [sea], which redundantly repeats *acqua* [water], is explainable as a trivialization of the archaic *âre* [airs]).

¶ The above analysis intentionally takes no account of the stemma proposed by the two philologists, which confirms it. In the stemma—which obviously will need to be carefully verified by other scholars—what I have regarded as secondary variants on the basis of Italian linguistic history of the fifteenth century go indeed back to branching point y, which accounts for only 25% of the surviving tradition.

I conclude with an example of diffraction *in absentia*. Among the many poems not included in the *Rerum vulgarium fragmenta*, but which tradition assigns to Petrarch, is a *frottola* entitled *Di ridere ò gran voglia*. This *frottola* is handed down, as far as we know, by some thirty fifteenth- and sixteenth-century witnesses, where it is often anonymous. In my opinion, it should be attributed (as some art historians use brands like “amico di Giotto” [friend of Giotto]) to a not otherwise identified “friend of Sacchetti”. In my own edition the witnesses—whose labeling and characteristics we do not need to go into here—read as follows at ll. 25-26:

L2 MA	un picholin disdegno me rimaso
le R4	un picc(i)olino isdegno me rimaso
R1	un picciol disegno me rimaso
b	un pochetin disdegno me rimaso
M1 t u	un piccioletto sdegno me rimaso
N2	un picciolo isdegno me rimaso
be Bo VP	un picciolin disdegno m'è rimaso

Given that the adjective-noun sequence *picc(i)olin(o)* / *picciol(o)* + *sdegno* / *disdegno* / *isdegno*, while not meaningless, is particularly sloppy and inelegant, even compared to the less than outstanding literary quality of the text, some familiarity with early Italian literature is all that is needed to recognize a literary cliché that very probably stood in this place in the text before diffraction occurred. This diffraction was apparently caused by a minor difficulty, namely, the *scriptio continua* “disdegno” for “di sdegno”.

A search in LIZ, s.vv. *rimaso*, *rimasi* etc., yields many examples of the cliché *mi è rimaso un poco di...*, which support the hypothesis I am about to put forward. These examples range from Cino da Pistoia (*Novellamente Amor* 10-11: “quel poco che *di vita* / gli rimase d’un’altra sua ferita”) to Boccaccio (*Teseida*, V 10: “E or *mi fosse un poco di speranza / rimasa*”; *Elegia di Madonna Fiammetta*, V,35: “mille modi di morte ho dimandati; li quali tutti *un poco di speranza*, che di te *m'è rimasa*, m’ha levati di mano”; *Decameron*, II 3: “gli confortò con lui insieme a vendere *quel poco che rimaso era loro e andarsene via*”) and saint Catherine of Siena (*Lettere*, 43: “Non dormite più [...] questo poco del tempo che *ci è rimaso*; perocché il tempo è breve, e dovete morire, e non sapete quando”).

Thus, the phrase to be restored after having been lost in the transmission of the *frottola* presumably followed the pattern *un poco di + vita / speranza / tempo* etc. [a little bit of + life / hope / time etc.] + “ethic” dative + 3rd person of the verb *rimanere* [to stay]. We can thus quite easily work our way back from *picholin disdegno* in L2 MA (and in the archetype) to the reading of the original, which must have been very similar:

un p[o]cholin di sdegno m’è rimaso [I was left with a little bit of disdain].

It is needless to say that *pocolino*, a modification of *poco*, is a relatively rare Florentinism in the literary language of the time.

I will return now to Contini’s essay of 1968, which also contains an important discussion of a short list of cases where the diffraction criterion has mere diagnostic significance. Although the “variety” of competing readings “seems suspicious”, sometimes Contini does not feel “able to provide a plausible solution” (CONTINI, 128 [= ID., *Frammenti*, II, 1013]). The scholar’s impeccable conclusion is that the short list of cases that adopting the new criterion fails to solve is “instructive [...] because it reminds us that the ‘reality’ we are pursuing, in textual criticism as in other fields, is the most parsimonious possible web of working hypotheses”:

The picklock presented here has picked many locks, but cannot open all doors. However, the bluntest of all tools is precisely the single manuscript, what [in the base text approach] is usually called the “best manuscript” (CONTINI, 134 [= ID., *Frammenti*, II, 1018]).

Bibliographical notes. Three of the four essays by Contini cited above are republished in CONTINI. In chronological order they are: CONTINI, 67-97, 99-134 (= ID., *Frammenti*, II, 957-985, 987-1018) e 3-66 (= ID., *Frammenti*, I, 3-62). The fourth, of 1955, has only been reprinted in ID., *Frammenti*, I, 265-280. See also L. Lazzerini, “Appunti e riflessioni in margine all’ecdotica di Gianfranco Contini”, *Anticomoderno* 3, 1997, 7-25: 14-16.

Other problems relative to the *Spagna Ferrarese*, a cura di V. Gritti and C. Montagnani, Novara, Centro di Studi Matteo Maria Boiardo-Interlinea, 2009, are discussed below (→ 3.8).

My classification of the witnesses of the *frottola* attributed to Petrarch can be found in P. Trovato, “Sull’attribuzione di ‘Di ridere è gran voglia’ (Disperse CCXIII). Con una nuova edizione del testo”, *Atti e Memorie dell’Accademia Patavina di Scienze, Lettere ed Arti*, cl. di sc. morali 110, 1997-98, parte III, 371-423.

3.3. On recent manuscripts, not necessarily worse than the others (*recentiores non deteriores*)

Novitas enim codicum non maius vitium est quam hominum adolescētia: etiam hic non semper aetas sapientiam afferit

Friedrich August Wolf, *Prolegomena ad Homerum*, 1795, 3

The problem in the case of each MS is like ‘a function of two variables’, (a) the character of its exemplar text; and (b) the skill or capacity of its scribe, who often combines the functions of copyist, critic and editor [...]. It follows from what has been said that *mere* antiquity of a MS. is no guarantee of the purity of its texts [...]. Our only hope therefore lies in a comparative study of MSS.

MOORE, xxiii-xxv

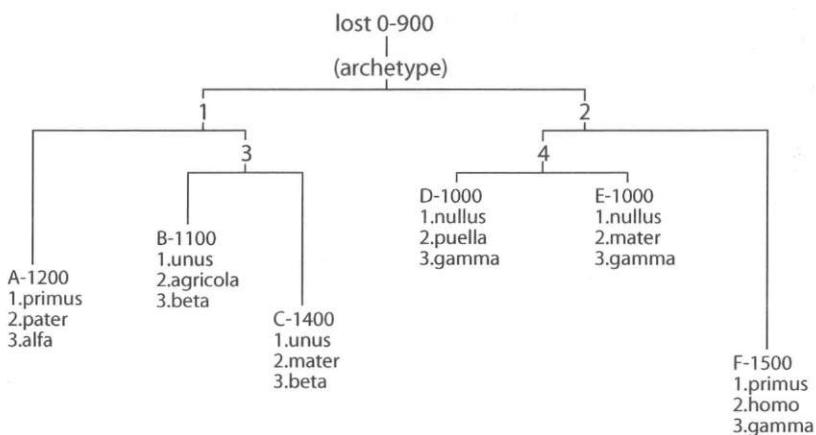
Never consider more recent manuscripts as necessarily inferior to older ones.

HUYGENS, 30

As I mentioned above, many nineteenth-century German classical philologists mistrusted the many witnesses transcribed by fifteenth-century *docti Itali*, because they suspected the Italian Humanists of overconfident recourse to conjecture. These philologists therefore preferred to work from a small number of early manuscripts, mostly from the time of Charlemagne or later (ninth, tenth or eleventh century). The temptation to trust only earlier manuscripts has seen a recent resurgence, especially among scholars faced with excessively rich, *overabundant* traditions. It should nevertheless be firmly rejected, for the reasons put forward by Hall in 1913 and further expounded by Pasquali in 1934.

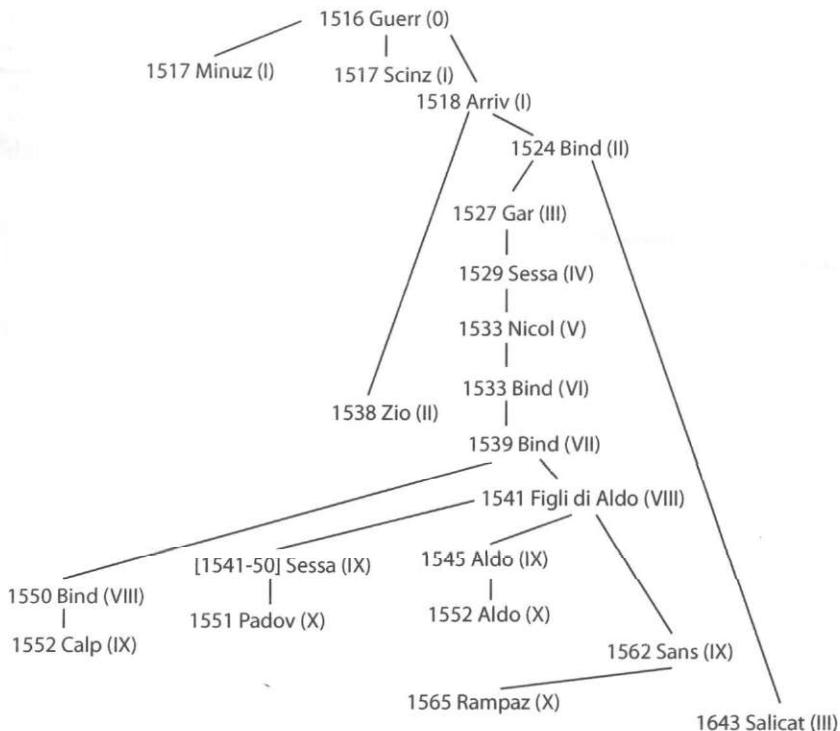
© The by now technical expression *overabundant traditions* was coined, as far as I know, by Emmanuel Amand de Mendieta in 1987 with reference to St. Gregory of Nazianzus.

Many “computerized” philologists disregard late copies. Ghislaine Viré, for example, only studies the 40 mss. of Hyginus transcribed between the ninth and fourteenth centuries, which she believes to be “moins corrompus que les manuscrits humanistiques”. Peter Robinson takes into account only witnesses of the *Canterbury Tales* earlier than 1500, which nevertheless run to 88. The issue is correctly addressed by Ben Salemans. In spite of having no direct knowledge of Pasquali (though he does use the works of a great student of Pasquali’s, Timpanaro), Salemans rightly observes, applying the majority principle to the following fictitious stemma:



The relatively young texts A and F, dating from 1200 and 1500 A.D., have the archetype reading ‘primus’, while older texts like D and E, both dating from 1000 A.D., have the derived, unoriginal reading ‘nullus’. In other words, the age of a text version or variant does not offer us trustworthy information about its originality. It is incorrect to assume that the oldest text version offers the archetypical or original readings (SALEMANS, *Building Stemmas*, 16).

I personally find that it is also didactically effective to use the stemma of a printed tradition as a model for a real tree (→ 2.3, 3.5), because the chronological distance between witnesses is almost always indicated explicitly and in the correct form by the witnesses themselves. Let us take, for example, the stemma of the *Regole grammaticali della volgar lingua* by the sixteenth-century grammarian Giovan Francesco Fortunio, for which I use Brian Richardson’s good edition:



The Roman numerals in parentheses, which I have added to Richardson's stemma (I, II, III etc.), indicate the rank of the copies, that is, their inevitable accumulation of errors (the edition of 1516, which was copied from a ms. provided by the author, here plays the role of the archetype). As is easily verified, the seventeenth-century edition on the lower right (of 1643) is only two steps away from the archetype, whereas the theoretically more promising editions of 1529 and 1533 on the central branch are down in ranks IV, V and VI. Some editions a century earlier than that of 1643, such as those of 1541 and 1545, are right down in ranks VIII and IX.

As a consequence, assuming high decimation of our real-tree model, if we decided to choose between surviving witnesses on the basis of their age, the seventeenth-century edition—which would have higher chances of survival than the other, because the value of T is low, and would certainly be among the highest-ranking surviving copies—would be disregarded by any philologist who aprioristically believed that earlier witnesses are *ipso facto* more genuine.

It is needless to say that the rank and position of witnesses in a stemma can only be determined *a posteriori*, through classification. This is true in all but a few exceptional cases when the *subscriptio* of a ms. or the colophon of a printed copy suggest a plausible relationship with an exemplar. Nevertheless these cases, too, *always* need to be verified through the genealogical method.

Bibliographical notes. HALL, 128-129; PASQUALI, 43-108 (Chap. “Recentiores non deteriores”); R. Browning, “Recentiores non deteriores”, *Bulletin of the Institute of Classical Studies* VII, 1960, 1-21; TIMPANARO, “Recentiores”; F. Vendruscolo, “Un ambiguo ‘motto’ pasqualiano: *recentiores, non deteriores*”, in *Studi in memoria di Giovanni Maria Del Basso*, Udine, Forum, 2000, 333-337.

Since few non-Italian scholars read Italian, some believe that “the famous adage ‘recentiores non deteriores’” was coined by Browning in 1960, as do, for example, C. Macé, P.V. Baret, “Why Phylogenetic Methods work: the Theory of Evolution and Textual Criticism”, in EoT, 101 note.

On overabundant traditions, see CHIESA, 101-105.

The stemma reproduced above is drawn from Giovan Francesco Fortunio, *Regole grammaticali della volgar lingua*, edited by B. Richardson, Roma-Padova, Antenore, 2001.

3.4. Contamination (or horizontal transmission) and extra-stemmatic contamination

In the simplest case, which will serve as a parameter for measuring abnormal cases, transmission is “vertical” (Pasquali’s term), that is, from copy to copy without deviations, and it is univocal, that is, it concerns a text that is fixed, with no alternatives. Pasquali calls “horizontal” or “transversal” a tradition in which more than an exemplar intervenes [*sc.* in a copy], by collation or contamination.

CONTINI, 22 [= ID., *Frammenti*, I, 19]

Contaminations become denser with the dissemination of a work. This is because: 1) the possibility increases of there being more than one witness of the work in the same *scriptorium*; 2) the probabilities increase that the codices used for the contamination are themselves already contaminated; 3) the reviser of the copies inevitably notices increasing differences between copies.

Segre, “Appunti...”, 72

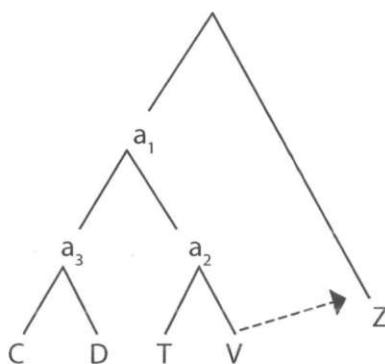
As is known, Paul Maas’s famous article *Indicative Errors and Stemmatic Types* (1937), published from 1950 on as an appendix to his manual, closes with the drastic observation:

Where texts were much read there is a tendency for contamination to creep in, and where contamination exists the science of stemmatics in the strict sense breaks down [...]. No specific has yet been discovered against contamination (MAAS [FLOWER], 61-62. The final sentence is even more expressive in the original German: "Gegen die Kontamination ist kein Kraut gewachsen": MAAS, 31).

As Contini explains in the epigraph above, one speaks of "contamination" when a witness does not limit itself to reproducing (usually adding further innovations) the characteristics of a single antecedent (in Pasquali's terminology: vertical tradition), but has significant errors in common with mss. from other families in the stemma (Pasquali, again, calls this horizontal or transversal tradition).

¶ This concept had already been formulated, as early as the eighteenth century, by philologists such as Gottlob Heyne and Johann Jacob Griesbach (TIM-PANARO, 44 [= ID. (MOST), 74]) and is also designated in English by the terms "conflation" or "hybridation".

In the following stemma, for example, ms. V, which has many errors in common with a_1 and a_2 , also has a number of errors, as well as good readings, that imply the consultation of a different witness, Z. This anomaly must be represented by a dotted or broken line, which, whenever possible (that is, whenever we are sure of which copyist took the initiative), should be oriented by drawing it as an arrow. Unlike the usage of many philologists, my personal preference is to start from the "contaminator"'s ms. and indicate which other witness it drew on, as in the figure below:



Contamination is a *perturbazione* (Timpanaro), that is to say, a disturbance, of transmission that can undermine attempts at classification. Save for especially simple cases (e.g., mss. or printed copies carrying alternative readings in the margins of the text, and thus clearly revealing the existence of a second tradition), contamination is detected only when the classification of witnesses is well under way. However, notwithstanding the pessimism of Maas and others, contamination has not always prevented textual scholars from successfully reconstructing ancient and modern transmissions affected by it (a broad range of examples can easily be found online by searching for “conflation of manuscripts”). As West observes:

The contamination may not be as total as it seems at first sight: some of the manuscripts do descend from an archetype directly enough for it to be reconstructed, and it is only the eclecticism of the others that confuses the picture (WEST 1973, 38).

Huygens, among others, is on the same wavelength:

Many complex manuscript traditions are more or less contaminated [...]. But in such cases too it usually remains possible to draw up a stemma, though details may be foggy and the constitution of the text will require even more skills from the editor (HUYGENS, 10).

In other words, given, as in our stemma, a manuscript V contaminated perhaps *deeply*, but not *completely*, it will be possible to specify, for instance, that V grafts errors (with, of course, correct readings) from Z or a relative of Z onto the text of its clearly distinguishable ancestor in the a_2 subfamily.

¶ Here and henceforth, to distinguish different types and degrees of contamination, I use the terminology of Segre, “*Appunti...*”.

There have even been some studies proposing possible “remedies against contamination”, notably Avalle, “Di alcuni rimedi...” (1961) and Froger. According to the latter:

Le moyen de résoudre l'anomalie consiste à se fonder sur la fréquence relative [sc. des accords] des groupes incompatibles, dont l'assemblage produit une irrégularité [...]. Pour choisir entre des groupes incompati-

bles, 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 abnormal 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.

[The way to solve this anomaly is to base ourselves on the relative frequency [[sc., 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 adopting the most probable interpretation [...]. Given, for example, the two frequencies 15 and 1, we sum them: $15+1=16$. The probability in favor 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, respectively 94% and 6%] (FROGER, 112-113).

In the same year, 1961, Segre ("Appunti...") introduced a useful distinction between contamination of readings ("the consequence of a collation [sc. of at least one collation] carried out on the ancestor of a codex") and contamination of exemplars, dwelling on the former and specifying that the latter lay outside the scope of his exposition:

When, 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, alternately, to only one of the groups that the two exemplars belong to (Segre, "Appunti...", 71).

Codicology and the study of the production of medieval books have evolved significantly in the last eighty years. Notably, beginning from Destrez (1935) and on to Fink-Errera (1962) and Pollard (1978), and then from Bataillon et al. (1988) to Soetermeer (1997) and then Murano (2005), scholars have become familiar with the so-called *pecia*-system,

a way of quickly multiplying textbooks that was widespread in European universities from the thirteenth to the fifteenth century. In a recent essay, which takes into account what historians of medieval books have taught us, Alberto Varvaro ("Considerazioni sulla contaminazione...", 191) rightly shifts the focus of the discussion to the causes of contamination, reviving the distinction between two types of perturbation in genealogical relationships:

1. contamination brought about by even partial reproduction of an *editio variorum* or an *editio variorum* in nuce (what Segre calls "contamination of readings" and Dutch stemmatologists "simultaneous contamination" or "*contamination simultanée*").
2. contamination brought about by the replacing, from a given point in the text onward, of one model with another (definable as "contamination of exemplars", or possibly even better as "contamination by juxtaposition of exemplars", or, again following Dutch stemmatologists, "successive contamination" or "*contamination successive*").

¶ On the subject of contamination of readings, MAAS [FLOWER], 8, § 10 already follows a path quite close to Varvaro's: "Contamination need not necessarily have come about through a scribe having two exemplars before him and giving now the text of one, now the text of the other; this is a very exhausting and, for that reason, unlikely procedure. What has happened is far more likely to have been something like this: in a manuscript, say F, the dissident readings of the other manuscript, which is not its exemplar—say A—are noted in the margin or between the lines; J in this case follows now the first reading of F, now the marginal or interlinear reading" etc.

Ignoring the precedents of Maas and Segre, Van Mulken curiously believes that the distinction she proposes between the two types of contamination was made possible "grâce à la dissection du procédé stemmatologique" developed at the Free University of Amsterdam: ("Les changements de parenté...", 108). To my knowledge, one of the first Romanists to have hypothesized, in essentially correct terms, contamination by juxtaposition of exemplars is Alexandre Micha, *La tradition manuscrite des romans de Chrétien de Troyes*, Paris, Droz, 1939, 2^e tirage: Genève, Droz, 1966, 206-209.

As Varvaro did not fail to point out, these two types of contamination are carried out, respectively, by two different categories of individuals, namely, copyists and readers. As a rule, the genesis of mss. with marginal variants is not "the result of the activity of the more scrupulous copyists, but of that of the most sagacious readers". These readers do not do "heavy work". They are not paid by output. They can "reflect on the text

they are reading”, double-check it against other copies, and, “note down at their leisure the alternative reading” that appears to be “more satisfactory” (Varvaro, “Considerazioni sulla contaminazione...”, 191).

As to the juxtaposition of exemplars, it is best to quote Varvaro directly:

Everyone should know that most medieval manuscripts are the work of professional copyists, and that they were copied quire by quire before being bound into a codex. Now, several copies of the same text are likely to exist in the workshops of professional copyists, more than in private libraries, especially if this text is in demand on the market [...]. In cases like the manuscripts of Dante of the *gruppo del Cento*, to me it seems beyond doubt that the workshop must have used many exemplars, because this was the only way available to the medieval book publisher of accelerating production, as it allowed several scribes to copy the same text simultaneously. It is likely that these copies were in loose quires, but even had they been bound it would not have made much difference.

The important thing is that, after interrupting work, for example for a night's rest, the copyist had no reason to pick up the exact same exemplar he had used the day before. If he had finished copying, say, the third loose quire, all he wanted was to resume work on a copy of the fourth loose quire. Again, it would have made no difference if he had worked on a bound ms. instead, because the same principle applies: what counted was whether he had access to another bound ms. or quire of the same work.

If this hypothesis hits the mark, it is hardly surprising that the final result is contaminated. Not in the sense that the text of the exemplar was modified through the inclusion of individual readings from another codex and another text family, but in the very different sense that membership of a text family can vary from one quire to the other [...] or, better, from one session of work to another, where the session of work may correspond or not to complete quires (id., “Considerazioni sulla contaminazione...”, 193).

Varvaro adds, possibly overoptimistically:

It is immediately evident that circumstances of this kind make the identification of the source of contamination much harder, or even impossible. But we need to draw an even more serious consequence. In long texts where contamination has been proved or is suspected, it is not prudent to build stemmata on a few erroneous variants found in a few passages. If affiliation has changed even once at the upper levels of the stemma, these stemmata have no correspondence with reality except as regards a specific portion of the text (id., “Considerazioni sulla contaminazione...”, 193).

Now, contamination of readings is typical of educated milieus. Contamination by juxtaposition of exemplars is typical of busy *scriptoria* and exclusively affects works of a certain length, which are impossible to copy in a single session of work. In the general absence of quantitative terms of comparison, it may be interesting to know that, out of 386 fourteenth- and fifteenth-century mss. of Dante's *Commedia* collated up to 2011 by my small team, 15% show this kind of contamination. And it will be comforting to know that, in the overwhelming majority of these cases, the copyists used only two exemplars, and as a rule the change of exemplar coincided with the change of canticle (or, much less frequently, with the change of quire).

In operative terms, if all the sources of a contaminated ms. or of a subfamily of contaminated mss. are preserved, there is no need to use the contaminated ms. or subfamily, which can be eliminated as *codices descripti* (→ 1.4), or rather *inutiles*. If, instead, a ms. that descends to some extent from a known exemplar contains high-quality variants not found in any known ms. or subfamily, we shall need to assume that “extra-stemmatic” (Timpanaro) or rather “extra-archetypal” contamination has occurred.

Indeed, as Timpanaro observes in his celebrated *Appendix C*:

There are correct readings at which no medieval 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: TIMPANARO (MOST), 179 [= TIMPANARO, 152-153]).

The explanation of the problem (which had been glimpsed, but not clarified, by dom Quentin and Bédier in the 1920s) is impeccable, and the limits within which textual scholars can legitimately suppose *extra-stemmatic*

contamination are also defined very persuasively on condition, however, that certain crucial notions of textual criticism, some of which I have already presented in this manual, are sufficiently clear. I will summarize these notions briefly, favoring didactic clarity over the variability of the data.

As the reader will remember:

- A) 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 determine its loss increases proportionally to the temporal distance (T) between us and the early copies.
- B) Every time it has been possible to cross-check data on print runs with those on the present survival of given incunables and *cinquecentine* printed in large numbers and formats, very high decimation rates were observed, between 77% and 100% (with *chapbooks*, i.e. booklets [It. *fogli volanti*, Spanish *pliegos sueltos*], the percentages would be even more impressive). It thus seems unreasonable to imagine that for thirteenth and fourteenth-century mss. (for which T is higher) decimation rates could be less than 80%.
- C) Contrary to what is often said, a *stemma codicum* of our time *does not* mirror (or only to a very small degree) the actual history of the transmission of a medieval text (→ 3.6), that is, what Fourquet, Castellani, Timpanaro and others call the real tree (the total tradition of a work). A stemma is simply the most effective depiction of the relationships between members of a random sub-group of the tradition, consisting of the mss. still in existence at the time when the philologist creates the stemma. These, as I have just said, can hardly comprise more than 20% of the mss. that made up the real tree.
- D) As a rule, most or all surviving mss. do not belong to the initial phase of the transmission of a work, which is inevitably meager, but reflect the vulgate texts (*vulgatae*) that followed one another over time, that is, starting from a moment (more or less far removed from the writing of the original) in which, to meet growing public demand, one or more copyists specialized in the reproduction of the work (→ 3.5). Examples could include, along with the better known copyists of the *Commedia* (the scribes of Parm, Pr, Vat, the “main hand” of the Cento group, and so on), Biagio Buonaccorsi, a friend of Machiavelli’s and a copyist of many of his works. From the textual standpoint, these mss. are usually rather far removed—that is,

separated by a high number of *interpositi*—from both the original and the archetype.

- E) In the Lachmannian and Neo-Lachmannian sense of the word, the archetype (which inevitably precedes the earliest preserved ms. in the stemma) is not as a rule an especially important exemplar prepared by the author for “publication”; i.e., it is not a synonym of the Latin *archetypum*. Rather, it is any lost ms., perhaps one situated in a low and marginal position in the real tree, from which *all surviving mss.* appear to derive. With Latin and Greek classics, the archetype is relatively easy to single out, because the chronological distance between the original and the surviving copies (and hence the number of *interpositi* and, accordingly, of errors) is as a rule a thousand years or not much less. With Romance texts, the archetype is sometimes only a few weeks later than the completion of a work, and thus hard to identify, because there were few acts of copying in between, and hence few errors (as in the case, for example, of Machiavelli’s *Vita di Castruccio Castracani*).

Let us return to the manuscript tradition of any long text of the Middle Ages or the Renaissance. Provided that there are reasons to do so, hypothesizing extra-stemmatic contamination for a text with a relatively large tradition simply means imagining that one of the copyists or readers whose copy has had the chance of coming down to us had the opportunity, at a time when T (and decimation) was very low or nil, to draw sporadically on one of the now lost mss. that formed the higher zone of the real tree.

¶ To represent extra-stemmatic contamination in the stemma, the broken or dotted line extending out from the contaminated witness will reach the lost witness, *y* or *w* or *z*, which conventionally should stand no lower than the archetype (for an example, → 8.2).

By common consent among specialists, contamination often baffles the programs for the automatic generation of stemmata used in computer-assisted philology.

¶ E.g.: Robert O’Hara, Peter Robinson, “Computer Assisted Methods of Stemmatic Analysis”, section “Limitations of cladistic analysis: contamination” (accessible online); MOTTA-ROBINS, LXXXVIII.

In this field, too, it is advisable to follow the scientific principle of simplicity or parsimony, and thus prefer more parsimonious hypotheses, “limiting to the minimum the places and sources of contamination and, other conditions being equal, involving the lower number of contaminations” (Avalle, “Di alcuni rimedi...”, 46-47).

Bibliographical notes. d.S. Avalle, “Di alcuni rimedi contro la ‘contaminazione.’ Saggio di applicazione alla tradizione manoscritta di Rigaut de Berbezilh”, in *La letteratura medievale in lingua d’oc nella sua tradizione manoscritta*, Torino, Einaudi, 1961, 159-178, later in id., *La doppia verità*, 35-51; C. Segre, “Appunti sul problema delle contaminazioni nei testi in prosa”, in SPCT, 63-67, later in id., *Ecdotica*, 71-74; FROGER, 112-113; M. Van Mulken, “Les changements de parenté dans le *Cligès de Chrétien de Troyes*”, in NOF, 103-114; A. Varvaro, “Considerazioni sulla contaminazione, sulle varianti adiafore e sullo stemma codicum”, in *Storia della lingua italiana e filologia*, 191-196; E. Tonello, P. Trovato, “Contaminazione di lezioni e contaminazione per giustapposizione di esemplari nella tradizione della *Commedia*”, *Filologia italiana* 8, 2011, 17-32 (the article also contains an up-to-date bibliography on the *pecia*). See also REEVE, *Manuscripts and Methods*, 211-219.

On *codices inutiles*: TIMPANARO, “Recentiores”.

On extra-stemmatic contamination, see the still vague indications of J.P. Postgate, “Textual Criticism”, in *Encyclopaedia Britannica*, Cambridge 1911¹¹, 708-715: 710 (“A special value attaches to a conflated codex when one of the MSS. from which it has been compounded has perished and its readings are thus otherwise irrecoverable”), dom Quentin and BÉDIER, 55 (“Encore devra-t-il [un critique] tenir compte d’une certaine possibilité, que le savant benédictein lui-même [dom Quentin], traitant de la *Vulgata*, a pris soin de mettre en relief, aux pages 94 et suivante de ses *Essais*: à savoir que tels et tels copistes peuvent avoir utilisé, non pas un archéotype, mais deux”). The problem is already made clear, for example, in a review by E.N. O’Neil, *Classical Philology* 51/1, 1956, 42-44: a ms. receives readings “by collation with some unknown extra-archetypal ms.” See TIMPANARO, 152-153 [= id. (MOST), 179-180]]; REEVE, *Manuscripts and Methods*, 37-42; P. Trovato, “Postille a una postilla. Il subarchetipo β della Commedia, i luoghi barbiani e la contaminazione extrastematica”, *Studi danteschi* 74, 2009, 307-315, from which some of the above sentences are drawn.

As far as I know, Reeve’s perplexities (terminological and otherwise) in his *Manuscripts and Methods*, 37-42 (“Timpanaro’s definition [...] leaves me wondering how anything can be *extrastematic* in the stemma of a whole tradition”), mainly regard the consequences that extra-stemmatic contamination has, according to Timpanaro, on bipartition, and not the fact, which Reeve also admits, that a medieval or Renaissance copyist could easily draw on early witnesses which have disappeared without leaving surviving copies. Although I believe the competing term “extra-archetypal contamination” to be more appropriate, I find it preferable to keep using the ter-

minology used by Timpanaro in 1965, just as one pays homage to Fourquet by retaining the expression “real tree”, although what it evokes is important, but not very *real*.

3.5. Archetypes and vulgate texts

The more recent the archetype, the likelier it is to be extant [...]. The greatest concentration of dead archetypes, for both Greek and Latin, are found in antiquity and then in periods of revival after the Dark Ages—the ninth century for Latin, AD 850-1000 for Greek.

WEITZMAN, *The Evolution*, 300-301

Not even the mathematically minded Paul Maas appears to be totally immune to the confusion between *archetype*₁ and *archetype*₂, discussed above (→ 1.5), when he writes:

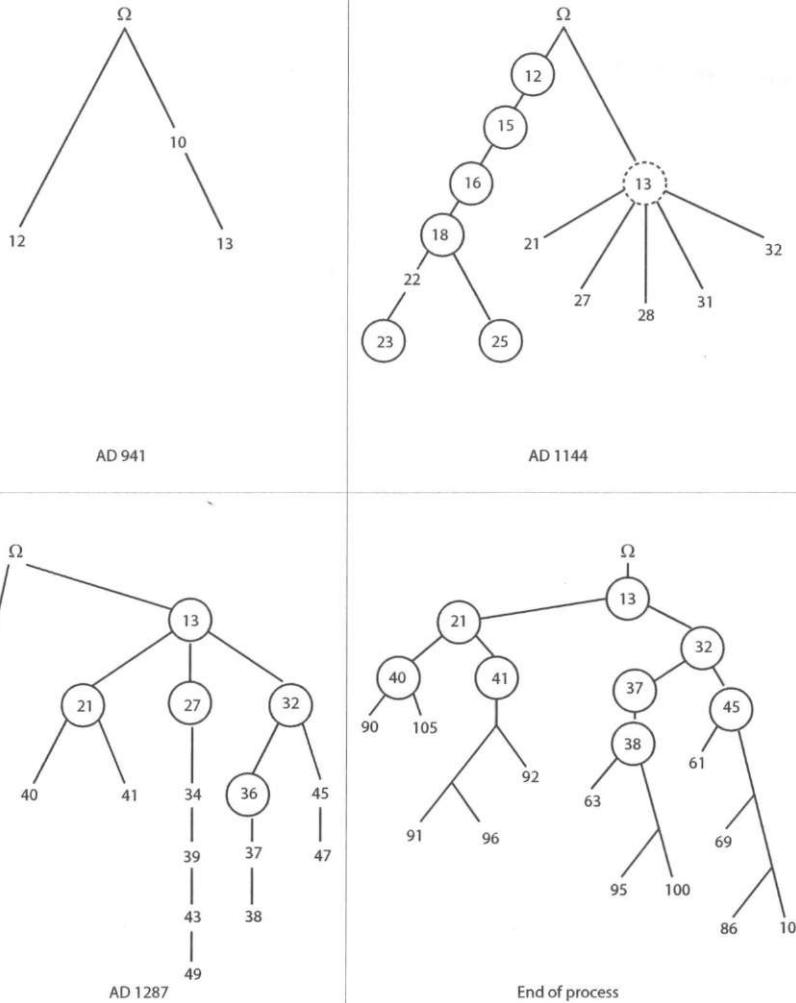
The exemplar from which the first split originated we call the *archetype*. The text of this archetype is free from all errors arising after the split and is therefore closer to the original than the text of any of the witnesses. If we succeed then in establishing the text of this, the *constitutio* (reconstruction of the original) is considerably advanced (MAAS [FLOWER], 2-3).

The first sentence would appear to refer to the formation of the real tree, that is, the history of the tradition, whereas the rest correctly describes the properties of the vertex of the stemma in Neo-Lachmannian stemmatics.

¶ Actually, although the formulation is not crystal clear, the context suggests that Maas is reflecting on the ramifications of the stemma and not on those of the real tree (an observation we owe to REEVE, *Manuscript and Methods*, 124. See also MONTANARI, 36).

Now, just as stemmata are prevalently two-branched, the high probability that the stemmata of classical or medieval works have a lost manuscript (Lachmann’s *archetypon*) at their vertex seems satisfactorily explained by the random but devastating effect of a high “decimation” of manuscripts. The “artificial” genealogical trees produced by Michael Weitzman’s PC in 1982, which I referred to in my discussion of Bédier’s paradox (→ 2.3), are didactically valuable as a means to make the concept of *archetype* in the Neo-Lachmannian meaning (our *archetype*₂) easier to digest.

As the reader will remember, Weitzman's computer generated 15 models of traditions. His software replaced fate by causing mss. to "be born" and "die" randomly, but on the basis of well-defined parameters. It thus altered the shape and size of the transmission over time, as exemplified by the four phases of one of his experiments which I reproduce again below:



I remind the reader that a circle indicates that the witness is "dead", that is, lost, and a dotted circle indicates that the witness is about to disappear. Many dead witnesses, being inessential for the study of the surviving tradition, are left out in Weitzman's diagrams.

Now, in AD 941, the tradition under examination is formed of two tiny branches, one constituted by copy 12, the other by copies 10 and 13 (1-9 and 11 are hence lost). By 1144, the left branch is almost completely extinct (only 22 survives). The right branch's earlier survivor, 13, has generated 5 descendants. Given that errors, as we know, and thus also indicative errors, insinuate themselves with increasing frequency into witnesses that are copies of copies of copies, the philologist using the genealogical method to classify the witnesses that still survive at the end of the process (from right to left: 101, 86 etc.) would reconstruct the text of the lost ms. 13, which would thus be the common antecedent of all surviving witnesses, i.e., the archetype.

But let us suppose for an instant that the end of the process was even more devastating for the real tree, and that all the witnesses of the left branch are extinct. On this second hypothesis, the archetype would be 32, which is placed lower, and hence further from the original.

It should be noted that the genealogically lower 32 would be an archetype riddled with a higher number of errors, that is, easier to recognize than 13. For obvious reasons, ease in identifying an archetype, being dependent on the number of errors it contains, is inversely proportional to the textual quality of the archetype itself.

Besides, the date of an archetype is by definition earlier than that of the earliest ms. contained in the stemma.

© MAAS [FLOWER], § 17, however, goes too far when he says: "the archetype must be earlier than the time of the earliest datable variant", because he fails to take into account contamination (→ 3.4), or the fact that the witness from which a fragment descends that is attested in papyri or the indirect tradition could contain only a small part of the errors of the archetype, and thus rank much higher than surviving tradition.

For example, let us consider the textual history of two works of Dante, who died in 1321. While the archetype of Dante's *Commedia* must be at least three months earlier than October 1330 (the date when the copying of the lost ms. *a* began), the archetype of the *Convivio*, which knew a revival possibly due to interest in his major work, is later by several decades (late fourteenth century). It will thus not come as a surprise that:

- 1) while the archetype of the *Commedia* is hard to make out (→ 3.7), the most recent editor of the *Convivio*, the late Franca Brambilla Ageno, managed to identify about a thousand probable archetypal errors in the treatise;
- 2) while modern philologists tend to underestimate the usefulness of so-called indirect tradition (quotations in other works, translations, etc.),

the quotations from the *Convivio* in one of the earliest commentaries to the *Commedia* (the early-fourteenth-century commentary known as the *Ottimo*), being pre-archetypal, are much more correct than the text handed down by the surviving mss.

Guidi and Trovato obtained, by other means, results not too different from Weitzman's illustrated above. They used some stemmata of printed works (which, as is known, are as a rule complete) as models of real trees, and variously modified them by assuming several different hypotheses of decimation (→ 2.3). With low decimation percentages (between 10 and 30%), the size and ramifications of the real tree do not vary significantly. Even copy no. 1 of the transmission, taken directly from the original, stands a good chance of surviving. With high decimation rates (between 70 and 90%), the less luxuriant branches and the copies closest to the original (which are inevitably less numerous than lower-ranking ones) disappear leaving no traces. In this case, too, the genealogical method would allow philologists to trace the surviving copies back to a common ancestor, more or less far removed from the first generation, that is, the archetype.

This is why it is correctly argued, as I observed in 1.5, that the Neo-Lachmannian archetype, "lacking any 'official status' or normativity" (Timpanaro), is a manuscript "randomly" identified by philologists while classifying surviving witnesses.

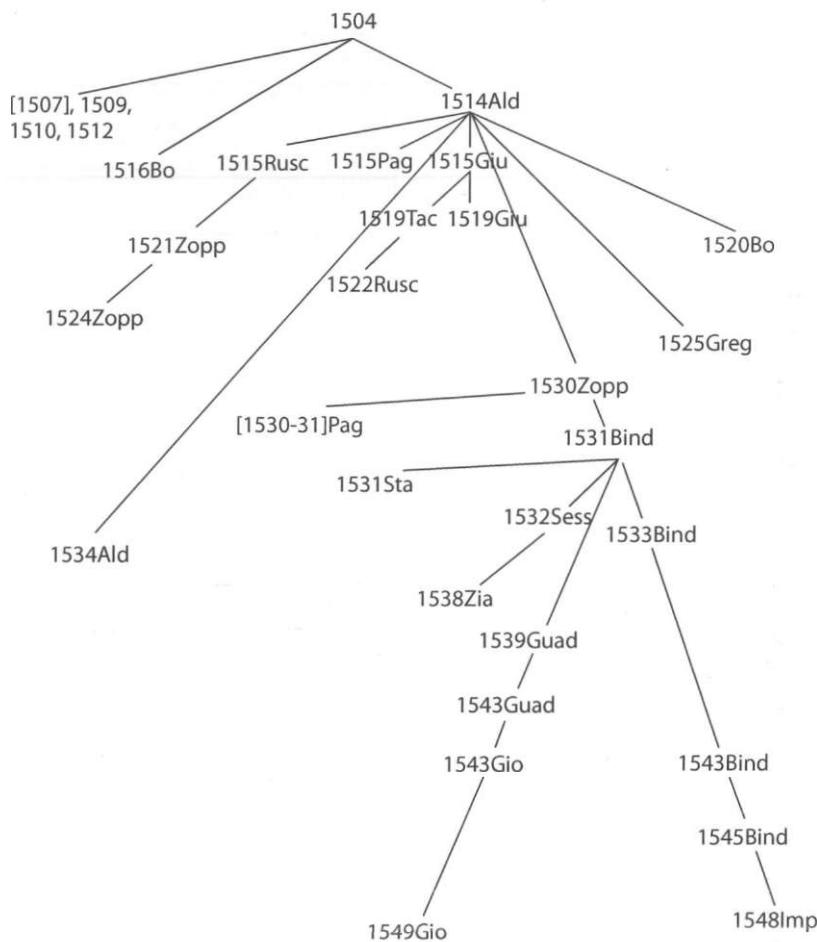
A comparison of a large number of stemmata shows that it is normal, or at least very frequent, for surviving traditions to be decidedly asymmetrical. Tradition almost never distributes itself equally among the various branches of the tree. Some branches are luxuriant, others slender and under-represented. Just as, according to certain popular writers on Darwinian evolution, the genes of well-adapted individuals tend to replace those of less well-endowed ones in the struggle for life, successful editions (vulgate texts, whether manuscript or printed) put their stamp on taste and the book market, and spread rapidly, to the detriment of products considered to be of poor quality, outdated, or too idiosyncratic (for reasons having to do with their language, size, *mise en page*, etc.).

In Italian the usual term for designating the vulgate text, the edition of a given text normally in circulation at a given time or in a given area,

is the Latinism *vulgata* (from *editio vulgata*). Obviously, for fairly successful works several *vulgatae*, i.e. vulgate texts, can be distinguished.

It is needless to say that the broad circulation of vulgate texts is in itself a powerful factor in their transmission to the later tradition. The most luxuriant branches of printed traditions suggest that from the sixteenth century on *vulgatae* follow one another at intervals of roughly ten to twenty years. When new vulgate texts meet public demand, the reproduction of earlier models becomes extremely rare.

Let us take, for example, the case of the second version of Sannazaro's *Arcadia*, which can be represented as follows:



After the quarto *editio princeps* (1504) and the editions modelled on the first edition (1507, 1509, etc.), the new vulgate text is constituted by the Aldine edition of 1514. After the Aldine, which is of course in octavo and italics, for fifteen years or so every editor follows the new standard, and thus reproduces the Florentinizing corrections of the Aldine (with the sole exception of an edition that is singular in many respects, viz., Bologna 1516, in the central branch). The Zoppino of 1530 is also based on the Aldine, that is, on the ancestor of the previous *vulgata* text, but for commercial reasons boasts about corrections and authorial additions, starting already from the title (*Arcadia di m. Giacomo Sannazaro nobile napolitano, con somma diligenza corretta, et nuouamente con la gionta ristampata* [that is, ...recently reprinted with additions]). All publishers bowed to the new standard, with the exceptions of Aldo's heirs, who, on the strength of their tradition and prestige, as late as 1534 were still reprinting their editions of 20 years earlier with no substantial changes.

It is hard to conceive that, unlike Italian printers of the sixteenth century, medieval copyists could possibly have had no concern for meeting the demand of the public. We need only consider dense transmissions such as that of the *Commedia*, where we get a glimpse of the coexistence of different models, on one or two columns, in *littera textualis* or chancery hand, with rubrics in *vulgare* or Latin, with or without illuminations or commentaries, etc. It is likely, on the other hand, that in pre-Gutenberg Europe the turnover of book models and formats was slower, and the marginalization of out-of-fashion witnesses less drastic, than in the new era of the printed book.

Bibliographical notes. On the notion of archetype, → 1.5.

On the notion of “vulgate texts”, see, on the one hand, the special issue of *Filologia italiana* 3, 2006, on the other, GUIDI-TROVATO.

On the printed transmission of the *Arcadia*, see Paolo Trovato, *Con ogni diligenza corretto. La stampa e le revisioni editoriali dei testi letterari italiani (1470-1570)*, Bologna, il Mulino, 1991 (anastatic reprint, Ferrara, Unifepress, 2009), 156-158, 197-200.

3.6. The shape of stemmata and the structure of real trees

Le stemma [...] n'est pas de façon stricte l'arbre généalogique réel de toute la tradition du texte. C'est un schéma dont nous espérons qu'il n'est pas infidèle à l'arbre généalogique réel mais qui n'en retient pas nécessairement toutes les ramifications [...]. Le caractère schématique du stemma provient en outre de ce que l'absence d'un assez grand nombre de témoins intermédiaires, disparus au cours des âges, ne permettra jamais de déterminer avec une certitude absolue si un manuscrit est fils d'un autre ou encore son neveu, ou son petit-fils.

Pierre Marie de Contenson, O. P., *L'édition critique des œuvres de S. Thomas d'Aquin...*, 65

It would be wrong to interpret a stemma as an exact historical picture of the history of the deliverance of the versions. One should bear in mind that a stemma is a *minimal* picture relating only to the text versions [i.e. copies] that still exist. Thus, a stemma can only be considered as a hypothesis about (a part of) the historic reality. On and around the lines of descent, we can imagine lost manuscripts whose contents are unknown.

SALEMANS, *Building Stemmas*, 14

Many believe stemmata to be accurate depictions of the historical vicissitudes of transmission, but it is the few who believe that this is *not* the case who are right.

The existence and spread of successive waves of vulgate texts is not a mere erudite detail, but a mechanism that has a strong impact on the work of philologists, and especially on their stemmata. The gradual thinning out, over time, of the original branches of a genealogical tree invariably spares the most popular *vulgatae*. Without disregarding the many alternative explanations put forward so far—contamination, polygenesis of innovations, etc.—which will help to complete our general picture of the factors at play, the prevalence of two-branched stemmata—that is, the frequent paring down of medieval and Renaissance traditions to only two branches—results, as the reader will remember, on the one hand from the high loss or decimation rate of manuscripts (which varies case by case, but is nevertheless always very high), on the other from the asymmetry usually observed between the various branches of transmission, some of which are represented by only one or two witnesses, others by bloated *vulgatae* (→ 3.3).

Now, if we knew the size and shape of the real trees, we could specify case by case the decimation rates that determine the transition from a, so to speak, “rich” transmission (where the archetype is preserved, along

with many witnesses arranged in more than two branches) to a poor one (two-branched stemma and loss of the archetype). Vincenzo Guidi has calculated these values, too, starting from our usual models of the real tree. Using as a model the three-branched printed tradition of Sannazaro discussed above, it is sufficient for the decimation rate to be above 51.6% for there to be a high probability of ending up with a two-branched stemma and the extinction of the archetype. In Guidi's own words, "the transition from one regime to the other is only a matter of probability of decimation".

Furthermore, as we realize every time we have access to adequate information, the surviving witnesses most often in our possession carry vulgate texts and are *editiones variorum*, that is, more or less contaminated texts copied in more or less well-organized *scriptoria*.

Whatever the decimation rates used in our experiments, it is intuitively evident that, given any real tree, with 2, 3, or exceptionally even 5 or 6 branches, the chances of survival for any of its primary branches (which we can call *real families*) will be:

- a. high if it is a family crowded with witnesses (and therefore in many cases with further branches of its own),
- b. low if it is a family with few members,
- c. very low, or non-existent, if it is a family composed of a single manuscript.

For example, if we take the three-branched stemma of the *Arcadia* reproduced in the previous section and assume an 80% decimation, the chances of survival for the central branch, which only has a single witness, are slim, and they are also poor for the four editions on the left branch, while the right branch, composed of 26 witnesses, stands a high chance of not disappearing.

Thus:

- d. we cannot affirm that the number of branches of a *stemma codicum* is totally independent of the number of real families. Nevertheless, the connection between the two series is anything but automatic, and essentially depends on two unknown quantities: the size of each primary branch (or real family), and the decimation rate.

- e. The correlation between the conservation of the archetype and the "richness" of the tradition, discussed above, could permit diagnostic deductions regarding the decimation of the traditions under examination. The conservation of the archetype—a rather rare event in classical and Romance philology—could mean that an especially popular tradition had a relatively low decimation rate. On the contrary, a sparse tradition, in the sense specified above (two branches and the loss of the archetype), will imply in most cases high decimation rates, between 80 and 99%.

I do not want to give the wrong impression: I still believe that the common-error method is the most powerful tool available to us for coming close to a lost original starting from n copies of unknown rank and quality. This faith, whose rational basis I hope I have at least in part explained, is not shaken by my awareness that many of our sparser stemmata (two-branched and sometimes even single-branched) must inevitably be the random result of the brutal pruning of real trees with three or more main branches that today are out of our reach.

What I find more worrying (albeit only to a certain degree, since we can only write history from the documents available) is the realization that not all the branches of our stemmata, whether two or more-branched, necessarily derive from as many real families. As we have seen, single-member or small families are condemned to disappear leaving no trace. We must admit that stemmata often show only the most populated branches (and even these dimly and only at their most robust splits) of real trees that originally had a completely different structure; i.e., they only show especially popular vulgate texts.

As if the lower-case letters in our stemmata or the infinite points of which their lines are composed were not enough, Contini's precept, so often repeated in our manuals, that a critical edition is a mere "working hypothesis" seems to find new and more decisive confirmation in this general downgrading of witnesses that are crucial to us, as on them depends the accuracy of our editions.

Bibliographical notes. These issues are discussed in more detail in GUIDI-TROVATO. See also P.M. de Contenson, O. P., *L'Édition critique des œuvres de S. Thomas d'Aquin. Principes, méthodes, problèmes et perspectives*, in *Probleme der Edition Mittel- und Neulateinischer Texte: Kolloquium der Deutschen Forschungsgemeinschaft*, Bonn, 26-28. Februar 1973, herausgegeben von L. Hödl und D. Wuttke, Boppard, Boldt, 1978, 55-73; SALEMANS, *Building Stemmas*, 14.

3.7. The partial obscuring of a hyparchetype or of the archetype

If α [i.e. the archetype] has split not merely into β and γ but also into K or still further branches, the text of α is guaranteed by the agreement of two of these branches.

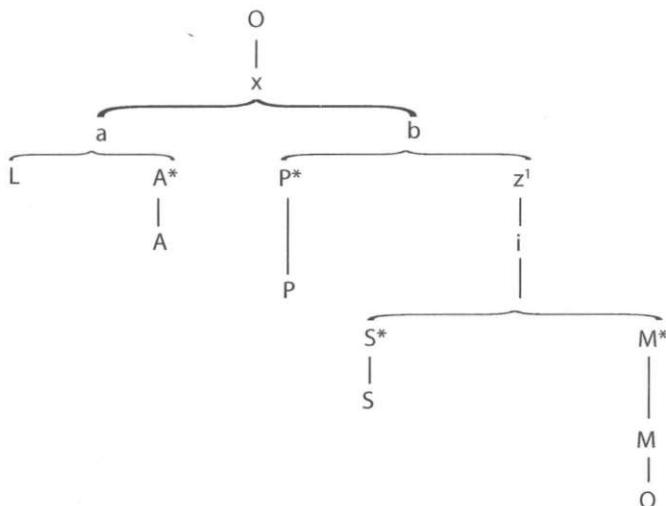
MAAS (FLOWER), § 9

The absence of a characteristic error can very often, given the character of contamination, be the (even polygenetic) result of conjectural corrections

SEGRE, *Ecdotica*, 73

As the reader will remember, the archetype of our stemmata is the “código o impreso [...] que transmita errores comunes a todos los testimonios conservados”, that is, “a lost copy marred by at least one error of the conjunctive type, from which the whole tradition derives” (\rightarrow 1.5). Consequently, one error in the archetype should be shared by the whole preserved tradition.

Now, let us take the following stemma:



This is not a fictitious stemma. It is the earliest stemma ever drawn up for an edition of a Romance text. It was created by Gaston Paris in 1872 for his study of *Saint Alexis*, and is thus the earliest among those criticized by Bédier in 1928 for their presumed arbitrariness.

© O designates the original, *x* the archetype. According to Paris, the letters followed by * indicate that he is sure of the existence of lost ancestors of witnesses A, M, P and S. These letters can strictly speaking be disregarded, because the lines of stemmata, like geometrical lines, are composed of infinite points.

As Contini illustrates, Paris uses two kinds of proofs to demonstrate the existence of *x*, namely,

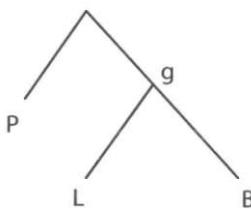
unacceptable readings (or readings regarded as unacceptable) that lived on in the whole tradition, and *non-canonical agreements* (LP vs. AS, LS vs. AP) that should be interpreted as (independent) conjectural emendations of a corrupted archetype, for example, hypermetric or deficient lines (CONTINI, 76 [= ID., *Frammenti*, II, 965]; my emphasis).

Here I do not intend to test the validity of Paris's demonstration (for Saint Alexis Contini proposes instead a three-branched stemma: L; PSM; A + V, a later discovery by Rajna). What I want to stress is that the use of partial agreements as a means to identify the archetype is not an abstract hypothesis, invented to lengthen this chapter by a couple of pages, nor a bizarre idea conceived by Paris alone. The method was not unfamiliar to classicists (it is sufficient to read Maas's passage quoted in the epigraph to the present chapter, and TIMPANARO, 153 [= ID. (MOST), 179]). It is fully consistent with the logical assumptions of the genealogical method, and often an inevitable choice for editors of medieval and modern texts, who work on traditions where the time that elapsed between the original and the copies available to philologists is sometimes very short, and what are undeniably archetypal errors can hence be counted on the fingers of one hand.

An easy-to-explain case of partial "obscuration" of the archetypes is that of the *Vita di Castruccio Castracani* by Machiavelli, an edition of which by my Dutch student Riekie Brakkee came out in 1986. Castruccio's biography is handed down in two mss., L and P—the latter is dated *a dì 28 ottobre 1520*, and is thus only a few weeks later than the original—, and by the *editio princeps* B (Roma, Blado, 1530), where the main text, the *Principe*, is accompanied by the *Vita* and other minor works by Machiavelli. The whole printed tradition both of the *Vita di Castruccio* and of the *Principe* itself derives from this edition, which was reproduced in its

turn—as was frequently the case with forbidden works—in many late sixteenth- and seventeenth-century mss. after Machiavelli's texts were put on the *Index librorum prohibitorum*.

After having proved, by pointing out an adequate number of conjunctive and separative errors, that L and B are related and independent of P, and having spotted what is certainly an archetypal error (§ 85 “Baldo Cecchi et Iacopo Baldini, tucti [et due] uomini di auctorità”; the role played by these two characters is confirmed by Giovanni Villani's chronicle, a favorite “source” for Machiavelli the historian), Brakkee suggests that two more passages where L and P display a common error against B are actually archetypal errors, obscured by the well-documented emending practices of the correctors of the *editio princeps*: § 32 *comunicò questo suo sdegno L, ...suo disdegno P] ...suo disegno B*; § 98 *millequattrocento cavagli L P] quattrocento cavagli B* (the number in B is confirmed by its having already been stated previously in § 95: “a band of four hundred horses [i.e. mounted soldiers]”). The higher levels of the transmission thus appear to have the following structure:



Possibly concerned with proving the need for a new edition, which is not too evident if we compare the two textual reconstructions in detail, the most recent editor of the *Vita*, Carlo Varotti, has returned to the old and groundless genealogical reconstruction of Mario Casella, who presumably on the basis of the same two errors drew up a different stemma, placing L and P in the same family and opposing it to B. Neither Casella in 1929 nor Varotti in 2010, however, has been able to explain, with their stemmata, why L and B share the following errors and innovations (I have marked the certain errors in L B with an *, followed by the correct or at least preferable reading in P):

4 assalto il Valdarno L B (assalto il Valdarno di sotto P);

*101 che la parte guelfa mantenesse lo stato di Toscana L B (...lo stato in Toscana P);

*102 perche gli era stato necessitato (necessario B) partirsi L B (perche egli era stato necessitato partirsi P);

148 Era ancora mirabile L B (Potrebbesi etiam dire di Castruccio come era mirabile P).

I will limit myself to quoting, in a somewhat abridged form, Brakkee's well-argued comments on the two errors:

In P, *stato* (§ 101) has the antiquated and very Machiavellian sense of "power", "government", which was to go out of fashion during the sixteenth century ("stato di Toscana" [...], at any rate, would be an anachronistic designation [...]). [...]

Although he frequently employs the compound forms of the verb *necessitare* [...] Machiavelli [...] shuns the clumsy construction "gli era stato necessitato" (see, for example, *Principe*, VIII [...]: "iudico che basti, a chi fussi necessitato, imitargli", "furono necessitati accordarsi con quello", "è sempre necessitato tenere il coltello in mano"). The error was already in g (upstream of which was possibly the undivided "perchegliera"). This is proved by the normalizing intervention [...] of B [...]: "perché gli era stato *necessario* partirsi" (Machiavelli, *La vita...*, 64-65).

I will add that, for the sake of scruple, I checked the distribution of the forms *egli* and *gli* in the *Vita* (23 and 82 occurrences, respectively) in the LIZ digital concordance, which was not available in 1986. In the lofty style of the *Vita*, *gli* (which in colloquial Florentine and in Machiavelli's own plays can also be a subject pronoun, along with *egli* / *e*) is the definite article (*gli amici*, *gli altri di casa...*) or the oblique case of the pronoun (*gli parse*, *gli trovò disposti*, *gli ridusse...*), with no exceptions.

I also doubt whether any early sixteenth-century copyist, confronted with a plain sentence such as "Era ancora mirabile", would have felt compelled to introduce into the text the unfashionable Latinism *etiam*, which, however, was normal in the prose of Machiavelli the "bureaucrat" ("Potrebbesi etiam dire di Castruccio come era mirabile").

Finally, if we go through the critical apparatus of the Brakkee edition, we shall find a number of confirmatory readings (→ 3.1) of BL, which cannot all be polygenetic, and which are consistent with the stemma I have just illustrated above. Notably, in the points of variation listed below, the ancestor of L and B, g, omits a number of monosyllables (73, 79, 87, 88, 95, 101, 128, 148), which in one case (128) are stylistically and semantically important, or modernizes archaizing constructions dear to Machiavelli and preserved in P, such as the agreement of the adverb in 84 ("mali contenti") or the type, still frequent in the *Istorie fiorentine*, "a casa Giano della Bella" (49, 152, 163):

- 49 in casa di Castruccio *g* (in casa Castruccio P)
 73 che si afortificava *g* (che e' si a. P)
 73 che poteva *g* (che e' poteva P)
 79 che si facevano *g* (che e' si facevano P)
 84 malcontenti *g* (mali contenti P)
 87 che non dovessi *g* (che e' non dovessi P)
 88 di riportarne la victoria *g* (di portarne la v. P)
 88 benché si confidassi *g* (benché ei si c. P)
 95 che si fosse insignorito *g* (ch'ei si fosse i. P)
 101 perché sapeva *g* (perché ei s. P)
 128 montare il primo a cavallo et ultimo scendere *g* (...et l'ultimo
 scenderne P)
 148 si truovono molte cose *g* (si t. di molte cose P)
 152 in casa di una meretrice *g* (in casa una m. P)
 163 in casa d'uno *g* (in casa uno P).

In sum, there is no reason to dispute the Brakkee stemma.

In the case of Dante's *Commedia*, too, our identification of the rather elusive archetype (we should remember that the poem was completed in 1321 and the earliest dated witness of which we have a surviving copy dates to 1330-31) so far rests on a single certain error common to the whole tradition, recently detected by Giorgio Inglese (*rocco* for *crocco* in *Purgatorio* XXIV 30). To this we can also add some twenty cases of weakly conjunctive errors which are nevertheless attested in all families of the tradition, with the exception of a few witnesses transmitting correct readings which must be considered as subsequent conjectural emendations. I should immediately caution the reader that, unlike the rather solid data available on the formation of the groups standing beneath the archetype (a good point of departure is provided by Barbi's 396 *loci*: → 9.1), these are often polygenetic innovations.

A rather typical example of the available proofs, or rather clues, regarding the archetype of the poem is *Paradiso* XXIX 100 (a high-diffraction passage included by Barbi in his canon, in spite of the fact that one of the variants is an exchange between *t* and *tr*, that is, *t* with a superimposed ~, a typically polygenetic paleographical exchange, very common in medieval transmissions in general, and in that of the *Commedia* in particular). The original reading, persuasively reconstructed by Vandelli

in 1902 (Vandelli, "Varianti...", 108) and reproduced by all subsequent editors, does not lend itself to doubt. According to Dante, those who argue that there was a solar eclipse during Christ's Passion lie:

Un dice che la luna si ritorse
nella passion di Cristo e s'interpose
per che 'l lume del sol giù non si porse,
e *mente*, ché la luce si nascose
da sé [...] (ll. 97-101)

[A preacher says that the moon changed place during Christ's Passion and blocked the sun's light, and he is a liar, because the light vanished by itself]

As Bruno Nardi observed and the best commentaries confirm, in this passage Dante echoes, sometimes literally, the *Historia scholastica*, that is, the Biblical commentary by the French theologian Petrus Comestor [Peter the Devourer (of books)], respectfully remembered in *Paradiso* XII 134: "Non fuit eclipsis solis, ut *quidam mentiti sunt* quod luna e regione fere est ad solem".

A sample-based collation of the whole ms. tradition confirms the broad dissemination and prevalence of the mistaken reading "e *mentre* che la luce si nascose" [and while the light vanished]. The reading is also found in the earliest commentaries of Dante, including Lana and the Ottimo.

The Cento group, the Vatican group and the codices of Boccaccio, that is, a large portion of the Florentine vulgate text, graft another certain innovation, "e *mentre* che *la luna* si nascose", a repetition of "la luna" (Germ. *Perseverationsfehler*, It. *errore di ripetizione*) in l. 97, onto the primary error *mentre che* for *mente che*. In this branch of the transmission (possibly in the subgroup that adds Buti's commentary to Dante's text) one also finds a popular, but arbitrary, attempt at correction, that is, a scribal conjecture ("et l'altro" or "et altri [sc. dice] che la luna si nascose"). The scribal conjecture (which some scholars call a "critical error") is reproduced in several late codices, but also in Bembo's Aldine edition, and was thence passed on to the later printed tradition (from Daniello to Foscolo, Witte, etc.).

The error *mentre* was corrected to *mente*, apparently by collation rather than conjecture, as early as the second half of the fourteenth century by the acute commentator Benvenuto da Imola ("nec dicas *mentre*, sicut textus multi habent"). Still, the correct reading *mente* rears its head here and there in the preserved manuscripts, often in the form of a revision of the text (for example, in La₂ Laur. 40.13₂ Marc. Zan. 51₂ etc.).

¶ The reader should be informed that revised readings always need to be accurately distinguished—for examples, in the cases just cited, with a subscript number—from the earlier textual stratum, the *scriptura prior*. Nevertheless, some textual critics (e.g., Petrocchi in his edition of the *Commedia*) do not distinguish between the different textual layers of an individual manuscript, i.e., the copy the first layer derives from, the one the second layer derives from, etc. This has devastating consequences for the stemma, that is, the genealogy of the witnesses.

More precisely, out of 600 manuscripts, only about 20 (not even 5%), have *mente*. These manuscripts are not closely related; on the contrary, they largely belong to different zones of the stemma, with the exception of a few codices that include Benvenuto's commentary, which I will distinguish with an *: Ang. 1102, Ashb. App. 3, Borg. 338, Can. 108 Cas. 251 Clar Eg Fior. II 1 43₂ La₂ Laur. 40.2 Laur. 40.4 Laur. 40.13₂ Laur. 40.20 Marc. IX 488 *Marc. IX 692 Marc. Zan. 51₂ *Par. 77 Ph Quer. A II 1 Ricc. 1033" Ricc. 1094 Trev.₂ Triv. 1046₂ V. Now, it is unparsimious to think that more than 500 mss. misread, each independently of the others, the right reading in their exemplars, *mente*, writing *mentre* instead. It seems likelier that the mistake, although easily committed, was made only once at the higher levels of the tradition, and more precisely in the archetype. As to the constellation of witnesses with the correct reading, its randomness suggests that they are the result of a combination of conjectural emendation and extra-stemmatic contamination. In fact, the strong interest of fourteenth-century readers in the text of the *Commedia* often led them to consult different copies, some of which are lost, and study available commentaries; we know this from the alternative readings written along the margins of many manuscripts.

More generally, Varvaro's observations about stemmatic hypotheses combining errors with poor conjunctive force and contradictory clues also apply to the *Commedia*:

The weakly conjunctive error is intrinsically polygenetic, so that in theory both the short and the long series [sc. 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 [sc. 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 (ID., *Identità*, 589-590; my emphasis).

In other words, even if almost none of the innovations that suggest the existence of an archetype for the *Commedia* is incontrovertible by itself, those discussed so far, taken together (the most recent list can be found in NP, 678-680), provide a reasonable, if not definitive, proof, at least in my opinion.

More generally, once the genealogy of witnesses beneath the vertex of the stemma has been scrupulously reconstructed (with its primary branches, its subfamilies, etc.), on the basis of deductions like those just discussed regarding the *Vita di Castruccio* and the *Commedia* we can also reasonably trace back to the archetype indicative errors which are missing from variable clusters of a few witnesses in the lower ranks of the stemma. Therefore, following in the wake of Maas's considerations quoted in the epigraph to the present chapter, and taking into account Varvaro's observations about "long series" quoted above, maybe it is time to reformulate the definition of archetype proposed above (→ 1.5), and repeated at the beginning of this section, in less abstract terms. Provided that the primary splits in the stemma are solidly reconstructed, I would propose the following:

The archetype is a lost copy marred by at least one conjunctive error (guaranteed by the agreement of the majority of the primary branches), from which the whole surviving tradition derives.

It goes without saying that problems of this kind (which of course can also be encountered at other splits in the stemma) are among the most difficult an editor is confronted with relative to the substance of a text.

Bibliographical notes. On *Saint Alexis*, see PARIS, and CONTINI, 99-134. On the *Vita di Castruccio*: Niccolò Machiavelli, *La vita di Castruccio Castracani da Lucca*, edizione critica di R. Brakkee, introduzione e commento di P. Trovato, Napoli, Liguori, 1986, 45-82; Niccolò Machiavelli, *La vita di Castruccio Castracani*, a cura di C. Varotti, in N. M., *Opere storiche*, a cura di A. Montevercchi e C. Varotti, Roma, Salerno Ed., 2010, XXI-XL. For several examples of editorial corrections introduced in the *editio princeps* B: Niccolò Machiavelli, *De principatibus*, testo critico a cura di G. Inglese, Roma, Istituto storico italiano per il Medio Evo, 1994, 142-146.

On Dante: G. Vandelli, "Varianti della edizione Alinari della 'Divina Commedia' in confronto con la edizione del Witte", in *Per il testo della "Divina*

Commedia", a cura di R. Abardo, con un saggio introduttivo di F. Mazzoni, Firenze, Le Lettere, 1988, 103-109; P. Trovato, "Fuori dall'antica vulgata. Nuove prospettive sulla tradizione della *Commedia*", in NP, 678-680; G. Inglese, "‘Crocco’ in *Purgatorio* XXIV 30?", *Filologia italiana* 6, 2009, 75-79.

On the subject, again, of relationships between witnesses, Jean Fourquet hypothesizes that in a tradition constituted by 3 mss. (L M N) a few slight innovations common to M and N may elude the philologist, who would draw a three-branched stemma from his or her classification of the manuscripts. He comes to the somewhat hasty conclusion that the “existence d'un stemma trifide ne peut jamais être démontrée” (“Fautes communes ou innovations communes?”, *Romania* 70, 1948-1949, 85-95: 86-89). It seems wiser to me to take into account this *exemplum fictum*—which is certainly possible, but not among the most frequent (only 3 manuscripts, 2 of which with barely detectable innovations going back to a common ancestor—among the difficulties that can arise when drawing up a genealogical tree.

3.8. Cases in which the genealogical method cannot be applied unless with special adaptations (short texts, texts with multiple versions, etc.)

If the MSS. are bad, with many manifest errors and many latent errors, one can draw a reliable stemma and use it to choose among the many latent errors. If, on the other hand, the MSS. are good, with few manifest errors, then no stemma can be drawn, but generally such good MSS. also have few latent errors and then the edited text will be comparatively reliable, even when the traditional method [i.e. Bédier's method] is used.

EKLUND, 13

Many manuscript traditions, particularly in the case of Greek and vernacular literature, have such a complicated and confused tradition, due to contamination or the existence of multiple archetypes or versions, that it is impossible to construct a stemma that works, or indeed a stemma at all [...]. But with the majority of classical Latin texts I think that a stemma can usually be a valuable instrument.

Leighton D. Reynolds, "Experiences of an Editor of Classical Latin Texts...", 7

In Timpanaro's words:

If the passage from Curtius [Georg Curtius, 1858-1862] that I have just quoted still displays an unshaken faith in the genealogical method, in

the last decades of the nineteenth century such a faith began to falter among both textual critics and linguists. In textual criticism cases of the perfectly successful application of the genealogical method were not lacking (and they have not been lacking even in times nearer our own) [...]. All the same, little by little scholars came to realize that the method achieved full success only in a relatively limited number of cases. All the manuscript traditions that were “too simple” (those represented by only one or two witnesses) remained outside its range, as did all those that were “too complicated” (those in which the copyists not only transcribed but also collated or conjectured so much than the kinship relations among the manuscripts were obscured): TIMPANARO (MOST), 123 [=TIMPANARO, 93-94].

Another case when it is hard to find sufficient errors to classify the tradition is when a text transmitted by several witnesses is very short, as, for example, in the case of a sonnet, which comprises merely 14 verses. In these cases, the less accomplished editors end up clutching at straws: they treat polygenetic variants as significant errors, or overhastily declare that the common-error method does not work. Actually, short texts are often handed down in more or less large miscellanies. The sequence of texts in these miscellanies usually depends on the taste or wealth of the copyist or the client and the books available. If several witnesses reproduce a more or less long series of short texts by the same author or several authors in the order *f a c b d e* (where the letters of the alphabet simply indicate a non-obvious sequence of texts), this characteristic sequence can legitimately be regarded as a special type of conjunctive innovation, and the collection of texts as a unit from a genealogical standpoint. As a consequence, if, for example, witnesses A, B and C show certain conjunctive errors in texts *f, a* and *d*, we shall be entitled—until otherwise proven—to regard these clues as also applying to texts *c, b*, and *e*, where no common errors are observable. (Of course, this criterion should be used with caution. It cannot apply to authorial collections, such as Petrarch’s *Canzoniere*, or to highly probable or obvious sequences of texts. In such cases, the normal order counts as an agreement in correct readings, i.e., it is of no consequence).

Even some Italian scholars have argued that the genealogical method cannot be successfully applied to literary works of a popular kind, such as many French *chansons de geste* or Italian *cantari* (chivalric poems). Their arguments partially resemble those of the great Spanish student of

juglaresca and *tradicional* poetry, Ramón Menéndez Pidal (1869-1968), which theorists of *mouvance* and enemies of authoriality are currently elaborating upon. The most important consideration here is that in this kind of tradition each version of a work deserves to be studied and edited separately. When the often itinerant minstrel makes the texts his own by transcribing them, he is inclined to modify them, expanding them and adapting them to the horizon of expectation of his public.

All this is beyond doubt. For example, a famous Florentine author of *cantari*, Antonio Pucci (d. 1388), whose second job was minstrelsy (he was the official town crier), used to “sign” the works he declaimed in the final verses, with formulas such as “al uostro onore | Antonio Puci ne fu dicitore” [to your honor | Antonio Puci was its teller] or “Antonio in rima ’l volse” [Antonio put it in rhyme]. A northern minstrel named Domenico modified the last verse to his own advantage (“Domenego rimar el volse” [Domenico put it in rhyme]), while a less brazen bard contented himself with removing the name of the by then no longer fashionable (or forgotten) author by modifying the rhymes of a final couplet as follows: “Al vostro onor detta è la canzon mia” [to your honor my song was sung], rhyming with *via*.

However, with the exception of such intentional variants, many of the variants found in *cantari* can be analyzed in terms of the usual phenomenology of the copy: they are normal transmission variants. Even Domenico De Robertis, whose authority is invoked by many editors as an alibi for producing what are little more than mechanical reproductions of this or that other witness, actually put forward a very different argument, where he distanced himself from the Bédierist tradition:

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 [sc. 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 (De Robertis, “Problemi di metodo...”, 124-125; my emphasis).

De Robertis also specified that even among *cantari* one finds a range of very different cases. For some, classification “is a way to embrace the panorama of tradition, but is not translatable into *recensio*”, whereas for others the tradition is “rather close-knit” (among the latter is the *Cantare di Piramo e Tisbe*, for which this great scholar, who recently passed away, hypothesizes a two-branched stemma with a securely identifiable archetype).

By writing that in some cases a classification based on the common-error method “is a way to embrace the panorama of tradition, but is not translatable into *recensio*”, De Robertis simply intended to say that, even when it is not possible to produce a Neo-Lachmannian edition of different versions (for example, because one is twice as long as the others, and in the added parts the eponymous hero takes a second seat to “local” heroes), a classification of common textual sections (the only ones where comparison is possible) will often reveal which of the competing versions is more likely to be closest to the original, and which furthest removed from it. This approach offers rational solutions to complicated historico-cultural *querelles* about the evolution of certain poetic “cycles”.

One tradition that at first sight appears to elude attempts at rationalization is that of the *Nencia da Barberino*. The text has been transmitted in “a handful of copies of very different length, order, character, and content: the 20-octave version in cod. Ashburnham 419 of the Laurenziana (= A) [...]; the 39-octave one in ms. Patetta 375 of the Vaticana (= P) [...]; a third, limited to 12 octaves, but where it is said that its anti-graph contained 37 more, in the cod. Conventi Soppressi B 7 2889 in the National Library in Florence [...] (= M); and finally the *editio princeps* printed in Florence (?) around 1490-92, in 51 stanzas (= V)” (as in the scrupulously conducted edition of the *Opere* by Lorenzo il Magnifico, by Tiziano Zanato, 159-160).

The question of the relationship between these different versions has often been raised. Drawing on the Neo-Lachmannian analyses and solutions of earlier students of the *Nencia* (applicable, of course, only to the octaves present in all versions), Zanato has identified the 20-octave version as the original, and attributed it to the young Lorenzo (1468?).

The identification by textual scholars of the version closest to the original has extremely important implications for literary history, too. The 20-octave version is the only one where the stanzas have a logical order, with a proemial octave, preludes to the protagonists’ falling in love, etc. The longer versions (which derive from vulgate text *y*, created by imitators who were not always very gifted) should thus be understood as early evidence of the immense popularity of *Nencia*, which “became the archetype of a genre of rustic poetry destined [...] to find practitioners well over a century after its birth, at least as late as the eighteenth century” (Lorenzo il Magnifico, *Opere...*, 159).

In the case of the *Spagna in rima* discussed above (one of the most popular poems on the adventures of Charlemagne's paladins in Spain), scholars again oscillate between two opposite interpretations. Giovanni Palumbo clearly sums up the question:

The poem has been handed down in seven manuscripts, all of the fifteenth century, and three incunabula, as well as a number of later printed books. These witnesses preserve two different versions. A long version in 40 cantos, known as *Spagna maggiore*, can be read in ms. P (Paris, BnF, Italien 567, the basis for Michele Catalano's well-known edition) and, with some perturbations, ms. L (Firenze, Biblioteca Medicea Laurenziana, Plut. 90 inf. 39), as well as in printed versions N (an unicum, without its opening and final leaves, and thus *sine notis*, preserved in Naples) and M (printed in Venice "for Bartholomio de Zani" in 1488). A shorter version, the so called *Spagna minore*, in 34 cantos, is fully preserved in ms. F and partially in ms. G in Como, which is missing its last part. The other manuscript witnesses have a mixed version, variously combining elements of the two main versions. This is also true of incunabulum B (printed in Bologna by "Ugo de Rugerij" in 1487). The main difference between the two main versions of *Spagna* lies especially in the treatment of two topical episodes in Charlemagne's Spanish campaign: the combat between Roland and Ferragut, to which the major version devotes four cantos (henceforth: C4) versus two in the minor version (= C2); and the defeat at Roncesvalles, narrated in eight cantos in the shorter version (= R8) vs. twelve in the major version (= R12). The main body of the poem—that is, cantos i 1-iv 2 and vii 20-xxviii 39 of the Catalano edition, corresponding in F to cantos i 1-iv 2; v 35-xxvi—remains, however, essentially stable (= S24).

The problems posed by the ordering and the reciprocal relationships of these two versions have spawned a vast and famous critical debate. I will only mention that Pio Rajna and Michele Catalano regarded the 40-canto version, the Florentine *Spagna maggiore*, as the original one, whereas Carlo Dionisotti staunchly argued for the priority of the minor, 34-canto version, preserved by ms. F. The latter hypothesis is the usually accepted one (Palumbo, "Spagna ferrarese...", 150).

The two editors of the *Spagna Ferrarese* (that is, the text contained in ms. F, copied for Borso d'Este) have tried to solve the problem by taking a Neo-Lachmannian approach, that is, by drawing up a stemma of the whole tradition. The stemma proposed by Valentina Gritti and Cristina Monta-

gnani suggests that *Spagna* did not originate in Tuscany, but in northern Italy, and that it was later Tuscanized and expanded. Since their classification is not based on the full text of the poem, which is very long, but only on a selection of *loci critici*, it has not put the question to rest. Still, it is beyond doubt that the “cultural” problem (Tuscany or northern Italy?) can only be addressed *after* having solved the philological problem of the shape of the stemma, that is the problem of the relationship between surviving witnesses.

Bibliographical notes. The second epigraph is from L.D. Reynolds, “Experiences of an Editor of Classical Latin Texts”, *Revue d’Histoire des Textes* 30, 2000, 1-15.

As I have already noted, we owe the first consistent presentation of a number of cases in which stemmatics alone is insufficient to solve the problems of a text to PASQUALI (→ I.5). EKLUND and REEVE, *Manuscripts and methods* offer important considerations on the stemmatic method and the limits of its application.

On *canzonieri* and such: M. Barbi, *Studi sul canzoniere di Dante*, Firenze, Sansoni, 1915 (reprint 1965); AVALLE, 89; CHIESA, 101; M.D. Reeve, “Dionysius the Periegete in miscellanies”, in *Il codice miscellaneo. Tipologie e funzioni*. Atti del Convegno Internazionale, Cassino, 14-17 maggio 2003, a cura di E. Crisci e O. Pecere, Cassino, Università degli studi, 2004, 365-378; P. Divizia, “Appunti di stemmatica comparata”, *Studi e problemi di critica testuale* 78, 2009, 29-48 (but I find the observations in the note on p. 42 somewhat specious: a theoretical model does not need to reproduce *all* the features of the phenomenon it intends to analyze); T. Zanato, “Per una filologia del macrotesto: alcuni esempi e qualche spunto”, in SPCT2, 47-72; M. Zaccarello, *Alcune questioni di metodo nella critica dei testi volgari*, Verona, Fiorini, 2012, 150ff.

On *cantari*, see D. De Robertis, “Problemi di metodo nell’edizione dei cantari”, in SPCT, 119-138; on Pucci’s works, → 4.3. On *Nencia*, see Lorenzo de’ Medici, *Opere*, a cura di T. Zanato, Torino, Einaudi, 1992, 155-174. On *Spagna*, see *Spagna Ferrarese*, ed. a cura di V. Gritti and C. Montagnani, Novara, Centro di Studi Matteo Maria Boiardo-Interlinea, 2009 (“Opere di Matteo Maria Boiardo. La biblioteca di Boiardo”, 1); G. Palumbo, “*Spagna ferrarese e Spagna in rima. A proposito di un’edizione recente*” and id., “Postilla”, *Medioevo romanzo* 35, 2011, 150-172, 177-178; C. Montagnani, “Risposta”, *Medioevo romanzo* 35, 2011, 173-176.

The case of medieval Latin hagiographical texts, too, seems similar to the previous entry. Paolo Chiesa has observed that “the variation between one witness and another has appeared, in the end, less prevalent than one would expect; [...] very often we find the keys to formulate convincing, or at least plausible, genealogical hypotheses” (P. Chiesa “Riflessione metodologica (non del tutto in margine)”, in *Le passioni dei martiri aquileiesi e istriani*, vol. II, a cura di E. Colombi, Roma, Istituto Storico Italiano per il Medio Evo, 2013, 127-132: 128 note).

As Andrew Peacock points out (“The Mediaeval Manuscript Tradition of Bal’amī’s Version of al-Ṭabarī’s History”, in PFEIFFER-KROPP, *Theoretical Approaches*, 93-105: 95), younger philological traditions tend to overestimate the presumed completeness of certain versions, but “this method of classifying the manuscripts is wholly inadequate”, because significant macro-variants (chapters or episodes or additional or missing groups of verses) can easily migrate, by contamination, from one zone to another of a tradition. It is safer to rely only on significant errors, at least in the first stage of classification.

3.9. Cases in which the genealogical method should not be applied (authorial variants)

As the reader will remember, in the passage quoted in the epigraph of the previous section, Reynolds mentions, among several complications that hinder the creation of a stemma, “the existence of multiple archetypes or versions”. If, after some time has elapsed, an author decides to modify a text to reflect new compositional ideas, new stylistic ideals, or changed grammatical criteria, the editors will have to distinguish clearly between the different versions A, B, C, etc. That is because, while variants introduced by copyists (transmission variants) constitute arbitrary textual layers that need to be identified and removed wherever possible, the editor’s failure to recognize authorial variants, or rather different authorial versions of the same text, results in a hybrid text that does not reflect in the least the plans and intentions of its author (→ 3. Appendix 1). On the contrary, the *critica delle varianti* [critique of variants] or *filologia d’autore* [authorial philology], that is, the study of the different versions and of changes made to a text by the author him- or herself over time, is a formidable way of entering a writer’s workshop and understanding how he or she worked, and what his or her aesthetic ideals were.

Above all, especially in the absence of incontrovertible evidence, such as the existence of two autograph drafts, the editors must have an uncommon familiarity with an author’s language and style in order to be able of applying the theoretically very clear distinction between (random) transmission variants and (intentional) authorial variants. The task is such that even consummate scholars can be drawn into error. Here is a single example, regarding the Latin writings of a well-known Humanist, Lorenzo Valla:

One of Valla’s most famous works, *De falso credita et ementita Constantini donatione*, has had the good fortune of being published in a good edition by W[olfram] Setz in 1976. Here the editor’s approach has mainly been that of an historian [...]. But the initial philological approach is valid and holds promise of a systematic editorial procedure, unlike previous edi-

tions. The merit of the Setz edition lies in its complete *recensio* of witnesses, finally approached with rigor. He catalogues, with sufficiently ample descriptions, 24 surviving manuscripts, a lost one, three copies with excerpts, and a sizable series of printed editions, including some of remarkable ideological density [...]. Setz carries out at least a partial collation, which leads him to some results: first of all, he identifies some errors unifying the whole tradition, then he singles out three groups of manuscripts on the basis of conjunctive errors (Regoliosi, "Per una nuova edizione...", 267-268).

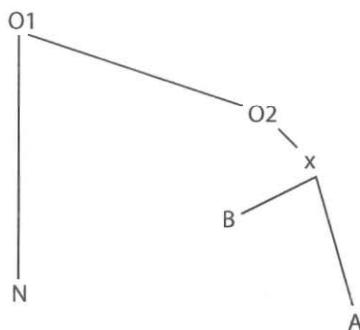
Setz, however,

without creating any stemma and thus without any "objective" criterion for the evaluation of the relationships between the three groups and of the different codices within the groups themselves, picks one manuscript per group [sc., Vat. Lat. 5314 (A), Vat. Ottob. Lat. 2075 (B) and Naples, National Library, VII D 25 (N)] and draws exclusively on these, alternating the readings of one or other without philologically founded motivation, and furthermore reports readings from the first printed editions, which are certainly important in a historical sense, but redundant from an editorial standpoint. Thus, while Setz's edition offers a good text, and at any rate a better one than the previous editions, his incorrect method involves inevitable errors or limitations (Regoliosi, "Per una nuova edizione...", 268).

Checking the apparatus of variants, Regoliosi recognized that AB introduces in the N text a type of correction typical of Valla, namely, the systematic replacing of *suus* with the more correct *eius / ipsius*, or its suppression (64.4, 81.22, 82.7, 87.15 etc.). The same opposition between witnesses (N vs AB) is observable in some classical citations ("ms. N and its group are characterized by three omissions, at 118.2-3, where a quotation from Pliny the Elder is missing, 146.18, where a parenthetical statement is omitted, and 149-150, where a long quotation from Varro is skipped": Regoliosi, "Per una nuova edizione...", 270).

Granted that Valla "systematically corrected his works over the years, notably as regards his use of possessives, of which he gradually became more aware, eventually writing *De reciprocatione sui et suus*" in 1449, the grammatical errors found in N indicate that this is an "early version, revised and corrected in the second version, attested by A B, to which the addition of passages from classical authors should also be ascribed" (Regoliosi, "Per una nuova edizione...", 270).

The conclusion is that what Setz saw as 3 families are actually 2 authorial versions. The schema is:



¶ It is needless to say that O1 stands for Original 1 and O2 for Original 2, that is, a version of O1 revised and corrected by the author himself.

The obvious consequences of the above stemma, that is, of the identification of two authorial versions of the *De falso credita et ementita Constantini donatione*, are that:

1. The new critical editor will have to decide preliminarily if he or she intends to publish the earlier version, O1 (Naples, 1440), or the more recent one, O2, probably dating from 1443-44, when Valla, “wishing to return to Rome, circulated the work among friends and men of the Curia, accompanying it with apologetic letters” (Regoliosi, “Per una nuova edizione...”, 270).
2. The agreement of AB against N (which would be worth 66% in the three-branched stemma proposed by Setz) could transmit, at least in some cases, not an authorial variation by Valla, but a fortuitous innovation by the copyist of x.
3. On the other hand, an agreement between codices of different versions (N + A or N + B) could be a good argument in defense of a reading (isolated readings in A or B will be, as a rule, arbitrary innovations by their respective scribes).

Bibliographical notes. On the *De donatione*: Lorenzo Valla, *De falso credita et ementita Constantini donatione*, herausgegeben von W. Setz, Weimar, Böhlau, 1976 (“Monumenta Germaniae Historica. Quellen zur Geistesgeschichte des Mittelalters”, 10); M. Regoliosi, “Tradizione e redazioni nel ‘De falso credita et ementita Constantini donatione’ di Lorenzo Valla”, in *Studi in memoria di Paola Medioli Masotti*, a cura di F. Magnani, Napoli, Loffredo, 1995, 39-46; M. Regoliosi, “Per una nuova edizione del ‘De

falso credita et ementita Constantini donatione,” in *Pubblicare il Valla*, a cura di ead., Firenze, Polistampa, 2008 (“Edizione Nazionale delle opere di Lorenzo Valla. Strumenti”, 1), 267-275.

On authorial variants in the Italian philological tradition: A. Balduino, *Manuale di filologia italiana*, Firenze, Sansoni, 1989³, 366-410 (Chap. “Varianti d’autore”); STUSSI, 147-246 (Chap. “Filologia d’autore”); *La filologia dei testi d’autore*, Atti del Seminario di Studi (Università degli Studi Roma Tre, 3-4 ottobre 2007), a cura di S. Brambilla e M. Fiorilla, Firenze, Cesati, 2009; P. Italia, G. Raboni, *Che cos’è la filologia d’autore*, Roma, Carocci, 2010; P. Italia, *Editing Novecento*, Roma, Salerno ed., 2013.

An especially delicate case of simultaneous occurrence of authorial variants and transmission variants is that of the *De pictura* (vernacular version) by Leon Battista Alberti, very competently addressed by L. Bertolini (Firenze, Polistampa, 2011).

3.10. More on the function of textual criticism

Se convendrá conmigo en que tanto el autor como el lector se merecen el mejor texto posible y en que el filólogo tiene la obligación social, moral y personal de intentar aproximársele con todos los instrumentos y los medios a su alcance. El primer deber de una edición es ser una edición: poner el texto en limpio, no calcarlo con borrones, lardones y cagadas de mosca

Francisco Rico, *El texto del “Quijote”...*, 46

At a time when a growing number of editors believe philology to be roughly synonymous with an uncritically imitative reproduction of a given manuscript, we should possibly ask again, brutally or naively (whichever adjective we prefer): Why do we need philology? The most common answer to this question, of which I will give some examples, with no ambition to be exhaustive, insists on the rigor of the process:

La critique des textes a pour but de retrouver, autant que possible, la forme que l’ouvrage auquel elle s’applique avait en sortant des mains de l’auteur. Ce but, elle ne l’atteint jamais complètement: elle s’en rapproche plus ou moins suivant que les conditions où elle s’exerce sont plus ou moins favorable [The aim of textual criticism is always to reconstitute so far as possible the form in which the work left the author’s hands. This aim is never completely attainable, and the degree of attainability varies] (PARIS, 8. English translation from REEVE, *Manuscripts and methods*, 61).

The manuscripts we possess derive from the originals through an unknown number of intermediate copies, and are consequently of questionable trustworthiness. The business of textual criticism is to produce a text as close as possible to the original (*constitutio textus*: MAAS [FLOWER], § 1).

The concern of textual criticism will be to recognize the phenomenon of textual variants and thereby reconstruct the original readings of the text (Heinrich Lausberg, *Romanische Sprachwissenschaft...*, I, 18).

The fundamental purpose of textual criticism or ecclitics is the publication of ancient and modern texts according to rigorously scientific criteria (AVALLE, 21).

Textual criticism provides rational procedures for advancing the most likely hypothesis about what the original was like and how, in rough outline, it was transmitted down to its preserved witnesses (STUSSI, 116).

Textual criticism is the discipline that... investigates the genesis and evolution of a work... studying its transformations in the course of time. Its objective is to publish a “reliable” text of a given work (CHIESA, 11).

But another requisite of textual criticism, which I personally regard as indispensable, pointed out by Barbi and Vandelli as early as the very beginning of the twentieth century, is set forth, for example, in Armando Balduino’s fine manual:

The editor, however, must *make the work readable to the modern reader*, and therefore must introduce punctuation... (Balduino, *Manuale di filologia italiana...*, 9; my emphasis).

This so to speak social aspect of the activity of a critical editor, addressed to a broader public, is reformulated in linguistic terms by Contini:

Any edition [...] of an autograph (or its equivalent) reproduces what is of interest and omits, intentionally or spontaneously, what is not of interest. In substance it is *the translation or adaptation of a historically delimited system into another system* (CONTINI, 15-16 [= ID., *Frammenti*, I, 14]; my emphasis),

and by others, such as Bourgoin and Vielliard:

Mais la *transposition du système du ou des témoins [...] dans notre propre système* est indispensable pour que le lecteur contemporain puisse percevoir aisément le texte” (BOURGAIN-VIELLIARD, 29; my emphasis),

and is stated especially clearly in Renzi and Andreose's manual:

Textual criticism aims to provide an edition of an ancient text that is accessible to the modern reader and, at the same time, conforms to the intention of its author (Renzi-Andreose, *Manuale di linguistica e filologia romanza...*, 268).

The eminent Arabist Wadād al-Qādī is also concerned with communication with the larger public:

The primary goal of editing is to receive a communication from the past. This communication [...] should be as accurate as possible [...] It should also be as accessible as possible to its target audience [...]. In this approach the scholar-editor's critical judgement is constantly called upon to balance the accuracy of the text with its accessibility to a different audience in a different time and place (Wadād al-Qādī, "How 'Sacred' is the Text of an Arabic Medieval Manuscript...?", 34-35).

In a very recent study, Lino Leonardi takes a similar stance. Philologists should provide

a text which, while meeting the requirements of science and elucidating the manuscript tradition, is not merely accessible to specialists, but recovers a work of the past for a contemporary public to read; one that does not reproduce a document, but interprets the tradition as a whole as a means to transfer its textual reality into something that is readable today. (LEONARDI, "Il testo come ipotesi", 6).

As a consequence, if our idea of textual criticism is purely and uncritically reproductive, we can do whatever we like. If instead—even disregarding the many possible distinctions when it comes to texts and editions—our idea of philology also incorporates services to be provided for the contemporary reader, an edition is essentially an approximation, an attempt to translate a text from a remote sign system to another that is more comprehensible for current readers, and, at the same time, free that text from as many defects in transmission as possible. In the second case, however, we have a yardstick for distinguishing between progress and regression, significant discoveries and superstitious beliefs.

It is a well-known fact that the so-called “New Philology” argues for the need for an edition “true to its model” (one that does not superimpose any modern convention on the materiality of the medieval artifact) and often derides traditional terminology and techniques (“archetype” seems to have become a bad word, and apparatuses of variants are eyed with suspicion). This has led to a growing diffusion of “imitative” or “diplomatic” editions, which reproduce, with arbitrary “improvements”, an arbitrarily chosen manuscript and “in fact abjure all interpretive requirements of the edited text” (ID., “Il testo come ipotesi”, 10 note 17).

The success of editions of this kind, which are rampant in Romance philology (possibly because they are easy to produce and not such as to require a high level of competence?), has prompted Leonardi to observe that “if we repeated Bédier’s statistics about editions of medieval texts—and, I dare say, not only of French ones—, we would discover that this is the formula that has become most widespread in Romance editing” over the last few decades (ID., “Il testo come ipotesi”, 10). There are no doubts, Leonardi observes,

about what the results of such a check would be, for example, in the case of four of the most popular and prolific series of early French editions, from the traditional “Classiques français du Moyen Âge” to the “Textes littéraires français” and the more recent “Lettres gothiques” and “Champion Classiques. Moyen Âge”. Whereas Bédier was confronted with a “forest of trees, all two-branched”, here we would be in the center of a plain with a boundless base (ID., “Il testo come ipotesi”, 10 note 17; my emphasis).

Leonardi discusses some glaring editing errors, and goes into a closely argued and very persuasive critique of the current vogue in the editing of early French prose romances. This tendency is actually beginning to make headway in Italian philology, too, as exemplified, for example, by a recent “uncritical” edition of Petrarch’s *Canzoniere*. This edition, remarkably, retains some non-pertinent traits of ms. Vaticano Lat. 3195, a partially autograph ms. recognized as the most authoritative witness of this famous collection of poems ever since the fifteenth-century Valdezocco edition. It has been reproduced photographically, more than once and in good quality editions, and has also been published in a scrupulous diplomatic edition.

Now if, for example, an editor flaunted as a merit of his or her “critical” edition the fact that it reproduces the absence of accents or apostro-

phes of a fourteenth-century copy in the name of respect for the “scribal version”, it would be easy to object that the disadvantages of such an approach far outweigh its advantages. Accents and apostrophes were just about unknown in the medieval West. They were first authoritatively introduced in Italian and European printing houses by the 1501 Aldine edition of Petrarch, edited by Pietro Bembo. A text without diacritics of this kind would tire a contemporary reader, who should be able to “percevoir aisément le texte” [easily perceive the text], and would not make up for this by providing any significant additional information about the author’s intention or the style of the text itself.

The same considerations apply to the practice (which some have extolled as a major scholarly advance) of beginning all the verses in Petrarch’s rhymes with an upper-case initial. This was actually common practice for the classics as early as Carolingian times, and was prevalent in European printing practices from the sixteenth through eighteenth centuries. It was still common in nineteenth-century poetry, including that of Foscolo, Leopardi and company, Gaston Paris’s *Saint Alexis* (1872), Moore’s *Divine Comedy* (1889), and the edition of Petrarch with commentary by Carducci and Ferrari (1899). The practice is still found in the twentieth century, at least occasionally (in Italy alone, for example, in Segre’s *Roland*, in Contini’s *Fiore* and *Detto*, and in the *Poeti del Cinquecento* edited by Gorni, Danzi and Longhi for the Ricciardi publishing house). The system is actually neither especially original nor especially useful. It is redundant as a means to signal the beginning of a verse, as this is already indicated by the beginning of a new line. Besides, it neutralizes the distinction between proper nouns, or words beginning a sentence, and all the other words.

Nevertheless, sometimes preserving graphic features that may seem unnecessary (e.g., most of the etymological or superfluous *h*-s frequently found in medieval and Renaissance mss., which are no longer used in contemporary Italian) becomes essential, or at least useful for a correct perception of the text. This is the case, for example, when the author uses acrostics or similar devices. In his *Viaggio d’Oltremare* (ca. 1348), the Franciscan friar Niccolò da Poggibonsi arranges the sequence of the initials of some consecutive chapters, beginning from the second and then again from the thirteenth, so as to reveal his identity. Thus, if the modern editor suppresses an etymological *h* at the beginning of chapter LXXXIII, modernizing *Hor* into *Or*, there will be an error in the second acrostic (formed from the initials of chapters XIII-LXXXVI). That is why in the available editions, which are also unreliable in other respects, the acrostic

yields the unsatisfactory TOSCOANA. The result is fully acceptable, however, if the editor maintains the spellings found in the manuscripts: FRATE NICOLA DI CORBICO DA POGI BONICI DEL CONTADO DI FIORENCA DE LA PROUINCIA DI TOSCHANA.

Bibliographical notes. The epigraph by Francisco Rico is drawn from his *El texto del “Quijote”. Preliminares a una edcótica del siglo de oro*, Valladolid, Centro para la Edición de los Clásicos Españoles, 2005.

The quotations of Lausberg, Balduino etc. are (respectively) from H. Lausberg, *Romanische Sprachwissenschaft*, Berlin, de Gruyter, 1969, 2 vols.; A. Balduino, *Manuale di filologia italiana*, Firenze, Sansoni, 1989³ (I ed. 1979); L. Renzi, A. Andreose, *Manuale di linguistica e filologia romanza*, Bologna, il Mulino, 2003; Wadād al-Qāḍī, “How ‘Sacred’ is the Text of an Arabic Medieval Manuscript? The Complex Choices of the Editor-Scholar”, in PFEIFFER-KROPP, *Theoretical Approaches*, 13-53.

The quotation of CONTINI, 15-19 [= ID., *Frammenti*, I, 14] is from a section entitled “Edizione interpretativa” [interpretative edition], but his observations are all the more applicable to critical editions.

Among typical examples of “imitative” editions, LEONARDI, “Il testo come ipotesi”, mentions E. Moore Willingham (ed.), ‘*La Mort le Roi Artu’* (*The Death of Arthur*) from the Old French ‘*Lancelot*’ of Yale 229, with Essays, Glossaries and Notes to the Text, Turnhout, Brepols, 2007 (rev. by L. Leonardi, *Medioevo Romanzo* 33, 2009, 437-440) and A. Overbeck, *Literarische Scripta in Ostfrankreich. Edition und sprachliche Analyse einer französischen Handschrift des Reiseberichts von Marco Polo* (Stockholm, Kungliga Biblioteket, Cod. Holm. M 304), Trier, Kliomedia, 2003 (rev. by S. Dörr, *Vox Romana* 66, 2007, 327-330). But the list could go on and on.

Before the spread of the so-called New Philology, the custom of reproducing common and non-pertinent scribal traits of mss. was regarded, and in my opinion rightly, as typical of fledgling scholars. Many such shortcomings can be found in the self-proclaimed “critical” edition of Petrarch’s *Canzoniere* by Giuseppe Savoca (Firenze, Olschki, 2008), reviewed by P. Trovato, “Su una recente edizione acritica del canzoniere di Petrarca”, *Filologia italiana* 7, 2010, 41-56.

On the spread of “Aldine” use of the accent and apostrophe in sixteenth-century typography, see P. Trovato, “Serie di caratteri, formato e sistemi di interpunzione nella stampa dei testi in volgare (1501-1550)”, in *Storia e teoria dell’interpunzione*, Atti del Convegno Internazionale di Studi, Firenze 19-21 maggio 1988, a cura di E. Cresti, N. Maraschio, L. Toschi, Roma, Bulzoni, 1992, 89-110 (= Paolo Trovato, *L’ordine dei tipografi. Lettori, stampatori, correttori tra Quattro e Cinquecento*, Roma, Bulzoni, 1998, 197-216), and, more generally, *Storia della punteggiatura in Europa*, edited by B. Mortara Garavelli, Roma-Bari, Laterza, 2008.

Marco Giola is preparing an edition of the *Viaggio* by Poggibonsi.

Appendix 1. The debate on authorial intention seen from afar

Peter Shillingsburg clearly summarizes the North American debate on the issue of authorial intention:

The concept of the author's final intentions is ancient, though its name and influence blossomed in the mid-twentieth century under the CEAA [i.e. Center for Editions of American Authors]. Nevertheless, it is a concept most editors have been uncomfortable with and that they find easier to use in practice than to defend in argument. It has furthermore been the central issue on which the Greg/Bowers school of editing has been attacked both by structuralist and poststructuralist critics and by Jerome McGann's social contract theory of works of literary art (SHILLINGSBURG, *Scholarly Editing*, 29).

This is a very interesting debate, but my impression is that some simple reformulations and some distinctions between different situations could partially bridge the rift between these different positions, which is magnified by the not always acceptable assumption that all editors believe "that normally the end product of composition can be and should be *one* text that best represents the work of art" (SHILLINGSBURG, *Scholarly Editing*, 13; my emphasis). The thought that many editors may know that a text could be transmitted, for example, in different authorial versions, does not seem to cross the mind of believers in this tenet.

To begin with, the typical situation of textual critics working on American authors is relatively simple: they have access to one or more printed editions made during the author's lifetime and, sometimes, the manuscripts or typescripts those editions were based on. In this case, the first thing to do is to ascertain whether the manuscript and the printed editions transmit the same version of the text—allowing for almost inevitable divergences in accidentals, often due to editors or typesetters. If the answer is affirmative, the editor will be able to choose without too many worries whether to prefer the manuscript (to highlight the individual peculiarities of the author) or this or that printed edition (shifting the focus, instead, to "historical" significance). A good apparatus of variants (preferably a positive one) will allow the more expert and interested readers to check which formal variants (i.e. accidentals) are found in each of the two (or three) witnesses and possibly to gain a sense of the peculiarities of each.

If the editor decides to use a printed edition for his or her text, cross-checking it with the manuscript will allow typos, if any, to be emended and indicated in the apparatus. And vice versa. More generally, the divergences between the readings in the text and those in the apparatus will allow the reader to gauge in the most rigorous possible way the impact of the so-called “social contract” with respect to the original text.

Of course, two or more different versions may well be authorial ones, approved by the author at different times (for example, the *Orlando Furioso* of 1516, that of 1521, and that of 1532; Chateaubriand’s three versions of his *Atala*; or Whitman’s many versions of *Leaves of Grass*). In this case, we would not be entitled to create a “texte unique et monstueux” (Bédier) by anti-historically merging the different variants. We would have to publish them separately—possibly in a digital text database—or decide which version is more urgently to be made available to the community of readers (→ 3.9), because in the case of multiple authorial versions it is not a matter of subjectively deciding which one is the “text that 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 texts and contrastively study their evolution.

* * *

Secondly, when dealing with great literary personalities—Cicero, Virgil, Dante, etc.—who wrote before the invention of print, even those who are very close to approaches such as that of the so-called “Death of the Author”, or Jerome McGann’s theories, should recognize that it is a primary interest of both textual critics and readers to have access to texts established in a non-superficial way, on the basis of tried and trusted criteria. Of course, in late antique, medieval or early modern manuscript traditions, a number of agents of textual change other than the author can be identified—copyists, correctors, rewriters, epitomators—who were active at different historical times. Nevertheless,

a radical skepticism, ruling out a priori any question regarding the original form of the text, its author, or the stages of its transmission and gradual change, threatens to warp our perspective. Indeed, the reception of a work—aiming at the interaction it created with its public at certain historical times, and thus the efficacy and productivity it had in specific milieus—can best be assessed and appreciated in comparative terms. Knowing

which text of the *Commedia* was being read in Florence in 1420 can be useful; but it is even more useful to know in what ways this text was different from that being read, at the same time, in Bologna or Milan, or from the one Dante had originally written (CHIESA, 134-135).

Now the only way to determine non-generically how copyists and re-writers changed a given text is to draw up a rigorous genealogical classification of all available witnesses, create a stemma, and date and place each editorial action, at least approximately.

As early as the Italian Renaissance, some writers tended to take control of the formal aspects of the printing of their work. In any case, major publishing companies had their own editors, just as today. Possibly, the most striking case of a takeover of a text by a series of editor-readers is that of Castiglione's work *Il Cortigiano* (Venezia, Aldo, 1528), whose definitive version was "prepared" for printing by the Venetian scholar Giovan Francesco Valier, who corrected the spelling and phonomorphology of the printer's copy (which is in Florence, Biblioteca Medicea Laurenziana, Ashburnham 409), regularized punctuation, and marked with a period to be followed by some blank spaces the transition from one subject to another (we owe the most important studies on this subject to Ghinassi and Quondam).

At any rate, McGann's notion that "works of literary art" are "actual" only when they are conveyed by means of the printed medium implies that some of the most outstanding and popular books ever written in the Western canon, from Homer to Virgil to Dante to Chaucer, had only a virtual or larval existence in the centuries that preceded the invention of print.

Furthermore, the burden of proving that any "historical" edition of Virgil or Dante—that is, a more or less justly famous late witness—is more useful for readers than an edition founded on a comparison of witnesses, with a view to reconstructing a text as close as possible to that written by the author, should rest with those who believe they can prove that most of the *Aeneid* is not by Virgil, or most of the *Commedia* is not by Dante, and not with those who seriously devote themselves to the study of the tradition of these texts.

Another objection to the criterion of authorial intention was advanced, again, by McGann on the subject of incomplete works or works lacking their author's last touch. My impression, however, is that even in these cases the criteria currently employed for texts reflecting the author's intention are applicable, with only slight reformulation. I am thinking of elementary definitions such as:

The editor should strive to reconstruct, insofar as this is possible, the state the text was in when, *for any reason* (loss of interest, death, or other circumstances beyond the author's control), *the author ceased to work on it*.

I will add that Dante's corpus of works alone includes two works that, though unfinished, are of the greatest cultural importance, namely, the Latin treatise *De vulgari eloquentia* and the encyclopedic work *Convivio*, both of which have been published in an exemplary way, although, for obvious reasons, no one believes that these editions have recovered 100% of the unfinished originals.

Bibliographical notes. Recent Anglo-American editorial theory and practice. A good anthology entitled *Anglo-American Scholarly Editing, 1980-2005*, with an introduction by P. Eggert and P. Shillingsburg, is available as a special issue of *Ecdotica* 6, 2009, 11-425.

How to deal with several authorial versions. Just to give a couple of examples, the second chapter of AVALLE's manual is divided into two parts: "Fenomenologia dell'originale" [phenomenology of the original] and "Fenomenologia della copia" [phenomenology of the copy], and STUSSI, too, draws a very clear distinction between texts preserved in several copies (Chapters 3 and 4) and "Filologia d'autore" [author philology] (Chapter 5). For further literature, → 3.9.

The theme of critical apparatuses is exhaustively discussed by A. Balduino, *Manuale di filologia italiana*, Firenze, Sansoni, 1989³, 233-247.

The quotation of Bédier is drawn from Jean Renart, *Le lai de l'ombre*, publié par J. Bédier, Paris, Firmin Didot, 1913, xxxviii ("Presque tous, sans même la considérer, se sont évertués à reconstituer O, 'l'archétype', souche réputée unique de toute la tradition manuscrite: risquant ainsi de procéder comme des éditeurs d'*Atala* qui combineraient en un texte unique et monstrueux les variantes de trois éditions, O₁, O₂, O₃, procurées toutes trois par Chateaubriand").

Authorial intention and social contract theory. In the Italian Renaissance, leading authors adapted the linguistic matter of their texts to the linguistic and stylistic precepts advocated by theorists such as Bembo. The influence of the social contract, principally on linguistic standardization, is observable especially in revisions of works by deceased fourteenth- or fifteenth-century

authors, or writers of modest education, such as the nevertheless brilliant Pietro Aretino. Some still useful indications on this vast subject can be found in P. Trovato, “*Con ogni diligenza corretto.*” *La stampa e le revisioni editoriali dei testi letterari italiani, 1470-1570*, Bologna, il Mulino, 1991 (repr. Ferrara. Unifypress, 2009); B. Richardson, *Print Culture in Renaissance Italy. The Editor and the Vernacular Text*, Cambridge, Cambridge UP, 1994.

On the tradition of Castiglione’s *Cortegiano*, after consulting Ghino Ghinassi’s works, it is inevitable to turn to A. Quondam, “*Questo povero Cortegiano*”. *Castiglione, il Libro, la Storia*, Roma, Bulzoni, 2000. Studies that provide interesting insights regarding the opposition between scholarly and popular editions of the work can be found in the forum “Forme e sostanze. ‘Il Cortegiano’ di Amedeo Quondam”, *Ecdotica* 1, 2004, 157-209. On the section-ending signs introduced by revisers in the printer’s copy, one can now read V. Guarna, “Per una nuova paragrafatura del ‘Libro del Cortegiano,’” *Filologia italiana* 10, 2013, 107-147.

Appendix 2. “Composite”, “hybrid”, “eclectic” texts. On the so-called “eclecticism” of reconstructive editions

The choice is necessarily a matter for editorial judgement, and an editor who declines or is unable to exercise his judgement and falls back on some arbitrary canon, such as the authority of the copy-text, is in fact abdicating his editorial function. Yet this is what has been frequently commended as ‘scientific’—‘streng wissenschaftlich’ in the prevalent idiom.

W.W. Greg, “The Rationale of Copy-Text...”, 28-29

In the conclusion to his most celebrated article, Bédier allows himself a rhetorical question:

Est-il légitime qu'il [sc. un éditeur] établisse le texte d'un ouvrage d'après un classement des manuscrits que lui-même estimerait seulement “acceptable”, logiquement “satisfaisant”? Oui, si ce classement l'invite à imprimer tel quel l'un des manuscrits conservés [...], non, si ce classement lui enjoint de construire un texte *composite* (BÉDIER, 69; my emphasis).

Actually, in his edition of the *Lai de l'ombre* of 1913 he had already cautioned that anyone who uses a stemma will inevitably produce “un texte étrangement *composite et bigarré*” (Jean Renart, *Le lai de l'ombre...*, XXXII; my emphasis).

Later editors of the *Lai de l'ombre* took this observation by the master almost literally. The examples that follow will suffice:

Given the greater dangers I believe are involved in a *composite* text, and also because of the occasional intrinsic interest in an individual manuscript of a work with multiple versions, I have chosen to use the ‘best manuscript’ approach and follow ms. E whenever possible (Winters, “Introduction”, in Jean Renart, *Le Lai de l’ombre...*, 21; my emphasis).

The text was edited twice [*recte*: 3 times, adding Bédier’s 1890 edition in the nineteenth century, by Francisque Michel and Achille Jubinal [...]. These editions seek an ‘authentic’ text, one which is made up from all extant manuscripts. The reconstruction of a *hybrid* text was no longer in fashion when Joseph Bédier published his edition of 1913. He attempted to conserve as much and correct as little as possible, a principle generally adopted by scholars today (Tudor, “Introduction”, in Jehan Renart, *Le lai de l’ombre...*, 7; my emphasis).

But other examples of this approach are also easily found in editions of other texts. In other words, starting at least as early as 1928, scholars began to contrast the “eclecticism” of reconstructive textual critics with the historical truth of a given witness. Ever since, words like “composite”, “hybrid” and “eclectic” have often been brandished as polemical weapons to discredit the methods of genealogical criticism, accused of pursuing an illusory objective, the *authentic* text, the one that is closest to the *original*. Of course, critics of eclecticism never distinguish between the naïve and sometimes rash reconstructionism of late nineteenth-century philologists and the much more rigorous, cautious and painstaking approach of the best reconstructive critics of the last fifty years.

Actually, as I observed above (→ 2.2), Bédier’s accusations had the very positive effect of forcing genealogical critics to rethink their editorial practices and stimulating an intense methodological debate. The presumed eclecticism of genealogically reconstructed texts is a result of the application of probability calculus (wherever the majority principle can be applied) and depends (in the case of an open *recensio* or of emendation) on explicit arguments, which other scholars can falsify.

As early as 1938, after admitting that Bédier’s warnings were warranted by the “inexperience, unawareness, and superficiality with which many set about such a difficult and delicate task” as textual criticism (*La nuova filologia...*, XX), Michele Barbi, on the strength, among other things, of his experience as editor of the *Vita Nuova*, denied general validity to the great Romanist’s objections, observing that:

It is not a matter of treating as certain what is only probable, or forcing everyone to accept our conclusions; but when one has illustrated to the scholars the state of things and the reasons for and against a given conclusion, has one not done what science allows and demands? Isn't it more arbitrary to choose a single text and put our full trust in it in every case, except where it proves, with manifest errors, that it, too, although to a lesser degree, represents a corrupt tradition [...]? Why should we give up considering each case for itself, and why should we not be allowed to substitute our cautious and reasoned judgment for that of a transcriber whose judgment [...] we do not see the reasons for and cannot measure the extent of? (*La nuova filologia...*, XXII-XXIII).

Other scholars argue along the same lines, e.g., Timpanaro:

Every time more than a copyist transcribes a model, “eclecticism” is objectively created, inasmuch as they make different mistakes in different parts of the text, with rare exceptions. To this random and irrational eclecticism we must oppose our choice, which is based on rational argument and therefore is not eclectic in the pejorative sense (TIMPANARO [MOST], 159 note [= TIMPANARO, 131 note]).

Whitehead and Pickford also stress the distinction between the irrational eclecticism of early witnesses and the eclecticism of scholars, which is supported—at least in the best cases—by scientific procedures and arguments:

Trust in the readings of a given manuscript is no more justifiable than trust in the inerrancy of a critical edition. Moreover, it is exaggerated to represent a manuscript as preserving a state of the text that has enjoyed an authentic existence, since what we are dealing with is in most cases something quite as composite as any reconstituted critical text produced by a scholar in the quiet of his study. A manuscript generally presents us with several strata of alteration and behind many readings there is a complex history of change (WHITEHEAD-PICKFORD, “Introduction to the *Lai de l’Ombre*”, 153).

Even in France, that is, in the epicenter of the beneficial earthquake caused by Bédier, a different and, in my opinion, more correct meaning of “eclectic” as applied to textual criticism has been gaining ground. In this new sense, “eclectic” is synonymous with “arbitrary”, “not scientifically proven”, “contradicting the stemma”. In their manual, for example, Bourgoin and Vielliard observe:

Si on établit un stemma, il faut effectivement s’en servir. Il est affligeant de voir des éditeurs consacrer des centaines de pages à établir un stemma

pour ne pas s'en servir pour l'établissement du texte, et choisir selon des principes “éclectiques” (BOURGAIN-VIELLIARD, 52).

Dominique Poirel goes even so far as to distinguish, in my opinion quite correctly, between “édition éclectique” (defined as the worse choice) and “édition stemmatique” (Poirel, “L'édition des textes médiolatins...”, 163, 166).

Not too differently, referring to printed transmissions, Fredson Bowers, one of the hallowed masters of Anglo-American Textual Bibliography, underlines the rationality implicit in the procedures theorized by Greg and historicizes McKerrow's diffidence toward eclecticism, cautioning that “the physical document thus reconstructed never existed, but that is of no account whatever”:

Greg's concept of the eclectic text limited by bibliographically generated principles has proved to be the great contribution of this century to textual criticism. McKerrow's conservatism was a reaction against the unprincipled impressionism of nineteenth-century editing, which had given a bad name to eclecticism. The most important result of Greg's theory of copy-text and of the methodology that flows from it is that it made *eclecticism* respectable when governed by scholarly principles that can be defined and can be applied to a number of different situations in a manner to meet with general approval (Fredson Bowers, “Multiple Authority...”, 459).

A significant number of textual critics studying Anglo-American literature seem to think along similar lines. In an essay of 1983—cited, among others, by BOURGAIN-VIELLIARD, 25, Thomas Tanselle cautions that, because of the changes made by the editor, a critical edition will be different (*viz.*, *must* be different) from all existing witnesses.

Shillingsburg impeccably comments on the wonderful misprint in a typescript by Faulkner, *Go Down, Moses* (“...the valley *rese,bled* a river”), which resulted in flaws and arbitrary corrections in all “historical” editions, and which should instead be corrected conjecturally, with a minimum expenditure of effort, to *resembled*, especially in the light of the fact that the /m/ and /,/ keys are adjacent on English typewriters:

We can imagine the process by which “rese,bled” [...] became “rose, bled”. The typesetter set “rese, bled a river” with a space after the comma; someone corrected this to “rose, bled”; and that is what every reader has thought Faulkner wrote (SHILLINGSBURG, *From Gutenberg to Google*, 180).

He then adds:

It is not what Faulkner wrote. That is a fact. And it is not what Faulkner meant to write. That is a well-informed conjecture. No textual artifact from Faulkner's lifetime has what Faulkner intended to write. And that is so frequently the case in the textual histories of Anglo-American works, that scholarly editors have developed an attitude toward surviving documents that influence what they believe to be their purpose [...]. Anglo-American editors conclude that the only way to produce a text of unmixed authority is to proceed *eclectically*, selecting the readings that represent one authority [...] rather than accepting the mixed results of the hasty commercial demands that produced the now extant historical documents (SHILLINGSBURG, *From Gutenberg to Google*, 180, 184)

Now I find that a great European conservative (I am referring, of course, to his approach to early French texts), the Neo-Bédierist Félix Lecoy, did not mean anything substantially different when he wrote:

Aucune copie, sauf exceptions notables (car il en existe tout de même de fort bonnes), n'est exempte de fautes, parfois grossières, mais aussi souvent plus subtiles. C'est là que peut, que doit s'exercer la sagacité de l'éditeur, qui est pris entre la nécessité de respecter son modèle et son désir de fournir au lecteur un texte lisible et cohérent (LECOY, "Variations", 469; my emphasis).

In sum, words like “eclectic” or “hybrid” may be brandished as weapons, but if so they are blunt ones.

Bibliographical notes. The epigraph is from W. W. Greg, “The Rationale of Copy-Text”, *Studies in Bibliography* 3, 1950-1951, 19-36.

Jean Renart, *Le lai de l'ombre*, publié par J. Bédier, Paris, Firmin Didot, 1913; Jean Renart, *Le lai de l'ombre*, Edited from Ms E [B. N. Nouv. Acq. Fr. 1104] by M.E. Winters, Birmingham, Alabama, Summa Publications Inc., 1986; Jehan Renart, *Le lai de l'ombre*, Translation and Introduction by A. Tudor, Text Edited by A. Hindley and B.J. Levy, Liverpool, University of Liverpool, Department of French, 2004 (“Liverpool Online Series: Critical Editions of French Texts”, 8); M. Barbi, *La nuova filologia e l'edizione dei nostri scrittori. Da Dante al Manzoni*, Firenze, Sansoni, 1938; D. Poirel, “L'édition des textes médiolatins”, in DUVAL, *Pratiques philologiques en Europe*, 151-173; F. Bowers, “Multiple Authority: New Problems and Concepts of Copy Texts”, in *Essays in Bibliography, Text and Editing*, Charlottesville, The University Press of Virginia, 1975, 447-487: 459.

On Tanselle's important contributions to textual criticism, see most recently N. Harris, “Riflettendo su letteratura e manufatti: profilo di George Thomas Tanselle”, *Ecdotica* 1, 2004, 82-115

PATTERSON, “The Logic of Textual Criticism”, 93 and note, proposes a distinction between the different meanings of “eclecticism”.

4. HIGHS AND LOWS OF COMPUTER-ASSISTED STEMMATICS

4.1. Eulogy of the PC and the current limits of computer-assisted textual scholarship

Il importe de bien délimiter le rôle [...] de la machine électronique dans la critique des textes: l'équivoque, sur ce point, serait désastreuse [...]. Totalement inintelligente, la machine ne fait que ce qu'on lui dit de faire, sans rien de moins ni rien de plus.

FROGER, 217

The quantity does not matter: what one may require from any computerized method is that it must be efficient and fast, whatever the quantity of data [...]. The data should be encoded in such a way that they may later be used in any possible way without being encoded again.

Marc Dubuisson, Caroline Macé, *Handling a Large Manuscript Tradition...*, 27

Because electronic publishing is incunabular, energetic, and exciting, it is surrounded by hype, exaggeration, ignorance, and skepticism. Fantastic and disastrous projects have taken over equipment and energy worthy of better causes. The Gutenberg Project, for example, well on its way to produce 100,000 free electronic texts by the year 2000, occupies the time of scores of persons [...] but is the product of abysmal ignorance of the textual condition. Its texts are unreliable, for they are insufficiently proofread, inadequately marked for font and formatting, and they come from who knows where, their sources unrecorded. Its perpetrators apparently believe that any copy of a given title adequately represents the work.

SHILLINGSBURG, *Scholarly Editing*, 161

Many works by computer-assisted philologists like to begin with the statement that traditional scholars do not have a sufficient understanding of computers and are hence incapable of realizing the most important innovations introduced by computer editions and the like. I am not sure whether the premise justifies such a drastic conclusion, but I have no problem admitting that, even though I have been using PCs for about thirty years, my computer skills are modest. I will therefore diligently comply with this cliché and avoid touching on issues that are somehow constitutive of digital philology, such as the rapid obsolescence of electronic editions (connected in its turn to the obsolescence and continuous replacing of one operative system by another) or the undeclared instability of online or DVD editions, which can be and sometimes are tacitly modified by the author-editor or by others after publication, even in some very important aspects. Instead, I will try to assess the strengths and weaknesses that are inherent in the present philosophical uses of this new tool, regardless of possible technical difficulties.

¶ On the correlation between computer obsolescence, readability of files, and increase of the costs of allowing enduring access to a text: G. Taylor, "Editoria", *Ecdotica* 7, 2010, 92-105: 98-99. See also D. Schmidt, "The vicious life-cycle of the digital scholarly edition", may 28, 2013 (blog: available at: <http://multiversinodocos.blogspot.it/2013/05/the-vicious-life-cycle-of-digital.html>)

It is beyond doubt that the PC, in all its applications, has already become, and may become more and more, a powerful ally of textual criticism, as well as of all the other human activities concerned with writing and reading. I am old enough to remember that until the late 1980s, whenever I wanted to correct a simple typo in the proofs of my first books, the typesetters had to set the whole print line all over again, often thereby introducing new typos. If it is true—and true it is—that every act of copying introduces new errors, other conditions being equal, a collation or list of variants produced by a PC *must* be a lot more correct than a typescript copied several times over by the author or his or her collaborators as the original project was modified and defined, and hence reset several times by the typesetters. As Peter Robinson explains, not without a dose of exaggeration: "With computer collation, you have to do one thing only: simply transcribe each witness, as accurately as possible, and the computer will do the rest" (ROBINSON, "Computer-Assisted Stemmatic Analysis", 73).

Not to mention the rarity of concordances that used to plague "old" philologists, with few exceptions. Concordances (invented, it seems, in 1230 by the Dominican friar Hugues de Saint-Cher for the purpose of

helping students of the Scriptures) are a very useful tool for assessing possible emendations to be introduced into a text. When, in the Seventies of the last century, I was a student of Italian literature at the University of Venice, there existed concordances of Italian texts only for Dante and Petrarch (usually drawn up by early-twentieth-century scholars). New and more complete concordances for Petrarch, and concordances for Boccaccio, Leopardi, and other authors, were compiled precisely during those years.

I am happy to say that the potential usefulness of the computer for textual scholarship was first glimpsed by a great Italian expert on St. Thomas Aquinas, Father Roberto Busa (Vicenza, 1913-Gallarate, 2011), in 1949, when computers were only used for computing. Today there are plenty of software programs that allow us to generate with ease concordances for any text we feed in, or search for isolated words or long strings of text in large databases of texts by a single author or several.

¶ Today, even the Search function on our PCs can be used as a crude, but infallible, concordance. In 1949 things were different. So Father Busa contacted Thomas Watson Sr., CEO of IBM, and persuaded him to help him with his research. In a few decades, he thus managed to put together the *Index Thomisticus*, a monumental lemmatization of the *opera omnia* of Thomas Aquinas and the texts most closely connected with him.

Similar projects include, for Italian vernacular texts, LIZ and TLIO, and the very recent BIZ, which are indispensable tools for anyone who studies the Italian Middle Ages or Renaissance; for France, FRANTEXT or the text databank of the *Dictionnaire du Moyen Français (1330-1500)*, Atilf; for Spain, CORDE; for the Dutch Middle Ages, the CD-ROM *Middelnederlands: Woordenboek en teksten*; and so on.

Another important advantage of the use of PCs in textual criticism is the low cost of digital data archiving (as I explained above, I will not discuss the “technical” issue of the costs involved in guaranteeing enduring access to digital editions) compared to the relatively high production and storage costs of the paper books. When dealing with long and largely rewritten texts such as French *chansons de geste* or compilations of chivalric romances in prose, or, in Italy, chivalric poems (*cantari*), the (sometimes chimerical) electronic edition of all the witnesses, possibly supplemented with digital images of all the pages of the manuscripts, is certainly a very appropriate solution for making all that is needed for the study of these texts available to the community of scholars, and facilitating comparisons between similar passages in different versions.

As scholars such as Ludo Jongen or Paolo Chiesa have observed, however, these undeniable advantages only hold for readers who have a solid philological background:

Les éditions synoptiques ne sont utiles qu'aux philologues chevronnés, capables en un clin d'œil de repérer les variantes. Pour les historiens de la littérature et les étudiants, ces éditions sont un vrai désastre: outre les difficultés qui accompagnent la lecture d'une édition diplomatique, ils doivent choisir eux-mêmes la leçon qui pourrait être "originale" (L. Jongen, "L'édition de textes médiévaux néerlandais aux Pays-Bas", in DUVAL, *Pratiques philologiques en Europe*, 55-76: 69).

[The reader] who lacks the time or tools to cope with this multiplicity, and simply wants to read and study the work, is also entitled to receive indications about which text he reasonably should read [...]. This is thus a responsibility we need to take on ourselves, probably today more than before, since today more than before it is possible to dodge this responsibility, thanks to the fact that multiple editions are easier to produce (P. Chiesa, "Non-neutralità dell'editore...", 293).

Furthermore, as Shillingsburg observes:

Without this foundation of collection and archiving, editors could not claim to be historical. *This archiving activity is not, however, editing. It is collecting, describing, cataloguing, and indexing* (SHILLINGSBURG, *From Gutenberg to Google*, 171; my emphasis).

Most importantly:

Linking in any electronic hypertext is accomplished by someone noticing something and creating a link or by some program identifying boolean similarities and constructing links automatically [...]. In order for there to be a link, someone had to think it would be a good idea first [...]. This is not necessarily bad. I'm not sure that I think there is an alternative to it. But it does raise the question, does it not, about whether the editorial work has been objective? Isn't that, after all, the argument for putting all the material forward with as little critical judgment as possible? *Is it not the case that every editorial job, even the electronic archive, is critical in nature?* (SHILLINGSBURG, *From Gutenberg to Google*, 156; my emphasis).

At any rate, while textual criticism has a millenary history, our attempts to use the PC to facilitate its tasks have just begun. To obtain more sophisticated results in the classification of witnesses, it seems we shall require more collaboration from the more experienced philologists, who are best qualified to assess the consequences in textual terms of the decisions made by the software programmers.

¶ See, for example, Reeve's account ("Editing Classical Texts with a Computer") of Ghislaine Viré's computer-assisted edition of Hyginus. There is also a glaring contradiction between two statements contained in the "Prologue" to *Studies in Stemmatology*, ix and xv. On the one hand we read (but today, almost twenty years later, no positive evidence for this has been forthcoming) that "if the computer enables us to work with 'difficult' text traditions, the entire stemmatological process has to be redesigned, and philologists have to learn to relativise their own decisions". On the other, the authors frankly admit that "the fundamental basis of any stemmatological procedure will always be the interpretative, intersubjective criteria which philologists apply when collecting the building blocks of the stemma, the variants [*recte*: the indicative errors]. *If a philologist does not critically evaluate the status of his variants, the entire building is subject to doubt*" (my emphasis).

Even overlooking other debatable elements of computer-assisted philological studies produced so far, no computer today is able to tell us whether a particular reading—one, say, that occurs very frequently in the witnesses—is an error or a correct and presumably original reading, or whether it is linguistically typical of a certain area in France, Italy, or medieval England. Furthermore, without the contribution of an expert philologist, no program developed by biologists and adapted to the needs of textual critics can produce a stemma capable of serving the purpose for which the stemma was invented in the nineteenth century, viz., to identify the readings that are most likely to be the original ones among hundreds and hundreds of equally acceptable variants, and do so on the basis of a restricted series of manifest errors.

If a stemma constructed using the common-error method is correct, the majority principle offers a rational basis for choosing, with the exception of some problematic passages, between readings that *very probably* were in the original and readings that *very probably* were not (→ 1.4). Now we all know very well that when statistics say that everyone eats half a chicken a week there are actually some who never eat chicken and

others who eat two. In our case, every variant chosen on the basis of the majority principle is more likely to descend from the archetype than the remaining variants. However—paraphrasing TIMPANARO, 155 [= ID. (MOST), 181-182])—if we believed that the application of the majority principle *always* allows us to single out the original reading, we would have reduced to zero an event that will have occurred in a minority of cases, but will nevertheless almost certainly have occurred. Thus, since the majority principle is based on probabilistic logic, let us admit that we thereby manage to determine the most probably authentic reading with a certain margin of error, say, of 10 or 15%.

In other words, when we competently employ the most rigorous possible stemmata, it is realistic to believe that we are correct in the choice between two or more interchangeable readings only 85 or 90 times out of 100. But what happens if, as in the case of digital editions inspired by cladistics—founded on polygenetic variants, that is, on “noise” (Margot van Mulke and others), rather than on significant errors (→ 4.2ff.)—the stemma (sorry, the phylogram or cladogram) *is content with being not too different* from the stemma that expert genealogical critics would have established? Elementary, my dear Watson: the choice suggested by the stemma is now no longer rational, but merely random. If competing readings in each point of variation are not too numerous, and one of the competing readings is the right one (and this is not always true, as in the case of diffraction *in absentia*: → 3.2), it is reasonable to believe that the variant chosen for the text is the correct one in, say, 30 or 40 % of cases.

Bibliographical notes. The second epigraph is from M. Dubuisson, C. Macé, “Handling a Large Manuscript Tradition with a Computer”, in EoT, 25-37.

An overview of Digital Humanities, where textual criticism, however, is only mentioned in passing, is provided by *A Companion to Digital Humanities*, edited by S. Schreibman, R. Siemens, J. Unsworth, Oxford, Blackwell, 2004.

Among Busa’s many works, the following is especially notable for being ahead of its time: R. Busa, “Rapida e meccanica composizione e pubblicazione di indici e concordanze di parole mediante macchine elettroniche”, *Aevum* 25, 1951, 479-493. The *Index Thomisticus Sancti Thomae Aquinatis Operum Omnium. Indices et concordantiae*, Stuttgart, Frommann Holzboog, 1974-1980, 56 vols., provides a complete index of every occurrence of every single word used in the works of St. Thomas. The work was later offered as a CD-ROM, and later a DVD. An online version can be consulted at <http://www.corpusthomisticum.org/>.

Up-to-date lists of the advantages computers offer to textual critics are provided by GREETHAM, *Textual Scholarship*, 357-360, and by F. Stella, “A digital policy for editions of medieval texts”, forthcoming, but a still acceptable illustration of the strengths and limitations of computer-assisted textual criticism, is provided by FROGER, 217-266. On the same subject, see also SHILLINGSBURG, *Scholarly Editing* and ID., *From Gutenberg to Google*. For further considerations, see G.P. Maggioni, “Esperienze wellsiane nell’ecdotica. Illusioni, disillusioni, prospettive”, in *Digital philology and medieval texts*, ed. by A. Ciula, F. Stella, Pisa, Pacini, 2007, 13-24.

For a long list of databases of Romance texts, see G. Giannini, “Corpora et édition (d’un guide de Terre-Sainte)”, *Laboratorio critico* 2, 2012 (online). Other resources for Latin etc. are listed by Stella, “A digital policy...”

Paolo Chiesa’s essay cited above is “Non-neutralità dell’editore e storicità dell’edizione. Qualche riflessione sulle *Res gestae Saxonicae* di Widuchindo”, in FERRARI-BAMPI, *Storicità del testo*, 285-298. Reeve’s essay “Editing Classical Texts with a Computer” is in ID., *Manuscripts and Methods*, 361-393. For further observations about the current limits (and the sensationalism) of computer-assisted philology, see ID., *Manuscripts and Methods*, 344-347.

4.2. A brief history of computer-assisted stemmatics

Unfortunately, the Lachmannian selection of common errors was not clear and repeatable, which made it unscientific.

SALEMANS, *Building Stemmas*

Rejecting the method of common errors, scholars like W. W. Greg, Henri Quentin and Vincent Dearing proposed looking at all moments of variation, not just obvious errors, in a tradition, and using algebraic logic or statistics to infer relationships.

ROBINS, “Editing and Evolution”

As William Robins argues, the genealogical method

depended on editorial judgment about which readings were “errors” and which were not. This difficulty has always been the biggest practical obstacle for the Lachmannian method—different editors of the same tradition could disagree about even the most basic decisions. The identification of errors, even of apparently egregious ones, is a matter of competent, intuitive, and *ad hoc* deduction, and accordingly varies considerably from one critic to the next [...]. Statistical approaches came to dominate biological systematics, as species were compared according to the number of features they had in common, according to the procedures of “numerical taxonomy” and “phenetics” [...]. Similar approaches emerged in textual criticism, especially to deal with very complex traditions like the books of the Bible (ROBINS, “Editing and Evolution”, 92 and 101).

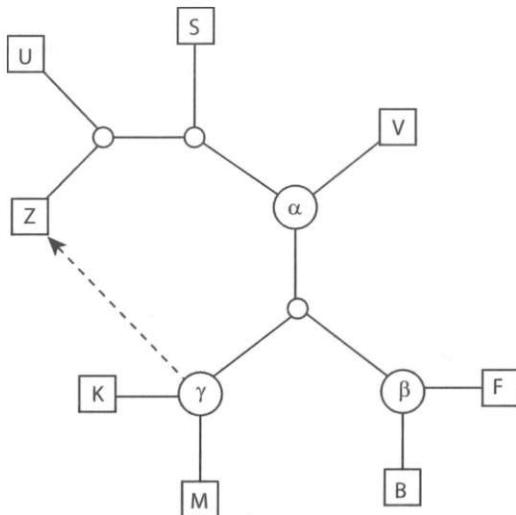
In fact, besides Bédier, other textual critics who flourished in the Twenties and Thirties of the last century, such as the Benedictine dom Henri Quentin, W.W. Greg, and Eugène Vinaver, criticized the common-error method. In 1926, dom Quentin (Saint-Thierry, 1872-Rome, 1935)—significantly, a Bible scholar, who had a huge manuscript tradition to deal with—developed a method of reconstruction of the original founded on the distribution of variants (not of errors), one, that is, where personal judgment had no influence. (As we know precisely personal judgment is essential in the common-error method, where one needs to distinguish between simple variants, polygenetic errors, and significant errors).

This approach was not too different from that of the great Shakespeare scholar W.W. Greg, who in 1927 published a book with a revealing title: *The Calculus of Variants: An Essay on Textual Criticism*. In this classic work, Greg suggests a series of mathematical rules for the construction of a stemma. Notably,

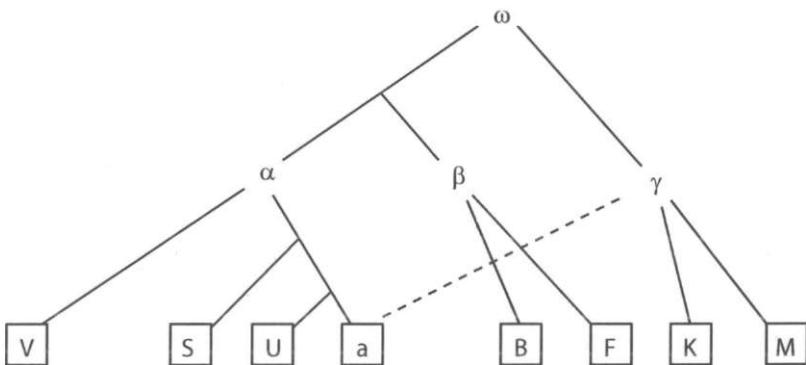
Greg claimed that only so-called type-2 variations give direct information about the shape (or chain) of a stemma: '[...] it will be apparent that it is only such variation as we see in type 2 that is fundamentally significant' (Greg 1927: 23). This type of variation occurs when text versions [i.e. copies, witnesses] show, at the same place in a text, precisely two competitive *variant readings* and when each variant reading (or variant) is represented in at least two text versions. Working with type-2 variations implies that four or more text versions are needed (SALEMANS, *Building Stemmas*, 11)

A conceptual mainstay of the method devised by dom Quentin is that the philologist should not immediately produce a complete stemma, that is, one with its vertex, the root of the tree, oriented in space. In the first phase, he or she should draw up a web of relations, a chain of witnesses (French *enchainement*). Only subsequently should that diagram be rooted by choosing a vertex, or root (French *orientation*). Only then will a stemma become scientifically effective, that is, a useful tool for choosing between variants (e.g.: FROGER, 78ff.; BOURGAIN-VIELLIARD, 52-53; MOTTA-ROBINS, LXXVII-LXXX etc.). The approach is different from that of "Lachmannians" (who are assumed to be, more or less by definition, anti-scientific, and would immediately try to pick out significant errors subjectively). In this alternative method, all variants are included in the classification and are weighted equally. As I have already noted, only after that the chain connecting the mss. has been constructed, will the philologist try to root the diagram.

Let us imagine that the structure reproduced below consists of a number of segments of wire, which are attached to the rings representing the splits, but free to rotate in any direction:



Once we have established, on the basis of further traditional-style investigation, the rooting of the chain, the structure will fall into place, as in a normal stemma. In the case under examination, among the many abstractly possible solutions, the stemma shown below is constructed assuming that the vertex, ω , should be placed between γ and the point that α and β go back to:



A curious and uninformed reader may ask what I mean exactly by “further traditional-style investigation”. Well, in order to turn the chain (which could be rooted in many different ways) into a selection tool, that is, a stemma under another name, traditional investigation consists precisely in the singling out of a small number of errors, usually redefined with synonyms to dissimulate the Neo-Lachmannian logic informing this part of the procedure.

¶ As is known (although some manifest limits of Quentin’s method were quickly pointed out by E.K. Rand, dom D. de Bruyne, dom John Chapman, Bédier, and Pasquali), some of the constituent elements of Quentin and Greg’s method, and especially their partial putting off of the distinction between errors and variants—certainly one of the most delicate phases of Neo-Lachmannian philological practice (→ 3.1)—to a later stage, witnessed a revival in the last decades of the last century, with the spread of the personal computer.

One of the first attempts to adapt Quentin’s method to the PC was made by another Benedictine, dom Jacques Froger, who in 1968 wrote an important manual, where he shows a sound knowledge of the history of traditional philology: *La critique des textes et son automatisation* (= FROGER). Greg’s theses, however, were further developed by an American textual scholar, the above-mentioned Vinton A. Dearing, first in his *Manual of Textual Analysis*, in 1959, then in *Principles and Practice of Textual Analysis*, of 1974 (= DEARING), and recently in *A Primer of Textual Geometry*, Xlibris Corp, 2006, which I have not yet gained access to.

Even more than the naïve reconstructive excesses of some genealogical philologists (excesses known only to the best informed Neo-Lachmannians), other factors created, in the last quarter of the last century, a decidedly favorable climate for the testing and spread of computer-assisted philological methods, inspired by Dearing and Froger’s pioneering works. These factors included: 1) the objective difficulty of a correct application of the common-error method, which requires long professional training (→ 6); 2) the mass spread of computers; 3) the converging vogues of the interest in scribal versions and the mistrust of authoriality; 4) the (prevalently North American) success of the above-mentioned pamphlet by Cerquiglini, a deconstructionist essay that suits typically, if not exclusively, American cultural orientations (New Medievalism, culture studies, gender studies, etc.), and has become the manifesto of the so-called New Philology; 5) the aspiration to a scientificity akin to that of biologists.

In particular, as we shall see more in detail in the following sections, during the last twenty years several computerized philologists have borrowed software (MacClade, PAUP etc.) and quantitative inquiry methods from cladistics, an important and relatively recent branch of evolutionary biology.

Bibliographical notes. The field is vast. I will therefore limit myself to a few select citations: dom H. Quentin, *Essais de critique textuelle (Ecdotique)*. Paris, A. Picard, 1926; W.W. Greg, *The Calculus of Variants. An Essay on Textual Criticism*. Oxford, Oxford UP, 1927; FROGER; PdO; DEARING; *Studies in stemmatology*; NOF; EoT; M.D. Reeve, “Shared Innovations, Dichotomies, and Evolution” (1998), now in id., *Manuscripts and Methods*, 55-103; ROBINS, “Editing and Evolution”.

For a brief discussion of dom Quentin’s positions, also taking account of contemporary reviews, see BIELER, 20-27; KENNEY, 134-138. As I mentioned above, some of the limits of dom Quentin’s methods were almost immediately pointed out by BÉDIER, 21-54. Cesare Segre provides an essential commentary on this discussion in his “Prolusione” to NOF, 11-17; 15-17.

Although frequent references to typescripts and punched cards evoke a technology today obsolete, many interesting articles can be found in PdO, which gathers the proceedings of an international conference of the CNRS held in Paris in 1978. Among the essays in this miscellany, I will cite, in order of appearance, at least those by Froger (who, remarkably, distances himself from certain aspects of dom Quentin’s method), Duplacy, Segre, Griffith, Dearing, Zarri, Kleinlogel, and the concluding round tables.

More or less during the same months when a renowned issue of *Speculum* celebrated, or rather looked forward to, the birth of a New Philology, the first fascicle of *Romance Philology* 45, 1991 (dedicated, more soberly, to textual criticism in the French, Italian, and Spanish traditions, with essays by Speer, Segre and Speroni, Blecua, Orduña, Faulhaber) contained an essay by Francisco Marcos Marín on computer-assisted philology.

In 1996, two Dutch students of Early French literature, Pieter van Reenen and Margot van Mulken, edited under the title *Studies in Stemmatology* a remarkable collection of papers presented in the course of the 1990s at *colloquia* held at the Vrije Universiteit in Amsterdam. The book, which opens with a “Prologue” by the two editors, is divided into three parts. Part 1 (“Methodological Approaches”) comprises the following studies: B.J. Salemans, “Cladistics or the Resurrection of the Method of Lachmann”; P.M.W. Robinson, “Computer-Assisted Stemmatic Analysis and ‘Best-Text’ Historical Editing”; E. Wattel and M. von Mulken, “Shock Waves in Text Traditions”; E. Wattel, “Clustering Stemmatological Trees”; E. Wattel and M. von Mulken, “Weighted Formal Support of a Pedigree”; A.D. van Reenen, “The Analysis of Early Islamic Traditions and Chains of Transmission”. Part 2 (“Subject-Related Observations”) comprises: J.B. Voorbij, “Medieval

Dossiers and Modern Stemmas”; W.R. Veder, “Textual Incompatibility and Many-Pronged Stemmatata”. Part III comprises “Applications to Manuscript Traditions”: K. Utheman, “Which Variants are useful in Discovering the Deep Structure of the Manuscript Tradition of a Text”; P. van Reenen and L. Schöslar, “From Variant to Pedigree in the *Charroi de Nîmes*”. The weakest part of the book is its uninformed and almost caricatural presentation of Maas’s genealogical method, attributed to Lachmann (e.g.: “Philologists usually restrict themselves to a brief résumé of all existing manuscripts [...]. In general, philologists do not believe that the outcome of stemmatological research will or can influence their choices of text edition”; “The [...] stemma is no longer seen [viz. by the editors] as an authoritative prescriptive scheme which an editor should blindly apply to his manuscript tradition”: vii, ix). The book is completed by a useful analytical index.

Ever since then, and ever more copiously, articles have appeared and conferences have been organized on the subject. For example, the Italian Accademia dei Lincei organized an international conference entitled *I nuovi orizzonti della filologia. Ecdotica, critica testuale, editoria scientifica e mezzi informatici elettronici*, held in Rome in May 1998, in whose proceedings (= NOF) essays by prominent genealogical philologists and paleographers like Bruno Gentili, Cesare Segre, Gian Battista Speroni are featured alongside essays by stemmatologists we have already met above, such as Robinson, Sallemans, and van Mulken. The *Revue d'histoire des textes* 31, 2001, contains three essays that are all remarkable, for different reasons: F. Woerther, H. Khonsari, “L’application des programmes de reconstruction phylogénique sur ordinateur à l’étude de la traduction manuscrite [*recte*: tradition manuscrite; evidently the titles of philological essays are not immune from scribal trivialization!] d’un texte: l’exemple du chapitre XI de l’*Ars Rhetorica* du Pseudo-Denys d’Halicarnasse” (pp. 227-240); C. Macé, T. Schmidt, J. Weiler, “Les classement des manuscrits par la statistique et la phylogénétique: le cas de Grégoire de Nazianze et de Basile le Minime” (pp. 241-273); L.R. Mooney, A.C. Barbrook, C.J. Howe, M. Spencer, “Stemmatic Analysis of Lydgate’s *Kings of England*: a test case for the application of software developed for evolutionary biology to manuscript stemmatics” (pp. 275-293). Macé, Schmidt and Weiler’s article describes the experimental use of a program for Multidimensional Scaling (MDS) used: a) for all variants; b) for all non-isolated variants (that is, excepting *lectiones singulares*); c) for a subset of significant variants, chosen on the basis of traditional philological criteria, and warns: “Les meilleurs résultats ont été obtenus sur base de variantes sélectionnées selon des critères philologiques” (p. 254).

A second miscellany of the Dutch “school” is also noteworthy (*Studies in Stemmatology II*, edited by P. van Reenen, A. den Hollander and M. van Mulken, Free University, Amsterdam / University of Nijmegen, Amsterdam, J. Benjamin, 2004), as is the collection EoT of 2006, where, among other things, Marc Dubuisson and Caroline Macé update their readers about their projected edition

of the 45 homilies by St. Gregory of Nazianzus (100 to 700 witnesses per homily), begun in 1999 ("Handling a Large Manuscript Tradition with a Computer", in EoT, 25-37). In the same book, there is a remarkably instructive account of a workshop held in 2004, where different scholars classified the same "artificial" tradition using different methods: BARET-MACÉ-ROBINSON, "Testing Methods".

Since 2007, the Germanic philologist Maria Adele Cipolla has been organizing an international conference on computer philology in Verona, whose fourth session was held in 2012.

Also noteworthy are some specialized journals, such as *Le Médiéviste et l'Ordinateur* (1979-2009), *Linguistica computazionale* (founded by Antonio Zampolli in 1981, discontinued in 2006), *Literary and Linguistic computing* (I, 1986-).

Abundant international literature, albeit inevitably destined to rapid obsolescence, is available online. A *Bibliographie générale — Littérature et informatique*, collected by A. Pano Alamán, can be found at http://textopol.free.fr/Documents/Bibliographie/Siat_Bibliografia.htm. Among dedicated sites, one could also consult <http://www.textualscholarship.org>, the semi-official organ of various computer-assisted philologists, including Peter Robinson (→ 4.3). (I visited both of these sites in the summer of 2011 and again in the spring of 2014).

An especially important site for our purposes is *A catalog of Digital Scholarly Editions*, compiled by P. Sahle (<http://www.digitale-edition.de/>), pointed out to me by Francèsa Tommasi, whom I thank again. In May 2014, Sahle listed 145 editions of English texts, 65 for the German area, 64 in Latin, 24 for France, 13 for Italy, and 28 for other areas.

Indications about the historical context I have summarily sketched above are found in GREETHAM, *Textual Scholarship*, 325ff. On North American Romance philology, see CARAPEZZA, *Ecodotica*, 698-700, who lucidly distinguishes between the popularity of Cerquiglini's pamphlet in America and the scant interest it aroused in Europe.

Another notable work is ROBINS' very recent overview ("Editing and Evolution"), of 2007, which is also interesting for its rich illustrations reproducing the earliest genealogical trees of biologists from Darwin on, and those of philologists; the latter, as far as I know, are also based on indications drawn from TIMPANARO. Robins, however, does not always correctly summarize all the key notions of genealogical criticism, and overestimates the importance of certain recent fashions and catchwords, such as mistrust of "authorial originality", invoking a plethora of venerable but hardly pertinent *auctoritates*, from Barthes to Guattari. On p. 92, for example, a sort of biunivocal correspondence is suggested between genealogical criticism and poor awareness of the concept of "scribal culture as a historical phenomenon", as if Pasquali had never explained, in 1934, that the history of the tradition is a fundamental prerequisite for textual criticism (→ 1.6), or as if the fact that a witness, of Homer, Dante, or Shakespeare, belongs to a given scribal culture—which is,

mind you, well worthy of being studied—did away with the need for having a text freed as much as possible from involuntary errors and intentional modernization by its copyists (→ 3.8). The bibliography of the article, too, which is very abundant for recent stemmatology and its relationship with biology, betrays a modest interest in traditional genealogical criticism, which is reconstructed on the basis of few, albeit important, reference works (Kenney; Pasquali; Reynolds; Timpanaro; West and a few others).

A still valid reflection on the current state of Romance philology in the Western world, going hand in hand with a firm assertion of the need not to lose sight of the *meaning* of medieval texts, is provided by C. Segre, “Leggere i testi del Medioevo”, in *Notizie dalla crisi. Dove va la critica letteraria?*, Torino, Einaudi, 1993, 297–309.

4.3. Peter Robinson's textual studies and pioneering editions

The test of any method is whether it works.

ROBINSON, “Computer-Assisted Stemmatic Analysis”, 81

Lachmannian logic is essentially based on the error criterion. Does going outside of the Lachmannian system mean going outside of the rule of logic, or does it not? If the answer is no, does there exist another logic, possibly even more solid, for the constitution of the stemma? Or can one do without the stemma altogether?

Cesare Segre, “Prolusione ...”, 17

In 1996, Cambridge University Press published *Chaucer: The Wife of Bath's Prologue on Cd-rom*, edited by Peter Robinson (new edition: 2006). As one reads on the web site of the publishing house:

The “Wife of Bath’s Prologue” on CD-ROM inaugurates a revolutionary new kind of ‘book’ and offers the first ever genuinely comprehensive record of the Chaucerian text. This electronic textual edition of a major work of literature contains a full record of all the original sources for the work, with sophisticated search software and scholarly apparatus. The CD-ROM presents transcriptions, collations, and some 1,200 digitized images of all 58 pre-1500 manuscript and print versions of Chaucer’s “Wife of Bath’s Prologue”—an important section of *The Canterbury Tales*. An advanced hypertext presentation system enables the user to locate the transcript of a particular line in a particular manuscript, the collation of a particular word, or the image of that line or word in a manuscript, with the greatest of ease. This is the first release in Cambridge’s Canterbury Tales on CD-ROM series, a significant innovation in textual bibliography.

The table of contents of this work provides a sufficiently clear description of the content of the CD-ROM and the system requirements:

The CD-ROM contains: • Transcriptions of all 58 pre-1500 witnesses of Chaucer's "Wife of Bath's Prologue" (54 manuscripts and 4 early print editions), fully encoded in SGML (Standard Generalized Mark-up Language). • Digitized images of the originals of 1,200 manuscript pages, useful for teaching purposes or for checking the accuracy of any transcription against the original. • Hypertext linking between files allowing the reader to call up the full collation of any word across the entire range of witnesses. • A lemmatized spelling database permitting scholars to trace every spelling of every word across the witnesses. • A description of each witness based on examination of the witness itself. • Transcripts of all of the glosses in every manuscript of the Prologue. • A powerful DynaText search and retrieval engine with user-friendly screen design. System requirements: The CD-ROM runs identically on PC Windows and Macintosh systems. All necessary search and display software and fonts are provided on the CD-ROM and do not need to be acquired separately.

The following equipment is recommended:

PC: 386 or later	Macintosh: system 7 or later
Windows 3.1+	
8 Mb of RAM	4 Mb of RAM
double-speed CD-ROM drive	double-speed CD-ROM drive

* * *

Actually, in quantitative terms this is not as spectacular an achievement as one may think, since the "Wife of Bath's Prologue" runs to 850 verses, so that 58 witnesses correspond to about 3.5 copies of Dante's *Commedia*. But, since we are dealing here with "a revolutionary new kind of 'book'", which has greatly influenced the youngest generations of scholars, what really counts is how practical and methodological problems are addressed.

Now in the same year, 1996, Robinson published an article giving some very useful indications about the procedures he followed. The article, however, also raises some perplexities. I will discuss those that are most important to me.

A) To exemplify affinities between cladistics and textual criticism, the author explains that "in the first line of the 'Wife of Bath's Prologue' 42 manuscripts read 'Experience' and 10 read 'Experiment"'; and continues:

We may presume that the ten manuscripts which read ‘Experiment’ are all descended quite independently of one other, and that they happened on this reading quite separately [...]. Or, we may presume that the ten manuscripts are descended from a single hyparchetype which introduced the reading ‘Experiment’. Cladistics would regard the latter explanation as simpler and, therefore, preferable; so, too, would a stemmaticist (ROBINSON, “Computer-Assisted Stemmatic Analysis”, 77).

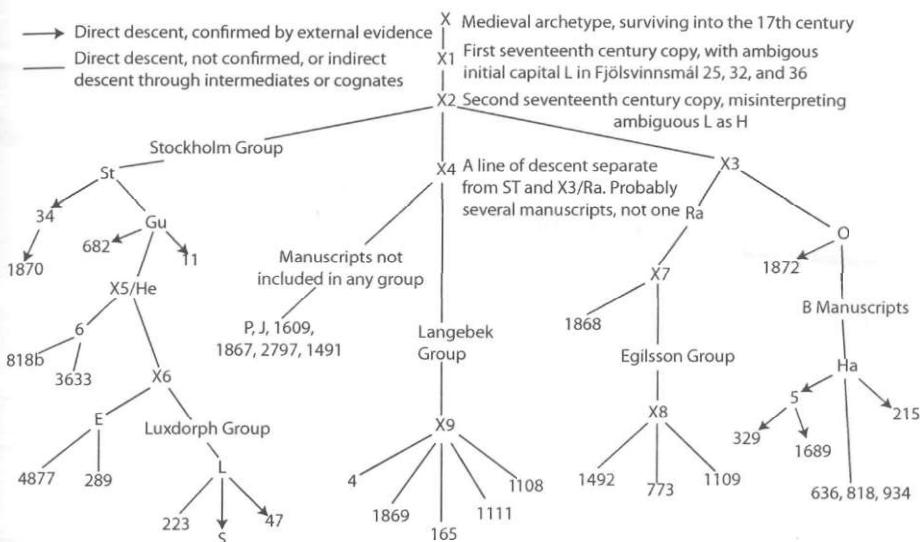
This line of reasoning is only abstractly correct. Actually, Latin paleography teaches us that *exp(er)i(men)t(um)* and *exp(er)ie(nti)a* are among the many abbreviations codified in the Middle Ages, which copyists of the time also adapted to modern languages. A hasty execution of an abbreviation sign could have misled several copyists independently of one another. Thus, the replacing of *experience* with *experiment*, or vice versa, is potentially a polygenetic innovation, one that only adds “noise” to the ensemble of variants to be compared. I wonder if an expert philologist would regard it as a significant error.

B) Robinson observes that

traditional stemmatics tries to identify which readings are ancestral [i.e., original] at only one point of the manuscript family tree. The single root or ultimate ancestor [i.e., the archetype]. Cladistics analysis, by contrast [...], aims not simply to reconstruct the ultimate ancestor but the whole history of the tradition, including the attributes of each ancestor throughout the tree. This has a remarkable and most powerful consequence (ROBINSON, “Computer-Assisted Stemmatic Analysis”, 78).

Actually, as I remarked above (→ 1.3) and a vast literature bears out, according to genealogical criticism a correct demonstration of the shape of a stemma requires specifying for each node in the stemma which innovations have occurred compared to the previous node.

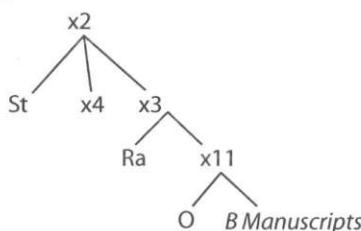
C) In the same article, 81–88, Robinson discusses the relatively recent tradition (“between 1650 and 1830”) of the Old Norse poems entitled *Svipdagsmál* (42 witnesses “about 1500 words in length”), which he studied for his doctoral dissertation (1991). He states that he included in the text of his edition, against the indications of his stemma, three distinct readings that are present in the B Manuscripts, but absent in the rest of the tradition, including the (presumed) ancestor of the subgroup, that is, ms. O (on the right in his stemma, which is reproduced below).



All that Robinson tells us in this regard is that these 3 readings should be restored as they belong to a series of 37 places where, in his opinion, the archetype is “incorrect”, and where he accordingly “emends the text”:

I, however, do not consider my duty to be finished with reconstruction of the hyparchetype [*corrige: archetype*]: as an editor, I believe I should use all the information at my disposal to reconstruct not the hyparchetype [*i.e. archetype*], but, as nearly as I can, the text as it was when it left the author’s hand. Indeed, there are 37 places in *Svipdagssmál* where I believe that the hyparchetype is incorrect, and where I emend the text accordingly (ROBINSON, “Computer-Assisted Stemmatic Analysis”, 97).

This inconsistency with the stemma (that is, with Robinson’s reconstruction of genealogical relationships) could be at least partially overcome by hypothesizing that the subfamily of the *B Manuscripts* in the right branch descends not from O, but from a lost, and more correct, related witness x11.



Now we are looking at a tradition where the T factor is relatively low, so the stemma may be very close to the real tree. Thus, even after this change has been made, it is not clear how a correct reading that is absent, from left to right, in St, x4, x3, and Ra could possibly resurface in the *B Manuscripts*. I hence find the three variants in question suspicious. Unless we presume that the readings were restored by the copyists by easy conjecture or that the exemplar of the *B Manuscripts* was partly based on a lost witness, and one that cannot be reconstructed within the stemma (extra-stemmatic contamination: → 3.4), we shall need to radically redesign the stemma, or resign ourselves to ascribing these three variants to the initiative of the same late copyist who demonstrates “an inclination to meddle in the text, without understanding of meaning, grammar or metre” (p. 96).

D) While it is the hope of every genealogical philologist to find, through a classification of the witnesses, “manuscripts close to the original” (i.e., with few errors) on which to found the reconstruction of the text, Robinson exposes the problem in a section surprisingly titled “The limits of stemmatics: manuscripts close to the original”:

As I remarked above, the problem of classifying this manuscript [i.e., J] and these other so-called ‘ungrouped’ manuscripts [i.e., P, 1491, 1609, 1867, 2797] is that they lack any strong affiliation with any other manuscripts (ROBINSON, “Computer-Assisted Stemmatic Analysis”, 88).

He then asks himself: “In traditional stemmatics, where do we put such manuscripts in a stemma; how do we use them when we edit?”, and eventually adds: “In my stemma, I had no better solution than to place them in a separate category, labelled ‘manuscripts not included in any group’ [...]. They form no such single line of descent, they share no unoriginal readings”. His frankly disconcerting solution is to group them together, including them arbitrarily in one of the three families he was able to single out, that is, x4. One cannot help remarking that such nonchalant manipulation of the data (“They form no such single line of descent, they share no unoriginal readings”) undermines the efficacy of the stemma.

In the light of the procedures I have tried to summarize in the previous chapters, a philologist should take a completely different approach. As the reader will remember, every act of copying introduces new errors. It is hence much easier to classify the lower levels of a stemma, which includes witnesses with many innovations, than the ancestors belonging

to the upper levels. Moreover, as I remarked above and as is confirmed in particular by the great number of *descripti* (i.e. mss. whose exemplars survive), the modern tradition of *Svipdagsmál* suffered only moderate decimation. As a consequence, there is a high likelihood that at least some of the early copies, with rather few errors, are preserved, perhaps even including the root of the tree (that is, the archetype). At least some of the witnesses that according to Robinson are proof of the "limits of stemmatics", because they do not have the same errors as the more numerous families and are hence hard to classify, could actually be high-ranking copies, not far removed from the archetype, and thus among the best copies of the surviving tradition. On this subject, the genealogical method offers a clear and parsimonious theory.

In confirmation of the above observations, Robert O'Hara's cladistic software, which was not misled by Robinson's methodological problems ("where do we put such manuscripts in a stemma; how do we use them when we edit?"), but was only based on variants in the tradition, places P at the highest level of Robinson's x_3 family, and the 1491, 1609, 1867 and 2797 foursome in very high positions within x_4 .

E) Granting that Robinson's stemma is correct, the editor of the poems would be dealing with a multi-branched stemma, that is, he or she would be in the ideal conditions for applying the majority principle. But Robinson does not seem to have correctly understood how the majority principle works ("the stemma [...] appeared to suggest some ten lines of descent", "it did not seem possible to use a stemma with so many lines of descent in any kind of 'majority verdict' stemmatic editing", etc.: ROBINSON, "Computer-Assisted Stemmatic Analysis", 89, 96).

Actually, as the reader will remember, the majority principle (→ 1.4) exclusively concerns primary branches, that is, those that descend from the archetype (in the stemma in question, only three). It is thus harder to accept Robinson's subsequent argumentations, such as:

If we cannot use the stemma to arbitrate between readings in this manner, how then can we use it? [...] A stemma [...] does have real use, though not quite the use proposed by traditional stemmatics. It gives a picture of the relationships among the manuscripts, and especially of the likely relative proximity of each to the archetype. The editor may use this information for two reasons. First, it may be used to guide the choice of a 'best-text': that is, a single manuscript which forms the base of the text of the edition [...] and thus its reading is to be followed at every point where

it is not clearly defective [...]. The second use of the stemma is to advise on the weight to be given to the alternative reading at those points where the chosen “best-text” is defective (ROBINSON, “Computer-Assisted Stemmatic Analysis”, 96–98).

Thus, Robinson’s unparsimonious classification appears mainly to serve the purpose of refounding (under the name of “best-text historical editing”: p. 98) the Pre-Parisian and later Bédierist notion of the *bon manuscrit*, whose limits are well known (→ 2). And if the base witness were manifestly erroneous, we would first of all need to adopt—as the rest of Robinson’s argumentation seems to imply—the plausible reading of the second ms. closest to the archetype, even if that reading were ruled out by the majority of the primary branches. But by proceeding thus we would be giving up, for no reason, one of the most solid foundations of genealogical criticism, namely, that, other conditions being equal, the original reading *is more likely* to be the one attested in the higher number of primary branches.

Robinson’s renaming of the stemma (“It is best described not as a ‘stemma’, but as a ‘table of relationships’”: p. 99) does not make this renunciation any less serious.

F) Robinson had famously promised to “buy lunch for anyone who could produce, using computer methods alone on this raw data, something close to the stemma [he] had spent five years making”. The solution that comes closest to his is the cladogram produced by Robert O’Hara (ROBINSON, “Computer-Assisted Stemmatic Analysis”, 85). Now, observing some divergences between his stemma and the cladogram, Robinson admits that O’Hara’s cladistic analysis “was not correct in all details” (p. 86). Actually, some differences concern not details but the structure of the upper levels (those that are decisive for establishing the “majority”). After discussing the above points, we shall have to ask ourselves whether the data are reflected more accurately by Robinson’s stemma or O’Hara’s cladogram, which produces radically different “majorities” from those of the stemma. Should further investigation confirm it, the shape of O’Hara’s cladogram would have dramatic consequences for the text to be established after Robinson’s preliminary one. Suffice it to say that, in Robinson’s stemma, the three primary branches, worth 33% each, are St, $x4$ and $x3$; in O’Hara’s differently three-branched diagram, the $x4 + x3$ group is derived from a single ancestor, which is thus worth 33% and is systematically placed in the minority by the agreement

of the other two branches, respectively constituted by St alone and all the other witnesses (in Robinson, only one family).

G) As Robinson himself declares, the stemma proposed in his dissertation is the fruit of a five-year-long study (p. 85)—which, incidentally, appears to be an incredibly long time to unravel what is, by his own admission, an “unusually simple” tradition (42 witnesses of a relatively short text, and which often declare what exemplar they were copied from). However, as Robinson himself admits, “it is difficult to determine from [his] stemma even how many lines of descent there are” (p. 89).

H) To conclude, let us return to the “Wife of Bath’s Prologue”. Robinson’s edition offers no indication of how the “unrooted tree” produced by the computer was turned into a rooted stemma. But our curiosity about how this decisive step was approached is satisfied thanks to further investigation by Michael D. Reeve:

One of the biochemists has been kind enough to tell me that the computer produced an unrooted tree and they rooted it by accepting the view of Chaucer scholars that the Hengwrt manuscript is close to the original; so that when the *Times* reported that the Hengwrt manuscript emerged as particularly close to the original, “emerged” was the reverse of the truth (REEVE, *Manuscripts and Methods*, 346).

More generally, as many of the considerations discussed above suggest, one gains the clear impression that almost all the debatable elements discussed so far, and possibly even the search for a computer-based alternative, arise from inadequate knowledge of traditional philological theory and methods. But, if it is true that “text-genealogical software, in fact, is a text-genealogical theory in computer shape” (Salemans, “Cladistics or the Resurrection of the Method of Lachmann...”, 6), it is hard to imagine that this inadequate knowledge could have produced software adequate to such ambitious tasks.

Bibliographical notes. The first epigraph is from C. Segre, “Prolusione”, in NOF, 11-17.

Among Robinson’s many works, I have used: R.J. O’Hara, P. Robinson, “Computer-Assisted Methods of Stemmatic Analysis” (also available online); *Chaucer: The Wife of Bath’s Prologue on Cd-rom.*, edited by P. Robinson, Cambridge, Cambridge University Press, 1996 (repr. 2006); ROBINSON, “Computer-Assisted Stemmatic Analysis”; A.C. Barbrook, C.J. Howe, N.

Blake, P. Robinson, "The phylogeny of the Canterbury Tales", *Nature* 314, 1998, 839 (also available online).

The impact of Robinson's works on younger scholars is well exemplified by the following statement: "The analytical task was immense, so the initial shape of the stemma was constructed with the help of computer software programmed to implement 'cladistic' algorithms devised by computational biologists to classify organisms based on their common descent. *The choice of cladistics to devise a family tree for witnesses to the text is justified* theoretically by deep philosophical similarities between cladistics and stemmatics and more practically by its success in editing the Canterbury Tales" (S. Carlson, "Dissertation Summary: The Text of Galatians and Its History", *Journal for the Study of Paul and His Letters* 2.2., 2012, 171-175; my emphasis).

However, severe misgivings about Robinson's experiments have been expressed by R. Hanna, "The application of thought to textual criticism in all modes—with apologies to A.E. Housman", *Studies in Bibliography* 53, 2000, 166-172; N. Cartlidge, "The Canterbury Tales and Cladistics", *Neuphilologische Mitteilungen* 102, 2001, 135-150; REEVE, *Manuscripts and Methods*, 345-346, 389-390, 392.

The last quotation is from B. Salemans, "Cladistics or the Resurrection of the Method of Lachmann", in *Studies in stemmatology*, 3-55.

4.4. The first cladistics-based edition of an Italian text: Antonio Pucci's *Reina d'Oriente*, edited by William Robins (2007)

First, editors will have to explain that a cladogram is just a particularly efficient way of organizing evidence about variation; it is not the same as a stemma, and in fact it need not be turned into a stemma. If the goal is to produce a compelling stemma, a cladogram will be just one of many sources of information.

ROBINS, "Editing and Evolution", 116

The core of cladistics is the permanent question which elements or characteristics in a species can be used to develop genealogies. The simple lesson taught by cladistics is that we must be very careful in using characteristics for genealogical research. For example, the fact that both swallows and flies have wings, does not imply that these birds and insects belong to the same family. The characteristic 'having wings' is not a trustworthy genealogical informant. Again, text-genealogists can learn from cladistics that they must be very careful in choosing variants to be the building stones of chains and stemmas.

SALEMANS, *Building Stemmas*, 6

In 2007, Attilio Motta and William Robins published two synoptical editions of the *Cantari della Reina d'Oriente* by Antonio Pucci. While the

Motta edition (a Bédierist edition of the more ancient and authoritative ms., K: formerly Kirkup, now in Florence, Biblioteca Nazionale Centrale, N.A. 333) lies outside the scope of the present chapter (→ 2), Robins declares that his edition is an attempt to “look for an innovative editorial solution bypassing the alternative between the earlier manuscript [i.e. the base ms., Bédier's *meilleur manuscrit*] and the text reconstructed on a stemmatic basis, in the persuasion that neither of these two approaches can, on its own, account for the ambivalence of Pucci's text and the figure of Pucci himself at the editorial level” (MOTTA-ROBINS, XIV-XV). The innovative solution consists, of course, in a genealogical classification conducted “not only employing the methods of traditional philology, but also experimenting with the ‘oriented’ quantitative methods of cladistics in replacement of the notion of error” (MOTTA-ROBINS, XVI), and proceeds along lines we could trace back to dom Quentin-dom Froger and Greg-Dearing.

Robins himself—who, as I mentioned above, is the author of a useful overview of cladistics and textual criticism (→ 4.2)—summarizes for Italian readers the basic assumptions and characteristics of these methods and their applications (MOTTA-ROBINS, LXXVIIff.). In particular, for Robins, too, the construction of the stemma is only the second stage of classification, which presupposes Quentin's distinction, by now well known to the reader, between: a) *enchainement* (building of a chain); b) *orientation* (rooting).

It is barely worth remarking that, aside from the foundation myths of cladistic stemmatology, which Robins appeared to believe in in 2007 (it is a well-known fact that the foundation myths of a city, a religious order, etc., are always quite radical), in practice the positions of the old and new philologists are actually not as far removed as one may think, because:

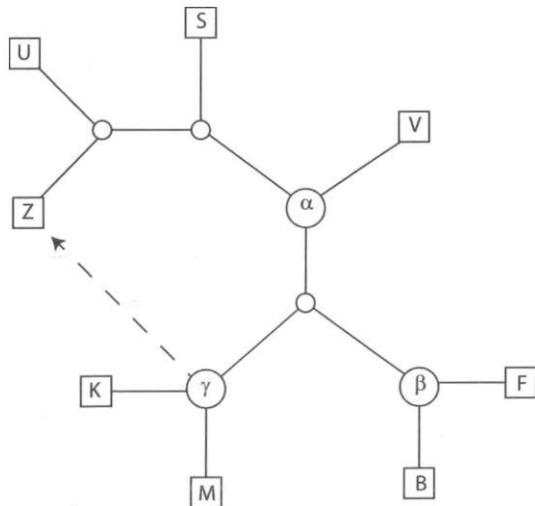
1. in spite of statements to the contrary (e.g., Van Mulken's that “a Lachmannian scholar always has a preconception of the original reading in the archetype”), even the notorious (Neo-)Lachmannians also limit themselves to collating in the first stage, after which they decide, usually on solid grounds, what can or cannot be an error amongst the long series of variants found in groups of mss. (potential families);
2. the more careful stemmatologists seem to have realized that, to build non-illusory stemmata, a few dozen significant errors are more useful than thousands of dialectal and inflectional variants, which have a highly polygenetic character.

¶ For example, Ben Salemans, whose arguments we shall be returning to later: 1) stresses that “only very few variants can be building tools”, 2) proposes acceptable rules for singling out significant errors; and 3) explicity refers, already in the titles of his works, to an update of “Lachmann’s method”.

But let us return to the Robins edition.

* * *

Here he proposes a first-generation type of cladistic analysis, very far removed from Salemans’ concessions to Neo-Lachmannism, and founded on all sorts of non-selected variants, such as *avendom’io / avendom’i*, *singnor / singnori*, *mi fè / mi fa*, *po che / ond’io / onde* (although occasionally with somewhat naïve justifications, as on pp. LXXIX-LXXX). Robins uses the cladistic software MacClade 4.06 to manage the 1450 points of variation of the 8 “complete” witnesses of the *Reina*. This program is capable of finding the Most Parsimonious Tree (= MPT), that is, the tree “requiring the least number of [...] independently introduced innovative traits” (MOTTA-ROBINS, LXXXVII). Robins thus ends up with the following chain:



Like Quentin and others before him, at this point Robins needs to *root* the stemma, and to do so he is forced to appeal to the very notion of error he had previously challenged. To this purpose, Robins singles out what he claims to be significant places, allowing him to determine “if not ‘er-

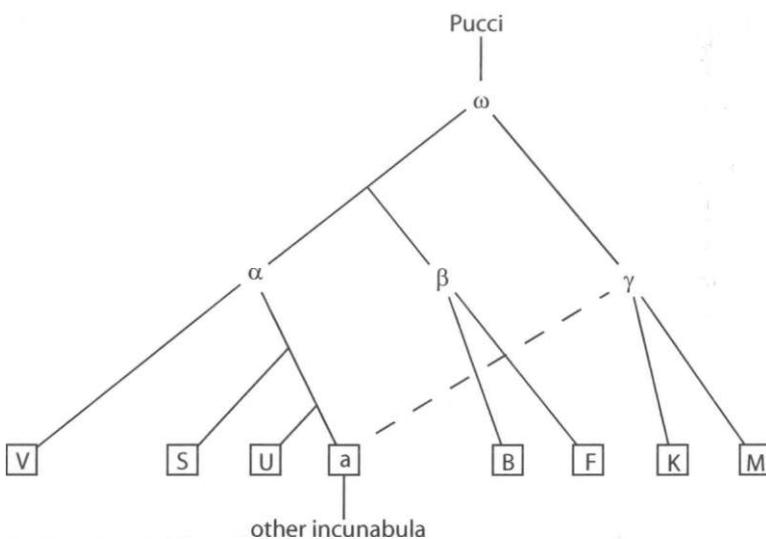
rors,' at least 'incorrect readings'" (this somewhat sophistical distinction is found on p. CII).

¶ A similar reluctance, following in the wake of dom Quentin, is found in Ghislaine Viré's computer-assisted edition of Hyginus's *Astronomica* (1992): "Though Viré often invokes that [i.e. Quentin's] tradition and tends to avoid words for 'error', her search for qualitatively significant variants is neither more nor less than a search for errors unlikely to have arisen co-incidentally more than once" (REEVE, *Manuscripts and Methods*, 367). In the "Prologue" to *Studies in Stemmatology*, x, the attempt to minimize and camouflage this contradiction is even more patent. Instead of referring to a search for error, the authors coyly speak of "judgments on the originality of variants": the new stemmatologists "work with 'unrooted' trees and show that Lachmannian subjective judgments to decide whether variants are directional or not are not necessary for the construction of trees, since *only a few judgments on the originality of variants are required for the rooting of the tree*" (my emphasis).

As far as I can tell, the whole procedure is intrinsically contradictory, as the authors ostensibly want to avoid subjective decisions, but the most important decision for the constitution of the text, that is, the rooting of the tree, is left to a few subjective decisions which are not explicitly discussed. What, indeed, if not the enumeration and discussion of *all* detected significant errors, can guarantee that, when a *few* errors are picked out to justify a given rooting of the stemma (errors that could well be due to contamination or polygenesis), there are not many others suggesting a more plausible and parsimonious rooting?

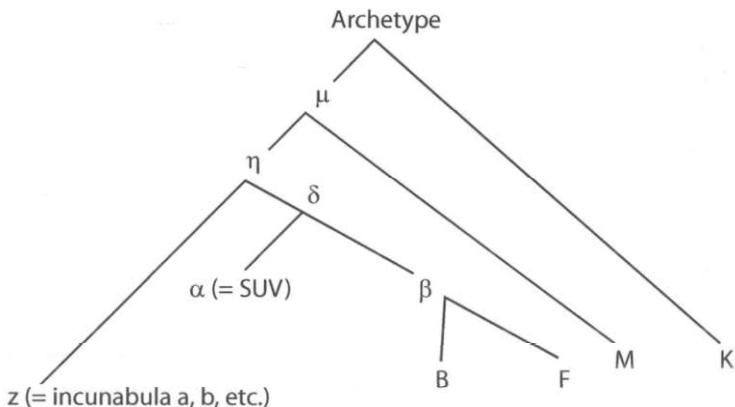
Most strangely, in the face of the general scarcity of significant errors invariably plaguing all traditions, Robins feels compelled to discard *a priori* all substantial variants found "in the final verse of a *cantare*" (MOTTA-ROBINS, CIII). His argument is that valedictions (which subsequent songsters often reformulated to update them, which involved deleting or replacing Pucci's "signature") are for some reason genealogically less significant than other parts of the text (MOTTA-ROBINS, CVII-CIX).

Below I reproduce the stemma he proposes on p. CXII, which is certainly, as he stresses, "preliminary", but no more so than any other similar diagram drawn by a philologist:



Robins' edition uses V as its base text, "chosen because almost complete (which guarantees a consistent linguistic texture) and because the copyist's Florentine contains few forms and almost no spelling that would have been unusual for Pucci" (MOTTA-ROBINS, CLXXXIII-CLXXXIV).

Basing him or herself on the rich apparatuses of variants supplied by Robins himself, and disregarding the mss. reduced to a few fragments, but including the almost complete ms. K (where the text begins at II 9.5, and octaves III 9-10 are missing), a Neo-Lachmannian like the present writer would rather incline to a stemma like the one shown below:



The stemma I ended up with is not, in abstractly morphological terms, too distant from that proposed by Robins himself, but it functions in a radically different way in view of the different percentage weights of K and M when the majority principle needs to be applied, which is almost always the case. I need hardly to add that the vulgate text η is the only zone in the stemma where strong contamination is detectable (an analytical demonstration of all the nodes can be found in Bettarini Bruni's article cited below).

The difference between these two stemmata of *Reina* is essentially a consequence of the different methods employed to create them. And, as far as I can tell, when compared they reveal another weakness in the method adopted to construct the first of them. It appears that, when the more rigorous (or less eclectic) cladists move on to Phase two (*rooting* the stemma), they are not inclined to, or cannot, question the pairs suggested by the computer-assisted chain, much less the assumption (which merely reflects the requirements of the computer programmers) that "all splits of a tree are two-branched" (MOTTA-ROBINS, LXXXVIII).

Actually, as Robins himself admits (p. LXXXVIII), there is neither a historical nor a philological reason why all splits should be two-branched (a ms. may have been copied 5 times, another one never). However, in Robins' stemma K and M continue to remain "attached" to a common node even after the *chain* has been *rooted*. But, while BF and SUV_z (by *z*, that is, *a* and *b*, Robins designates the first incunabula) can easily be confirmed to be relatives in Neo-Lachmannian terms, too (thanks to a long series of conjunctive errors), the frequent agreements between K and M—the presumed γ family singled out by the MacClade software in the chain-building stage (pp. XCIVff.)—on close examination appear to be agreements in good readings or at least equally acceptable ones. (I include, of course, the only three cases, II 36 7, III 19 3, and IV 16 3, that Robins regards as significant and discusses on pp. CV-CVI; → 6.2, where the most interesting of these three cases is discussed).

If we do not give up drawing conclusions from the presence or absence in the text of Antonio Pucci's "signature" (this is an additional "rule" followed by Robins, the reason for which is frankly difficult to grasp), it is notable that Pucci's signature at the end of *cantari* II, III and IV only occurs in K, along with M alone in two cases, and in U alone in a single case (the first *cantare* is missing in the all-important but incomplete ms. K). We know that Pucci usually "signed" his works. Thus, in this case, K (followed by M) preserves better than other mss. the original late-fourteenth-century text.

Furthermore, even in cases of broad diffraction (→ 3.2), K has, alone or together with one or another witness, what is in all likelihood the genuine reading. For example:

*III 17 6 ben per non diviso K + SB] ben per non divisi M, per mio aviso U, quel bel fiordaliso z, ben per noviso F

*IV 35 7 barbani (: Romani) K] balbani U, cani U, baroni sovrani S, burbani B, baroni M

The reading *per non diviso* of K+SB is a juridical technical expression often employed by Pucci (*Glossario MOTTA-ROBINS*, s. v. *diviso*). As we can tell from available text databanks, *barbani*, which only occurs in K, is already a *démodé* term in Tuscany in the second half of the fourteenth century (TLIO only has examples in Bono Giamboni and Guido da Pisa; no Tuscan example is recorded in LIZ).

If we kept working from significant (i.e., non-polygenetic) errors or innovations—as Anna Bettarini Bruni, who has studied all of Pucci’s works for many years, has done with the *Reina d’oriente*—, we would note that, unlike K, M shows many innovations or errors in common with the more recent tradition. As I mentioned above, the consequences of this difference in classification are hardly unimportant. While Robins’ stemma yields an open *recensio* (the node from which α and β branch out is worth 50%, like γ, and the editor can thus freely opt for one or other of the two hyparchetypes), in the stemma that I will call, for expediency’s sake, Bettarini Bruni-Trovato, the K + M or K + η agreements are worth 75% of the stemma, and thus allow us to reconstruct to a great extent the readings of the archetype, that is, the earliest version of the *cantare* we can trace.

As Justin Steinberg observed, regarding a different subject, “the genealogy of texts and codices that emerges from any hypothesized stemma, however tentative, can nevertheless, if read historically and not only hierarchically, illustrate the important trajectory of a work from composition to reception, author to public” (Justin Steinberg, *Accounting for Dante...*, 8). As a consequence, this more accurate identification of the archetype is not merely of interest to textual scholars. It also allows a better assessment of subsequent manipulations of the text (*editiones* U, S, etc.). It is necessary and helpful for language historians, for students of “scribal cultures”, and for historians *tout court*. Besides, our genealogical reconstruction of the text closest to Pucci’s lost original would not prevent us, if we so wished, from *also* providing digital editions of all the manuscript witnesses and of early printed copies.

At this point, one would like to know why Robins decided to disregard so valuable an element such as the presence of Pucci's "signature" in K and, in residual form, in only two other witnesses, since the replacing or removal of the author's name clearly depend on a change in the context in which the copyists worked, namely, on an increase in chronological and/or geographical, i.e., cultural, distance. So, in these places at least, it is easy to distinguish between original and secondary readings. All this seems to bring us to Ben Salemans' argument that the computer programmers who help textual critics in the application of cladistic routines often too nonchalantly suggest the removal of variants that do not quite fit expectations:

Most of the times the development of chains by these mathematicians is undocumented. That is a serious problem. For instance, *mathematicians throw away rather easily variants that cause 'bias' in a chain*. I have never seen a (theoretical) justification about this (de)selection of variants. If there is no justification of the removal of 'difficult' variants, *this has to be considered as an uncontrollably subjective, and therefore unscientific activity!* (Salemans, *The remarkable struggle...*; my emphasis).

Bibliographical notes. MOTTA-ROBINS. On Robins' edition of the *Reina d'oriente*, A. Bettarini Bruni, P. Trovato, "Dittico per Antonio Pucci", *Filologia italiana* 6, 2009, 81-128 (more precisely, the diptych is constituted by P. Trovato, "Di alcune edizioni recenti di Antonio Pucci, del codice Kirkup e della cladistica applicata alla critica testuale" and A. Bettarini Bruni, "Esercizio sul testo della *Reina d'oriente*. È possibile un'edizione neolachmanniana?"). I would like to acknowledge here Robins' courtesy and intellectual honesty in taking into account the objections of traditional philology (W. Robins, "Cladistics and Italian Philology", forthcoming).

Margot van Mulken's statement about Lachmannians is in VAN MULKEN, *The Manuscript Tradition of the Perceval*, 13, and the quotation of Justin Steinberg is taken from his *Accounting for Dante. Urban Readers and Writers in Late Medieval Italy*, Notre Dame, University of Notre Dame Press, 2007, 8.

On the excesses of computer programmers, I quoted from B. Salemans, "The remarkable struggle of textual criticism to become truly scientific...", [2002] in http://www.knaw.nl/agenda/pdf/Abstract_ben_salemans.pdf (later in *Language and Linguistics E-Books Online*, Collection 2011, Brill).

4.5. Prue Shaw's digital editions of Dante's *Monarchia* (2006) and *Commedia* (2010)

Two theoretical advantages are suggested for this approach [i.e., a computerized approach]. First, objectivity: no judgments of value are entailed, whereas the genealogical method calls for decisions as to the correctness of readings or textual states. Second, the possibility of mechanization: long and elaborate calculations involving thousands of variants may be performed by a computer. This possibility is especially attractive to New Testament critics, who are confronted with about 5,000 manuscripts of the Greek text as well as versions in other languages and patristic citations. *In practice, however, these advantages are to a large extent illusory.* An “objective” (i.e., undiscriminating) treatment of all variants in a literary text such as Ovid’s *Metamorphoses* (of which more than 300 manuscripts exist) without regard to their metrical and stylistic quality would be a self-evident waste of time and produce merely confusion. The critic cannot abrogate his critical function, which implies discrimination, at the very beginning of the critical process. Moreover, the preparation or programming of a text for treatment in this way, whether mechanical aids are used or not, is long and laborious, and one must consider whether in a given case the results justify the expenditure of effort.

KENNEY, “Textual Criticism” (my emphasis)

A realistic position is probably that these phylogenetic methods offer a valid approach to manuscript analysis, but that their real value comes in their power to assist the textual scholar rather than to attempt to replace the scholar’s expertise

WINDRAM et al., 444

Several digital editions have seen the light in the wake of Peter Robinson’s pioneering efforts, even outside the Canterbury Tales Project. I will briefly examine, again from a strictly philological standpoint, two recent DVD-ROMs offering digital editions of two works by Dante based on the software produced by Robinson.

© Gian Paolo Renello, who is more competent than myself on these matters, has kindly supplied a brief discussion of some technical aspects of these DVD-ROMs: → 4. Appendix.

The two DVD-ROMs are devoted, respectively, to the *Monarchia*, Dante’s Latin treatise on the best form of government (2006), and the *Commedia* (2010). The first is published by Scholarly Digital Editions

(SDE) and the Società Dantesca Italiana, the second, again, by Scholarly Digital Editions, and by SISMEL-Edizioni del Galluzzo. In both, a very competent English scholar, Prue Shaw, is responsible for the philological part. Shaw studied at Florence with Contini (one of the top Neo-Lachmannian philologists of the last century) and has been studying Dante and especially the transmission of the *Monarchia* for some forty years.

The first DVD-ROM offers—along with a very well informed philological Introduction supplemented with phylogenograms, i.e., unrooted stemmata in the manner of dom Quentin—accurate transcriptions of all the 21 witnesses known to Shaw as of 2006.

¶ A twenty-second witness, which had eluded Shaw's previous investigations and belongs, as is often the case, within the vulgate text, has been studied by Shaw herself (“Un secondo testimone londinese della *Monarchia*”, *Studi danteschi* 76, 2011, 233–264), and by Diego Quaglioni (“Un nuovo testimone per l'edizione della *Monarchia* di Dante: il Ms. Add. 6891 della British Library”, *Laboratoire italien* 11, 2011, available online since February 2012).

The transcriptions are linked to high-resolution digital images of the corresponding pages in the manuscripts. These are certainly fundamental tools for any philologist, but not that innovative after all.

¶ For example, although the project has been interrupted for lack of funds, the Società Dantesca Italiana has long been offering, on the website www.dante-online.it, a few dozen integral transcriptions of mss. of the *Commedia* which can be cross-checked with linked digital images of the mss. themselves. A major improvement in the DVD under discussion, however, is that the transcription can be viewed on screen side by side with a high-resolution digital image of the corresponding ms. page, which obviously makes their comparison much easier.

A much more promising function, one that we could even call revolutionary, is the one entitled “Vbase”. This is a databank of variants capable of identifying all variants shared by two or more witnesses in just a few minutes. This is a very handy and effective tool both for the cladistic textual scholar, who is satisfied with knowing which mss. have most variants in common, and the traditional philologist, who is only interested in the significant errors common to several witnesses.

¶ Both the traditional and the new textual scholars in the mold of Ben Sallemans (→ IV.6) will also have to do some non-computer-assisted work to distinguish significant errors in a long list of variants case by case. Their task will undoubtedly be facilitated, however, by the fact of working on an already preselected textual subgroup.

The program, as I was saying, is rather easy to use and takes only a few minutes, depending on the magnitude of the requested data, to answer our queries. But can we say that by following this path the textual scholar who compiles such a DVD-ROM actually saves him or herself time? Judging by the editorial history of the *Monarchia*, the answer would appear to be negative. The digital edition signed by Shaw in 2006 actually differs in very few points from the (paper) text published by Shaw herself in 1995. If the text only contains marginal changes, this means that the "new" digital approach did not significantly modify Shaw's own genealogical assessment of 1995, when she had analyzed the tradition of the *Monarchia* using the Neo-Lachmannian toolset of her mentor Contini (common errors, stemma, majority principle, etc.). Much of the ten years from 1995 to 2006, instead, was employed to produce encoded transcriptions allowing Robinson's software to "read" the text and find the variants common to the witnesses selected by the users of the disk.

The DVD-ROM relative to the *Commedia*, however, contains—along with a well-informed and in places excellent Introduction—transcriptions of only 7 witnesses, and, for copyright reasons, the computerized images of 6 of the 7 witnesses (as is known, the *Commedia*—to which Chap. 9 of the present book is devoted—is a very long text with a very rich tradition). The editorial history of this second DVD-ROM is not too different from that of the first. It too is based on a classification and an edition produced with traditional methods, those of Federico Sanguineti (2001), of which Robinson's programs allow *a posteriori* verification. But, while in the case of the *Monarchia*, save for one unintended exception, the whole tradition is presented in the DVD-ROM, including witnesses regarded as *descripti*, in the case of the *Commedia* the 7 witnesses account for little more than 1% of the tradition (without counting fragments, of course). As a consequence, the indications found in Shaw's new work only concern a rather small subgroup of the tradition of the *Commedia*, although some of the included witnesses, those designated as Ash, Rb, and Urb, will probably still be central for the reconstruction of the text in the years to come. Nevertheless, it must be said that the overall architecture of the CD-ROM offers significant improvements: one can study not only the texts of the mss., but also, through Vbase, the texts of the two most recent scientific editions, that of Petrocchi and that of Sanguineti, measuring the variations of each from their respective base witnesses.

In this case, too, one wonders if this CD-ROM actually allowed the philologist who put it together to save time, that is, if anything has really changed since 1968, when Froger wrote: "Il faut avouer [...] que la collation des manuscrits et la recherche des relations généalogiques vont plus vite et entraînent moins de frais si on les fait tout simplement à la main" (FROGER, 219). To classify about 200 mss. of the *Commedia*, collating them in the 396 *loci critici* selected by Michele Barbi (→ 9.1), Sanguineti employed about ten years (for another ca. 350 mss., Sanguineti limited himself to checking a very small number of *loci critici*). My team of volunteers completed its collation of all of Barbi's *loci* and another 100 or so points of variation in the same 550 manuscripts in 5 years and a half (most of the work was done by 4 people). To make fully computer searchable a mere 7 witnesses, Shaw employed, it would seem, 3 years. After all, if the professional copyists of the fourteenth century, who were fast, employed from 3 to 6 months to finish a copy of the *Commedia*, it seems hard to imagine that a philologist can produce more than 3 or 4 accurate transcriptions in a year, and do so with the encoding that the PC requires. This means that another 100 years or so would be required to offer all of the 550 transcriptions.

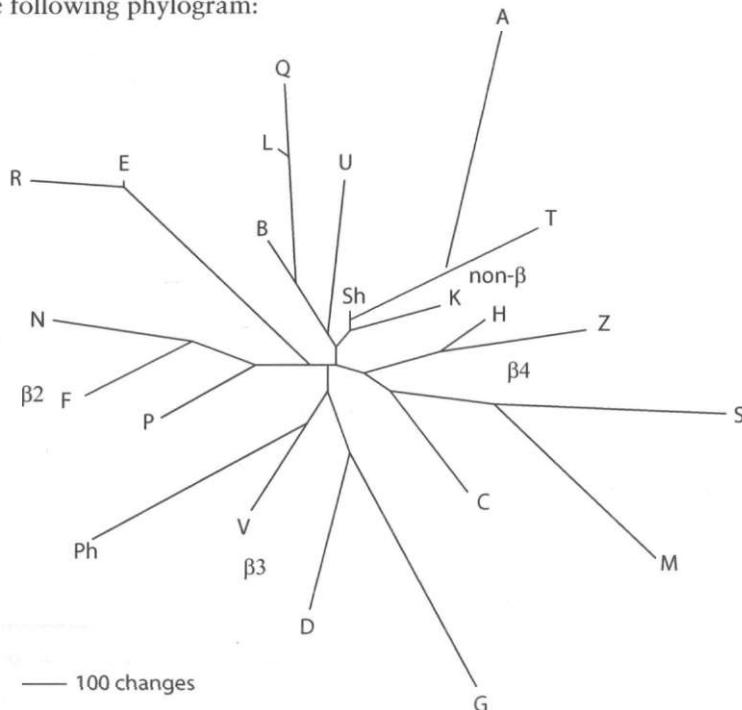
Thus, the first conclusion of this very summary examination is that the digital philology of Shaw and Robinson does not save a textual critic who wishes to emulate them any significant amount of time. I will readily admit, however, that users of any of the two DVD-ROMs interested in studying the tradition of the respective works in depth now have the possibility, which would hardly have been imaginable before, of checking the work of their predecessors in just a few hours, and if they so wish exploring new paths equally rapidly in the attempt to find alternative groupings.

Let us now come to a second, fundamental aspect of these editions, namely, phylogenograms, that is the unrooted stemmata generated by cladistic programs in the first processing stage. With the exception of Salemans, who, as we shall see, drastically selects variants in the search for possible significant innovations, that is, indicative errors, most first-generation computer-assisted philologists—for example, Robins—use the full range of variants of the surviving tradition, except for spelling differences. As cautioned by the abstract (necessarily a synthesis and possibly just a touch self-promotional) of a methodological article on the *Monarchia*

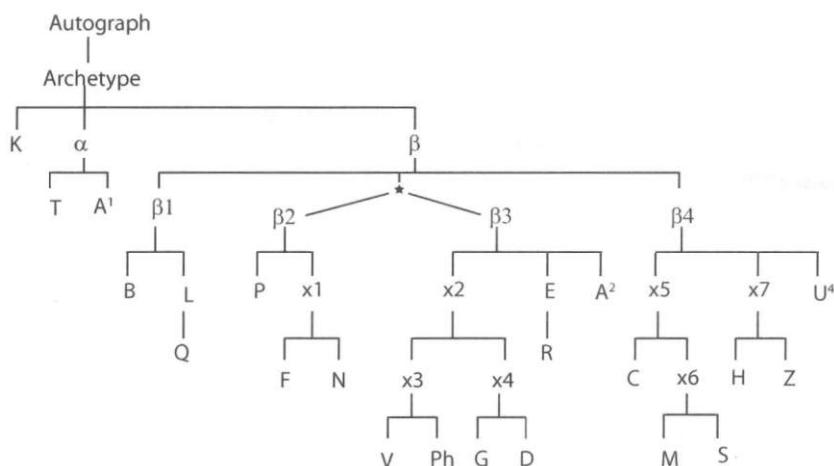
signed by biologists Heather F. Windram and Christopher J. Howe, as well as Robinson and Shaw:

Dante's *Monarchia* [...], which survives in 20 manuscripts and the *editio princeps*, has been studied extensively by scholars using traditional analytical methods to establish textual transmission. It was selected as a suitable tradition for a blind study to test the application of computer-based phylogenetic methods to the stemmatic analysis of manuscript relationships. Our results show that *these methods—maximum parsimony, NeighborNet and the Supernetwork algorithm—are capable of producing stemmata in very close agreement with those produced by traditional stemmatic analysis, including the identification of texts that change exemplar in the course of copying*. The phylogenetic methods can correctly indicate the affiliations both before and after the point of exemplar change. The maximum chi-squared method (developed to detect recombination in DNA sequences) is able to indicate the region of exemplar change, allowing the precise location to be ascertained by textual analysis (WINDRAM et al., 443; my emphasis).

Comparing all the variants contained in Vbase, the program generates the following phylogram:



In fact, if we compare the phylogram with the *stemma codicum* proposed by Shaw herself in her paper edition of 2010:



we shall realize that “very close agreement” does not mean identity, and it is a well-known fact (we have already remarked on this for the anonymous Old Norse *Svipdagsmál* and Pucci’s *Reina d’oriente*) that even slight modifications to the shape of a stemma can have very important implications for the constitution of a text. In the case of the *Monarchia*, too, some differences are especially important, because they concern primary branches ($K + T$ A^1 are traced back to the same split), and we do not know how things would have gone if a good text, established manually, had not already been available.

Now admittedly, although it draws on pre-existing paper editions, the digital philology of Prue Shaw and Peter Robinson is decidedly more sophisticated than that employed by Josephine Brefeld in her unimpressive attempt to study medieval guides to the Holy Land, and also more than the interesting, but not impeccable, approach of William Robins to Pucci’s *Reina d’oriente* (→ IV.3), and, in absolute terms, it is anything but uninteresting. However, the legitimate enthusiasm of these founders of a very promising methodology should not give rise to ambiguities regarding the limits of digital stemmatology. The fact that many of the variants used by the software are polygenetic suggests that a number of the agreements between witnesses detected by the software are purely random.

As Shaw repeatedly warns, and as we read in the useful four-author article I have cited several times, "Dante's *Monarchia* as a test case", it is entirely reasonable to argue that, "although computer methods cannot make any literary or textual judgment on the material, they can work very efficiently in conjunction with other methods [i.e. traditional genealogical methods] to facilitate an understanding of a textual tradition" (WINDRAM et al., 460). But there are more complex things—identifying the archetype, designing the upper levels of the stemma, analyzing contamination, etc.—that these programs could not do even if they only employed indicative errors. As these scholars themselves admit:

A realistic position is probably that these phylogenetic methods offer a valid approach to manuscript analysis, but that their real value comes in their power to assist the textual scholar rather than to attempt to replace the scholar's expertise (WINDRAM et al., 444).

The above-mentioned workshop held in Leuven in 2004 also concluded on a cautious note:

Firstly, the advantages of computerized methods are clear: they are objective, reproducible, able to deal with a large amount of data [...]. Secondly, *computerized methods do not seem capable, of themselves, of producing the most complete account of witness relationships. We have mentioned the need for scholarly analysis to determine direction of variation and hence tree orientation: this seems beyond the scope of computer methods. Further, computer methods have difficulty with polytomy (more than two witnesses copied from a single source) [...]*. Taken together, this suggests that a partnership of computer methods and traditional analysis might give the best results (BARET-MACÉ-ROBINSON, "Testing Methods", 281; my emphasis).

Shaw expresses similar views on the relationship between digital editions and traditional methods in her own digital edition of *Monarchia*:

PAUP does not unequivocally prove either the two-branch or the three-branch stemma hypothesis: both could be justified with reference to the phylogram. The t[three]-branch hypothesis places the archetype (the point of origin from which the whole surviving tradition descends) at the junction where three branches break off. But it would be possible to argue that the point of divergence could be slightly further along that line to-

wards the β grouping, and if we were to place it there then there would be only two branches. Only traditional scholarly investigative techniques establish, in my view beyond any shadow of doubt, that the *t[bree]-branch hypothesis* is the one which most accurately accounts for the data. The electronic stemma needs to be interpreted with a lively awareness of the significance and weight of traditional scholarly procedures (Chap. V. "The Methodology of the Edition"; my emphasis).

¶ *T[bree]-branch* instead of *two-branch* is a correction by Shaw herself, "Un secondo manoscritto londinese...", 262)

However, the experience that Prue Shaw and Peter Robinson have acquired in the field of digital philology seems to have boosted their trust in the new methods of investigation, as one senses especially in Chap. VI of the introduction to the *Commedia*, co-signed by Robinson ("The Phylogenetic Analysis", in SHAW), and decidedly out of line both with the equilibrium and balance informing the previous chapters, and with earlier considerations by Robinson himself (e.g. id., "Computer-Assisted Stemmatic Analysis", 72: "Where we might suppose that increasing knowledge and sharper tools might bring us more certainty, they actually bring us only doubt. In place of decision, we have new kinds of indecision").

For example, the partial identification of the eclectic Petrocchi text with the archetype of the surviving tradition is disconcerting, as well as simplistic ("Of course, we do not have that archetype. But we do have the Petrocchi text, and one might fairly suppose that for a great many readings, Petrocchi is likely to have chosen the archetypal form": "Introduction", Chap. VI. "The Phylogenetic Analysis", in SHAW). It would be too easy to reply, à la Talbot Donaldson, that if we really had had the archetype of the about 550 witnesses of the *Commedia* since 1966-1967, one would not see the use of a DVD-ROM published in 2010 that only investigates the relationship among 7 mss.

No less disconcerting, and even more naïve, is the immediately preceding hypothesis that the agreement of at least 4 witnesses out of 7 in the DVD-ROM can be traced back to the archetype of the surviving tradition, rather than, as seems beyond doubt here, to the hyparchetype α (to which Ash LauSC Ham Mart Triv can easily be traced):

We might hypothesize that the 'c2' form [sc. the correct form of LauSC including erasures and marginal corrections etc.] [...] is closer to one witness in particular, and one very important witness, the archetype of the whole tradition ("Introduction", Chap. VI. "The Phylogenetic Analysis", in SHAW).

This hypothesis seems indeed perilously close to the fascination of philology from the sixteenth to the early eighteenth century with variants found in most witnesses, instead of the majority of branches, as is required by the mature textual criticism of times closer to our own.

As I hinted above, I believe that most of this section, if not all of it, was written, not by Prue Shaw, who has an insider's knowledge of (Neo)-Lachmannian philology, but by Peter Robinson, a champion of computer-assisted textual criticism, but whose knowledge of the basic rules of Lachmannism, as we have seen, is not equally solid (→ 4.3).

¶ A sort of digital stamp or, if we prefer, conjunctive error that recurs in Robinson's judgments on Neo-Lachmannism is his enthusiastic reprise of a confused and historically unfounded observation by the New Critic and eminent student of Chaucer E. Talbot Donaldson, as in the following passage of Robinson's Introduction to the DVD-ROM, Chap. VI: "There is the argument elegantly expressed by Talbot Donaldson: if one can determine the original at every point, than why bother with any further analysis?". For example, see ROBINSON, "Computer-Assisted Stemmatic Analysis", 73-74 (→ 1.2).

In passing, with all due respect to the brave effort and expertise put by Kane and Donaldson into their edition, the criteria they employed in the classification of the B version of *Piers Plowman* betray a previous misunderstanding of the Neo-Lachmannian method, which discards polygenetic variants precisely because they increase "noise" within the data: "If an attempt to classify manuscripts by recovering their genetic relation is to have any chance of producing accurate results it must observe two principles: that the evidence for genetic relation is agreement in *unoriginal* readings; and that to draw conclusions from any selection of such evidence (made on whatever grounds) is not just potentially but probably misleading. Therefore classification of manuscripts cannot be safely undertaken until the whole text they contain is for practical purposes fixed" ([William Langland,] *Piers Plowman: the B version* [...] An edition in the form of Trinity College Cambridge MS B.15.17, corrected and restored from the known evidence, with variant readings, by G. Kane and E.T. Donaldson, London, The Athlone Press-University of London, 1975, 17). The procedure followed is in many respects subjective and circular: 1. The stemma is implemented in retrospect on the basis of the indiscriminate collection of *all* alleged conjunctive errors; 2. a variant reading, even if polygenetic and equally acceptable (for example, *is/ys so; to/for to; a/the*), is considered a conjunctive error when it does not occur in the text established by its editors. Unsurprisingly, the result is "the almost always conflicting, often obscure quality of the evidence" (p. 19). An obvious consequence of the procedure adopted, such as "the extreme frequency of convergent variation in the transmission of *Piers Plowman* manuscripts"—which is comparable with that of any manuscript tradition of the same sheer volume—is presented as no less than "the second result of the analysis" (p. 63). See also R. Hanna III, "On Stemmatics", in *Pursuing History. Middle English Manuscripts and Their Texts*, Stanford, Stanford UP, 1996, 83-93: 85-89.

Last but not least, I have wondered why, in Robinson's judgment, LauSC (that is, a relatively late and unreliable manuscript, a laboratory product by philologist Filippo Villani, which continuously mixes up variants from different traditions) is closer to the archetype of the *Commedia* than U, that is, a manuscript whose exceptional textual quality has been unanimously acknowledged by the best specialists, from Barbi to Petrocchi and Sanguineti (→ 9). I have likewise wondered why Josephine Brefeld, starting from a small group of 6 copies of the guidebook *Peregrinationes totius Terresancte*, including three incunabula, judged that the late incunabulum of 1481 she designated as NY was "the best representative of the cluster".

¶ In her master's dissertation on the *Peregrinationes*, Elisabetta Armellin has proved instead, using traditional methods, that VERONA, VIENNA and almost all the mss. have more genuine readings than the printed books, with the exception of a few mss. that descend from incunabula, such as LONDON (E. Armellin, *Alcune guide tardotrecentesche e quattrocentesche...*).

To conclude, I have also wondered why William Robins chose the colorless V as the base witness for his Pucci's edition, rather than K or M, which are so much closer to the archetype (→ 4.3).

No generalizations can be drawn from just three cases, and the three scholars do not provide sufficiently clear explanations on this point, but I cannot help thinking that this odd preference for late witnesses carrying the vulgate text (Brefeld even prefers a later edition to the earliest surviving incunabulum) results from a common distorting factor that is built into their programs for the automatic generation of stemmata. Since there is no way to distinguish between agreements in polygenetic variants (the "noise" or "confusion" discussed above), agreements in correct readings (which are genealogically non-significant) and agreements in error (the only significant ones), the software invariably ends up singling out as the best representative of the Most Parsimonious Tree *the witness that is quantitatively "closer" to most of the others*, not the one with less errors. In other words, even a prodigiously long series of good readings in one or two isolated witnesses competing with as many erroneous readings in most copies would be interpreted by the program as a list of individual deviations, or deviations of a small group, and this would be enough to preclude to the most genuine witnesses the role of "best representatives of the cluster", which would be assigned, instead, to the most typical descendants of the most popular vulgate text, copies of copies of copies...

The fact remains that these new frontiers of textual scholarship offer easy access to an enormous amount of data that are extremely interesting for the future of our studies, and stimulate even traditional philologists to suggest corrective measures to make computer-assisted textual criticism more reliable. We should hence be deeply grateful to Peter Robinson, Prue Shaw and their collaborators both for their commitment and for the truly promising results achieved so far.

Bibliographical notes. Dante Alighieri, *Monarchia*, edited by P. Shaw, An electronic edition on DVD-ROM, Leicester-Firenze, SDE Scholarly Digital Editions-Società Dantesca Italiana, 2006; SHAW.

On Shaw's digital editions: P. Chiesa, "L'edizione critica elettronica della *Monarchia*: la filologia elettronica alla prova dei fatti", *Rivista di studi danteschi* 7, 2007, 325-354; P. Trovato, "La doppia *Monarchia* di Prue Shaw (con una postilla sulla *Commedia*)", *Ecdotica* 7, 2010, 193-207; P.G. Beltrami, review of SHAW, *Medioevo Romanzo* 36, 2012, 216-219; M. Giola, review of SHAW, *Studi danteschi* 78, 2013, 381-384; V. Ribaudo, "Nuovi orizzonti dell'ecdotica? L'edizione elettronica della *Monarchia* e della *Commedia* di Prue Shaw", *L'Alighieri* 42, 2013, 95-128, besides Renello's essays cited in the Appendix to the present chapter.

J. Brefeld, *A Guidebook for the Jerusalem Pilgrimage in the Late Middle Ages. A Case for Computer-Aided Textual Criticism*, Hilversum, Verloren, 1994, is sternly reviewed by S.D. Westrem, *Speculum* 72/1, 1997, 116-119, and P. Trovato, "Per le nozze (rinviate) tra storia e filologia. Sulle vulgate di alcuni pellegrinaggi tre- e quattrocenteschi (Leonardo Frescobaldi, Mariano da Siena, Alessandro Rinuccini) e sulle guide di Terrasanta", *Filologia italiana* 3, 2006, 31-76: 66-73. See also E. Armellin, *Alcune guide tardotrecentesche e quattrocentesche per i pellegrini in Terrasanta. Per uno studio della tradizione dei testi*, University of Ferrara, 2008-2009 (unpublished master's thesis, supervised by P. Trovato).

For further perplexities regarding the computer-assisted textual studies produced so far, see REEVE, *Manuscripts and Methods*, 345-346. To conclude, I cannot but acknowledge the new awareness Robinson appears to have developed lately: "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*". (P. Robinson, "What Digital Humanists don't know about Scholarly Editing, and Scholarly Editors don't know about the Digital World", paper presented at the conference *Social, Digital, Scholarly Editing*, University of Saskatchewan, July 11, 2013-

July 13, 2013, accessible online under the title “Why digital humanists should get out of textual scholarship”. My emphasis).

4.6. A computer-using Neo-Lachmannian editor: Ben Salemans

This paper demonstrates that a rather new approach in computerised genealogy is possible and necessary, in contrast to the unscientific tendency of many ‘computerised’ text-genealogists to develop text-genealogical software without a clear theoretical basis [...]. We must not be too impressed by ingenious software that produces text-genealogical trees [...]. *The problem is not drawing a text-genealogical tree, with or without the computer, but developing a “trustworthy” text-genealogical tree [...].* Text-genealogical software, in fact, is a text-genealogical theory in computer shape [...]. When the software produces incorrect results, one or more element in the theory must be inaccurate.

Salemans, “Cladistics or the Resurrection of the Method of Lachmann...”, 5 (my emphasis)

First, when I started my text-genealogical research, I was fascinated by one fundamental question: why do stemmatologists claim that text-historical trees can be built with all kinds of textual variants? I understood how Lachmannians used their specially selected common errors, most often quite eye-catching variants, as hereditary scars passed on to the descendants. Once we have found these scars we can determine the text-historical relationships between the text versions. Unfortunately, the Lachmannian selection of common errors was not clear and repeatable, which made it unscientific. The second phase started when I was studying current, modern alternatives for the method of Lachmann. I was, and still am, surprised by the easy, nonchalant way in which modern inductive stemmatologists use variants. Often the status of variants, the textual differences, seems to have become unimportant to them. *They simply consider each textual difference as an objective, easily observable (objective) fact, although they sometimes exclude small or unimportant variants for unexplained reasons.* They gather these objective facts and introduce them in statistical-mathematical software which builds, in an objective way, a tree out of them. But is such a tree a chain or a stemma, a text-historical tree?

In inductive research, the objective facts must be related to the goal of the research. If I want, for example, to predict the weather, I can gather all kinds of objective facts in and around my house: paperclips, stones, papers, etc. It is obvious that I will not be able to predict the weather with these facts, even though they are *objective*. In other words, a goal-

oriented justification is necessary for the selection of objective facts in inductive research. *I dare to say that, until now, a scientifically necessary justification for the use of all the variants as building stones for historical trees has not been presented in inductive stemmatology. Some inductive, statistical stemmatologists admit that their trees are not historical trees, but trees which show the spread of the variants in the text versions. In that case, I simply do not see the virtue of such trees* (my emphasis).

The writer of these lines is not, as one might think, a traditional philologist with an urge to confront the newer trends, but the Dutch scholar Ben Salemans, who holds a very original and stimulating position in the field of computer-assisted stemmatics. The long quotation above is from SALEMANS, *Building Stemmas*, 8-9, his rich doctoral thesis (of 2000) on the transmission of the *Lanseloet van Denemerken* (a Dutch text of which 2 manuscripts and 12 printed editions, dating between the fifteenth and the eighteenth century, are preserved).

In his dissertation, opportunely titled *Building Stemmas with the Computer in a Cladistic, Neo-Lachmannian, Way*, Salemans succinctly recapitulates the evolution of textual criticism and presents a series of observations that any Neo-Lachmannian philologist could subscribe to. (Significantly, above I referred mainly to Salemans' own criteria, as well as those of my student Caterina Brandoli—which largely agree with his, but are drawn from the observations of traditional philologists—to make clear the difference between variants and significant errors; → 3.1.).

To summarize the content of his dissertation, I will quote its introduction:

This study covers five themes: 1. it offers a global introduction to stemmatology; 2. it is interdisciplinary and pays attention to biological-cladistic genealogical concepts; 3. it discusses hot text-genealogical items like contamination and the type-2 limitation; 4. it is concentrated on the *recensio*, not on emending text passages; 5. it pays attention to differences between deductive and inductive text-genealogical research. These five themes are the environment in which the main subject of this book is discussed: my method to build text-genealogical trees, which I call *automated deductive stemmatology* [...]. This subject covers the largest part of the book: in chapter 3 the basic text-genealogical principles and characteristics are explained and developed, in chapter 4 they are applied and in chapter 5 they are evaluated (SALEMANS, *Building Stemmas*, 8).

And, again in his words:

In chapter 4, the fourteen *Lanseloet van Denemerken* texts will be entered into the computer software that I wrote in the computer language SNOBOL/Spitbol. As far as I know, this was the first time that a computer was able to analyse texts according to a deductive text-genealogical theory. The scientific advantages of the computer as analysis instrument are evident. Once the theory (the characteristics of variants) has been programmed into the computer, it is repeatable, it will be performed consistently and it can be checked afterwards. The main advantage of letting the computer perform a theory is that we can be sure that, in our *Lanseloet* case, it will apply all the programmed characteristics to all the ten thousands of variants consequently and quickly. It is almost impossible to treat such a large amount of variants by hand without making mistakes. The automation of the procedure to detect the variants to build a text-genealogical tree with, is also important for inductive text-genealogists, because they usually work with variants which are classified by hand (SALEMANS, *Building Stemmas*, 10).

Of course, Salemans, too, agrees with an axiom of the practitioners of the new method (and of Quentin before them), that is, the distinction between the arranging of witnesses in a chain and the rooting of the said chain. However, while in the most common applications of cladistics to philology, because of an aversion to the notion of error, the very delicate phase of rooting tends to remain in the shade (Robinson, for example, refers the reader to the databank of variants), the Dutch scholar discards non-significant variants and only works with selected material, i.e., pairs of presumably monogenetic variants.

¶ For precision's sake, Salemans, too, prefers to replace the traditional designations *correct* or *genuine reading* and *errors* with more politically correct expressions such as *original common readings* and *derived non-original common changes*, but, as I have already remarked above, the substance remains the same.

Lexical innovations aside, the result is precisely "a list of potential type-2 variation formulas from which a text tree can be built". And simply by reading through the list of significant variants selected by Salemans' program—largely coinciding with the typologies of traditional philologists—a reasonably experienced reader would be able to spot, almost unfailingly, a number of indicative innovations, and thus root the chain.

As the author argues, "*these formulas have to be judged systematically by philologists*, especially on their grammatical [...] and historical-philological adequacy" (Salemans, "Cladistics or the Resurrection of the

Method of Lachmann...”, 36, my emphasis). The importance of the philologist’s judgment is reasserted, albeit in more nuanced tones, in Salemans’ dissertation:

The advantage of creating a stemma in this ‘modern’ way is that the interpretation (judgment) of the originality of a reading is not necessary to draw the chain. In this way, the main problem with the method of Lachmann is solved for the most part. However, the second step, the orientation of a chain into a stemma, is still based on the knowledge of the (un)originality of usually two or three readings (SALEMANS, *Building Stemmas*, 20-21).

Likewise, while other digital editors snub *lectiones singulares* (unique readings) in witnesses assuming that, since these are found in abundance in the whole tradition, they are useless, Salemans, by his own admission, recovers them in a second stage by using them, as an orthodox Neo-Lachmannian would, as separative errors:

First, I will use type-2 variation to build the chain. Then, I will try to find out whether some of the texts might be intermediate nodes. I will use type-1 variations for that purpose (ID., *Building Stemmas*, 27).

At a second reading of Salemans’ dissertation, one gets the impression that—aside from his competence as a programmer and his skill in incorporating the basic rules of Neo-Lachmannian stemmatics into his program—he shows an uncommon ability to make the most of all his traditional philological knowledge without giving up the advantages offered by the new technologies.

Aside from inevitable minor divergences on other questions—such as, as usual, the notion of *archetype* (ID., *Building Stemmas*, 17-18, 264-265) or the reification of the opposition between genealogical, that is monogenetic, variants, and parallelisms, that is polygenetic variants (ID., *Building Stemmas*, 30), which should be decidedly toned down (there are more or less monogenetic variants and more or less polygenetic ones)—I only feel compelled to express two reservations, which do not intend to question in any way the usefulness and importance of Salemans’ work.

First observation. Salemans, too, believes that the only usable significant variants are binary ones, where a reading *y* is opposed to a reading *z*:

I am convinced that complex variations, with three or more competitive variants, are almost useless for the development of chains and stemmas. In other words, I think that philologists should in general only work with the mentioned special type of variance. This severe limitation is called the *type-2 limitation* (ID., *Building Stemmas*, 5 e passim).

Now I understand that, in the present state of technology, binary variants are more compatible with the “logic” of computers. It is to be hoped, however, that computer-assisted philologists may also avail themselves of complex variations, with three or more alternatives. Wherever possible, an analysis of complex variations (i.e., diffraction, lucidly theorized by Contini: → 3.2) can provide valuable clues for the orientation of a chain and the building of a stemma. And I think that Salemans himself could easily incorporate this *desideratum* in his programs.

My second reservation, however, concerns a more general aspect of Salemans’ approach. Very honestly, Salemans repeatedly states that his work essentially concerns only one stage of the investigation, namely, *recensio* (in which he includes, like Maas, classification and stemma-building) and the consequent reconstruction of the archetype. But what happens if the archetype is deeply flawed? Do we nevertheless stop at this level for fear of making mistakes, and give the reader a text full of absurdities? Or do we admit that the usefulness of the PC necessarily ends here, and address the problem by formulating a series of hypotheses founded on our knowledge of the language, of poetical conventions, and of the author’s stylistic habits, and on the cultural and historical data in our possession, and thus allow other, more accomplished scholars to confirm these hypotheses or falsify them? We shall return to this problem in Chap. 6.

At any rate, among the studies I am familiar with, Salemans’ seem to me to be, to date, the most mature and promising fruit of computer-assisted philology—which is admittedly in its infancy and is certainly destined to progress. I conclude this chapter with a recommendation by Alfredo Stussi, which seems to me to be fully in line with Salemans’ own aspirations to a computerized philology with “a clear theoretical basis”:

Digital editing should not slip from the grasp of experts in textual criticism. It should not be monopolized, that is, by commercial interests and computer programmers who may be very skilled, but lack philological experience. Otherwise, what will happen is that a bad edition will become popular, maybe even become the vulgate text, and this only because, once

the traditional paper medium has been replaced by the magnetic one, the wonder of the medium distracts from careful evaluation of the message (Stussi, *Relazione conclusiva...*, 289)

Bibliographical notes. B. Salemans, “Cladistics or the Resurrection of the Method of Lachmann”, in *Studies in stemmatology*, 3-55; id., “The old text-genealogical method of Lachmann updated with the help of cladistics and the computer”, in NOF, 115-125; SALEMAN; B. Salemans, “The remarkable struggle of textual criticism to become truly scientific...” (2002), in http://www.know.nl/agenda/pdf/Abstract_ben_salemans.pdf.

Stussi's words are from A. Stussi-E. Raimondi, “*Relazione conclusiva*”, in NOF, 289-299.

Appendix. On the programs used for the digital edition of the *Monarchia* and the *Commedia*, by Gian Paolo Renello

The DVD edition of the *Monarchia* captured the attention of scholars because it included a number of noteworthy innovations. Their primary intention was to favor complex searches and analyses of Dante's treatise, well beyond the elementary use of the search function, for example, of a word-processing program.

- a) These innovations included, on the one hand, the application of the methods of cladistics, with their unrooted phylogenograms, to the philological field as a means to determine the relationships between 21 out of 22 witnesses of the treatise (the copies included in the digital edition are provided in diplomatic transcription, in image, and, of course, in normalized form).
- b) On the other hand, they included the introduction of a hypertext providing an information retrieval system capable, in theory, of investigating every single aspect of the tradition by employing a specific query language in the database, capable of accounting for every possible occurrence of every single word in the copies of the text.

For a discussion of the former innovation, summarily examined in the above pages, I refer to Renello, “L'edizione della *Monarchia*...”; id., “A proposito della *Monarchia*”; Ribaudo, “Nuovi orizzonti dell'ecdotica? L'edizione elettronica della *Monarchia*...”, where the potential contribution of cladistics to philological studies is discussed precisely starting from the very edition cited above. For the latter, aside from referring the reader to the works just cited, I would make the following observations.

The system used for reading, analyzing and querying the documents is based on two programs: the first, called *Anastasia*, recreates a web server on a computer, allowing navigation in the text “as if” one was on the Internet. The second is *Vbase*, the query engine created by Peter Robinson, through which the webserver provides the information requested by the reader/user. This, in my opinion, is the most useful and interesting tool offered by the DVD.

It is *Vbase*, for example, that allows all the variants in the text to be singled out, thus doing an excellent collating job, which, thanks to the computer technologies available today, can all be viewed simultaneously on screen, line by line. And it is *Vbase* that allows a comparative examination of variants in individual witnesses, or groups of witnesses selected at will. In its turn, the behavior of different witnesses in relation to a specific variant can be represented graphically by using so-called “variant maps”, that is, phylogenograms, or orientable diagrams showing the closeness or distance of witnesses with respect to the variant in question. Finally, it is noteworthy that the DVD also contains, along with transcriptions of all known witnesses at the time of publication (including the *editio principis* of 1559), page-by-page photographic reproductions, with which the transcriptions can be cross-checked.

* * *

This digital edition of the *Monarchia*, however, also has some significant shortcomings, both in terms of content and usability. As regards the former aspect, suffice it to say that the DVD totally lacks reproductions of ms. H, and the sheets from 4r to 9v of ms. N.

¶ This used to be true also of the online version of the treatise, but was later remedied.

As to the latter aspect, aside from individual coding errors that do not always offer the user an accurate overview of the behavior of the witnesses (as Renello, for example, points out in “L’edizione critica della *Monarchia...*”, 157 and note 2, and id., “A proposito della *Monarchia...*”, 155-156), both programs, *Anastasia* and *Vbase*, show serious flaws in their functioning and in memory management. Often, even in response to not especially complex queries, the system returns error pages after sometimes exasperating delays. Additionally, the preconfigured queries are incomplete: the search for variants of β overlooks witness E, its *descriptus*

R, and U; that for non- β witnesses overlooks A₁, possibly because the critical edition, although it clearly distinguishes it from witness A₂, treats both only in terms of whether they belong to family β or not. No information about these issues and their possible solutions is provided in the DVD, leaving the reader in doubt regarding the editors' approach in preparing the digital version of the text.

Of course, these shortcomings do not change the fact that this remains a very interesting project, which only requires a small number of improvements, certainly beginning from *Anastasia*, a program that became *open source* in 2005, but since then has practically never been updated. The program lacks, for example, functionalities such as the ability to export the results of queries in the XML format, or in other user-manageable formats, to avoid the need to repeat queries. It also lacks tools for conducting more sophisticated statistical text analyses.

The DVD of the *Commedia* improves on some of the above limitations of the edition of the *Monarchia*, especially as regards the graphic interface and waiting times (but, as the reader will remember, the witnesses included in this edition are only seven, about a third of those of the *Monarchia*, with the addition of Petrocchi and Sanguineti's critical texts).

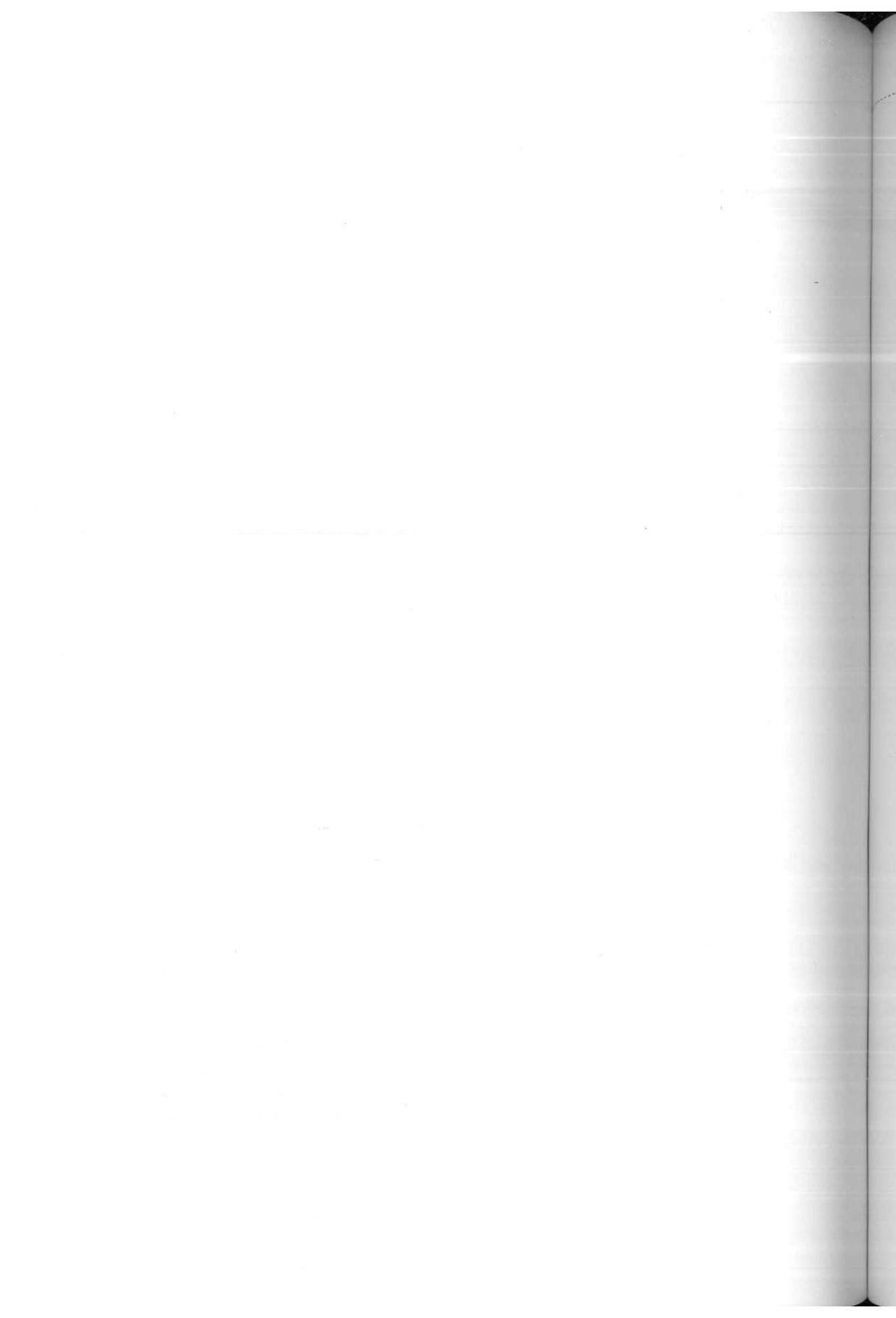
As to the interface, I find it an excellent idea to create a link for every word of the text in each witness. With a simple click on any word, a window opens showing the variants of all other witnesses at that variation place. The user thus does not need to repeat the search going through actual collation, which is now carried out in a different way.

This is not a secondary improvement, because there are two main problems with digital editions, and especially critical ones: creating an elegant, fast and reliable data processing system, and providing access to it through an intuitive, simple and equally fast graphic interface.

However one intends to deal with these issues, a critical electronic edition is, in my opinion, a work in which authoriality is split up among different roles, ranging from the work of the philologist when the text is constituted (and here the methodological problems discussed in this manual come into play, such as Neo-Lachmannism vs. cladistics), to the encoding phase, the creation of query tools, and finally the creation of an adequate graphic interface, which, as a rule, will be the only part of the work immediately perceptible to the user. Each of these roles requires specific skills, but none can "function" without being closely coordinated with the others.

P.S. (2014). After a couple of years, I have resumed the *Monarchia* and *Commedia* DVDs. I must admit that the problems encountered since I first availed myself of them punctually recurred. Particularly, one must face even longer waiting times and a greater number of timeout errors. This is especially true for the *Monarchia* DVD, which seems to be more defective than the subsequent electronic edition of the *Commedia*. Nevertheless, both DVDs are affected by previously unencountered technical problems concerning Windows 7 and Windows 8.1. Once the program Anastasia is launched, the running application is inexplicably closed by the system. According to Robinson this is due to a well-known Windows bug. He illustrates the problem himself at <http://www.sd-editions.com/anastasia/fixes/>, and explains that once encountered, the problem remains insoluble.

Bibliographical notes. G.P. Renello, “L’edizione critica della ‘Monarchia’, *Italianistica* 40, 2011, 141-180; id., “A proposito della ‘Monarchia’. Note in margine al ritrovamento del ms. Additional 6891”, *L’Alighieri* 53, 2013, 115-156; V. Ribaudo, “Nuovi orizzonti dell’ecdota? l’edizione elettronica della *Monarchia* e della *Commedia* di Prue Shaw”, *L’Alighieri* 42, 2013, 95-128.



5. THE CRITICISM OF LINGUISTIC FEATURES IN MULTIPLE-WITNESS TRADITIONS

5.1. Core and patina

Everybody knows that the first and essential difference between the medieval tradition of classical texts and that of vernacular texts lies in the radically different linguistic tradition the transcriber operates in. In vernacular traditions, scribal interventions on the language of their exemplars are in fact customary and organic procedures. The language is a living one and remains such for the whole course of the manuscript tradition, and also in the first printed books [...]. Today, the textual scholar is increasingly drawn to questions of linguistic and cultural history and increasingly forced to develop the required competence, and the historian of the language is increasingly involved in philological work.

Gianfranco Folena, "Filologia testuale e storia linguistica...", 59-60

Many words and proper names are written in more than one way, and whatever system we adopt, it will always be too rigorous.

HUYGENS, 31

As the reader will probably have gathered already, it is just about impossible to practice textual criticism at an acceptable level, whether on works in Latin, French, or German, without adequate knowledge of the evolution over time and space of the language of the text being studied, and of the linguistic varieties of its copies. Texts written in "living" languages, in particular, pose special linguistic problems, because, as Paris explained so well, and as Folena and Varvaro have further stressed, the copyists continually modify the language (and sometimes also the style) of these texts, adapting

them to the usage of their time and region. According to Contini, formal, that is linguistic, reconstruction (i.e. what Gaston Paris called *formes* and Anglo-American textual bibliographers call *accidentals*) is often more complicated than the reconstruction of textual substance, which the same Paris called “critique of readings” (CONTINI, 64–65 [= ID., *Frammenti*, I, 59]):

There are no limits to the linguistic information useful to the editor of a text (I will mention at least the identification of the dialect stratum to which formal variants responsible for substantive errors belong: CONTINI, 149 (= ID., *Frammenti*, I, 75)).

In the tradition of a much-copied work like the *Commedia*, essentially written by Dante in the Florentine idiom used in his generation, with moderate hospitality to archaisms, we find, for example, alongside the copious Florentine production, many copies where the original language is adapted to north Italian linguistic usage, others with Umbrian or Marche infiltrations, or features that are Tuscan but not Florentine (e.g., Pisan or Sienese), and others (overall only a few) revealing the hand of southern Italian scribes. Furthermore, in the light of the very detailed linguistic studies of early varieties of Tuscan conducted by Arrigo Castellani and his school, we know that various Florentine forms are anachronistic for Dante’s time, or unlikely to have been used by him, and must necessarily stem from the practice of Florentine copyists of later generations.

Similar linguistic variability is observable in France, as early as its earliest hallowed works. A fine example of the conservatism of lateral areas (confirming Pasquali’s criterion: → 1.6) is the earliest version of *Roland*, its archaic version in *laissez assonancées*. This is preserved in the Oxford manuscript, written in Anglo-Norman, “daté des environs de 1125 [...] ou de la seconde moitié du XII^e siècle” (Segre), and its authenticity is largely confirmed by manuscript V4 in the Marciana in Venice, a northern Italian product (“franco-vénitien du XIV^e s.”). The other preserved copies, transcribed in France or northern Italy—all in more modern *laissez rimées*, although here and there they retain traces of their original metrical scheme—conspicuously reveal their provenance from different areas and linguistic milieus (Franco-Venetian, “francien avec des formes lorraines sporadiques”, from the west, “probablement bourguignon”, “avec des traits de l’est”, “probablement lorrain”, “transcrit dans l’est ou le nord-est de la France”, not to mention a “traduction norroise”). To these we could easily add a number of equally variable Italian, French, and Spanish examples.

It is expedient to designate as *core* (French *fonds*, It. *fondo*, literally ‘bottom’) the original language of a text, and as *patina* (It. *patina* or *colorito* or *vernice*, sometimes also in the plural, as if referring to several layers of painting added to the original language) the linguistic sedimentation that is certainly due to the copyists—although ever since the time of Paris and Rajna, the first term has occurred more frequently in French studies, the second in Italian.

Bibliographical notes. On the relationship between textual form and substance: PARIS, 7ff.; G. Folena, “Filologia testuale e storia linguistica”, in SPCT, 17-34, later in id., *Textus testis. Lingua e cultura poetica delle origini*, Torino, Bollati Boringhieri, 2002, 59-77; A. Varvaro, “Critica dei testi classica e romanza. Problemi comuni ed esperienze diverse”, in ID., *Identità*, 567-612.

Besides the essay by Folena quoted above, other studies of the connections between philology and linguistic history include CONTINI, 149-173; A. Stussi, “Filologia e storia della lingua italiana”, in id., *Lingua, dialetto e letteratura*, Torino, Einaudi, 1993, 214-234; TROVATO, “Storia della lingua e filologia”; MENGALDO, “Filologia testuale”.

On the copies of *Roland*, see *La Chanson de Roland* S, 47-56.

On the geographical distribution of part of the tradition of the *Commedia*, see, most recently, NP, 49-94, 229-241; S. Bertelli, *La tradizione della Commedia. Dai manoscritti al testo. I. I codici trecenteschi (entro l'antica vulgata) conservati a Firenze*, presentazione di P. Trovato, Firenze, Olschki, 2011 (a new volume is forthcoming). On Dante’s language, see P. Manni, *La lingua di Dante*, Bologna, il Mulino, 2013.

On the metaphor of *patina*, P. Trovato, “Un campo metaforico del linguaggio filologico. *Colorito, patina, strato e simili*”, *Lingua e stile* 37, 2002, 287-313.

Some recent studies of the question in the field of Romance literature can be found in *Medioevo Romanzo* 33/1, 2009, 3-72, and in *Transcrire et/ou traduire. Variation et changement linguistique dans la tradition manuscrite des textes médiévaux*, Actes du congrès international, Klagenfurt, 15-16 novembre 2012, publiés par R. Wilhelm, Heidelberg, Winter, 2013.

5.2. The formal reconstruction of texts

While only a sophisticated Lachmannism is adequate as far as the recovery of substance is concerned, the problem of formal reconstruction remains more open, certainly not for lack of progress, but as a consequence of very diverse historical situations. We should not, however, fail to highlight, among the relatively conservative factors, an increased cultural respect for graphic phenomena.

CONTINI, 173 (= ID., *Frammenti*, I, 97)

As Paris lucidly explained, the genealogical method, so useful for the reconstruction of the substance of a text, is useless when it comes to its linguistic characters. Let us take, for example, a Florentine work disseminated all over Italy, whose surviving tradition can be schematized with a five-branched *stemma codicum*, where the branches are represented, respectively, by witnesses A (Florentine) B (Ferrarese) C (Milanese) D (Neapolitan) E (Neapolitan). It is easily imaginable that the two codices produced by the Neapolitan copyists, D and E, will agree in a large number of polygenetic variants, such as the reduction of *quella* to *chella*, metaphonies such as *niro* and *niri* for *nero* and *neri*, and metaphonic diphthongization (*castiello* instead of *castello*), etc. As I have already argued above, these agreements (fatally overestimated by many computer-assisted editors) are irrelevant for the purposes of classification.

If the distinction between critique of readings (to be addressed with the help of the *stemma*) and critique of forms (to be addressed by other means) authorized arbitrary attempts by nineteenth-century Romance philologists to reconstruct the language of the original, Bédier's criticism, in this case fully justified, providentially put a stop to the practice of "language reconstructions which had been customary since 1872" (FOULET-SPEER, 28).

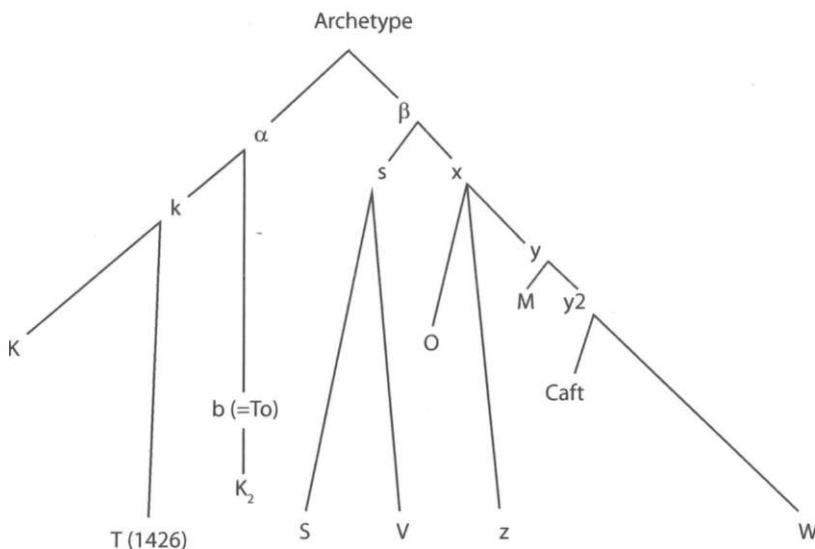
Once the *stemma*—that is, the genealogy of the surviving tradition—has been designed, the simplest solution for formal reconstruction, as well as the solution most respectful of the historicity of individual witnesses—which goes back to Paul Meyer—, is to choose as a base manuscript or copy text (It. *testimone base*) a ms. as close as possible, both from a geolinguistic and a chronological standpoint, to the author's language. Other conditions being roughly equal, we will prefer, of course, a stemmatically "high-placed" witness, one not too different in its substance from the text to be edited.

I have just mentioned the Oxford *Roland*, "transposition en français insulaire d'une oeuvre composée dans une idiome peut-être très différent", for which it is plausible to imagine that copyists from different areas placed themselves "entre le poète et son tardif copiste anglonormand" (Bédier, "De l'édition...", 489). In this case, given the impossibility of reconstructing the original language of the text, the most reasonable solution is that rightly advocated by Bédier and adopted by Segre: to preserve the linguistic *facies* of ms. O.

In the case of the *Lais* by Marie de France, possibly the most famous author of French poems in England, Anglo-Norman is not a *patina*, but the *core*. Fortunately for posterity and philologists' peace of mind,

the largest and most genuine collection of her work is preserved in H (London, British Library, Harley 978), a thirteenth-century manuscript produced in the British Isles, and hence one that is also linguistically reliable (remarkably, only one other surviving ms. of the most widely copied poem in the collection, the *Lai de Lanval*, i.e. C, is in Anglo-Norman; S and P have, respectively, a Francien and a Picard patina).

Let us consider instead—of course, after Barbi’s massive *eliminatio codicum descriptorum* has made this possible—the stemma of a Florentine work, Dante’s *Vita Nuova*, drawn up by Barbi in 1907 and retouched in minor particulars by Trovato in 2011:



Since the very old β -family is Umbrian, and the earliest Florentine ms., K, seems to be only some thirty years later than the lost original (1296?), the most recent editor, Stefano Carrai, wisely adhered to the language of the witness he chose as basic text, that is, K (the famous Chigi ms. L VIII 305, a fundamental source for the Stilnovo and the poetry of Cecco Angiolieri).

\mathcal{C} K is by the hand of the same Florentine copyist who worked in the Thirties of the fourteenth century and is known as the “main hand” of the *Cento* group: 19 copies of the *Commedia* by this copyist are known at present (see, most recently, Bertelli, *La tradizione...*, 84-95).

In the case of the *Principe*, or rather *De principatibus*, by Machiavelli, a Florentine himself, the situation is somewhat more complicated. If Kristeller's *Iter italicum* had not encouraged the study of an important Florentine ms., D—it alone, now in Munich (Universitätsbibliothek, 4° cod. ms. 787), seems to account for one whole branch of the tradition, but the reciprocal relationships between the main branches are uncertain—we would need to choose between the Gotha codex, which is also very correct and independent of the others, but with a decisively northern patina, and the Florentine vulgate text y, constituted by some fifteen witnesses that share, among other things, a number of extensive lacunae. Giorgio Inglese, the most recent editor of Machiavelli's treatise, hence chose ms. D as his basic witness.

For Dante's *Convivio*, composed in the first decade of the fourteenth century, but whose surviving tradition is much later (no earlier than the end of the 1300s), Franca Brambilla Ageno used not one but four mss., chosen among the most conservative ones, working from the reasonable—but strictly speaking not irrefutable—assumption that the language actually used by Dante for this encyclopedic treatise was only moderately Latinizing.

To establish sounds and forms we use, of course, the earliest codex of α, Vb [Città del Vaticano, Biblioteca Apostolica Vaticana, Barberiniano Lat. 4086] and the best and possibly earliest codex of β, L⁴ [Firenze, Biblioteca Medicea Laurenziana, XC sup. 134]. Unfortunately, these two manuscripts regularly diverge in their spelling and morphology. Vb is resolutely vernacular in its graphic habits [...], but often retains earlier forms than those found in L⁴ [...]. L⁴, in its turn, is more regular and constant in orthography and forms, but tends to introduce recent forms and shows frequent Latinization [...]. The issue of choosing between the two series is especially delicate, because one may indeed believe that D[ante] used Latinizing forms in a treatise entirely modelled on Latin treatises, but one could suspect a Humanist influence on the spellings of L⁴, which is rather later than Vb.

For these reasons, we add to Vb and L⁴ two manuscripts which, independently of their position in the stemma, in some cases have proven especially conservative, that is, Ash [Firenze, Biblioteca Medicea Laurenziana, Ashburnham 842] and L [Firenze, Biblioteca Medicea Laurenziana, XL.39] (F. Brambilla Ageno, "Introduzione" to Dante Alighieri, *Convivio...*, I II, 901).

The situation of thirteenth-century Sicilian school is even more complicated, as is that of the *Nencia da Barberino* in 20 octaves—which scholars usually ascribe, on reasonable grounds, to Lorenzo il Magnifico, and which is, linguistically, a parody of the vernacular of Mugello—or the untitled comedy (known simply as *Comedia*) by the fifteenth-century Sicilian author Caio Ponzio Caloiro. In all three of these cases, in the course of transmission copyists from other geographical areas or with different linguistic sensibilities tended to obliterate the original linguistic character of the text, substituting forms that deviated less from what they were used to for many forms of the stylish Sicilian used by lyrical poets at the court of Frederick II, of Lorenzo's artificial Mugellan vernacular, and of Caloiro's colloquial Sicilian. This is why the original linguistic character of these texts is only partially preserved. But the fact that Sicilian or Mugellan forms are found, more or less regularly, in both branches of these traditions (which are all two-branched), guarantees that they are indeed *core* (that is, linguistic features that need to be highlighted and preserved, as far as possible) and not *patina* (which should be removed, whenever possible).

Hence the solution Oiva J. Tallgren suggested and Contini adopted for the lyrics of the Sicilian Giacomo da Lentini—which is handed down, with very partial exceptions, in manuscripts produced in Tuscany—viz. to reproduce these poems “in their traditional garb [i.e., in the language of the manuscript tradition, that is, that of the Tuscanized version handed down in the surviving thirteenth-century *canzonieri*], but with the highest degree of Sicilianness directly attested in the codices” (PD, I, 47). Indeed Contini incorporated in the text graphic and phono-morphological Siciliannisms, even when preserved only in a single codex, since these singular readings cannot be a result of the initiative of Tuscan copyists, but must necessarily go back to earlier nodes in the tradition.

Similarly (we owe this observation to Barbi), Florentine forms preserved in Umbrian or northern manuscripts of Dante's Italian works cannot be *patina*; they must go back, at the very least, to the exemplar used by the copyists and, if they are well attested in the upper levels of the stemma, to the archetype. For example, most of the copies of the *Commedia*, which are dominantly Florentine, have the non-Florentine forms *ternaro* and *Cavrara / Cravara / Cravrara*. Only a few northern and genealogically very high mss. have the phonetically Florentine forms *ternaio* and *Capraia*. Since Dante died in Ravenna and it is from there

that the circulation of his poem first began, *ternaro*, *Cavrara* etc. are explainable as unconscious modifications by some northern copyists, which were then inherited by the subsequent Tuscan tradition (we can confidently reject as anachronistic and non-parsimonious the alternative idea that *Capraia* and *ternaio* are arbitrary hyper-Florentinisms polygenetically produced by northern copyists of the fourteenth century).

“Diverse historical situations” (Contini), which change for each text, make it impossible, as the reader will have surmised, to provide a universally valid recipe for linguistic reconstruction. But manuals provide a vast range of examples of decisions made by the best editors on these matters. It is thus not difficult to adapt solutions that have already been tested, at least in part, to the peculiarities of the tradition one is studying.

Generally applicable criteria include, in my opinion:

1. No reconstruction of the linguistic character of a text based on the stemmatic distribution of forms is admissible;
2. It is advisable to hold in the highest regard, until the contrary is proved, the anomalies found in the manuscript which the scrutiny of tradition has reasonably induced us to select as our basic text as far as language is concerned (Varvaro, “Autografi non letterari...”, 267)

Bibliographical notes. On how philologists have dealt with linguistic character from Paris to the present day, see P. Trovato, “Da Gaston Paris ai New Philologists Qualche riflessione sul trattamento della veste linguistica nelle edizioni di testi romanzi”, in *Transcrire et/ou traduire. Variation et changement linguistique dans la tradition manuscrite des textes médiévaux*, Actes du congrès international, Klagenfurt, 15-16 novembre 2012, publiés par R. Wilhelm, Heidelberg, Winter, 2013, 17-27.

The quotations about *Roland* are from J. Bédier, “De l'édition princeps de la *Chanson de Roland* aux éditions les plus récentes. Nouvelles remarques sur l'art d'établir les anciens textes”, *Romania* 63, 1937, 433-469; 64, 1938, 145-244 and 489-521.

On the *Lai de Lanval*, Marie de France, *Le lai de Lanval*, Texte critique et édition diplomatique des quatre manuscrits français par J. Rychner [...], Genève-Paris, Droz-Minard, 1958; *Les lais de Marie de France*, publié par J. Rychner, Paris, Champion, 1978.

On Giacomo da Lentini: *I poeti della scuola siciliana*, I, *Giacomo da Lentini*, edizione critica con commento a cura di R. Antonelli, Milano, Mondadori, 2008, XCI-XCII (Antonelli follows Tallgren and Contini's approach). For the other Sicilian poets, however, “no attempt was made to select [...]

apparently more Sicilianizing forms”, because, while “Giacomo has a dominantly multi-witness tradition, [...] most of the other Sicilians [...] [have] a dominantly single-witness tradition, which involves not necessarily identical editorial strategies” (*I poeti della scuola siciliana*, II, *Poeti della corte di Federico II*, edizione critica con commento diretta da C. Di Girolamo, CXI, CXVIII).

On the *Vita Nuova*, see Dante Alighieri, *La Vita Nuova*, per cura di M. Barbi, Firenze, Società Dantesca Italiana, 1907, or *La Vita Nuova di Dante Alighieri*, edizione critica per cura di M. Barbi, Firenze, Bemporad, 1932; S. Carrai, “Quale lingua per la *Vita nova?*”, *Filologia italiana* 4, 2007, 39-49; Dante Alighieri, *Vita nova*, introduzione, revisione del testo e commento di S. Carrai, Milano, Rizzoli, 2009; P. Trovato, “In margine a una recente edizione della *Vita Nuova*. Schede sulla tradizione del testo”, *Studi e problemi di critica testuale* 81, 2010, 9-15. On the copyist of K, S. Bertelli, *La tradizione della Commedia. Dai manoscritti al testo. I. I codici trecenteschi (entro l'antica vulgata) conservati a Firenze*, presentazione di P. Trovato, Firenze, Olschki, 2011.

On the *Convivio*, Dante Alighieri, *Convivio*, a cura di F. Brambilla Ageno, Firenze, Le Lettere, 1995, 2 vols. (divided into 3 tomes).

On *Nencia*, Lorenzo de’ Medici, *Opere*, a cura di T. Zanato, Torino, Einaudi, 1992, 155-174. On Caliro, B. Baratelli, F. Brugnolo, “Un siciliano a Venezia. Per un’edizione della ‘Comedia’ di Caio Ponzio Caliro”, *Bollettino del Centro di studi filologici e linguistici siciliani* 16, 1990, 19-66.

On the *Principe*, Niccolò Machiavelli, *De Principatibus*, testo critico a cura di G. Inglese, Roma, Istituto storico italiano per il Medio Evo, 1994.

On the *Commedia*, NP, 709; P. Trovato, “Un problema editoriale: il colorito linguistico della *Commedia*”, in *Storia della lingua italiana e filologia*, 73-96.

Over the last fifty years, there have also been some attempts to use stemmas for a reconstruction of linguistic features, from Giuseppe Porta (Anonimo Romano) to Guglielmo Gorni (*Vita Nova*). These attempts are mainly interesting for having stimulated the formulation of new arguments against this approach, which seems illusory: see for example Livio Petrucci’s review of the Porta editions, *Studi Medioltini e Volgari* 28, 1981 (actually 1983), 207-225; A. Stussi, “Dialettologia, Storia della lingua, Filologia”, *Rivista italiana di dialettopologia* 11, 1987, 101-124; TROVATO, “Storia della lingua e filologia”; V. Formentin, “Approssimazioni al testo e alla lingua della *Cronica d’Anonimo Romano*”, in *Leggere gli apparati. Testi e testimoni dei classici italiani*, edited by G. Raboni, Milano, Unicopli, 2012, 27-71. Some useful examples and observations, in spite of the “apologetic” tone of the article, can be found in G. Gorni, “Restituzione formale dei testi volgari a tradizione plurima. Il caso della *Vita nova*”, *Studi di filologia italiana* 56, 1998, 5-30.

The final quotation is from an important article by A. Varvaro, “Autografi non letterari e lingua dei testi (sulla presunta omogeneità linguistica dei testi)”, in *La critica del testo. Problemi di metodo ed esperienze di lavoro*, Atti del convegno di Lecce, 22-26 ottobre 1984, Roma, Salerno Ed., 1985, 255-267.

5.3. Some problematic cases

It was the philology of idealism that struck the most decisive blows against the indiscriminate custom of translation (especially of langue d'oïl texts) based on the localization of authors [...]. Attention was drawn to the languages called, with a Dantesque word, *illustri* [...] or, with a reference to antiquity, *koináí*, or, finally, *scriptae* (Gossen) [...]. This demystified the efforts of so many German graduate students, who tended to surmise birth in *Grenzgebiete* (frontier districts) [...] for legions of writers of an epoch who incorporated features of their own [dialect] [...] within the framework of another literary dialect [...]. Pure linguistic systems revealed themselves to be relatively rare, and in any case to be a working hypothesis to be handled with the greatest caution.

CONTINI, 40 [= ID., *Frammenti*, I, 36-37]

Whereas distinguishing between *core* and *patina* is relatively simple when we know the name and homeland of the author, in other cases it can be very hard to determine which linguistic stratum belongs to the author and which to the copyist(s).

In the case of a work that was very popular from the fourteenth to the sixteenth century, the book of (fantastic) travels attributed to the fictional English knight John Mandeville (Jean de Mandeville), it was long believed that its Italian translation, of which manuscript copies and printed editions from a variety of places survive, was a Tuscan work with local patinas added by copyists from peripheral areas, especially in northern Italy. Recent studies by Giulia Pellecchia have confirmed a suspicion I originally conceived because I was struck by the fact that some north Italian words (such as *fiamada* "cow dung", *oredelli* "borders") were preserved in Tuscan manuscripts, and was hence inclined to ascribe the translation to a Northern center with a receptive attitude towards French literature, such as late fourteenth- or early fifteenth-century Ferrara. Pellecchia, while wisely using the short cut of *loci critici*, has produced a rigorous classification of the whole Italian tradition known to us, proving, among other things, that the Florentine ms. FN, the basic text of the nineteenth-century edition by Francesco Zambrini, is a *codex descriptus*, copied from a Florentine incunabulum, which descends in its turn from an intricate transmission of northern Italian printed books and, earlier, manuscripts.

The linguistic mix of the monumental *Spagna Ferrarese* (a favorite repertoire of chivalric situations and motifs for Boiardo's *Inamoramento de Orlando*) is decidedly problematic. Long regarded as a sort of northern

translation of a lost Tuscan *Ur-Spagna*, the poem has recently been presented, instead, by its editors Gritti and Montagnani as an autonomous creation by a northern writer who had some knowledge of the Tuscan tradition of the *cantari*, and especially of Antonio Pucci. It is thus quite understandable that solid scholars such as Lucia Bertolini and Giovanni Palumbo, who have briefly re-examined the main elements in favor of each of these two alternatives, recommend further investigation.

A very complicated situation, such as to provoke doubts or second thoughts even in scholars of the caliber of Gianfranco Contini and Maria Corti (but at the time the standard collection for Early Venetian published a few years later by Alfredo Stussi—*Testi veneziani del Duecento e dei primi del Trecento*—was still not available), is that attested in codex Hamilton 390, today in Berlin, formerly owned by the Saibante family. The Saibante-Hamilton manuscript (by a single thirteenth-century hand) is an exceptionally rich and old collection where many of the earliest literary works in northern Italian are gathered. The collection includes, among other things: *Proverbia que dicuntur de natura feminarum* (which most believe to be Venetian), the works of Uguccione da Lodi and Gerardo Patecchio (from Cremona), and northern translations (also believed to be Venetian) of the *Disticha Catonis* and the *Panphilus*.

Contini titled the introduction to his well-known 1960 edition of the *Proverbia* “Anonimo veneto (?)”, with an eloquent question mark (PD, I, 521). No less hesitant in this regard is his introductory note to the *Proverbia* in his *Letteratura italiana delle origini*, 127: “Contained in an important Venetian manuscript, and possibly indeed of Venetian origin (rather than Lombard, as has also been argued)”.

An alternation of resistance and surrender to the Venetianess of the text can even be glimpsed in references to the Saibante ms. by our foremost specialist of Early Venetian philology and dialectology, Alfredo Stussi, who for two decades remained unshaken in his sober determination to stress the distance between the Hamiltonian texts and the Venetian of the thirteenth century, but later apparently had fewer doubts.

For his first phase, see, e.g., his *Testi veneziani...*, XXXIV, XLI, etc. or his 1988 essay in *Medioevo romanzo*, which I quote from:

Speaking of the *Proverbia*, Corrado Bologna relates, without pledging himself to either, the conflicting opinions of Contini and Maria Corti, who incline to a linguistic localization, respectively, in Venice and in Tre-

viso. Without going into technical details, I will only observe that *in that text, as in the others declared as early as Tobler to be Venetian, difficulties are created by a much more extensive apocope of final vowels than one would expect, that is, not only after liquids and nasals, but also after plosives and sibilants.* Furthermore, on the basis of internal evidence, the *Proverbia* probably date from the last quarter of the eleven hundreds, which is to say that they are earlier by about a century than the codex that has preserved them. Thus, the possibility that they underwent repeated transcription makes it all the more necessary to perform a comparative diagnosis on all the segments of the codex, which includes, as is well known, non-Venetian texts such as the *Libro* by Uguccione da Lodi and the *Splanamento* by Girardo Patecchio da Cremona [...]. [The] problem [has] essentially [been carried no further] [...] since its first exploration by Adolf Tobler a century ago (Stussi, "Per la storia e la geografia della letteratura italiana...", 121; my emphasis).

Conversely, in his fine 1995 review "Medioevo volgare veneziano", Stussi calls the *Proverbia* "The first literary text that can be regarded, in spite of some anomalies, as having a prevalently Venetian linguistic form".

On the other hand, another top-notch scholar, Pier Vincenzo Mengaldo, has recently produced a careful analysis of the same text where he begins by declaring: "I am convinced that the *Proverbia* are Lombard. One can see this quite clearly in spite of the poor condition of the text, or else its difficulty (many of its terms remain *unica* or hard to explain": MENGALDO, "Filologia testuale", 23).

In June 2011, an interdisciplinary conference was held at the universities of Milan (Statale) and Pavia in the attempt to shed light on the venerable Saibante-Hamilton manuscript. It is the conviction of the present writer, who was among the conference participants, that all the texts contained in the manuscript, whatever their origin, show at least two patinas, a broadly Venetian one (to which we owe, for example, the adverbs in *-mentre* and the opposition between the male *ogno* and the female *ogna* [each, every]) and a broadly Lombard one (characterized, among other things, by widespread apocopes and syncopes, and forms such as *ig(i)* (< ILLI), which are found in just about all the texts, although in different percentages.

It is barely worth adding that these discussions are not an end in themselves: distinguishing between the core and one or more patinas is a necessary preliminary to correctly editing and, thus, correctly interpreting a text.

Bibliographical notes. On *scriptae*, i.e., the written languages used over more or less extensive areas, see C.T. Gossen, *Französische Skriptastudien. Untersuchungen zu den nordfranzösischen Urkundensprachen des Mittelalters*, Wien, Österreichische Akademie der Wissenschaften, 1967 (“Österreichische Akademie der Wissenschaften. Phil.-hist. Klasse. Sitzungsberichte”, 253); H. Goebel, “Qu'est-ce que la scriptologie?”, *Medioevo romanzo* 2, 1975, 3-43; G. Holtus, A. Overbeck, H. Völker, *Luxemburgische Skriptastudien. Edition und Untersuchung der altfranzösischen Urkunden Gräfin Ermesindes (1226-1247) und Graf Heinrichs V. (1247-1281) von Luxemburg*, Berlin-New York, De Gruyter, 2003 (“Beihefte zur Zeitschrift für romanische Philologie”, 316; repr. 2011).

On the Italian Mandeville: TROVATO, “Storia della lingua e filologia”, 30-31, and especially G. Pellecchia, “Il volgarizzamento italiano del Voyage di John Mandeville e i suoi rapporti con la redazione francese”, *Medioevo romanzo* 31, 2007, 345-380.

On the *Spagna in rima*, L. Bertolini, “Ferrara dell’Umanesimo. Vecchia e nuova letteratura in volgare (1440-1500)”, *Nuova informazione bibliografica* 3/2011, 491-520; G. Palumbo, “Spagna ferrarese e Spagna in rima. A proposito di un’edizione recente” and id., “Postilla”, *Medioevo romanzo* 35, 2011, 150-172, 177-178; C. Montagnani, “Risposta”, *Medioevo romanzo* 35, 2011, 173-176.

Contini’s popular anthology *Letteratura italiana delle origini* was first published in Florence by Sansoni in 1970.

On Venetian texts: A. Stussi, *Testi veneziani del Duecento e dei primi del Trecento*, Pisa, Nistri-Lischi, 1965.

On the Saibante-Hamilton codex, after Stussi’s reflections (“Per la storia e la geografia della letteratura italiana del medioevo”, *Medioevo romanzo* 13, 1988, 115-125: 121; “Medioevo volgare veneziano”, now in id., *Storia linguistica e storia letteraria*, Bologna, il Mulino, 2005, 23-80: 39-45) and MENGALDO, “Filologia testuale”, we await the proceedings of the conference of 2011, edited by Maria Luisa Meneghetti. In the meantime, see M.L. Meneghetti, S. Bertelli, R. Tagliani, “Nuove acquisizioni per la protostoria del codice Hamilton 390 (già Saibante)”, *Critica del Testo* 15/1, 2012, 75-126.



6. THE INELUCTABILITY OF CRITICAL JUDGMENT (CHOICE OUT OF VARIANTS, CONJECTURE)

6.1. In praise of the working hypothesis (and of work)

Anyone who is afraid of giving an uncertain text had best confine himself to dealing with autograph manuscripts.

MAAS (FLOWER), § 18

Everywhere the possibility of an emendation depends on a lucky coincidence; but it is only the properly equipped scholar who can grasp it. The reader should study Bentley's first essay, *The Epistula ad Millium*. Admittedly it offers no guidance as to method.

MAAS (FLOWER), § 32

The better the judge of [...] a reading knows the language and habits of thought of the ages that transmitted this reading, or that may have coined it, the sounder his judgment will be. The best critic of a Greek text handed down by Byzantine tradition will be the one who, besides being a perfect Hellenist, is also a perfect Byzantinist. The best editor of a Latin author transmitted in Medieval or post-Medieval mss. will be the one who is as knowledgeable about the Middle Ages and Humanism as he is about the author and his language and times. *Such a critic is an ideal that no one can perfectly incarnate, but that it is everyone's duty to strive to come near to.*

PASQUALI, 123 (my emphasis)

We probably all wish that in the near future textual critics who want to provide texts as close as possible to the original can choose case by case whether it is more expedient for them to deal with the classification stage

by using—or, if need be, perfecting—Neo-Lachmannian software (such as Ben Salemans') or to use traditional systems of error selection, such as the well-tested short cut of *loci critici*—using, of course, a word-processing program or a database to keep their material in order.

Today, given the very long time and substantial funding required for the transcription and markup of witnesses, not to mention that there are only a handful of philologists in the world, I expect that for a long time yet the former system—involving the full transcription and encoding of all copies (→ 4)—will be a luxury reserved for relatively short texts with a relatively sparse tradition. The latter approach, or rather one or other combination of the first and second methods, such as the approaches described by Marc Dubuisson and Caroline Macé (“Handling a Large Manuscript Tradition...”), will instead be the dominant solution when confronted with very long texts with a superabundant tradition, which are often the very texts that have hitherto been judged indispensable, and that we passionately keep reading (Homer, the Bible, Dante, etc.).

¶ I personally find it convenient to record the noteworthy data of collations in Excel sheets. For each variation place (adequately numbered by verse or prose line, one by one) I list in distinct lines on the sheet *all* non-polygenetical significant variants attested in the tradition (collation is free from the constraints of type-2 limitation). I also record in different columns the behavior of each witness: the value is 1 if the witness has the same reading as the collation text (→ 1.2), 0 if it diverges from it. It goes without saying that when the 0 and 1 recur in two or more columns in the same sequence or an almost identical one, they indicate genealogically related witnesses (Figure 3).

In order to facilitate classification work, in autumn 2012 Cristina Bonzanini and Gian Paolo Renello developed software based on this grid in response to a short wish-list drawn up by my team. The beta version is described by G.P. Renello, “Un programma per la classificazione “Computer-Assisted” delle copie della *Commedia* e di altre tradizioni sovrabbondanti”, in NP₂, 207–222. A demo of the alfa version, which can be used by other scholars, will be published in few months on the website www.dantelab.eu. Obviously, with this software the responsibility for the selection and assessment of variants rests fully with the philologist. The PC is only used to speed up the classification phase and make it more accurate.

6.1. In praise of the working hypothesis (and of work)

A	B	C	D	E	F	G	H
			U (366)	E (= Est. 474)	F	Ub (365)	Pad. 67
1.21.113	WP	mille dugento con sessanta sei m. d. un con sessanta sei m. d. con settanta et sei m. d. con cinquanta sei	1	1	1	1	1
1.22.6	WP	feder torneamenti e correr giostra feder tornegiamenti e correr giostra ferir di tormamente accorrer g. far di torneamenti f.t. e muover giostra	1	1	1	1	§
1.22.58	WP	Tra male gatte era venuto 'l sorco Tra male gatte era arrivato 'l sorco tra male gaite era abattuto il s. tra male gente tra male branche	0	1	0	0	§
			1		1	1	

Figure 3. Column A refers to the variation places (e.g. 1.21.113 means *Inferno* XXI, l. 113). Symbol W at column B inform that the reading of the collation text (P, i.e. Petrocchi) is already in W(itte). Column C contains the collation text (darker background) and the variant readings that occur in each variation place. Columns D, E ff. refer respectively to the behavior of the various mss. (symbols U, E, F, etc. at line 2).

As far as I am concerned, I am all for the spread of team work, in textual criticism as elsewhere, having personally experienced on several occasions the advantages it offers. (The teams should consist of small groups of well-trained and motivated people who are capable of abiding by the guidelines, which project directors should immediately make explicit).

But let us also hope that we shall continue to emend texts, with ever increasing rigor. In 1992, Dembowski rightly observed that the growing popularity of the “one-manuscript method [...] made any emendation, except that of the most obvious errors, somehow illegitimate” (DEMBOWSKI, “The ‘French’ Tradition”, 531). As early as the beginning of the 1960s, Bieler had already denounced the fashion of over-credulity:

To suspect a passage or even a whole text was considered a proof of critical acumen during the last century [i.e. nineteenth century]; as a reaction, the fashion to-day is over-credulity. Here as elsewhere the critic must strive for the Aristotelian mean. He is expected to be cautious in his decision and unambiguous in his statements (BIELER, 40).

But even though the prevalent trend in many national schools of philology is to *conserve* the basic texts in the state they are in, i.e., to provide mere transcriptions illuminated by rare suggestions of possible al-

ternatives, the first duty of an editor remains that of providing his or her readers with the best possible text, to try to create a balance between the excessive interventionism of seventeenth- and eighteenth-century classicists or late-nineteenth-century Romanists and the superstitious conservatism revived today by Bédier's lazier followers.

If it is true that "textual criticism provides [...] rational procedures whereby to formulate the most probable hypothesis as to what the original was like, and provide a rough outline of the evolution of its transmission down to the preserved witnesses" (Stussi) and even "aims to provide an edition of an ancient text that is at once accessible to the modern reader and conforms to the intention of its author", as Renzi and Andreose very effectively write (→ 3.10), we should be ready to face more difficult trials. While the earlier stage of classification can be facilitated or made automatic by computer, there are stages in editorial work that are not mechanical or serial, and hence cannot be delegated to machines.

From a reconstructionist perspective (but also, among the more conscientious Bédierists, to find rigorous criteria for singling out a "bon manuscrit"), a compulsory phase, and one calling for solid training and a critical mind, is the detection of indicative errors in long lists of variants. Although computer-assisted editors prefer not to insist on this point (and thereby acknowledge their debt to the hated genealogical criticism), the ability to identify, among a series of alternative variants, some readings that are certainly unacceptable, i.e. errors, is indispensable even for rooting an unrooted chain, turning it into a stemma:

To orient a tree by locating the position of the common ancestor or archetype (which in theory could lie at any node or along any branch of the tree), *a hypothesis based on some other kind of information will be necessary* (MOTTA-ROBINS, LXXXVIII; my emphasis).

For other examples of the embarrassment with which practitioners of cladistics treat the "old" notion of indicative error, → 4.4.

Furthermore, a genealogical critic needs the ability to choose rationally between alternative readings every time the majority rule is not applicable, and one is hence confronted with an "*open recensio*".

But the quintessential element of the interpretative and critical aspects of textual philology is precisely the passage—which is inevitable

for anyone who wishes to provide a good edition—from the text of the archetype, reconstructed thanks to *recensio*, to the text of the original, to be reconstructed by conjecture (*divinatio, emendatio ope ingenii*) in all the places where the archetype is “corrupt”. As I mentioned above, *we cannot stop at the identification of the archetype*, which is in any case often problematic. If the archetype of a classical, medieval or Renaissance text can be defined, it may be badly disfigured by mistakes and lacunae, as Maas warned:

The process of *recensio* [...] leads us as a rule either (1) to a surviving codex unicus, or (2) to an archetype [...]. If the tradition [i.e. the archetype] proves to be corrupt, we must attempt to remedy it by conjecture (MAAS [FLOWER], §§ 13-15).

In this sensitive stage of editorial work, our task is to determine, on the basis of our knowledge and acumen, which readings common to tradition are (very probably or certainly, depending on circumstances) original, and which are not (very probably or certainly, depending on circumstances), and, so far as possible, to try to remedy the flaws in the text.

I will now briefly recapitulate and comment on these aspects of Neo-Lachmannian philology, which are as obvious as they are crucial. The distinction between significant errors and genealogically useless variants can be illustrated fairly exhaustively by a relatively restricted set of rules (→ 3.1) or (which is ultimately the same thing) by a series of algorithms incorporated in a PC software (which, however, should be designed by a textual scholar and be fed with high-quality transcriptions). Nothing of the kind is imaginable, however, when we need to choose between variants that at first sight are equally acceptable, or, even worse, when we need to emend the archetype (or the unique witness).

Still, the need to go beyond the transmitted readings, that is, the archetype, in the quest for the original reading does not betoken arrogance in traditional philology; on the contrary, it is an almost ineluctable pillar of any textual criticism worthy of this name, including both Bédier and dom Quentin’s philology and Robinson and others’ computer-assisted investigations. It does not matter that, as the reader will remember, scholars of the present generation are usually reluctant to grant this point or at least surprisingly silent about it; indeed, even the most solid manuals limit

themselves to general indications (see for example FOULET-SPEER, 85; BOURGAIN-VIELLIARD, 67-68; STUSSI, 131-132; CHIESA, 95-99).

¶ A field of study in which resistance to conjecture is, for perfectly understandable reasons, very strong, is that of textual criticism of the New Testament. In this regard, it is a real commonplace to evoke the richness of the manuscript tradition: e.g., “It is doubtful whether there is any reading in the New Testament which requires to be conjecturally emended. The wealth of attestation is almost invariably bound to be preserved by at least one of the thousands of witnesses” (F.F. Bruce, *The Books and the Parchments*, 5th ed., London, Marshall Pickering, 1991, 169-70, cited in WEGNER, *A Student's Guide*, 240); “We have so many MSS of the NT and [...] these MSS contain so many variant readings that surely the original reading in every case is somewhere present in our vast store of material” (E.J. Epp, “Decision Points in Past, Present, and Future New Testament Textual Criticism”, in *Studies in the Theory and Method of New Testament Textual Criticism*, edited by E.J. Epp, G.D. Fee, Grand Rapids (Mich.), Eerdmans, 1993, 17-44: 31). But the argument of the “wealth of attestation” needs not to be taken as gospel, and on the contrary we may permit ourselves to suspect that, as may be the case for every other text copied by human hand, these thousands of witnesses descend from few ancestors, more or less marred by errors.

It is hardly worth mentioning here that in the face of difficulty science (all sciences) does not stop, but moves forward on the basis of clearly formulated and argued working hypotheses. Scientific hypotheses (and hence also hypotheses as to whether a given reading is original or secondary) can be confirmed or falsified, to use Popper’s terminology, by subsequent studies.

The characteristics of a good conjecture are in substance those summarized by Chiesa:

A good conjecture is one that allows decisive improvement of a text, especially as regards its meaning, while perfectly matching the style and language of the author. Here, too, the principle of parsimony applies: an extravagant conjecture, that is, one that assumes that in the transmitted text there is a complex or not easily explicable innovation, will be acceptable only if the textual improvement it affords is notable; on the contrary, a good conjecture is one that offers a considerable improvement in a passage at a very low “cost” (for example, through a minimal and very reasonable adjustment of the text) (CHIESA, 96)

I should add that, as Maas has already argued, no matter whether the conjecture is confirmed or proved false, scholars will have obtained val-

able information that can be applied to similar cases: “as every conjecture provokes refutation, this at all events advantages our understanding of the passage, and only the best conjectures will win acceptance” etc. (MAAS [FLOWER], § 18).

* * *

According to Segre, the advent of logic, typical of (Neo-)Lachmannian approaches,

would suggest seeing in the history of editing the intertwining of two moments: that of rationality and that of *iudicium* or, as it is commonly called, of interpretation. I understand the term *iudicium*, extending the meaning it has assumed in textual criticism, as standing for all the practices and devices whereby the experience of texts and a noble routine prevail over the norms of logic. And it is clear that transcriptions, collations, the singling out of errors, and critique of linguistic features all pertain to *iudicium* in this sense; to rationality, on the other hand, belong the delineation of stemmas and the choice of readings, in sum, what is called [...] stemmatics (Segre, “Prolusione”..., 11).

However, the strict oppositions Segre posits for reasons of didactic clarity are actually much more nuanced in reality. In the most difficult decisions, *iudicium* and rationality often go hand in hand, supporting one another in unpredictable ways.

Here I cannot help remembering (this is only apparently a digression) something that haunted me in the early years of my university education. Some of the first sections in Heinrich Lausberg’s manual of Romance philology are gathered under the heading “Requirements for the study of Romance linguistics”. In § 13, for example, the author states, with Teutonic thoroughness:

The study of Romance linguistics presupposes good traditional training in the domains of language (knowledge of Latin, Greek, Hebrew, modern French, English, and German), history and literature, as well as logical faculties acquired through the study of mathematics.

In §§ 16 and 17, Lausberg adds that a good Romanist should strive for knowledge of the “historical-geographic environment of the Romance languages”; that “one can distinguish two environmental sectors (with fluid boundaries): 1) the linguistic environment [...]; 2) the cultural environment”. He then adds that “the cultural environment of

the Romance languages has three sectors (with fluid boundaries): ‘primitive’ folklore (1), ‘profane’ culture (2), Christian culture (3)”, for which he recommends, among other things, “regularly repeated reading of the Latin *Vulgata* (e.g., in the edition *Bibliorum Sacrorum iuxta Vulgatam Clementinam nova editio*, ed. by A. Gramatica, Città del Vaticano, 1951), the *Missale Romanum* [...], and the *Breviarium Romanum* [...], this last, however, with the old Psalter of the Vulgate (not the one in modern Latin, used optionally since 1945)”.

Needless to say, my first reading of these pages as a student caused me to doubt what I had up to then presumed to be my vocation for literary studies. Even today, a good thirty-five years after graduation, I am afraid I still meet only a small part of Lausberg’s requirements. I am comforted, however, by his observation that “these requirements are not such as to impede the embarking of this study; they only stand as imperatives whose achievement, in each phase of the study itself, broadens and deepens attainable knowledge of Romance linguistics” (§ 13).

Likewise, to produce a good edition it is not enough to have good software: one must have worked, and worked hard. As Van Reenen and van Mulken observe, in spite of having produced an interesting computer-assisted classification of the *Yvain*, Salemans was forced to stop before the evaluation stage:

In the evaluation stage, the computer results, and consequently the text genealogical theory, is (*sic*) evaluated; however, this evaluation presupposes perfect knowledge of the authentic language in the extant manuscripts, *in casu* the Old French dialects. Since Salemans does not possess such knowledge, he will comment on the evaluation stage when applying his approach to a field he is more familiar with, the fourteen versions [i.e. copies] of the Middle Dutch *Lanseloet van Denemerken* (“Prologue”, in *Studies in stemmatology*, xi).

Actually, the knowledge required for distinguishing between a plausibly original reading and a secondary one is not merely linguistic; it can range, for example, from the history of law to theology. Not unlike the ideal reader postulated by narratologists, a good philologist should share the “mental encyclopedia” of the authors he or she is working on.

In other words, textual criticism calls for the blend of hard work and intuition prescribed by the scientist Thomas Edison (1847-1931): “ninety-nine percent perspiration, one percent inspiration”. As in all fields, if the amount of work done is equal, it is the most brilliant and committed

scholars who find the best solutions. As in all fields, the cogency of the results (what, seen from afar, appears as inspiration, intuition, magic, whereby the most skillful conjecturers are described as “great artists, who are able to combine the rigor of method [...] with genius, and often imagination as well”: CHIESA, 96) depends on, or rather is directly proportional to, the solidity of the philologist’s training and how much of his or her own, and other scholars’, experiences (work) he or she is able to command.

Unfortunately, or fortunately, there does not exist for textual criticism anything equivalent to an Oscar or Nobel Prize, whose assignment in any case seems often susceptible to political or economic influence. Bédier, with his usual verve, noticed that “si l’évidence est le ‘critérium de la certitude,’ on n’a pas encore découvert le critérium de l’évidence. Ce qui est évident pour Pierre ne l’est pas toujours pour Paul” [if obviousness is the “criterion of certitude”, the criterion of obviousness has not yet been discovered. What is evident to Peter is not always so to Paul]. Anyway, while we cannot be sure that Peter and Paul are equally accomplished scholars, truth is the daughter of time; fashions aside, the best solutions and editions ultimately end up being acknowledged as such by the community of scholars.

Thus, even if some philologists or post-philologists regard the emendation of a text as a deplorable pastime, many conjectures born of *iudicium*—that is, the competence and intelligence—of classical and modern philologists (or, more appropriately: many working hypotheses) have been confirmed by early witnesses discovered at a later date or fresh readings of wholly or partly illegible documents with new technologies, which carry precisely the conjectured reading. Not unlike what happens in physics nowadays, the best working hypotheses are later proved true.

Bibliographical notes. M. Dubuisson, C. Macé, “Handling a Large Manuscript Tradition with a Computer”, in EoT, 25-37; C. Segre, “Prolusione”, in NOF, 11-17.

Lausberg’s manual is *Romanische Sprachwissenschaft*, Berlin, de Gruyter, 1969, 2 vols.

The quotation from Bédier is drawn from his “De l'édition princeps de la *Chanson de Roland* aux éditions les plus récentes. Nouvelles remarques sur l'art d'établir les anciens textes”, *Romania* 63, 1937, 433-69; 64, 1938, 145-244, 489-521: 160.

As to subsequent confirmation of working hypotheses, for the classical languages and medieval and Humanist Latin, conjectures later proved right by mss. unknown to the conjecturers are very numerous: MAAS [FLOWER], § 40;

S. Timpanaro, “Delle congettture” (1953), in *Contributi di filologia e di storia della lingua latina*, Roma, Ateneo & Bizzarri, 1978, 673-681 (also useful on the changing attitudes of Classicists to conjecture); ID., 40 [= ID. (MOST), 71]; S. Mariotti, *Scritti medievali e umanistici*, Roma, Edizioni di Storia e Letteratura, 1976, 127-152; id., “Validità e limiti della critica congetturale. Qualche esempio dall’Apocolocintosi di Seneca”, in *Scritti di filologia classica*, Roma, Salerno ed., 2000, 509-522; CHIESA, 97. On Greek texts, see KENNEY, 140 and note 2. In Old-Slavonic studies, some hypotheses of André Vaillant, who produced an edition of Nicodemus’s Gospel on the basis of very few mss., were confirmed by one of the many witnesses unknown to him (G. Ziffer, “Appunti sul Vangelo di Nicodemo paleoslavo”, *Slovo* 60, 2010, 867-873).

It is a well-known fact that, albeit in a less complicated scenario, the old contrast between scholars who regard it as their duty to emend flaws in the tradition (such as many classicists and Italian and Spanish Romanists) and scholars who adhere to the form of the text preserved in surviving witnesses (such as Bédierist Romanists or some Medieval Latinists) has resurfaced, to some degree, in recent methodological disputes between editors of Anglo-American texts of the nineteenth or twentieth century. Here the role of the Italians is played by the “Anglo-American scholarly editors” and that of the French Romanists by the “German historical-critical editors”, whose positions are set out *Contemporary German Editorial Theory*, edited by H.W. Gabler, G. Bornstein and G. Borland Pierce, Ann Arbor, The University of Michigan Press, 1995. On this subject, see SHILLINGSBURG, *From Gutenberg to Google*, 173-188, and ID., *Scholarly Editing*, 56, which I cite: “The editor has a third choice: to edit a text that does for the authors what he expected to have done for him [i.e. by a publisher], but avoids the extraneous alterations imposed by a publisher in his normal but misguided undertaking of the production process. The result, as I have said elsewhere, ‘will be much closer to «what the author did» than blanket acceptance of copy-text forms.’ *It should be remembered, too, however, that the result of any emendation is a critical text*” (my emphasis).

A chapter on Post-Philology, which frankly does not appear to be decisive for the future of textual criticism, can be found in P.C. Ingham, M.R. Warren, *Postcolonial Moves: Medieval through Modern*, New York, Palgrave Macmillan, 2003.

6.2. The art of editing ancient texts. Identifying the correct reading out of two or more competing variants

The quintessence of textual evaluation is the selection from the different transmitted readings of the one reading.... which is the most appropriate to its context. Within the process of this selection, the concept of the “context” is taken in a broad sense, as referring to the language, style and content of both the immediate context and of the whole literary unit in which the reading is found.

Emanuel Tov, *Textual Criticism of the Hebrew Bible*, 3rd ed., Minneapolis, Fortress, 2001, 309, cited in WEGNER, *A Student's Guide*, 125

Every text implicitly refers to a universe of knowledge which may even be shared by a contemporary reader, but becomes less and less obvious with the passing of time. To produce a satisfactory edition, the philologist should recover, if possible, this true mental encyclopedia in its entirety.

Varvaro, *Prima lezione....*, 100-101

Here and in the next section I will limit myself to providing, in chronological order, some examples drawn from texts, mostly medieval or Renaissance, written in different languages. These are all, in my opinion, indisputably clear examples. I have added some basic explanations to give at least an idea of the range of expertise that can come into play in the *art* of editing early texts.

In the title and text of this section (as well as in general usage), *art* is a potentially polysemic word: in Latin titles, such as that of Jean Le Clerc (alias Johannes Clericus)'s book *Ars critica, in qua ad studia linguarum Latinæ, Græcæ, et Hebraicæ, via munitur; veterumque emendandorum, spuriorum scriptorum a genuinis dignoscendorum, & judicandi de eorum libris ratio traditur* (1696) or in HUYGENS's recent manual (*Ars edendi: a practical introduction to editing medieval Latin texts*), *ars* prevalently, if not exclusively, means "technique", as it usually does in Latin. In Bédier ("Réflexions sur l'art d'édition les anciens textes"), Frank ("De l'art d'édition les textes lyriques") and Contini ("l'arte di pubblicare i testi antichi"), a mystical or at least Romantic sense prevails, of art as the possession of exceptional abilities, of the gift of divination, although not without a tinge of self-irony. As far as I am concerned, self-irony is without question: as I said, I believe that, IQs being equal, *iudicium* and the ability to emend depend on, or rather are a function of, the work previously done by the editor.

* * *

It is barely worth reminding the reader that choosing between several competing readings and emending them can be relatively easy when dealing with the texts of great stylists or very learned authors, much less so when the texts were produced by authors with a not clearly definable style and of modest culture. As a very perceptive non-philologist has

acutely observed, “the editorial process is enabled by three assumptions: that there is an original; that the original is qualitatively different from an unoriginality in which it is submerged [i.e. the incorrect readings of the various copies]; and that it has a meaning that is enacted in the language at every point” (PATTERSON, “The Logic of Textual Criticism”, 111).

At any rate, commentaries and explanations relative to textual decisions—which editors should always make explicit—provide the community of scholars with sufficient elements to falsify or confirm the editors’ hypotheses about problematic passages in the text. This is a strictly scientific approach, but only on condition that all important changes made by the editor are communicated (for example, in the introduction) and discussed (in the apparatus, in the commentary, or in a list).

CHIESA, 111-112, offers a useful list of cases when “the identification of innovations is problematic”, including that of “texts written in a not very stylized language, that is, not subject to constant rules”, and appropriately reminds us that Eduard Fraenkel deemed it impossible “to reconstruct ‘non-creative’ texts”, that is, texts by modest personalities.

From a historical perspective, it should be remembered that the practice of finding adequate explanations for less obvious decisions, commenting on them in a second level of the apparatus, and thus distancing oneself from the concise apparatuses of classicists, was theorized and introduced into the Italian philological tradition in 1907, by Michele Barbi in his critical edition of the *Vita Nuova* (see also Pio Rajna’s *De vulgari eloquentia*). Among the most important “Italian” critical editions of the last fifty years that have a second apparatus, i.e. an apparatus complemented by critical comments, I will mention at least the *Commedia secondo l’antica vulgata*, a cura di G. Petrocchi (Milano, Mondadori, 1966-1967, and later editions), and the *Chanson de Roland S.*

Like translations, whose utility is stressed by VARVARO, *Identità*, 612, continuous commentaries are the true test of an editor’s competence and command of every passage in the text, as one can see from *Il commento ai testi. Atti del Convegno di Ascona, 2-9 Ottobre 1989*, a cura di O. Besomi and C. Caruso, Basilea, Birkhauser, 1991. As Varvaro himself observes: “The operation of constituting the text cannot be performed, under any circumstances, unless the editor fully understands the text itself. As a consequence, at the time of constituting the text the editor must be in possession of the information that will later be needed by the reader for correctly understanding it [...]. Every text implicitly refers to a universe of knowledge which may even be shared by a contemporary reader, but becomes less and less obvious with the passing of time. *To produce a satisfactory edition, the philologist should recover, if possible, this true mental encyclopedia in its entirety. Why should he refrain from placing this recovered material at the reader’s disposal, forcing him to not understand or only approximately understand, or to do over again the work the philologist has done?*” (Varvaro, *Prima lezione*, 100-101; my emphasis).

Whenever necessary, in the first column I will show the secondary reading(s), in the second the correct reading. For the reader's convenience, the respective bibliography is provided after each example rather than at the end of the section.

* * *

According to manuals and experience, the errors that are most likely to be significant are lacunae. But when a word, or even an extensive portion of text, is missing in some witnesses, this does not necessarily mean that we are dealing with a lacuna. However, to classify a tradition (with the Neo-Lachmannian method) or orient a stemma (in computer-assisted philology), we must be able to distinguish between lacuna and interpolation. A student of the Gothic Gospels, Marusca Francini, clearly summarizes the controversial story of what appears to be a long interpolation in the Gospel of John, 7:53-8:11 (the well-known story of the adulteress or *Pericope Adulterae*):

The passage on the adulteress is missing from the best Greek manuscripts [...]. A and C have a lacuna at this point, but probably did not contain the pericope, because there could not have been enough space in the missing sheets to include it. It is absent from the Syriac tradition and the Arab version of the *Diatessaron*, as well as Coptic translations. It is not present in the Georgian version and in some Armenian manuscripts. In the West, the passage is absent from the Gothic version and in several codices of the *Vetus Latina* [...]. Among the Greek Fathers, for at least 1000 years none refers to the pericope. The first to mention the passage is Euthymius Zigarbenus (twelfth century), who comments on it, but declares that the more accurate copies of the Gospel do not contain it [...]. The earliest Greek manuscript containing it is *codex Bezae* (D, fifth or sixth century), along with various witnesses of the *Vetus Latina* [...]. The passage is probably a piece of "fluctuating tradition", which came from oral tradition and circulated in certain parts of the western Church, and *was hence included in several codices in several places, independently of one another*.

© M. Francini, "L'edizione della Bibbia gotica tra testo vivente e tradizione quiescente", in FERRARI-BAMPI, *Storicità del testo*, 313-340: 321-322 (my emphasis).

With the same clarity, Francini recalls the case of another apparent lacuna, which frequently recurs, for instance in John, 7:37-39, and requires theological knowledge to be properly understood:

Among the emendations created for doctrinal reasons, there is the frequent addition of “holy” to the word “Ghost”, due to a confusion between “Ghost” and “Holy Ghost”, which were not the same thing [...]. Probably the theological subtlety was not always grasped, and one hence understood “Ghost” as meaning “Holy Ghost”, with the consequent addition of ἅγιος.

¶ M. Francini, “L’edizione della Bibbia gotica tra testo vivente e tradizione quiescente”, in FERRARI-BAMPI, *Storicità del testo*, 322-323.

The following example refers to the *Tractatus de locis et statu sancte terre Jerosolimitane*, an anonymous description of the peoples, religious and military institutions, vegetation, and animals of the Holy Land, written in Latin, presumably in the town of Acre (now in northern Israel), in the first quarter of the thirteenth century.

Omnis [abbates] mitarci sunt Ch, omnes qui numerati sunt H, omnes habent infulas M N Narr. O.	126n omnes mitrati sunt B W
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The original reading—only recognizable as such by a student who has some knowledge of medieval church institutions—is certainly *mitrati* (abbots of *nullius dioeceseos* abbeys, that is, abbeys not under episcopal jurisdiction, were *mitrati* ‘miter-wearing’, as bishops still are today).

The new computer-assisted methods would recommend discarding this variant, because it is complex and not a “type-2 variation”. Actually, in this case, as in others, diffraction (→ 3.2) allows very parsimonious hypotheses about the chain of variants. Rather than a trivialization of the not especially difficult *mitrati* of witnesses B W, the alternative reading *habent infulas* (‘they have miters’) appears to be a critical error, that is, the reaction of an intelligent copyist, and one well informed about the prerogatives of abbots, to the non-existent and meaningless *mitarci* or the no less meaningless, although grammatically acceptable, *numerati*. The two readings just cited, respectively the result of a partial paleographical misunderstanding and poor conjecture, occur in early witnesses such as Ch and H. The sequence of change was thus probably *mitrati sunt > mitarci sunt Ch, numerati sunt H* (either of the two variants, both unacceptable for different reasons, may have led to the subsequent one) > *habent infulas M N O Narr.*

¶ We shall be taking a closer look at the transmission of this treatise (→ 7).

The tradition of the *Lai de l'ombre* [the *Lai* of the Reflected Image] by Jean Renart, one of the most sophisticated French medieval poems about the dynamics of seduction, was used by Joseph Bédier to argue that, since ten or so different stemmas of the work are possible, none of which is manifestly wrong, the Lachmann method does not work (→ 2). Actually, Bédier contented himself with a classification limited to very few points of variation and the tradition of the *Lai* appears to be less impervious to the reasoning inherent in the genealogical method than Bédier thought. Let us consider the following variant:

350 bele tres douce amie chiere CG + DE	bele tres douce dame chiere AB + F
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Bédier, who discusses this variant in connection with eight other agreements, in his opinion fortuitous, of AB + F, dismisses it as negligible: “Cette liste [...] ne comporte [...] que de ces faits infinitésimaux qu'on peut, qu'on doit, qualifier de ‘quantités négligeables’” (BÉDIER, 41). As is known, Bédier’s general conclusion is that the variants opposing A and E, which he judges to be equally acceptable, can be explained as authorial variants.

Now, it is undeniable that in lyric tradition (as in real life) the case of a *dame*, a lady, who is also the *amie*, the lover, of a knight is anything but uncommon, and the two terms can be interchangeable. But is this the case in the *Lai*? In this poem, a knight insistently courts a *dame* of high lineage. At this point in the story, the lady does not regard him as an *amis* (ll. 618-619 “Ja n'est il mie mes amis / Et si pens je qu'il le cuide estre”) and “n'est encor preu en la nasse” by the seducer (l. 726). As a consequence, the knight is very careful not to address her with the improper, or even counterproductive, word *amie*: 350 “Bele très douce dame chiere”; 371 “Damé’, fet il”; 399 “Ha! dame, merci, por pitié!”; 412 “Douce dame”; 451 “Dame’, fet il”; 474 “Bele gentiz dame honoree”; 511 (o 512) “Ha! dame’, fet il, ‘mort m'avez””; 537 “Dame’, fet il, ‘por moi garir””; 544 “Dame de biauté et manière”; 577 “Dame’, fet il, ‘a vo congié!””; 646 “Dame’, fet il, ‘je cuit bien fere”; 714-715 “A foi! Bone aventure ait hui / Ma dame, a cui je sui et iere!”; 734 “Douce dame’, fet il”; 859 “Dame, jel prendrai”.

Only at ll. 886 and 895 does the knight finally say the word *amie* (actually, *douce amie*). At this point, after the *dame* has refused the compromising gift of a precious ring, the knight ingenuously declares that he intends to present it, after this refusal, to another woman; this woman turns out to be the image of the *dame* reflected in the water of a well. Of course, in these verses *amie* does not refer to the lady, but to the *ombre*,

to which the knight throws the ring. At l. 901, the knight again addresses the *dame*: “‘Veez’, fet il, ‘dame, or l’a pris’”.

The final capitulation, when it is the *dame* who calls herself the knight’s *amie*, only comes about at ll. 930ff., when the dame, won over by the knight’s words and *courteous*—that is, nobly disinterested—behavior, declares:

[...] Biaus douz amis,
tout ont mon cuer el vostre mis
cist douz mot et li plesant fet
et li dons que vous avez fet
a mon ombre en l’onor de moi.
Or metez le mien en vo doi.
Tenez, jel vous doing comme amie.

In other words, the only acceptable reading at l. 350 is that of A B and F, *bele tres douce dame chiere*.

¶ For a classification of the tradition of the *Lai*, → 8; see also P. Trovato, “La tradizione manoscritta del *Lai de l’ombre*. Riflessioni sulle tecniche d’edizione primonovecentesche”, *Romania* 131, 2013, 338-380.

The next example is drawn from one of the most popular medieval encyclopedias, Brunetto Latini’s *Tresor*:

Creons nous la Sainte Trinité du père et du fil et du saint esperit en une seule personne	Creons nous la Sainte Trinité du père et du fil et du saint esperit en une seule [sustance, non mie en une seule] persone
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From a theological and doctrinal point of view, as the most recent editors of the *Tresor* have observed, the reading handed down by the manuscripts is an “absurd reading that has taken in all editions (except, paradoxically, Gaiter’s very bad one of the *Tesoro*, which however no one has profited from)”. In their own words: “Our text rests on the idea that this is a *saut du même au même*, based on Pseudo-Ambrose [...]: *et haec tria unus est Deus; non tamen in unius singularitate personae, sed unius Trinitatis essentia constans*”.

¶ P.G. Beltrami, “Per il testo del *Tresor*. Appunti sull’edizione di F.J. Car-mody”, *Annali della Scuola normale superiore di Pisa* s. III, 18, 1988, 961-1009: 969-970; Brunetto Latini, *Tresor*, a cura di P.G. Beltrami, P. Squillaciotti, P. Torri e S. Vatteroni. Testo a fronte, Torino, Einaudi, 2007, XXVII-XXVIII.

Let us move on to an example in Dante, one where one of the variants violates the context, the situation described by the narrator (*Purgatorio* II 118). In the island of Purgatory, the newly arrived souls relax, surrounding the musician Casella, who melodiously sings a song by Dante himself. Cato, the guardian of Purgatory, immediately enters the scene and harshly scolds the idlers (ll. 120-123). The new arrivals flee towards the mountain. The following verse refers to the situation preceding Cato's arrival:

Noi <i>andavam</i> tutti fassi (andavam fassi eravam β + Est Est. it. 196 V Pad. 2) e attenti g (- Est Est. it. 196 V) + + p (- Clar Ricc. 1119) + Ph₂ Clar Ricc. 1119 + Ph₁ + α (- Co Ham Pa) + Co Ham Pa.

It is easy to see that the reading *andavam* [we walked] disagrees with the poet's indications, and is especially unacceptable in the light of Cato's subsequent reproaching of the souls for the fact that they are *standing still* ("Che è ciò, spiriti lenti? / qual negligenza, quale stare è questo?" [What is this, slow souls? / what is this laxity, this standing still?]).

© NP, 681.

Another example from *Purgatorio* (XXIV 36). In this case of broad diffraction as well, it is an analysis of the context that exposes the errors—or, if we wish, innovations:

1a) che piu parea <i>di me voler certecca q;</i> 1b) <i>di me veder conteça Ph + Eg Lo Ricc; 2a)</i> <i>di</i> <i>me aver cierteça Ricc. 1119 + Co;</i> 2b) <i>di me aver contezza Est Pad. 2 + Clar + b Fi</i> <i>Ga La Parm Po Pr Tz Vat;</i> 2c) <i>di me veder certeça V + Stocc t + Mo]</i>	<i>di me voler contezza (voler di</i> <i>me c. Ol) β + g (- Est n; miss-</i> <i>ing in Bud Gamb. D II 41) + a</i> <i>La₂ Lau Laur</i>
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Although the correctness of the reading in the right-hand column is suggested by the U-Triv agreement (the variant is common to both branches of his stemma), Petrocchi prefers 2b, "di me aver contezza" [to know more things about me]. But the right reading is rather that of β etc., "di me voler contezza" [to want to know things about me]. This is confirmed by Dante himself a few lines later (ll. 40-41): "O anima [...] che par si vaga l di parlar meco" [Oh soul [...] who seems so keen / to talk to me]. This is indeed a recurrent psychological situation during Dante's voyage (for example, "vidi un che mirava l pur me come conoscer mi

volesse” [I saw someone gazing | at me as if he wanted to know me], “a l’ombra che parea più vaga | di ragionar, drizza’mi” [I turned to the shade | who seemed most wishful to converse]).

¶ NP, 682.

Here is an example from the *Cantari della reina d’Oriente* by Antonio Pucci, III 19 1-4. According to William Robins, the left-hand variant is preferable to the right-hand one, and should even be regarded as a *lectio difficilior*: “The reading of BFSVZ contains the news that the false letter from Donna Berta appears to be addressed by the queen of the Orient to the king; it is preferable to the *lectio facilior* of KM” (MOTTA-ROBINS, CV):

E fe’ fare una lettera mostrando che la mandasse la vecchia reina, nella qual contenea, <i>al re parlando</i> : “Sappi figliuol che la mie vita fina...” BFSVZ	breve parlando: KM
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Actually, not even the most careless of readers could doubt that the letter of the “vecchia reina [old queen]”, which begins “Sapi, figliuol... [Son, you must know...]”, is addressed to the king, who has just talked with the courtiers in octave 17 and with Donna Berta in octave 18. What calls for an explanation, if anything, is the fact that an official letter, albeit forged, sent from a queen to her son the king, completely lacks the protocol formulas canonical in medieval letter-writing. In the two most authoritative witnesses, K and M (where *breve*, an adjective used adverbially, is the equivalent of *brevemente* [briefly], that is, a syntactical archaism), Pucci declares that he has stuck to the substance. “Breve parlando” is thus a case of agreement in a good reading and hence cannot be used to argue that the two manuscripts are relatives. The reading in BFSVZ, however, is one of the many trivializations found in η, the ancestor of the vulgate text, whose existence has been impeccably proved by Anna Bettarini Bruni (→ 4.3).

¶ A. Bettarini Bruni, P. Trovato, “Dittico per Antonio Pucci”, *Filologia italiana* 6, 2009, 81-128.

In the *Spagna ferrarese*, VII 36 5, which I quote from the above-mentioned Gritti-Montagnani edition, Charlemagne instructs Salomon of Brittany on how to go about exterminating the traitorous Germans. But

the reading of the Ferrara ms. (= F), which has “‘Monzogia, San Donixe’ *uo* chiamare”, contradicts an essential detail in the narration: the massacre is done in silence, without calling the usual invocations and pass-words (*Montjoie, Saint Denis*):

E se tu senti ‘sta note passare nessuna gente, fia chi vole e come, di dimandarli già non t’impaçare e non voler saper com’abian nome. ‘Monzogia, San Donixe’ <i>uo</i> chiamare.	[n]o.
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In the light of VIII 5 1ff. (“Senza gridar ‘Mongiogia’ o dir niente, / cristiani insieme seguivan ferendo...” [Without calling ‘Monjoie’ or any other cry, the Christians kept on wounding the enemy...]), I propose to correct that inconsistent *uo* to *no*, that is, “do not”. It is hardly worth adding that this is an absolutely banal paleographical slip, and that even today in many northern Italian dialects, including Venetian, *no* + infinitive is perfectly grammatical.

¶ *Spagna Ferrarese*, a cura di V. Gritti e C. Montagnani, Novara, Centro di Studi Matteo Maria Boiardo-Interlinea, 2009 (“Opere di Matteo Maria Boiardo. La biblioteca di Boiardo”). Gritti kindly informs me that the reading of the Como ms., possibly the most authoritative witness of *Spagna* after F, confirms my conjecture, and Montagnani includes the passage among the places in the text certainly to be corrected in her reply to G. Palumbo’s review article cited above (C. Montagnani, “Risposta”, *Medioevo romanzo* 35, 2011, 173-176).

In 1986, the medieval historian Marie Luise Bulst-Thiele published a useful edition of the *Opuscula* by the theologian Johannes von Frankfurt. These include, among other things, an itinerary for pilgrimages to the Holy Land in 1426-1427, also detailing the indulgences to be gained thereby. This itinerary declares itself to have been copied from a *tabula* kept in Jerusalem. The text of the itinerary essentially coincides with the *Peregrinationes totius Terresancte*, probably compiled by the Franciscans of the *Custodia Terresancte* in the late fourteenth century, and widely circulated in both manuscript and printed form in the fifteenth.

As is often the case with texts edited by historians, the reader finds no indications about the relationships between witnesses, nor about the criteria followed for reconstructing the text. Whether out of mistrust of the genealogical method or a lack of philological expertise, in spite of having access to three witnesses, respectively in Berlin (= B), Hamburg

(= H) and Mainz (= M), Bulst-Thiele firmly adheres to the earliest ms., that is, B, without emending even its most patent errors and lacunae. The following is a typical example. Here Bulst-Thiele adopts the reading in the left-hand column (the symbol [], to indicate a lacuna, is mine):

Item a predicta civitate Masare per tres dietas in terra Egipti est quedam patria nomine [] Almarath, ubi est capella ubi beata virgo stetit per septem annos B (= ed. Bulst-Thiele, 38)	Item a predicta civitate Masare per tres dietas in terra Egipti est quedam patria nomine <i>Menphel(e)utum</i> , <i>in quo est monasterium Iacobitorum nomine</i> Almarath, ubi est capella ubi beata virgo stetit per septem annos H M
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One can object that the additional words in H and M are not arbitrary additions, but unquestionably go back to Johannes's text. As the right-hand column shows, where the part that is missing in B is highlighted in italics, the copyist of B has incurred a typical copying accident, an omission by homoioteleuton or *saut du même au même*, in this case from the first to the second *nomine*. Furthermore, the mentions of the town of *Menphel(e)utum* and the monastery of Almarath are confirmed, with minimal formal variants, by dozens of witnesses of the *Peregrinationes totius Terresancte* independent of Johannes's transcription.

© Johannes von Frankfurt, *Opuscula*, herausgegeben von M.L. Bulst-Thiele, Heidelberg, Winter, 1986 ("Editiones Heidelbergenses", 22), 30-61; B. Saletti, *L'invenzione dei luoghi santi e la politica delle indulgenze in Terrasanta (secoli XI-XV)*, Università del Salento, a.a. 2011-2012 (unpublished doctoral thesis, supervised by R. Delle Donne), 375.

The *Celestina*, one of the masterpieces of Spanish theater, has a rather complicated tradition and, as far as I know, even the very recent edition by Lobera, Serés and others has not managed to fully unravel this tangled mass. Here I will only dwell on an especially thorny passage, and one that has given rise to much scholarly debate. It is on p. 29, lines 9-11 of the edition cited (act I). On the left I show the reading of the so-called *Tragicomedia* (the many editions augmented with five acts that appeared from 1506 on), used as the basic text by the editors, on the right that of the *Comedia* in 16 acts, transmitted by the earlier witnesses: the so-called "manuscrito de palacio" (Mp) and incunabula B and C.

<p>¡ Oh bienaventurada muerte aquella que deseada a los afligidos viene! ¡Oh si viniése agora, <i>Crato y Galieno medicos, sentiriades</i> mi mal! ¡ Oh pie- tad celestial inspira en el pleberico corazon...</p>	<p>o bien aventurada muerte aquella que de- seada. alos aflitos viene [...] <i>sentirias</i> mi mal / o piedad de <i>seleuco</i> [...] Mp, O si vinisssed agora <i>Eras 7 Crato medicos sen-</i> <i>tiriades</i> mi mal. O piedad de <i>silencio</i> in- spira en el pleberico corazon... B</p>
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To limit ourselves to widely circulated and always available sources for the classical anecdotes the *Celestina* alludes to, I will cite, in the footsteps of Menéndez Pidal, Valerius Maximus, of whose work about thirty incunabula, mostly Venetian and Milanese, are preserved. Here is Valerius Maximus' version of the facts, somewhat abbreviated:

Seleuci regis filius Antiochus nouercae Stratonices infinito amore cor-
reptus, memor quam inprobis facibus arderet, impium pectoris uulnus
pia dissimulatione congebat. itaque diuersi adfectus isdem uisceribus
ac medullis inclusi, summa cupiditas et maxima uerecundia, ad
ultimam tabem corpus eius redegerunt. [...] Sed hanc tristitiae nubem
Leptinis mathematici uel, ut quidam tradunt, *Erasistrati medici* prouin-
dentia discussit: iuxta enim Antiochum sedens, ut eum ad introitum
Stratonices rubore perfundi et spiritu increbrescere eaque egrediente
palle<sce>re et excitatiorem anhelitum subinde recuperare anima-
duerit, curiosiore obseruatione ad ipsam ueritatem penetrauit [...] protinusque id *Seleuco* exposuit. qui carissima sibi coniuge filio cedere
non dubitauit, quod in amorem incidisset, fortunae acceptum refer-
ens, quod dissimulare eum ad mortem usque paratus esset, ipsius pudori
inputans. subiciatur animis senex, rex, amans: iam patebit quam
multa quamque difficultia paterni adfectus indulgentia supera<ue>rit”
(5.7.ext.1; c. p3r).

The same story, still centered on the sagacity of the physician Erasistratus, is also narrated in detail in Plutarch's *Lives* (*Life of Demetrius*, XXXVIII)—of which ISTC records six incunabula of the Latin translation and a rare incunabulum with a Spanish translation by Alfonso Fernández de Palencia (Sevilla, Compañeros alemanes [Paulus de Colonia, Johann Pegnitzer, Magnus Herbst and Thomas Glockner], 2 July 1491)—and in Petrarch's *Trionfi* (*Tr. Cup.*, II, 94-126), a printed version of which was in circulation, with a learned commentary by Bernardo Illicino. (As to Apianus Alexandrinus, which only circulated in Greek, and the *Novella di Seleuco e Antioco* by Leonardo Bruni, where the name of the physician is modernized as Filippo, for these reasons they are not relevant to the passage under scrutiny.).

Whatever the classical or Humanist “source” this passage of the *Celestina* draws on, the author’s habit of adhering faithfully, as a rule, to the *auctoritates* he refers to (a practice that we could qualify as “Humanist”, in the broad sense of the term) and the fact that *Mp*, unfortunately damaged at the very point that concerns us here, has *o piedad de Seleuco* and, in the singular, *sentirias mi mal*, leave no doubts that the original of the 16-act *Comedia* presented one name and one name only, even if perhaps in a slightly garbled form (*Erasyrato?* *Erasycrato?*): that of Seleucus’s physician.

The erroneous splitting of the name *Erasycrato* into *Eras y Crato*, the consequent pluralization of the verbs (“vinissé”, “sentiriades”), and the misreading of *seleuco* as *silencio* (an *n* is an upside-down *u*) are ascribable to the typesetters of the first edition of the *Comedia*. The subsequent replacing of *Eras y Crato* with *Crato y Galieno* and the correction of the absurd *pietad de silencio* to the trivial *pietad celestial* are equally arbitrary “improvements” introduced in the *Tragicomedia* by the printing house correctors, unbeknownst to Rojas, who, as was customary in such cases (we have only to think of version C of the *Orlando Furioso* by Ariosto), presumably limited himself to bringing to the printing house a printed copy of the “old” *Comedia*, with pen corrections here and there, and a few handwritten quires containing the “new” parts.

¶ As I mentioned above, the editors of the 2000 edition of *Celestina* are of a different opinion: Fernando de Rojas (y “antiguo autor”), *La Celestina. Tragicomedia de Calisto y Melibea*, edición y estudio de F.J. Lobera y G. Serés, P. Díaz-Mas, C. Mota, I. Ruiz Arzálluz y F. Rico, Barcelona, Crítica, 2000, CCXXXVII-CCXXXIV, 29-30 notes 43-44. Donald McGrady gives yet another interpretation of the passage in “Two Studies on the Text of *Celestina*”, *Romance Philology* 48, 1994-1995, 1-21; not being much aware of the intermediate copyists and typesetters, he is inclined to ascribe all the errors in the text to poor Rojas. The reading of *Mp* is forcefully defended, however, by A. Blecua, “Sobre el ms. de palacio de La *Celestina*”, in *Estudios de crítica testual*, Madrid, Gredos, 2012, 207-231: 212-213, 229-230. See also A. Roncaglia, *Principi e applicazioni di critica testuale*, Roma, Bulzoni, 1975, 138-139, though he does not know of *Mp*.

For the influence of classical literature on *Celestina*, it is sufficient to refer the reader to the commentary in the 2000 ed. and the anonymous and very abundant sixteenth-century glosses of the so called “*Celestina comentada*” (*Celestina comentada* edición de L. Fothergill-Payne (†), E. Fernández Rivera y P. Fothergill-Payne con la colaboración de I. Corfis, M. García, F. Plazolles, Salamanca, Ediciones Universidad de Salamanca, 2002).

In the first version of the *Cortigiana* by Aretino (1525), some readings can usefully be compared with the second version, prepared by the author for printing some ten years later. The second version (= *II Cort*)—drawn up by Aretino on the basis of a copy of the first that was certainly independent of the only preserved ms. (Firenze, Biblioteca Nazionale Centrale, Magliabechiano VII 84: given the symbol Mg)—is useful, wherever comparison is possible, as indirect tradition. See, for example, at III 6 3, the following allusion to the homosexuality of high Roman prelates:

Bench'io credo che lo faccino perché, potendo toglier moglie, togliano marito e cavansi le voglie assai meglio e non dà contro le leggi Mg Ed. Petrocchi [Nevertheless, I believe they do so because, being allowed to get a wife, they get a husband and satisfy themselves better and it is not against the laws]	Bench'io credo che lo faccino perché, [non] potendo toglier moglie, togliano marito... Edd. Innamorati and Romano
---	---

The passage in the left-hand column makes no sense (in Rome priests *cannot* get married), but can easily be restored thanks to the parallel passage in *II Cort*, III 6 5: “E questo avviene perché, *non* sendo lecito il tor moglie, si to’ marito; e con sì bel modo si cava ognuno le sue voglie, e non dà contro a le leggi”. One should hence restore the passage of the first version as [*non*] *potendo toglier moglie...*, as Innamorati and Romano did.

© Pietro Aretino, *Cortigiana* (1525 e 1534), a cura di Paolo Trovato e Federico Della Corte, Roma, Salerno ed., 2010, 168.

* * *

Though a more stringent analysis could oblige me or others to reconsider some of the above arguments, in my opinion this list of examples is a sufficient illustration of how it is possible to put forward scientific hypotheses (that is, hypotheses that can be confirmed or falsified) about the originality or, at least, the correctness of a reading vis-à-vis one or more competing variants. In other words, while Griffith and others have observed that within the genealogical method “the stemma is necessarily based on errors shared by two or more MSS, yet it is not always clear which of the competing readings are errors”, the problem does not appear to lie with the Neo-Lachmannian genealogical method, but with the greater or lesser competence of editors, who do not always match up to the “requirements” listed with Teutonic thoroughness by Lausberg (→ 6.1), or take the trouble to familiarize themselves with the author’s mental encyclopedia.

6.3. The art of editing ancient texts. The emendation of the archetype (or of the single witness)

Conjecture has at various moments in history held a place of honour in our repertory of editorial paradigms [...]. But at the same time, conjecture is a foundling, strangely lacking a well-defined history, theory, methodology.

Kari M. Kraus, "Conjectural Criticism...", 10

As in the previous section, I will limit myself to providing some examples of conjectural correction, hoping this will be didactically useful. Needless to say, depending on circumstances, both the decision to emend and the decision to preserve can be fully justifiable:

It is indeed only in comparatively modern times that the true function of an editor or textual critic has been recognized to be the discovery of what the author actually *did write*, not the suggestion of what he *ought to have written*. The process of emendation is thus to be applied to a text only when there is good reason to suppose that corruption of the original has occurred (MOORE, viii).

Here, too, no fixed rule can be given. One will have to decide case by case, taking into account the state of the text and that of the manuscript tradition. If there is reason to think that the archetype is a text that is very close to the author [i.e., to the original] and not very innovative, one will try to emend as little as possible; if, on the other hand, the archetype turns out to be marred by many clear innovations, then one will feel authorized to intervene with more freedom (CHIESA, 98).

In the only witness of the anonymous canzone *Quando eu stava in le tu' cathene* (the earliest known Italian poem, discovered by the great explorer of libraries Augusto Campana in the 1930s, and rediscovered many decades later by Alfredo Stussi in a church archive in Ravenna) line 6 reads: "Nullomū cūsillo de penare cōtraquelke plasalsosg^{re}" (the orizontal bar over the vowel, or *titulus*, stands, usually, for *n*, or, exceptionally, a short word beginning with *n*).

Stussi emends this to *Null'omo n[on] cunsillo de penare l contra quel ke plas' al so signore* and interprets it as: "I do not advise anyone to strive against what pleases his lord". But, as Claudio Giunta observes, the double negation *null'omo non* as a direct object at the

beginning of a sentence is never attested in the rich *corpora* of early Italian.

Giunta proposes a fairly parsimonious emendation, which, while retaining a meaning consistent with feudal ideology, also makes the sentence acceptable from a grammatical standpoint (*Null'om [c]un cunsillo de penare...* [No man of sense should strive against the will of his lord]) and offers a possible explanation of the error, founded on the phenomenology of copying: by haplography, he argues, the sequence *cū cūsillo* was inadvertently simplified to *cūsillo* by one copyist, and a later copyist introduced an *ñ* (that is, *non*) to fill out the metre of the verse.

Actually, the corruption Giunta hypothesizes here could also be explained differently, for example by assuming that a scribe inverted 9 (*cum*) and *ñ* (*non*) while copying a poorly readable exemplar or one with many abbreviations. Furio Brugnolo suggests trying out “other possible solutions, starting from the paleographically less invasive ones, such as, for example, *Null'om eu consillo...*, with a horizontal bar above the *u* which could exceptionally have made up for the unwitting omission of the *e* in *eu* (which is the normal form, found throughout the text, for the first person subject pronoun)”. This different textual reconstruction would thus lead us back to the meaning proposed by Stussi: “I advise no man to oppose...” (Furio Brugnolo, “Lirica italiana...”, 205).

Of course, it is quite possible that neither of the above emendations precisely restores the original, but both of these conjectures meet a fundamental requirement of what is called, for good reason, textual *criticism*: “Of course it is far more dangerous for a corruption to pass unrecognized than for a sound text to be unjustifiably attacked” etc. (MAAS [FLOWER], § 18).

¶ To the already copious literature on the two poetic texts of the Ravenna parchment, cited in V. Formentin, *Poesia italiana delle origini*, Roma, Carocci, 2007, 139-177 (“Storia linguistica italiana”), we should now add F. Brugnolo, “Lirica italiana settentrionale delle origini: note sui più antichi testi”, in *I trovatori nel Veneto e a Venezia*, Atti del Convegno Internazionale, Venezia, 28-31 ottobre 2004, a cura di G. Lachin, presentazione di F. Zambon, Roma-Padova, Antenore, 2008, 171-204 (= F. Brugnolo, *Meandri. Studi sulla lirica veneta e italiana settentrionale del Due-Trecento*, Roma-Padova, Antenore, 2010, 5-43).

In the *Novella del picchio senese* by Luigi Pulci, recently edited by Nicoletta Marcelli on the basis of two sixteenth-century editions (F and V) and a late fifteenth-century ms. (Cs), a fool from Siena wants to honor the Sienese pope, Pius II, by clumsily presenting him with a wood-pecker, which a farmer has sold to him as a parrot (a literary tradition

that spread from Florence has it that all citizens of Siena are fools). To gain audience with Pius II, the fool turns to an acquaintance who has become a powerful member of the Curia and asks him for news of the pope. Here editions F and V, independently of the manuscript, have a manifestly poor reading, *santino huomo* (it should be noted, that in the texts in the TLIO and LIZ corpora, up to the seventeenth century *Santino/Santini/Santina* is only a first or last name, never an adjective). Cs, instead, has a totally unacceptable one, that is, the non-existent *cattruohuomo*. Disagreeing with the editor, who supposes the error to be due to an obscure Sienese idiom incomprehensible to Florentine copyists, I believe that we are faced with a typical case of diffraction *in absentia* (→ 3.2). The right reading, which can parsimoniously explain the other two, is different from both that of FV and that of Cs, and it comes very close in spelling to that of Cs:

18 Che è di chel <i>santino huomo</i> di messer Enea? Ègli vero	Che è di chel catt[i]-
che sia facto papa? F V] Che è di chel <i>cattruohuomo</i> ...? Cs	uo huomo...?

According to my hypothesis, in his attempt to stress his familiarity with the pope, the fool inappropriately employs the very common phrase *cattiuo huomo* [scoundrel], and the expression was miscopied in Cs or in the archetype of the tradition, where the copyist misread the *i* in *cattiuo* as an *r*, as found in Cs. We cannot know which was the reading found by Giovan Battista Doni, the editor of F and V, in his exemplar, but one might suppose that he had commercial reasons for preferring the more trivial and somewhat censorious *santino huomo* both to the meaningless *cattruohuomo* and to the disrespectful and possibly dangerous *cattiuo huomo*.

© N. Marcelli, “La Novella del picchio senese di Luigi Pulci. Studio ed edizione”, *Filologia italiana* 8, 2011, 77-101.

In the *Celestina*, ed. Lobera et al., 38, r. 10, one reads of Minerva’s sexual association with *el can* (“No has leido de Pasife *con el toro*, de Minerva *con el can*?”). Now, as the reader will remember, the author (or authors?) of the *Celestina* has a solid Humanist education and draws freely on repertoires, without ever poking fun at them, and no classical author ever attributed bestial affairs to Minerva. On the other hand, some mythographers mention an attempt by Vulcan to rape Minerva, and the fully regular form *Ulcan* for Vulcan is also attested, according to the invaluable CORDE (which I consulted in February 2012), in the almost coeval Alfonso de Toledo (1453-1467).

Sempronius's hearty peroration can thus find a suitable conclusion, if only we emend the incongruous *el can* to [U]*lcan*, with a very parsimonious correction that Paolo Tanganeli suggested to me. Thus, Pasiphae's coupling with the bull exemplifies sexual unions with *brutos animales*, and Minerva's with Vulcan, sexual unions with *acemileros*, that is, low-ranking partners (Vulcan, after all, is only a blacksmith):

¡Desesperas de alcanzar una mujer, muchas de las cuales, en grandes estados constituidas, se sometieron a los pechos y resollos de viles acemileros, y otras a brutos animales. ¿No has leido de Pasife con el toro, de Minerva <i>con el can?</i> ...de Minerva con [U] <i>lcan</i> ?
--

Any editors who chose to retain *el can*, as the early editions do, would not only be going against the author's encyclopedia (in this case, mythological tradition) and rhetoric, but also taking no account of the requirements of context.

¶ Fernando de Rojas (y “antiguo autor”), *La Celestina. Tragicomedia de Calisto y Melibea*, edición y estudio de F.J. Lobera y G. Serés, P. Díaz-Mas, C. Mota, I. Ruiz Arzálluz y F. Rico, Barcelona, Crítica, 2000, 38. A rival, but less parsimonious emendation (“de Pasife con el toro, [Semiramis] *con el ca[vallo]*”), is suggested by A. Blecua, “Sobre el ms. de palacio de *La Celestina*”, in *Estudios de crítica testual*, Madrid, Gredos, 2012, 207-231: 217-218, 228.

In the above-mentioned ms. Mg, the only witness of the first version of the *Cortigiana* by Pietro Aretino (II 13 2), the need for emendation (*farà > far[i]a* [will make > would make]) is suggested by the author's *usus scribendi*:

Ma mi pare così veder che messer Maco <i>farà</i> impazire d'alegrezza una coperta e che gli scoppia se non sta tre mesi legato Mg	<i>far[i]a</i>
--	----------------

Whimsical exaggerations of this kind, with the verb in the conditional form (and always in the non-Florentine form derived from the infinitive + HABEBAM), are a stylistic quirk of Aretino, often occurring in *I Cort.* For example: III 8 3 “Quei sonetini di mastro Pasquino [...] *farianno* arosire la vergogna”; II 21 1 “Una di queste burle *faria* rignovenire il Testamento Vecchio”; IV 13 1 “ce dice cose [Maco], cose che *faria* ralegrare la maninconia” and “Maestro Andrea li fa credere cose che *fariano* bugiardo il Vangelo”; V 15 6 “Quella vacca è [...] cotta sì mangioldamente che *faria* fugire la famme a l'abstinentia”; V 15 7 “ci fanno una colla in su lo stomaco che *amazaria* una statua”; V 15 8 “ti assalta un

caldo, creato in quelle sporcherie d'ossame [...], che *spaventaria* la rabbia, non che l'appetito”; V 22 6 “con esse [sc. le donne] non *averia pacientia* un pilastro, che mille anni tiene una colonna adosso” etc. Especially notable is II 19 1 “Gli è capitato in buone mani [...]. Uno *giuntaria* l’usura e l’altro *faria impazire* la sapientia Capranicha”. We should thus emend as follows: “messer Maco far[i]a impazire d’alegrezza una coperta”.

© Pietro Aretino, *Cortigiana* (1525 e 1534), a cura di P. Trovato e F. Della Corte, Roma, Salerno ed., 2010, 167.

Should anyone be inclined to believe that the transcription of a manuscript for a traditional or digital edition is a mere formality, it is worth remembering that one of the first and most delicate forms of *interpretatio* is determining word boundaries. (Significantly, Segre, quoted at the beginning of this chapter, argues that transcription “pertains to *iudicium*”). In the comedy by Aretino just discussed, the word division found in its single ms., Mg, at II 15 1 (and retained by some critical editors) is trivializing.

E non mi fa profetizare el vino, che, Dio gratia, <i>sa dacqua</i>	s’adacqua	Innamo-
(sa d’acqua Petrocchi Romano) in modo che ’l cervello sta	rati	
in cervello Mg [The wine, which, God be thanked, <i>tastes of</i>		
<i>water</i> , doesn’t set me raving: my brain is perfectly in order]		

Parallel passages in Aretino himself (we need go no further than the description of the *tinello* where the courtiers eat in the first version of the *Cortigiana*, V 15 8: “*El vino* di poi ti ristora? Per mi’ fe’ che è meno stomachevole una medecina! È *adacquato* di acqua tepida, stata un giorno in vaso di rame...”) require us to read, with the first editor of the ms., Giuliano Innamorati, “s’adacqua” [is watered down].

© Pietro Aretino, *Cortigiana* (1525 e 1534), a cura di P. Trovato e F. Della Corte, Roma, Salerno ed., 2010, 167.

The first editors of the first version of the *Cortigiana* (V 12 3) also understood an *el* [the] in the single witness (= *il* in fourteenth-century Florentine and standard Italian) as *e'l*, that is, *e il* [and the].

odio più le femine che l’acqua e ’l vino Innamorati Petrocchi	el vino Mg Ro-
[I hate women more than water and wine]	mano

The reading of Innamorati and Petrocchi disregards the fact that the mysogynist Rosso is also a great drinker (I 14, II 1, III 8 etc.), and thus

spoils the comic effect of the antithesis, constructed on at least generically Boccaccian models such as “Sé così vago di noi [sc. donne] come il can delle maze” [you are as fond of us women as the dog is of getting a hiding]. We thus need to read, with the ms. and Romano: “odio più le femine che l’acqua el vino” [I hate women more than water hates wine].

© Pietro Aretino, *Cortigiana* (1525 e 1534), a cura di P. Trovato e F. Della Corte, Salerno ed., 2010, 172.

Concluding his manual, after a short list of examples of more or less successful conjectures, Maas observed, with his usual levelheadedness:

Here I must close. I realize that the selection and arrangement of these examples is arbitrary. But 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 (MAAS [FLOWER], § 40).

I find, however, that one of the opening sentences of Robert Huygens’s manual *Ars edendi* possibly offers a more encouraging conclusion for the present chapter, focused on the deploying of abilities that are at once individual and homegrown, such as *tact, intuition, application* (PARIS, 13):

Editing texts is an art. And as with all arts, you have to be at least moderately talented, or you will be a failure. *The talents you do have can be developed, indeed will be definitely developed, through exercise and practice* (HUYGENS, 9; my emphasis).

Bibliographical notes. It would be relatively easy to find a number of statements paralleling Huygens’. I will limit myself to quoting an equally appropriate passage from WEGNER, *A Student’s Guide*, 125 (“Evaluating the Internal and External Evidence”): “This is where skill, knowledge and balance on the part of the text critic are needed to determine the most plausible reading of a text. Intuition and common sense must guide the text critic; at the same time, informed judgments must be based on familiarity with types of copyist errors, manuscripts, versions and their authors. Bruce Metzger quips, ‘To teach another to become a textual critic is like teaching another to become a poet.’ While natural talent is certainly a factor, the skills and rules related to textual criticism can be honed and sharpened by regular interaction with the texts”.

The epigraph is cited from K. M. Kraus, “Conjectural Criticism: Computing Past and Future Texts”, *DHQ: Digital Humanities Quarterly* 3/4, 2009 (<http://www.digitalhumanities.org/dhq/vol/3/4/000069/000069.html>).



PART 2
PRACTICAL APPLICATIONS



7. A SIMPLE TRADITION. THE *TRACTATUS DE LOCIS ET STATU SANCTE TERRE JEROSOLIMITANE*

7.1. The state of the question

Robert Huygens's observation that the rendering of William of Tyre's chronicle in British Library: Royal 14.C.X and Cambridge, Magdalene College: F.4.22 amounts to a medieval critical edition, applies also to their rendering of the treatise [sc. *Tractatus de locis et statu sancte terre jerosolimitane*]. This rendering is edited below. Limitations of space permit the indication of only the more significant divergences in the manuscripts utilized by Thomas and Neumann.

Kedar, *The Tractatus de locis et statu sancte terre...*, 123

In an essay of 1998, reprinted in 2006, the famous historian of the Crusades Benjamin Kedar has made new sense of the contradictory data and judgments regarding a short but very interesting anonymous medieval work, the *Tractatus de locis et statu sancte terre Jerosolimitane*, and produced a useful edition of it. However, although scholars have accepted Kedar's theses, the text remains fraught with difficulties regarding its date and even its actual length.

The treatise, which begins with the words "Terra jerosolimitana in centro mundi posita est, ex maiori parte montuosa...", is preserved in a few dozen thirteenth, fourteenth and fifteenth-century mss. It was published in the nineteenth and twentieth century, repeatedly, and erroneously, as a work by Aimar the Monk, or else as part either of an account of a pilgrimage beginning with the words "Ego ivi de Accon in Caifa que

est sub monte Carmeli” (attributed by Wilhelm Anton Neumann to the so-called Innominatus V), or of an *itinerarium terre sancte* whose incipit reads “Si quis de Iope in Ierusalem ire voluerit” (attributed by Reinhold Röricht to the so-called Innominatus IX).

According to Kedar—who, being a historian, does not base his study on philological analysis, but on an assessment of references to facts or institutions found in the text—most of the treatise was written during the last two decades of the Latin kingdom of Jerusalem, and at any rate before its fall in 1187.

The enumeration in the present tense of the Latin canons and monks of Jerusalem’s churches points to the period before Saladin’s conquest in 1187; the same is true of the list of the barons. The *terminus a quo* is provided by the mention of the bishopric of Hebron and the archbishopric of Petra, both founded in 1168 (Kedar, “The *Tractatus de locis et statu sancte terre...*”, 119).

Still regarding the date of the text, he does not fail to observe that a passage relative to the recent history of the Armenians (“Armeni nuper Romane ecclesie promiserunt obedire dum rex eorum a Maguntino Archiepiscopo romane sedis legato coronam suscepit”; my emphasis) refers, as Neumann had already remarked, to the crowning of Leo II in January 1198. However—unlike Neumann, Golubovich and Grabois—Kedar does not judge that the date of composition of the treatise should therefore be lowered to after 1198:

This statement on the Armenians, phrased as it is in the past tense, stands out starkly in a treatise that consistently uses the present tense in describing the country; it may therefore be deemed an addition (Kedar, “The *Tractatus de locis et statu sancte terre...*”, 120).

Actually, Kedar’s argument for a high date, before 1187, is not so cogent. Other Crusader texts postdating the loss of Jerusalem (e.g. Jacques de Vitry’s *Historia orientalis*) still describe the institutions of the Latin kingdom in the present tense, evidently in the hope that these institutions will soon be restored when the city is reconquered. But let us suspend our judgment on the presumed interpolation. We will return to it in the final section, after having attempted a genealogical classification of the witnesses.

Bibliographical notes. Benjamin Z. Kedar, “The *Tractatus de locis et statu sancte terre Jerosolimitane*”, in *Franks, Muslims and Oriental Christians in the Latin Levant. Studies in Frontier Acculturation*, Aldershot, Ashgate, 2006, n° II, 111-132.

7.2. An attempt at genealogical classification based on partial data

Anyone who uses a critical edition should be able to grasp with ease the criteria followed and the decisions made at each level. A critical apparatus serves this purpose no less than a detailed introduction. Especially if placed at the foot of the page, the apparatus allows convenient comparison of the readings accepted in the text with those discarded.

STUSSI, 136

Like other historians, Kedar is understandably more interested in the texts he sets out to analyze and interpret than in the methods used to establish them. In an appendix to the article quoted above ("The Tractatus de locis et statu sancte terre...", 123-131), he provides a generally good edition of the *Tractatus*, based on 7 witnesses, and especially on the ms. designated as B. I cite his data, but have not checked them.

B = London, British Library, Royal 14.C.X, ff. 1ra-3rb (thirteenth c., first half).

G = Berlin, Preussische Staatsbibliothek, Görres 111, ff. 105r-106v (thirteenth c.). Fragments.

H = Heiligenkreuz, Stiftsbibliothek, n° 88, ff. 156ra-156v (thirteenth c.). Incomplete, used by Neumann.

M = München, Bayerische Staatsbibliothek, Clm. 17060, ff. 68r-76r (thirteenth c., first half). Base ms. of the Thomas edition.

N = München, Bayerische Staatsbibliothek, Clm. 4351, ff. 203rb-204va (fifteenth c.). Used by Thomas and printed by De Sandoli.

O = München, Bayerische Staatsbibliothek, Clm 5307, ff. 120v-130v (fifteenth c.). Used by Thomas.

W = Cambridge, Magdalene College, F.4.22, ff. 1ra-3va (thirteenth c., first half)

Kedar does not dwell on his reasons for choosing ms. B as his base manuscript, even though he himself defines it as "a medieval critical edition", and it could hence be the worst copy, that is, the one with the greatest abundance of arbitrary innovations introduced by some unknown medieval colleague of ours. Neither does he tell us whether the non-alphabetical order of his presentation of the mss., B W M H G N O, reflects his judgment on their degree of correctness, from high to low, although this is very probably the case. However, the edition does have an apparatus of variants (more precisely, a negative apparatus), although a confessedly selective one (in his own words: "Limitations of

space permit the indication of only the more significant divergences in the manuscripts").

Now if we were to produce a *true* critical edition of the *Tractatus*, first of all we would need to carry out a census of the witnesses of the work by consulting the ms. catalogues of the principal libraries. We would then fully collate all the witnesses and classify them (→ 1.2). For our purposes, however—that is, for spot-checking Kedar's textual procedures, which he does not make explicit, and the reliability of his text—his own partial apparatus of variants will suffice, combined with a few other witnesses that were especially easy for me to access and collate, that is:

Ch = Charleville-Mézières, Bibliothèque municipale, ms. 275, ff. 217va-219vb (fourteenth c., first quarter). Described by Donnadieu, as it includes the *Historia orientalis*: J. Donnadieu, "L'Historia orientalis de Jacques de Vitry. Tradition manuscrite et histoire du texte", *Sacris erudiri* 45, 2006, 379-456: 382; Jacques de Vitry, *Histoire orientale*, par J. Donnadieu, Turnhout, Brepols, 2008, 47-48. (Thanks go to Wiseon Kim for providing a reproduction of the folios we are concerned with here).

Narr. = *Narratio de statu Terre Sanctae* (*incipit*: "Jherusalem, gloria Judee metropolis, in medio mundi sita est, cui ab oriente..."). Edited in S. De Sandoli, *Itinera Hierosolimitana Crucesignatorum*, Jerusalem, Franciscan Printing Press, 1983, III, 374-390: 380-390. This collection includes the whole text of the *Tractatus*, and some scholars of past centuries believe it to be part of the so-called "third book" of the *Historia* by Jacques de Vitry, where the text of the *Tractatus* takes up Chap. III to the end. On the spurious nature of the *Narratio*, see Kedar, "The *Tractatus de locis et statu sancte terre...*", 113-114 with notes.

Vr = Verona, Biblioteca Capitolare, CCCXVII, ff. 19r-26r (copied in 1458 by the famous copyist Felice Feliciano). Edited by G. Golubovich, *Biblioteca bio-bibliografica della Terra Santa e dell'Oriente francescano*, Quaracchi, Collegio di S. Bonaventura, 1906-1927, V, 407-408. The text is drastically abbreviated compared to the current one, corresponding to the first 2 pages of the Kedar edition.

By way of example, or rather experiment, to show how even a preliminary stemma can facilitate certain deductions (which interested readers could, or indeed should, verify for the full length of this text), I will also take another couple of witnesses into account, limiting myself to the data provided in the catalogues they appear in:

Hann = Hannover, Niedersächsische Landesbibliothek, ms. XXXVII 1806, ff. 1r-10v (ca 1200). Described in: *Handschriften der Niedersächsischen*

Landesbibliothek Hannover, herausgegeben von H. Hartel, F. Ekowski, Wiesbaden, Harrassowitz, 1982, II, 276.

Upps = Uppsala, Universitetsbibliotek, C 43 (fifteenth century, Sweden). Described in: M. Andersson Schmitt, M. Hedlund, *Mittelalterliche Handschriften der Universitätsbibliothek Uppsala*, Stockholm, Almqvist & Wiksell International, 1988, I, 286-289.

In the course of its transmission, the text of the *Tractatus* underwent a number of adaptations, including some very extensive ones. Still, the group of mss. preserved in French and German libraries that we designate as *ch&* (Ch H M N Narr. O) quite evidently constitutes a family showing a particular abundance of conjunctive innovations. These innovations are also shared, to the extent that a comparison is possible, by G and the Italian Vr, which, let us not forget, contain only extracts from the treatise.

Among the certain errors in *ch&*, which I will mark with an * and briefly discuss, and its confirmatory readings (→ 3.1), for brevity's sake I will mention only the following (here and hereafter, references such as 126c are to the page and note of the Kedar edition):

Table 1. Indicative errors and confirmatory readings of *ch&* (Ch H M N Narr O)

126c satisfactionem condignam B W] penitentiam condignam Ch H M N Narr. O, missing in Vr;

Penitentiam (referring to the punishment reserved for Templar knights who have committed serious offenses, such as cowardice in the face of the enemy) is a more obvious variant in ecclesiastical milieus; *satisfactionem* appears to be juridically more appropriate.

*126n omnes [abbates] mitrati sunt BW] omnes mitarci sunt Ch, omnes qui numerati sunt H, omnes habent infulas M N Narr. O, missing in Vr.

As the reader will remember (→ 6.2), the genuine reading is *mitrati* (abbots of abbeys *nullius dioeceseos*, i.e., not under episcopal jurisdiction, were “miter-wearing”, like bishops today).

© As is often the case, the notion of diffraction (→ 3.2) allows editors to put forward hypotheses about the chain of variants. The reading of M N O *habent infulas* [have miters] would seem to be a critical error, i.e., the reaction of an intelligent copyist to the meaningless *mitarci*, or to the possibly conjectural *numerati*, both of which occur, through partial graphic misreading, in the upper levels of *ch&*.

The following seems to be the most remarkable among the innovations that distinguish a subfamily of *ch&*, designated as *h&* (*H + mn&*, that is, *H + M N Narr.* etc.):

Table 2. An innovation in h& (H + mn&)

*125 *y balcanum BW + Ch] balzaus H, balzano MN, baucant Narr., balza O, missing in Vr*

The Lat. *balcanum*, whence *balcanifer* “standard-bearer” (Ducange), designates the bicolored (*balzano*) standard of the Templars, called *baussant, bauçant* and similar things in French sources. Early French and Italian parallels can be found, respectively, in Tobler-Lommatsch, s.v. *baucenc* (where the term refers almost exclusively to horses), and in TLIO, s.v. *balzano*. Barbarisms such as *balzano / baucant* are unlikely to have occurred in the original text without the use of metalinguistic markers such as *vulgo, vulgariter*, as the treatise usually respects the basic rhetorical prescriptions of Latin *puritas*.

Finally, I will point out what are certainly innovations in another subfamily of *ch&, mn&* (*M N Narr. O*):

Table 3. Indicative errors and confirmatory readings in mn& (M N Narr. O)

125z *et devote B W + Ch] om. mn&*

*126f *parce comedentes B W + Ch H] pariter comedentes mn&*

The error is manifest: the Templars are praised for eating *parce*, that is, frugally, and not *pariter*, that is, all in the same way.

*126g *milites boni, cum ipsa militia B W + Ch H] om. mn&*

The lacuna damages the text.

*126n *omnes mitrati sunt BW] omnes mitarci sunt Ch, omnes qui numerati sunt H, omnes habent infulas mn&*

This critical error in *mn&* has already been discussed above, in *Table 1*.

*127r *Jacob B W + Ch G H] om. mn&*

The lacuna damages the text.

130n *ex illis non sunt mille B W + H] om. mn&*.

130o *in omnibus B W + Ch] om. mn&*

130 *sicut templarii in lege sua B W + Ch] om. mn&*

Here are some further observations on the tradition of the text, and especially on some descendants of the *ch&* family. To begin with, all the mss. (including B and W) have many singular errors (*lectiones singulares*), which prove that none of them can be disregarded as a copy (*descriptus*) of any other witness known to us (→ 1.4).

In M and O, the treatise is followed by the so-called *De excidio regni et regibus Jerusalem* (Kedar, "The Tractatus de locis et statu sancte terre...", 112), which is evidence that the two copies are to some extent related. The text of O, preserved in a fifteenth-century ms., is, however, especially rich in interpolations and updates added by a direct eyewitness in the Holy Land itself. These innovations cannot be earlier than 1213, which is the date of the murder of the son of Bohemond IV of Antioch (Kedar, "The Tractatus de locis et statu sancte terre...", 132-133). Nevertheless, O preserves good readings, where M N and others share an innovation (e. g. 128w, 129g, 130k, 130m, 130s).

In the rest of the subfamily *mn&* one finds useful clues that allow a sub-group to be singled out, in which we can also include witnesses such as Hann and Upps, up to now not used by previous scholars (some segments of them can be read in the catalogues where they are described). N also shows extensive interpolations, as in the last paragraph (*inc.* "Terra Jerosolimitana semper variis casibus exposita fuit...", *expl.*: "...puniens eos in presenti, ne damnet in futuro"), but the same lengthy addition, which is certainly conjunctive, is found in Narr., as well as in Hann and Upps. Should a fuller collation confirm these clues, we should be able to distinguish, within the vulgate tradition, a subfamily *n&*, constituted by Hann N Narr. and Upps.

Ch (with its fragmentary relatives G and H, whose genealogical position a full collation might determine more accurately) is the least incorrect mss. of the vulgate family *ch&*. Given the scarcity of the available data, all we can say of the fragment Vr is that it belongs to *ch&*.

* * *

Let us now come to the two mss. with the least number of patent errors, B and W. These two mss., both preserved in England, are unaffected (again according to Kedar's apparatus) by the innovations found in *ch&*. Precisely because of the high quality of their texts and the scarcity of variants in Kedar's apparatus, we cannot conclusively determine their relationship. Aside from some confirmatory readings, I believe I have found at least one conjunctive error that sets B and W against the whole *ch&* family. Thus *Table 4* encourages us to suspect the existence of a common ancestor of BW, *b&*.

Table 4. Indicative errors and confirmatory readings in b& (BW).

124c Syria B W] Syria et mare cipricum (ciprinum Narr., Cyprum Vr) Ch H M N Narr. O Vr

The absence of the phrase “et mare cipricum”, unnecessary in this context, could indicate that it was added in *ch&*, and thus does not count as an indicative error.

124k *om.* B W] per omnia Ch H M N Narr. O Vr

Same as above. The phrase “per omnia” (“Grecis in fide et sacramentis *per omnia concordantes*”) could indeed be an arbitrary addition introduced by *ch&*.

*124m circa ecclesiastica instituta B W] contra ecclesiastica instituta Ch G H M N Narr. O Vr

The text of B W reports that the Armenians follow the practices of the Latin church in some cases, those of the Greek church in others, “in die apparitionis [sc. the Epiphany] nativitatem Christi celebrantes et multa alia circa ecclesiastica instituta facientes”. From the perspective of Western Christians, however, which is that of the author of the *Tractatus*, the “ecclesiastica instituta”, that is, the teachings of the church, are necessarily those of the Latin church, and celebrating the Nativity on the day of the Epiphany is hence “*contra ecclesiastica instituta*”.

125c Alii sunt Jacobiti B W] A. s. Jacobini Ch G H M N Narr. O Vr

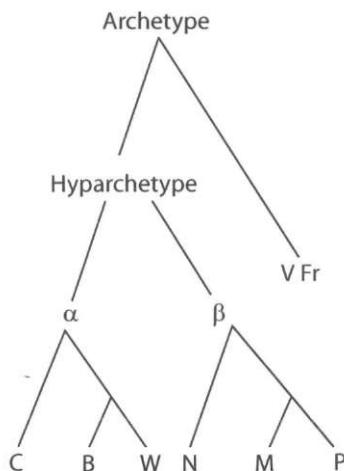
125z daviticum canitur canticum B W] daviticum illud comuniter concidunt Ch + M H N O.

B W seem to trivialize, but the possibility cannot be excluded, especially in Latin, that a learned scribe should have introduced a more elegant variant.

Strictly speaking, the error *circa* for *contra* is polygenetic, as it may result from misunderstanding an abbreviation (the word *circa* could be abbreviated as something like *č* or *č̄*, *contra* as something like *čč*); it is thus not sufficient evidence that B and W belong together. On the other hand, the treatise is rather short and hence relatively lacking in indicative errors. However, as I suggested above, a classification of associated texts can provide valuable information, especially if a short text is part of a non-obvious sequence of heterogeneous texts that occurs in several witnesses (→ 3.8), being unlikely that the same collection arose polygenetically.

In our case, we are especially fortunate, because B and W have been collated by one of the foremost experts on textual criticism in the field of medieval Latin, Robert B.C. Huygens, who used them for his edition of William of Tyre. According to Huygens, the text of William’s *Chronicon* in B and W has been very judiciously corrected, and can be regarded as a true edition. With Huygens’ book in hand, we learn that:

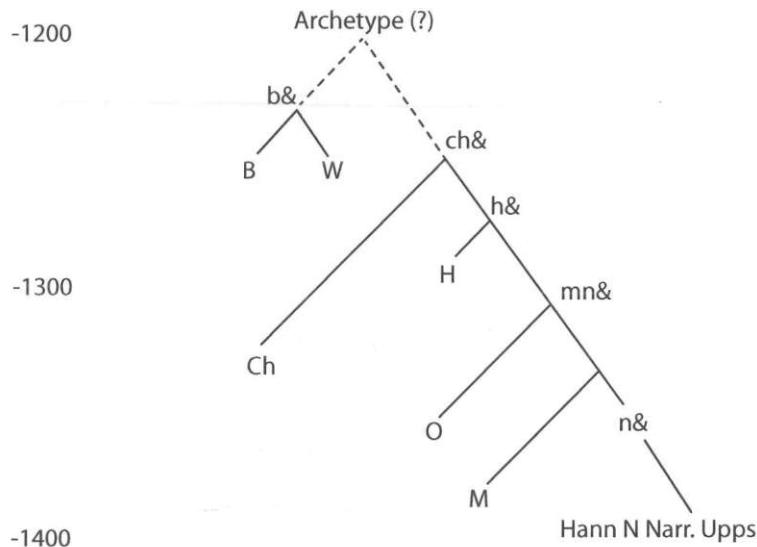
- the textual quality of the *Chronicon* in W is “de beaucoup inférieure à celle de B”, although both mss. are from the first half of the thirteenth century (Guillaume de Tyr, *Chronique...*, 21);
- the two mss. belong to subfamily α , a low-ranking and textually negligible group in the surviving tradition of William’s *Chronicon*, which can be graphically represented as follows (it is hardly worth adding that, as in our case, it is normal for two or more miscellaneous witnesses to be negligible, say, for text a and fundamental for text b);



- the scholar upstream of copies B and W, who put together the collection constituted by the *Tractatus*, the *Chronicon* of William, *Continuatio historie tractate*, *Cathalogi quorundam magnatum de quibus mencio habita est superius*, was a sophisticated intellectual with great philological skills (whom Huygens praises in heartfelt terms: “jamais je n’ai rencontré un éditeur médiéval aussi qualifié que ce collègue d’il y a plus de sept cent cinquante ans”: Guillaume de Tyr, *Chronique...*, 22);
- in B and W the text of the *Tractatus*, offered at the beginning of the collection as propedeutic reading, is followed by a series of true bibliographical references provided as recommendations for further reading (“De nonnullis eorum que iam supra commemorata sunt plura in sequenti opere scripta reperiuntur. Videlicet de Turcis in libro primo, capitulo septimo. De situ terre Ierosolimitane in libro octavo, capitulis quatuor prioribus” etc. (Guillaume de Tyr, *Chronique...*, 22 note 42);

5. the exemplar from which copies B and W are derived was certainly produced in England, or rather by an Englishman, after 1199 and presumably before 1216, since in the *Cathalogi* "l'avènement de Jean sans Terre au trône anglais (1199) est mentionné bien que ce roi ne joue aucun rôle dans la continuation", whereas no mention is made of his death (1216).

All the above considerations confirm my hypothesis that B and W descend from a lost progenitor *b&*. Thus, mainly in the light of the variants in Kedar's apparatus, we can draw the following two-branched stemma:



Of course, this stemma (and probably the text as well) could be significantly improved by the inclusion of new witnesses and more generous collations—we have only to consider e. g. that the mss. known to Thomas cannot be traced back beyond the quite uncorrect text of *mn&*—, but this should not significantly affect the relationships between the witnesses investigated so far. I prefer to connect the primary branches *b&* and *ch&* only by a tentative broken line, having found no errors common to the two families so far (but neither have I noticed such variants as to justify the hypothesis of two versions, whether au-

thorial or not, which could explain the theoretically improbable absence of an archetype).

As I explained above (→ 1.5), the archetype is by definition the point in the stemma with less errors than any other witness, the one least far removed from the original. Its existence is easy to prove when we are working on Greek or Latin classics, whose earliest complete mss. are usually of the ninth or tenth century, that is, some ten centuries later than the originals. In the case at hand, however, while it cannot be proved, it remains relatively probable in view of the effects of decimation (→ 3.5-7). On the other hand, if it is true that mss. B W and H are of the first half of the thirteenth century, and Ch of the early fourteenth, the number of copies (and consequently of errors) separating the archetype, if any, from the original should not be too high.

An agreement of *b&* with *ch&*, and particularly with the higher levels of the latter (Ch H), would put all our doubts to rest in the case of competing variants. In all cases where equally acceptable readings of *b&* and *ch&* compete, we should follow *b&*, which appears to be closer to the original and avoid capricious oscillation between the two branches of the stemma (as the reader will remember, this cautious approach has become generalized as a reaction to Bédier's methodological objections: → 2.2).

Bibliographical notes. Guillaume de Tyr, *Chronique*, édition critique par R.B.C. Huygens, Identification des sources historiques et détermination des dates par H.E. Mayer et G. Rösch, Turnhout, Brepols, 1986, 2 vols.

7.3. Some chronological and historical implications

J'ai déjà exposé dans mon *Rapport de 1877*, combien il était embarrassant, dans la plupart des cas, de fixer une date à peu près exacte à la rédaction de textes anonymes, reproduisant presque toujours dans le même ordre la description de lieux dont l'aspect extérieur, aussi bien que les traditions, ont toujours revêtu, depuis le Moyen-Age jusqu'à nos jours, ce caractère immuable [...].

The Count Riant, "Preface", in *Itinéraires à Jérusalem...*, ix

Returning to the issues raised by Kedar, viz., those of the chronology and length of the text, it is worth adding a couple of observations based on my textual analysis. Even admitting the absurd hypothesis that *b&* and *ch&* (which are essentially identical, except a few sentences at the

end) are two successive versions of the same work—not necessarily by the same author —whose relative chronology is unknown to us (i.e., we do not know whether *b&* precedes or follows *ch&*), my brief investigation, although only preliminary in character, corrects Kedar's historical reconstruction in two not insignificant respects.

In the first place, since *all* the witnesses of the *Tractatus* examined so far contain the reference to the Armenians and the crowning of Leo II in 1198 (“Armeni nuper Romane ecclesie promiserunt obedire...”), these textual elements cannot be dismissed as an interpolation. On the genealogical hypothesis formulated above (→ 7.2), the passage is in the archetype; on the absurd hypothesis I have just put forward at the beginning of this section, it is in both versions. It is of course possible, as the eminent historian argues, that the original text lacked the reference to the Armenians (→ 7.1), and that this was introduced by a copyist-rewriter upstream of the preserved tradition. But we miss a “good reason to suppose that corruption of the original has occurred” here (I quote again MOORE, p. viii), and the *Tractatus*, as it is concretely and historically transmitted in the known mss., cannot be earlier than 1198.

Furthermore, we need to reconsider the nature of a second, long variant, which Kedar rightly dates after 1189-91. This variant is found only in *b&*, at the end (*inc. “Saracenorum plurime sunt gentes...”, expl. “... Maurorum, Garamantum, Magitrogoditorum et aliorum plurimorum”*). In line with his dating of the treatise “before Saladin’s conquest in 1187”, Kedar judges these pages, too, to be an interpolation, albeit an interesting one (“a survey of the Saracen forces Saladin amassed in front of Acre during the siege of 1189-91”). Nevertheless, they are consistent with the overall structure, the lexicon and style, and the post-1198 date of the treatise as we read it today. In stemmatic terms, these sections—entitled *De diversitate Saracenorum et hostium Christianitatis [...], De Syris, De Egyptiis etc.*, and which Kedar has edited and discussed in a separate essay (“A Western Survey of Saladin’s Forces...”)—have a 50% probability of being an integral part of the original text of the *Tractatus* as we know it.

In either case, the work must have presumably been put together some time after 1198, probably at Acre (the capital of what remained of the Kingdom). If the long text of the *Tractatus*, preserved today only by the collection *b&*, already appeared in the archetype, the author could have been identical with the learned English compiler of *b&* (datable after 1199 and before 1216). In that case, the final sections of *b&* would have been expunged from *ch&* (possibly also produced at Acre, some years

later) because they described the Saracens' numerical and organizational superiority during the siege and thus ran counter to the requirements of Christian propaganda.

Bibliographical notes. Besides Kedar's essay cited in the above sections, see his "A Western Survey of Saladin's Forces at the Siege of Acre", in id., *Franks, Muslims, and Oriental Christians in the Latin Levant. Studies in Frontier Acculturation*, Aldershot, Ashgate, 2006, n° VII, 113-122.

The epigraph is drawn from *Itinéraires à Jérusalem et descriptions de la Terre Sainte rédigés en français aux XI^e, XII^e & XIII^e siècles*, publiés par H. Michelant et G. Raynaud, Genève, Julles-Guillaume Fick, 1882.



8. A TRADITION OF AVERAGE DIFFICULTY. JEAN RENART'S *LAI DE L'OMBRE*

8.1. The state of the question. From Bédier to the present (1890-2012)

From Bédier to the present there has been no dispute as to the principal groupings as determined by common variants [sic]: A and B form a pair, as do C and G, which are both heavily Picard and set apart from all the others in that respect, while ABCG in turn probably form a group to be contrasted with DEF.

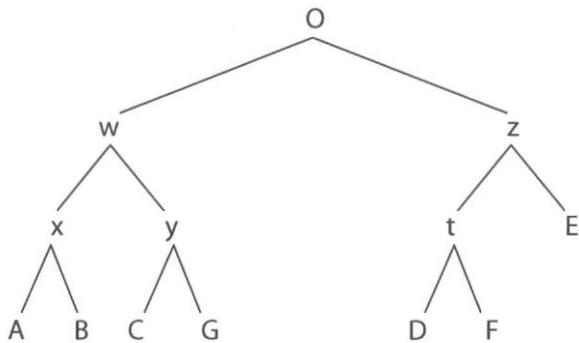
Margaret E. Winters, "Introduction", in Jean Renart, *The "Lai de l'ombre" ...*, 2

It is exaggerated to represent a manuscript as preserving a state of the text that has enjoyed an authentic existence, since what we are dealing with is in most cases something as composite as any reconstituted critical text produced by a scholar. A manuscript generally presents us with several strata of alteration and behind many readings there is a complex history of change.

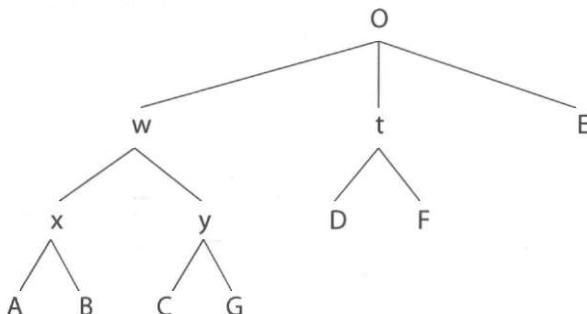
WHITEHEAD-PICKFORD, "Introduction to the Lai de l'Ombre", 153

As is known, in 1890 Gaston Paris reviewed an edition by his most promising student, Joseph Bédier, of Jean Renart's *Lai de l'ombre*. Bédier applied to this sophisticated poem (already edited by others on the basis of a single witness) the new reconstructive method recently introduced by Paris himself to Romance philology (→ 2.1). According to Paris, the work of Bédier, who "reconstructed" the text starting from A, but drawing on the whole tradition, was excellent in every respect, with the sole exception of the two-branched stemma, essentially the one below (for the sake

of accuracy, I should add that ms. G was studied and recognized as an independent relative of C only in 1913):



For the patriarch Paris (who, of course, worked on the materials available in the first Bédier edition), the stemma that best reflected the tradition of the *Lai de l'ombre* was rather a three-branched one. Incidentally, this would have simplified the task of editing greatly by allowing the application of the majority principle, but—as I incline to interpret a passage of Bédier's 1913 edition (p. xxxii)—it would have required accepting into the text, for instance, some hardly original variants in the mss. DEF:



Stung to the quick, Bédier soon began to rethink the costs and benefits of the method followed by Paris in 1872, which as early as 1913 he designated with a highly inaccurate formula, but one that was to enjoy great popularity: “la méthode de Lachmann” (“depuis quarante-cinq ans environs [...] la méthode ici maniée sert à publier les anciens textes français”). The first result of these years of rethinking was a new edition of the *Lai*, based on A, which came out in 1913. Here Bédier reworked the

stemma of 1890 (with some changes in the selection of indicative errors). However, after arguing that the method adopted almost inevitably resulted in two-branched stemmas, he began to put forward some serious reservation about the scientific correctness of the method:

Qu'il se soit agi d'une chanson en cinquante vers ou d'un roman en dix mille vers, et que le texte fût conservé en trois manuscrits seulement ou en deux cents, tant de critiques opérant chacun de son côté ont abouti, à leur insu, au même résultat: sur les quatre-vingts arbres généalogiques dont se compose actuellement ma collection, il en est soixante-dix-huit qui sont à deux branches seulement, *x* et *y* [...]. Pour qui n'accepte pas volontiers le merveilleux, il n'y a qu'une autre façon imaginable d'interpréter cette loi [sc. the preponderance of two-branched stemmata]: elle signifie que l'on est en présence non point de faits réels de l'histoire de la transmission des textes, mais à l'ordinaire de phénomènes qui se passent dans l'esprit des éditeurs de textes: auquel cas un nombre indéterminé, mais peut-être considérable, d'éditions de nos anciens textes sont fondées sur des classements erronés en partie et illusoires (Jean Renart, *Le lai de l'ombre...*, XXVI-XXVII).

Bédier's second and even more devastating attack on "Lachmann's method" was the famous article he published in *Romania* in 1928 and then again, together with an edition of the *Lai* based on E, in 1929 (BÉDIER). Here he declared his preference for the pre-scientific method of editing based on a *bon manuscrit*: not a reconstructed text, but a document whose historical existence was not in question, to be corrected only where this should prove absolutely necessary. As I hinted at above, by virtue of Bédier's authority and extraordinary eloquence, the essay of 1928-1929 caused a possibly irreversible split in the world of textual critics and, beyond Bédier's own intentions, paved the way for the typical thesis of New Age cultural relativism that it is impossible to find one's way in the *mare magnum* of medieval variants and rationally sort them all out, from Zumthor's *mouvance* to Cerquiglini's unmanageable *variance*.

Thereafter, and for a long time—in spite of the fact that in 1890 Romance textual criticism was still in its infancy, and the reconstructive method was to undergo significant theoretical revision (from Maas to Timpanaro) and continual refinements—no one tried any more to use the common-error method to study the tradition of the *Lai de l'ombre*, and, as I noted above (→ 2.4), only Bédierist editions of the poem were produced, founded on one or other of the two witnesses the French master had judged to be the best, A and E.

Now it is worth remarking that both the 1890 and the 1913 editions were produced after what even a benevolent critic would call a very summary classification. The evidence produced in 1913 to prove the existence of families *w*, i.e., ABCG (4 likely innovations), and *z*, i.e., DEF (2 likely innovations), is all concentrated in a very short section of the text, about 100 lines out of almost 1000 (ll. 511-608). What we have learned in the meantime, however, about textual criticism and especially about contamination, requires us to ascertain whether the relationships between the witnesses remain constant throughout the length of the text. If the text is long, the cautious scholar will fully collate several sections of the text far removed from one another. If the text is relatively short, as in the case of the *Lai*, one should begin by drawing up a list of *all* recognizable variants of substance and then carefully looking for indicative errors.

Needless to say, even Bédier's essay of 1928-1929, although it presents the reader with a whirlwind of stemmas alleged to be equivalent, does not constitute a true step forward in the classification of the *Lai*.

As to subsequent editions, they all accept Bédier's conclusion that it is impossible to produce a satisfactory classification of the tradition of the *Lai*. No editor, however, has sought to ascertain whether other conjunctive errors exist, such as to confirm or, if so it turned out, falsify the fragile dossier of evidence backing up the stemmas discussed by the French master.

Bibliographical notes. The first epigraph is a quotation from Jean Renart, *Le lai de l'ombre*, Edited from Ms E [B. N. Nouv. Acq. Fr. 1104] by Margaret E. Winters, Birmingham, Alabama, Summa Publications Inc., 1986.

Gaston Paris's review was published in *Romania* 19, 1890, 609-615.

I quote Bédier from Jean Renart, *Le lai de l'ombre*, publié par Joseph Bédier, Paris, Firmin-Didot, 1913.

For a list of Bédierist and Post-Bédierist editions of the *Lai*, → 3.4.

8.2. A new attempt at classification

On peut dire, à considérer la masse de ces constructions que l'on dénomme *stemmata codicum*, que presque toujours les principaux groupements de manuscrits y apparaissent déterminés de façon très juste, ceux que l'on aligne au bas du tableau: la base de la construction, le rez-de-chaussée, est solide. Mais il en va autrement des parties hautes [...]. Et pourtant, c'est d'elles seules, de la façon dont elles sont disposées, que dépend le sort du texte.

BÉDIER, 71

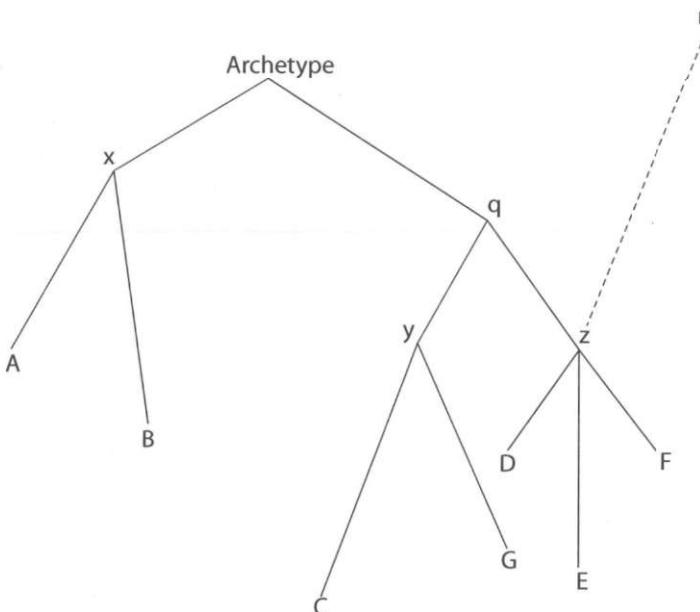
In order to avoid the vicious circle, one must use exclusively manifest errors to build up the stemma, and latent errors must under no circumstance be used in this process. Unfortunately [...] there are a fair number of editions in which the stemmata are to some extent based on latent errors [...]. *The problem is serious, but is not a consequence of the stemmatic technique but a consequence of the misuse of this technique. The only cure is the improved training of future editors.*

EKLUND, 12-13 (my emphasis)

A recent attempt at classification extended to the whole body of the *Lai* confirms the existence of the subfamilies *x* (= AB), *y* (= CG) and *z* (DEF), already identified by Bédier in 1890, and for which new evidence can easily be added. However, it also suggests a radically different solution for the upper levels. Bédier's original stemma would seem to be an example of the validity of Bédier's own statement, quoted in the epigraph, that, as a rule, "la base de la construction, le rez-de-chaussée [viz., of nineteenth and twentieth century stemmas], est solide. Mais il en va autrement des parties hautes". A simple perusal of the rich apparatuses of some of the available editions (the first two by Bédier, and those of Orr, Lecoy, and Winters) reveals an impressive number of oppositions between mss. A and B on the one hand (for which it is preferable to retain the traditional designation *x*) and subfamilies *y* and *z* on the other (let us designate the "new" family formed of *y + z* as *q*).

I have already discussed above (→ 6.2) the trivialization *dame* > *amie* (l. 350). This is one of the most evident errors conjoining the descendants of *q* (the *dame* will become *amie*, that is, "lover" of the knight only towards the end of the *Lai*). Here I will limit myself to observing that, in my opinion, the surviving relatives of *q* agree in error or innovation nine times and witnesses A + B (*x*) nine times (Trovato, "La tradizione manoscritta...", 352-356). Furthermore, in another forty or so cases "the opposition *q* || *x* is represented by equally acceptable readings [...]: in Bédier's own words, the groups "se forment sans qu'il soit possible de décider de quel côté est la faute" (Trovato, "La tradizione manoscritta...", 357ff.). We might even grant that some of the readings in *q* and *x* that I regard as errors are only latent errors, as Eklund would call them, or worse, are not errors at all, and should hence be downgraded to equally plausible readings, lacking genealogical significance. These readings would thus join forces with the other 40 passages which are listed in the work I have just cited where AB and CDEFG have equally acceptable readings.

Although in the case of equally acceptable variants it is impossible, by definition, to determine which of a pair is the innovation, the variant in *x* or that in *q*, it seems highly unlikely that *all* the innovations were introduced in the hyparchetype *x* and none (except some of the nine we have hypothesized above) in the lost ancestor *q*. The evidence for the *x* / *q* opposition, distributed along the whole length of the poem, remains overwhelming. At least in my opinion, the following stemma is a fairly accurate picture of the relationships between the surviving witnesses (*r*, on which more below, stands for a lost "source"):



Retrospectively, it is easily remarked that, unlike the stemmas of Bédier and Paris, where *x* was worth respectively 25% or 16.5%, the stemma I propose on the basis of 18 common errors, supported by a very high number (40) of confirmatory readings, that is, readings peculiar to the same groups, helps to make better sense of Bédier's own well-argued judgments of 1890, and Lecoy's of 1979, on the overall superiority of *x* and, within it, of *A* over the descendants of *y* + *z*. As the stemma shows, *A* and *B* are textually more genuine because they are genealogically less far removed from the archetype.

I would add that, while the archetype of the *Lai* contains a handful of Picard linguistic features, ascribable, as far as we know, to Jean Renart himself, and 50% of branch *q* (i.e., the very corrupt *y*) is written in

Picard—the language of A is Francien. In Pasqualian terms, one would be inclined to stress that, as is almost always the case, peripheral areas (in this case, with respect to the center from which Jean Renard's poem can be presumed to have radiated) are textually more conservative.

But, instead of vacuously congratulating ourselves on this result, which corrects Bédier on the strength of a method that in his more circumspect form only took hold after his ruthless *j'accuse* against the textual critics of his time, we should try to understand why such a conscientious and extremely acute scholar did not deem it useful to follow the promising path he himself had pointed out in 1913, that of the “quatre-vingts passages environ où CGDEF s'opposent à AB” (Jean Renart, *Le lai de l'ombre*..., XXXII), and why in his essay of 1928-29 he reaffirms, as he did in 1890 and 1913, that “une barrière (II) sépare AB + CG de DF + E” (BÉDIER, 21).

In my opinion, the most likely reason for his inability to call into question his youthful classification of 1890 is the fact that in ll. 562-565 AB and CG share, against DEF, one of the most certain proofs of a close genealogical relationship, viz., an extensive lacuna, and, as was well known even in Bédier's time, lacunae are very seldom transmitted intentionally, through contamination.

561-	Dont ele se repente au loing.	Dont ele se repente au loing.
567	<p>[<i>A celui qui ert en grant soing</i> <i>Du pensser ou ele ert entree,</i> <i>A mout bele voie monstree</i> <i>D'une grant cortoisié fere]</i></p> <p>Amors, qui en tant (tentent D) maint (mal C) besoing A esté voiseuse (sages AB) et soutille ABCG</p>	<p><i>A celui (de celui DE) qui ert en grant soing</i> <i>Du pensser ou ele ert entree,</i> <i>A mout bele voie monstree</i> <i>D'une grant cortoisié fere</i></p> <p>Amors, qui en tant maint afere</p> <p>A esté voiseuse (cortoise D, viseus F) et soutille DEF</p>

This is undoubtedly the most sensitive passage for any discussion of the *Lai*, and the one on which the whole edifice of Bédier's first two editions rests. It is advisable to proceed one step at a time, and ask ourselves first of all if this is really a lacuna. A superficial scholar interpreting the surrounding verses of ABCG may argue that there is nothing missing. The rhyme *loing* at l. 561 is picked up by *besoing* at l. 566 in some current editions. But BÉDIER, 60-61 (“Troisième conflit”), rightly observes that the lines only found in DEF fairly connect with a detail in l. 547. And the state of tradition does not allow the hypothesis of a mere (polygenetic) *saut du même au même*, from an *-oing* rhyme to another *-oing* rhyme, because if we restore the missing verses we lose the rhyme with *besoing*.

The fact is that our great ancestors, including Bédier, founded all their reconstructions on just a few manifest errors. After Bédier, but also after Barbi, Maas, Pasquali, Timpanaro, we draw up lists as exhaustive as possible of non-polygenetic variants in our effort to work out the overall “system” of innovation and conservation in the witnesses. In this case, the high number of significant innovations connecting DEF to CG and the presence of our lacuna in 2 branches of the tradition compel us to ascribe the omission of ll. 562–565—possibly instigated by the homeoarchy *Dont... Dune...*—to the archetype itself, and their restoration in DEF to extra-stemmatic contamination. But this is a typical problem of twentieth-century “mature” textual criticism (→ 3.4), which has addressed with determination the issue of possible “perturbations” (Timpanaro) of the stemma. In other words, we are looking at a far from infrequent case of a partial obscuring of the archetype, similar in every respect to those discussed above in the present manual (→ 3.7).

As to the variants in l. 565, there are only two possibilities: either the rhyme *besoing* is original and the leap from 562 to 565 caused the loss of a verse in *-oing* after l. 564, which the manipulative copyist of DEF remedied with rhymes in *-fere*, or, as seems more parsimonious, the archetype not only had the primary error (the loss of the 4 verses), but also “critically” edited the rhyme of v. 566 (*maint afere > maint besoing*) to restore the pattern of rhyming couplets, *aabbcc...*, disrupted by the flaw.

It is barely worth mentioning that the notion that Jean Renard’s (or Dante’s) contemporaries could have access, for their collations, to much earlier and less corrupt mss. than the few witnesses of the vulgate traditions that have survived down to our day is anything but extravagant, since the T-value (the temporal distance) was then very low. Notably, to minimize the risk of lacunae, some especially careful copyists would count the verses in poetical texts, and would sometimes note down these calculations in the margins of their copies.

Bibliographical notes. P. Trovato, “La tradizione manoscritta del *Lai de l'ombre*. Riflessioni sulle tecniche d'edizione primonovecentesche”, *Romania* 131, 2013, 338–380.

On extrastemmatic contamination and the obscuration of the archetype → 3.4 and 3.7.

The quotation from Bédier is from Jean Renart, *Le lai de l'ombre*, publié par J. Bédier, Paris, Firmin-Didot, 1913.

For completeness’ sake, it is only fair to note that in 1979, using exclusively cluster analysis methods, Patricia Galloway anticipated my

conclusions. While she did not work on the whole range of variants ("A full analysis of all the variants Bédier used would be necessary for a valid comparison..."), but only on the information supplied by Dom Quentin (30 points of variation between ll. 166 and 233, and hence not including the lacuna that had misled Bédier), Galloway observes: "There is obviously no conflict between the results of all methods tested on the data that was used. This would seem to indicate that the structure of the data is very clear and is represented faithfully by the results of the analyses. These results all imply that A and B form the most closely-related group, C and G the next, and D and E the next; that F is closer to the DE group than to any of the others; and that the DEF group is closer to CG than to AB" (P. Galloway, "Manuscript Filiation and Cluster Analysis: the *Lai de l'ombre*", in PdO, 87-95: 91). Although Galloway rightly takes care to stress that her experiments, founded on variants, not indicative errors, point to "similarities rather than descent", her stemmas on p. 93 reveal even more clear-cut convergences. Galloway's essay was reprinted—with few changes and the title "Filiation, Classement, Cluster Analysis: *Lai de l'Ombre*"—in *Le Médiéviste et l'ordinateur* 7, 1982, 10-14. I have not managed, however, to gain access to her article "Clustering variants in the *Lai de l'Ombre* manuscripts. Techniques and principles", *Association for Literature and Language Computing Journal* 3, 1982, 1-8, based on a "larger random sample" of the *Lai*, which "still did not agree" with Bédier's results (I quote from Galloway, "Filiation, Classement, Cluster Analysis...", 12).



9. A VERY COMPLICATED TRADITION. DANTE'S *COMMEDIA*

9.1. The state of the question (1862-1891). Witte, Moore, Barbi

We should at any rate obtain within reasonable time a '*vindemiatio prima*': we should eliminate a large number of MSS. as not worth the labour of a complete collation; we should have selected a definite and perhaps manageable number containing a text more or less pure and comparatively little tampered with. Next, those MSS. so selected might then by a division of labour be subjected to a complete and searching collation, and the mass of evidence so gathered would be available for the great work of the reconstruction of the text.

MOORE, xxxi

Italian philology of the nineteenth century, until, say, the last quarter of the century, was not philology [...] That is why Michele Barbi regarded himself as a pupil, not of his great teacher Alessandro D'Ancona, but of Girolamo Vitelli, and a great Romanist, Pio Rajna, [...] drew on the rules of classical philologists [...]. But the balance was soon overturned [...] as vernacular texts proved [...] a much more fertile and certain experimental field than the field of classics.

Augusto Mancini, *Giorgio Pasquali e l'unità della critica filologica*, in *Studi Pasquali*, 3-4

The lyrical poets who were fashionable when Dante was young—those of the Sicilian school, Guittone, Guinizelli, and the other Stilnovists—are preserved in just over twenty witnesses dated between the late thirteenth

and the sixteenth century. Dante's *Vita Nuova*, whose tradition was classified in an exemplary way by Barbi in 1907, is transmitted in less than 100 manuscripts. The *Commedia*, however, is preserved in about 800 manuscripts and dozens of early printed editions. It thus has a decidedly overabundant tradition, although today about a quarter of the surviving copies are reduced to fragments comprising only a few folios.

If we assume an absolutely realistic decimation rate such as 80% (→ 2. Appendix), we must imagine that the real tree of the *Commedia* (the real tree, as the reader will remember, is the whole corpus of manuscripts that ever existed, including those that are not preserved) consisted of at least 6,400 copies. These terrifying numbers—unequalled in European manuscript traditions of the time, with the exception of that of the *Bible* and a few other books for *clerics*, in Latin or Greek—are the first reason why classifying the tradition of the *Commedia* (a work over 14,000 verses long) is so complicated.

If we remember that reading a couple of cantos (about 150 lines each) in a manuscript of the *Commedia* would take about an hour, simply reading the full 100 cantos in each of the about 600 non-fragmentary surviving copies (let alone collating, recording variants, or performing any other more specific and onerous tasks) would require 30,000 hours, i.e., 3,750 8-hour working days; minus weekends, a total of about ten years. One understands why the history of Dante scholarship has also been a history of short cuts devised to pare down the task to more manageable dimensions.

But the problems confronting students of the tradition of the *Commedia* are not merely of a quantitative nature. Many manuscripts carry series of alternative readings, one in the text and one or more in the margins. Others juxtapose alternative readings within the text itself (double readings). Many of the parchment manuscripts have been corrected here and there over erasure (Lat. *rasura*), that is, after abrasion of the original reading (Lat. *scriptura prior*) with a knife. In some cases, glosses, i.e. marginal comments (e.g., *Virgilio* for *lo savio mio* or *fiume* for *l'altra [acqua]*) have been mistaken for textual variants and included in the text. In other cases, scribes who had already copied the *Commedia* unconsciously replaced some parts of the verse (formulae introducing direct speech, synonymous pairs, and such) with variants attested elsewhere in the text, which had lingered in their memory. A significant part of the tradition of the *Commedia* (for some, the whole tradition) is, or at least has appeared to be, inextricably contaminated and impervious to genealogical rationalization.

The first scholarly editor of the *Commedia* was the German jurist Karl Witte (Lochau, 1800-Halle 1883), who, after collating a broad selection of passages from about a hundred witnesses, decided to move away from the *textus receptus* (the Aldine edition edited by Bembo in 1502, more or less modified). Witte used 4 manuscripts, designated as A B C D, corresponding to the following currently used symbols (Lat. *sigla*), introduced by the Petrocchi edition: A is ms. LauSC, the above-mentioned "edition" drawing on several witnesses by Filippo Villani, a Humanist of the circle of Coluccio Salutati; B is the no less famous Vat, a ms. possibly sent as a gift by Boccaccio to Petrarch; C is Berl.; D is the Caetani codex. In the light of my own team's investigations, today we know that Berl, Caetani and LauSC, can all ultimately be traced back to the same zone of the tradition of the *Commedia*, a relatively popular one, but not of outstanding textual quality, viz., a thoroughly contaminated constellation of manuscripts, not far removed from Vat and the so-called Vatican group. Nevertheless, Witte's text (1862, republished in the same year in an *editio minor* with slight changes) constituted a significant advance and was a decisive influence on all subsequent editions, with the possible exception of the very recent one by Federico Sanguineti (2001).

The second edition that one should not fail to mention is the monumental one by the Reverend Edward Moore (Cardiff, 1838-1916), Principal of St Edmund Hall, one of the oldest colleges in Oxford. It was planned in 1879 and published in 1889 by Cambridge University Press. Some letters of Moore to Witte—to whose memory the edition is dedicated—shed light on the progress of this undertaking from 1879 to 1882 (Lindon, "Notes on the British Contribution...").

Besides providing a description of witnesses ("Account of the manuscripts examined or collated": MOORE, 511-679), which usefully complements the still fundamental *Bibliografia dantesca* by Paul Colomb de Batines (1846; anast. reprint Roma, Salerno ed., 2008, 3 vols.), Moore—who reproduced the text of Witte's *editio minor* (MOORE, XLVII)—supplemented the text of the *Inferno* with a large apparatus of variants. The apparatus draws on 17 mss., chosen among those kept in the rich libraries of Oxford, Cambridge and London (the text and apparatus are at pp. 1-254).

Moore also provides variants carried by a large number of witnesses for many *loci critici* both in *Purgatorio* and in *Paradiso*: pp. 257-504). In his words, “nearly half of that total [sc. the *circa* 500 mss. that constitute the surviving tradition for Moore] have in the case of the more important passages been examined” (MOORE, “Preface”, page not numbered).

The work, complete with useful synoptic tables and appendices, is preceded by important “Prolegomena” (MOORE, v-xlv) expressing philological conceptions very advanced for the time, as beffited an accomplished classical philologist (in 1875 Moore had published *Aristotle's Poetics, with Notes*), although only applied to the “discussion of selected passages” (the text of Moore's *Inferno* faithfully adheres to that established by Witte).

Some remarkable features of Moore's approach, in order of appearance, include:

- his interest in indirect tradition, that is, in our case, ancient commentators (the most innovative biblical scholars—maybe in the wake of Bentley's project, mentioned, among others, by TIMPANARO, 33-34 [= ID. (MOST), 63-64])—used quotations by the Fathers of the Church to “date” different versions of the New Testament);
- his certainty that most of the witnesses carried a *lezione volgata* [vulgate version] and that the great problem of textual criticism (whether in Dante or not) was “to get behind this ‘lezione volgata’” (MOORE, vii);
- his notion that the traditions of works in *volgare* [vernacular] are those more subject to free variation by copyists (Paris had already expressed a similar notion, although the distinction between more or less *quiescent* traditions, like those of the classics, and more or less *active* ones, like those of Romance texts, was only introduced in 1970, by Alberto Varvaro, “Critica dei testi...”);
- his firm rejection of the interventionist philology of the renowned Cambridge classicist Richard Bentley (1662-1742), for reasons not unlike those later adduced by one of the most sophisticated scholars of our time, Sebastiano Timpanaro; Bentley, Timpanaro argues, is indeed “one of the most brilliant Classical philologists of all time”, but “in the overwhelming majority of cases, [his] hundreds of conjectures on Horace are ‘corrections’ not of the transmitted reading but of the poet” (TIMPANARO, 24-26 [= ID. (MOST), 55-56]);
- his recognition of the widespread tendency for scribes to innovate at the graphic and phonomorphological level and in word order, and trivialize when faced with difficult words or constructions;

- his clear explanation of how a gloss could be incorporated in the text (“The word *Virgilio* was originally a *gloss* to explain the reference of *Lo savio mio* [my learned master] and was mistaken for a correction of *in ver lui* [*Inferno* XII 16], and so introduced into the text”: MOORE, xx);
- his drawing on the works of the contemporary New Testament philologists Brooke F. Westcott and Fenton J.A. Hort both for the concept of *conflate reading*, “i.e. a reading formed by the *combination* of readings intended to be *alternative*” (It. *doppia lezione*), a typical byproduct of the contamination of readings (→ 3.4), and for the distinction between mss. from a good tradition (“good foundation text”) disfigured by scribal mistakes, and poor mss. faithfully copied by the scribes (“deeply adulterated texts” etc.: ID., xxi-xxiii).

¶ On this distinction, already formulated by the best eighteenth-century philologists, see TIMPANARO, 39 [= ID. (MOST), 69];

- his confident adoption of common errors as “proof of connection between the MSS.” (MOORE, xxiv);
- his downplaying of the criterion of “mere antiquity” of a ms., which is no “guarantee of the purity of its text”, and that of the numerical prevalence of a reading (ID., xxiv-xxv); and his criticism of the practice of only indicating in the apparatus the number of mss. offering a certain variant (ID., xxvii);
- his appreciation for Mussafia’s collations and sagacious analysis of the Vienna and Stuttgart codices in 1865 (ID., xxix).

In 1894, Moore also published an edition of all of Dante’s works (*Tutte le opere di Dante Alighieri*) with Oxford University Press. This edition was repeatedly updated and remained an unsurpassed model for decades, thanks in part to Paget Toynbee’s indexes, added to the third edition (1904). Although Moore’s text of the *Commedia* was largely based on the one published by Witte in Berlin, many passages were modified on the basis of collations and observations by Moore himself and by other *Dantofili* of his time (Lindon, “Notes on the British Contribution...”, 233-234).

* * *

Another milestone of Dante studies was not an edition, but a mere list of points of variation selected with great ability by the recent graduate Michele Barbi (but only signed by the then more authoritative scholars Adolfo Bartoli, Alessandro D'Ancona and Isidoro Del Lungo). This was a tool designed for tracing back with ease most of the manuscripts to the different families.

The list, known as the “canone barbiano” or the “400 luoghi del Barbi” (actually 396), was meant to allow Dantists to cooperate internationally, collating mss. kept in their cities of residence—this at a time when traveling was time-consuming and expensive, and the very reliable means of reproduction available today were still a long way in the future. Although it must be acknowledged that Barbi was elaborating on and refining ideas that had already been put forward by Witte, Mussafia and MOORE, xxxiv-xxxv, his approach was exceptionally modern. What Barbi was promoting was the application to Dante scholarship of an improved version of a collaborative method that had possibly been inaugurated by Witte himself and had been adopted, among others, by the celebrated Theodor Mommsen for the manuscripts of Livy (Lindon, “Notes on the British Contribution...”, 230; Reeve, “Gruppenarbeit...”, 316-317). But, unlike classicists, only very few Dante scholars responded positively to the invitation, which appeared in 1891 in the *Bullettino della Società Dantesca Italiana*, the organ of the newly established (1888) Società dantesca italiana.

As Barbi repeatedly declared, and as a recent study by Caterina Brandoli confirms, his short cut was based not on a random selection but, on the contrary, on a carefully pondered list of passages whose effectiveness for singling out families he had already tested on mss. in Florentine libraries and the materials published by Witte, Mussafia and Moore. It was a very acute application of the method of *loci critici*: non-exhaustive collation, limited to a predefined series of variation places—a short cut that is only justified in the face of very long texts with overabundant traditions. As Barbi himself was to specify:

The point here is above all to ascertain whether, besides the two families of codices that have manifested themselves so far, which also need more accurate verification [*sc.* Casella's families α and β ; → 9.2], there exists some tradition independent of them. I am increasingly convinced that, to facilitate this research, one needs to carry out that first cull among the great mass of the codices of the *Divina Commedia* for which, forty years ago, I devised and prepared a selection of four hundred passages for the Società

Dantesca. *It was the result of long study and thus not determined a priori, that is, randomly, as were some other selections proposed around the same time* (Barbi, “La nuova filologia...”, 34; my emphasis).

Certainly Barbi was fully aware that this was not the only necessary collation; it was merely a preliminary operation, but one that would be sufficient for picking out from the vastness of the tradition, on the one hand, all mss. belonging to the most numerous families, on the other, the more promising isolated witnesses—*independent of or predating the formation of the vulgate texts—which could then be subjected to a more “accurate examination”*:

But it is true that a partial examination will leave out the best evidence even from the simplest groupings, and that, in the face of the oscillations we see in that table [i.e. the table which precedes the present quotation], nothing certain can be concluded regarding the relationships between the groups, because we do not know what results the examination of the remaining 14,113 verses in the poem would yield (Barbi, review of Fiammazzo, 156-157; my emphasis).

Among the copies most in need of accurate examination [...] I find one should include: [...] four manuscripts which Vandelli thought should be assigned to α [i.e. Casella's α family] rather than to β [Casella's crowded β family]; that is, the Riccardiano-Braidaense [...]; Vatic. Urbinate 366 [...]; Madrid L-110 [...]; Palermo, National Library, XIII G 1 (Barbi, “Il codice di Francoforte...”, 181).

¶ The Madrid ms. (Mad) and the Riccardiano-Braidaense (R or Rb) are mss. of great importance. Ms. Urbinate 366 (Urb or U) is unanimously regarded today as the most authoritative witness of the *Commedia*.

As Gianfranco Folena observed, the background of the Witte-Moore-Barbi line, “a German-English-Italian philological *iter*” that founded modern Dante philology, is “the whole history of European culture” in the nineteenth century:

The Romantic philology of Witte, born in Lochau near Halle at the dawn of the century, nourished on Protestant *philologia sacra* and the juridical-historical culture of the Restoration, the empiricist philology of the Reverend Mr. Moore, born in Cardiff in 1835, nourished on classics and the study of New Testament, and the positive [i.e., positivist] philology of Barbi, born in Sambuca Pistoiese in 1867 (Folena, “La filologia dantesca...”, 26-27).

Bibliographical notes. For an introduction to the tradition of Dante's works, some still useful publications include G. Folena, "La tradizione delle opere di Dante Alighieri", in *Atti del Congresso Internazionale di Studi danteschi*, Firenze-Verona-Ravenna, 20-27 aprile 1965, Firenze, Sansoni, 1965, I, 1-78, and C. Ciociola, "Dante", in *Storia della letteratura italiana*, diretta da E. Malato, X, *La tradizione dei testi*, Roma, Salerno Ed., 2001, 137-199. Some recent reviews on the most recent textual studies are cited in the final *Notes* to the present chapter.

On Witte, there exists a very in-depth essay by Folena himself, "La filologia dantesca di Carlo Witte" (1967), now in id., *Filologia e umanità*, a cura di A. Daniele, Vicenza, Neri Pozza, 1993, 25-52.

On the Reverend Dr. Moore's Dante studies: J. Lindon, "Gli apporti del metodo di Edward Moore nei primi decenni della Società Dantesca Italiana", *Filologia e critica* 14, 1989, 105-120 (later in *La Società Dantesca Italiana 1888-1988. Convegno Internazionale. Firenze 24-26 novembre 1988*, Palazzo Vecchio-Palazzo Medici Riccardi-Palagio dell'Arte della Lana, a cura di R. Araldo, Milano-Napoli, Ricciardi, 1995, 37-53); A. Baicchi, "Uno scritto inedito di Edward Moore relativo al "Comentarium" di Guido da Pisa", in *Miscellanea di studi danteschi in memoria di Silvio Pasquazi*, Napoli, Federico & Ardia, 1993, I, 33-43; J. Lindon, "Notes on the British Contribution to the Nineteenth-Century Rise of Dante Studies: Edward Moore and the Text of the Commedia", in *Dante the Lyric and Ethical Poet. Dante lirico ed etico*, ed. by Z.G. Baranski and M. McLaughlin, Oxford, Legenda, 2010, 227-239. I have so far not managed to consult A. Baicchi's master's dissertation *Gli studi danteschi di Edward Moore*, Università degli Studi di Pisa, academic year 1985/86.

To appreciate the modernity of the positions adopted by Moore, who did not hesitate to side openly with the Biblical philologists Westcott and Hort against the zealots of the *textus receptus* of the New Testament, one needs to realize how cautiously that issue was treated in the academic milieus of the time, as is clearly illustrated in the book *The Oxford Debate on the Textual Criticism of the New Testament held at New College on May 6, 1897*, London-Oxford-Cambridge 1897 (anast. reprint: Kessinger Publishing, s. a.).

Varvaro's essay quoted in the text is "Critica dei testi classica e romanza. Problemi comuni ed esperienze diverse", *Rendiconti dell'Acc. di Archeologia Lettere e Belle Arti di Napoli* 65, 1970, 73-117, later in ID., *Identità*, 567-612.

On Barbi, one can consult the entry "Barbi, Michele", by Francesco Mazzoni, in the *Dizionario Biografico degli Italiani* (also available online).

On Mommsen's collations of Livy, see M.D. Reeve, "Gruppenarbeit an Handschriften" (1987), now in ID., *Manuscripts and Methods*, 315-321.

Barbi's canon of *loci critici* was published with a preamble by some scholars more authoritative at the time: A. Bartoli, A. D'Ancona, and I. Del Lungo, "Per l'edizione critica della 'Divina Commedia'", *Bullettino della Società Dantesca Italiana* 5-6, 1891, 25-38: 28-38. A useful reading is Caterina

Brandoli's essay cited above, "Due canoni a confronto: i luoghi di Barbi e lo scrutinio di Petrocchi", in NP, 99-214.

The other essays by Barbi quoted above are: his review of Antonio Fiammazzo, "Nuovo spoglio del codice Lolliniano di Belluno e raffronti con altri 'del Cento'" (Bergamo, Istituto italiano d'arti grafiche, 1897), *Bullettino della Società Dantesca Italiana* 4/9, 1897, 137-158; "Il codice di Francoforte e la critica del testo della *Commedia*", *Studi danteschi* 23, 1938, 180-182; *La nuova filologia e l'edizione dei nostri scrittori. Da Dante al Manzoni*, Firenze, Sansoni, 1938 (anast. reprint with a bibliography of Barbi's writings edited by S.A. Barbi and an introduction by V. Branca, Firenze, Le Lettere, 1994).

9.2. The state of the question (1921-1967). Casella, Vandelli, Petrocchi

A choice of manuscripts, in my opinion, can only be based on their age. [...]. There is no obstacle of a concrete nature to choosing such a criterion, nor does Pasquali's "*recentiores non deteriores*", which documents an exception, not a rule, apply to the *Commedia* or to the great majority of the textual situations of fourteenth-century texts [...]. If the editor can prove that, as transmission develops, both vertical and horizontal corruption of the text increases, and if he or she manages not only to prove that the early codices are all the more free of error and corruption, but also that all the commonly accepted readings and those that are traditionally agreed on by earlier editors come from early codices, and that recent texts show a disruptive process in action, there will be no possible objection.

Petrocchi, "Proposte...", 111-112

Even in a brief overview like the present one, the names of Giuseppe Vandelli and Mario Casella cannot be left out. Vandelli (Modena, 1865-1937), after moving to Florence with his family in 1883, completed his studies at the Istituto di Studi Superiori Pratici e di Perfezionamento, where one of his professors was his high-school teacher in Modena, Pio Rajna, an old acquaintance of ours (→ 1.1). Rajna suggested to his high-school student a dissertation on the manuscript tradition of *I Reali di Francia* by Andrea da Barberino.

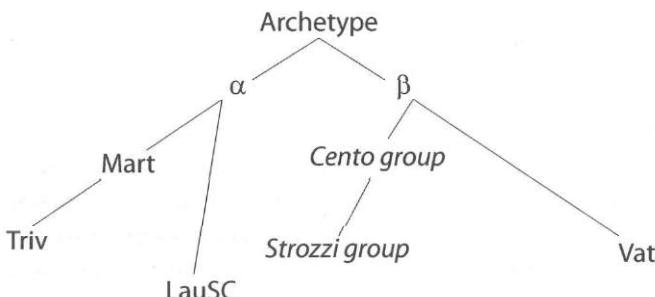
Vandelli's many important studies on Dante, recently collected in a posthumous book, culminated in his critical text of the *Commedia*, published along with Dante's other work in *Opere di Dante* by the Società Dantesca Italiana for the centenary of 1921 (Firenze, Bemporad).

Though an accomplished paleographer and a learned commentator on Dante (he produced a radically updated and very popular revision of Scartazzini's nineteenth-century commentary), Vandelli was unable to make rational sense of the apparently contradictory philological data that emerged from his collations of witnesses. Thus, in spite of his training as a genealogical critic, he decided to reconstruct the text of the *Commedia* line by line, forgoing the advantages of stemmatics. His edition, which he retouched here and there in the years that followed and which was often reprinted by Hoepli with Scartazzini's commentary and his own, is nevertheless anything but inferior, and many of his studies are still valuable.

Casella, too (Fiorenzuola d'Arda, 1886-Firenze 1956), enrolled in the University of Florence, where he was educated in the school of Pio Rajna, Ernesto G. Parodi and Guido Mazzoni. He collaborated on the edition of the critical text of Dante's works published by the Società Dantesca in 1921, for which he compiled the *Indice analitico dei nomi e delle cose*. In 1925, like Pasquali, who was of the same age, he was one of the few signatories of the *Manifesto degli intellettuali antifascisti* drawn up by Benedetto Croce. In 1931 he gave some courses at Columbia University in New York. From 1949 on he directed the journal *Studi Danteschi*, founded by Barbi in 1920.

In 1923, Casella produced a remarkable edition of the *Commedia*, published by Zanichelli, followed in 1924 by a philological study including a stemma of the tradition, which is decidedly outdated today but nevertheless remains historically significant.

His stemma often misleads less expert scholars, because the designations α and β , which would later be taken up again, but with different meanings, by the subsequent editors of the *Commedia*, both refer to the Florentine tradition, which is numerically superior, but which I am not alone in regarding as very corrupt:



More precisely, this stemma accounts only for the denser strands of the earlier Florentine tradition (the *gruppo del Cento*, the Vatican group) and for three other still controversial witnesses, namely, LauSC (the base text of the Witte edition), Triv, the earliest dated Florentine codex (of 1337), and Mart—an Aldine edition with variants drawn from a lost ms. of 1330-1331, which had been studied by Vandelli a couple of years earlier (1922).

Another jurist who, like Witte, converted to literary studies, Giorgio Petrocchi (Tivoli, 1921-Roma, 1989), undertook a much more ambitious and important work, whose shortcomings, however, have been making themselves felt for some time. Petrocchi nevertheless has the great merit of having provided in his edition a complete collation of 27 mss. chosen from among the earliest (or those believed to be such). Possibly unconsciously, he sought to differentiate himself from Barbi. He therefore revived, with some adjustments, the notion already formulated by Carlo Negroni in the nineteenth century that the whole tradition later than the mid fourteenth century was much more contaminated than the earlier tradition, and hence untrustworthy and unusable.

The short cut devised by Petrocchi (methodologically more dangerous than Barbi's, because it sacrificed so many *recentiores* that were not necessarily *deteriores* until proved so → 3.3) was, as one can easily imagine, that of focusing on the first generations of manuscripts—the vulgate text, or rather the several vulgate texts, in circulation during the first thirty years or so after Dante's death—; a criterion already announced in the title of his important edition of 1966-67 (*La Commedia secondo l'antica vulgata* [The Comedy according to the early vulgate text]). While Negroni had proposed 1350 as a (conventional and unsubstantiated) cutoff date, for Petrocchi the uncrossable threshold was 1355, because that is when, he claimed, Boccaccio's *editio* started to spread, an event that in his opinion increased contamination exponentially.

Petrocchi's edition (included in the prestigious Edizione Nazionale delle Opere di Dante published by the Società Dantesca Italiana) consists of a first introductory volume followed by one volume for each *cantica*. The text has two levels of apparatus, the first with the variants, the second with comments on editorial decisions (Figure 4). The Introduction (almost 700 pages) contains a census of manuscripts and a far-ranging

vii 6 - 22

Così scendemmo ne la quarta lacca,
pigliando più de la dolente ripa
che 'l mal de l'universo tutto insacca.

18

Ahi giustizia di Dio! tante chi stipa
nove travaglie e pene quant' io viddi?
e perché nostra colpa sì ne scipa?
Come fa l'onda là sovra Cariddi,

21

16. >*Noi de<scendemo La* (lez. origin. quasi certamente *Cosi scendemo*); *Cossi Mad Pa Rb; scendemo Ash Cha Fi Lau Lo Pr Ricc Tz, cademo Co, sciendemo*

Mad, sendemo Pa Rb, sciendemmo Po; lacha Laur Mad Pa Rb

17. *prendendo Vat; pu Mad; dolente<via>ripa Mad*

18. <*tucto*> *tucto Ham, tanto Rb; nsacca Cha Ham Mart Vat, sciaccha Eg, ensacca*

La, insacca Laur Mad Rb

19. *A (o Ha) Ash Ham La Laur Pa Po Rb Si Urb; iusticia Mad, giusticia Urb;*

diddio Co; tanta che Ash, tanto qui Co, tante che La Pa Urb, chi tante Mad, quante

che Rb

20. *noui trauagli Ash, nuouo trauaglo Rb; nuoue >trauagie e< Eg; nuoue pene e*

trauagle Co Pr; penne Mad Pa; uidde Laur, uidi Mad Pa Pr Rb

21. *se ne scipa Cha Co Eg Fi Lau Lo Parm Po Pr Ricc Tz, se ne stipa Ham*

La Laur Pa Urb, se ne scippa Mad, se ne schipa Rb

22. *Comme Pa; lunda Pa; soural Ash Po; cariddi Ash Pa, caridde Laur, caridi*

Mad Rb

16. La variante di correzione in La ha avuto una storia molto limitata; cfr. in Cass. *Noi* e in Cambr. Mm. 2 3¹ *discendiamo*.

17. *prendendo* di Vat è equipollente, ma contestualmente *facilior* (cfr. Inf. XII 28; XXIV 61 ecc.).

19. *tante chi stipa*: 'chi stipa tante ... se non tu?'. La lezione a testo prevale indubbiamente sulla variante espressa da La Pa Urb, che darebbe un senso più contorto, perché meno collegato in forma diretta con l'esclamazione *Ahi giustizia* (cioè: *tant'è che stipa*, 'è tanto grande la giustizia divina da stipare', 'ammassare' ecc.), e tale da lasciare ingiustificato il corrispondente *quante*. La variante si è comunque sviluppata in una fase più tarda (*tante e* in Bol. Un. 589, Can. 115 ecc.) ed è ammessa da Benvenuto.

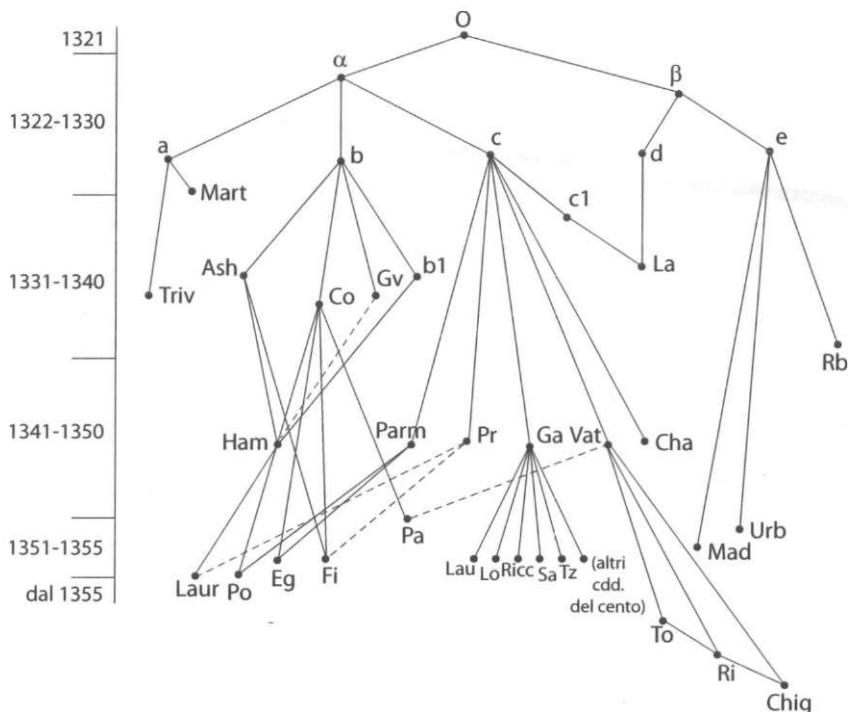
21. Il richiamo del v. 19 ha prodotto il trascorso di Ham La ecc.; *se ne scipa* dà un significato insoddisfacente ('se ne sciupa', 'se ne strazia') rispetto alla lezione di Ash Mart Triv Vat.

22. *che sopra* nell'Udinese del Seminario.

III

Figure 4. Dante Alighieri, *La Commedia secondo l'antica vulgata*, a cura di G. Petrocchi, 2, *Inferno...*, p. 111.

(but not faultless) discussion of their reciprocal relationships, graphically summarized in the following stemma:



For a long time, scholars found some features of the stemma convincing, such as the splitting of the tradition into a northern branch, β , and a Florentine branch, α , independent of the former, although these features disagree with what we know about Dante's life and the early dissemination of the poem. Other arguments—e.g., Petrocchi's confirmation, following Vandelli, of the existence and importance of subfamily a , his realization of the close relationship between Ash and Ham, or his stressing of the exceptional importance of Urb, or U—still hold water today.

And so it was that, within three decades or so, Petrocchi's selection of 27 early vulgate copies was mistaken—by an understandable but risky metonymy—for the totality of the pre-Boccaccio tradition, of which it is merely a random sample (the surviving mss. that meet Petrocchi's definition of “antica vulgata” are close to one hundred). The 1966-1967 edition became, not the most up-to-date “working hypothesis” about the *Commedia*, but the text of the *Commedia*. And the editor's reconstruction was

understood as *the* history (the only possible history) of the transmission of the *Commedia*.

Now Petrocchi's stemma, which seems to establish once and for all the genealogy of every witness under examination, and was long regarded as a model, is actually (as I have already observed elsewhere) "more symbolic than to the point", not only because, for understandable reasons of working economy, Petrocchi did not always study in depth the reciprocal relationships of different copies, but also because many of his solutions have revealed or are revealing themselves to be unfounded.

For example, from left to right:

- a. it is very doubtful whether Ham, however contaminated, derives *recta via* from Ash;
- b. Laur, Po, Eg, Fi, Pa, which are earlier or only slightly later than 1350, *cannot* derive from Co, which is a codex of the late fourteenth century (and hence much later than Petrocchi thought) and full of separative errors (→ 1.3), which invalidate that area of the stemma;
- c. Gv, too, erroneously dated *mcccxxxvii* (1337), is a late codex—of 1387 or 1437, depending on whether the copyist skipped an *l* or, by haplography, a fourth *c*—and cannot in any way have influenced Ham, which is certainly of 1347;
- d. the existence of *b1* is unprovable (at least 2 mss. are needed for reconstructing a lost ancestor);
- e. Lau Lo Ricc Sa Tz *cannot* derive from Ga, which is relatively late (1347) and above all shows many separative errors;
- f. Vat and Cha are to a certain extent relatives of Parm, Pr and Ga, but above all they share an ancestor, *vat*, with another five mss. by the same hand not collated by Petrocchi (Pomaro, "L'officina vaticana...");
- g. To Ri Chig, which anyway do not belong to Petrocchi's early vulgate tradition, *do not* derive from Vat, but from a relative of Vat (on this point, one can now read Mecca, "Il canone editoriale dell'antica vulgata di Giorgio Petrocchi...");
- h. as in the case of *b1* (point d), the existence of *c1* and *d* cannot be proved;
- i. witness La, of 1336, *cannot* be ascribed to β (as is almost always the case when he is dealing with manuscript revisions, Petrocchi does not distinguish the original textual layer of La and the revisions of La₂, by a later hand, which contaminates La by using a relative of the Frankfurt codex, Franc);

- j. Mad and Rb do not share indicative errors only with Urb—which therefore remains the only member of the β family in the Petrocchi stemma—and both probably descend from a common ancestor $x1$.

Finally:

- k. Petrocchi did not prove the existence of the ambiguous O (archetype? original?), on whose nature a sometimes Byzantine discussion has flared up among Dantists. His failure to prove the existence of an archetype allows the possibility that there existed two different (both authorial?) versions of the *Commedia*, which philologists should keep distinct.

Fortunately, Petrocchi was the first to mistrust his own stemma, and so omitted to draw from it the inevitable consequences in Neo-Lachmannian terms:

1. He did not group the converging readings of the various manuscripts in the families and subfamilies indicated in his stemma (in his negative apparatus, each reading different from that in the text is followed by the alphabetically ordered designations of all the mss. it appears in, while the symbols α , β , a, b etc. are never used);
2. He did not eliminate the manuscripts which according to his stemma are *descripti* (from left to right: Ham, Laur, Po, Eg, Fi, Pa, Lau, Lo, Ricc, Sa, Tz etc.).
3. He almost never applied the majority principle. Indeed, he systematically ignored his own stemma in the constitution of the text, which he largely established case by case, *à la* Vandelli.

Thus (philosophers would say: by heterogenesis of ends), Petrocchi fully preserved a hefty and valuable mass of variants, largely substantive, handing it down to later scholars. He collated his set of witnesses (22 complete or almost complete, 2 respectively containing the *Inferno* and the *Paradiso*, plus 3 fragments) with commendable accuracy (except for cases of early revision) for all the 14,233 verses of the poem. The apparatus of variants in his edition, and its second level of apparatus (where he explains, or tries to explain, his textual decisions, referring to earlier literature and often appealing to the experience and advice of Gianfranco Contini) constitute, and will continue to consti-

tute, an extraordinary springboard for any study of the tradition of the *Commedia*.

Bibliographical notes. *La Divina Commedia*, testo critico a cura di M. Cassella, Bologna, Zanichelli, 1923; id., "Studi sul testo della *Divina Commedia*", *Studi danteschi* 8, 1924, 5-85; Dante Alighieri, *La Divina Commedia*, testo critico della Società Dantesca Italiana, riveduto, col commento scartazziniano rifatto da G. Vandelli, Milano, Hoepli, 2008 (anastatic reprint of the twenty-first edition); id., *Per il testo della 'Divina Commedia'*, a cura di R. Abardo, con un saggio introduttivo di F. Mazzoni, Firenze, Le Lettere, 1989; G. Petrocchi, "Proposte per un testo base della 'Divina Commedia'" (1955), now in id., *Itinerari danteschi*, premessa e cura di C. Ossola, Milano, Franco Angeli, 1994, 104-133; Dante Alighieri, *La Commedia secondo l'antica vulgata*, a cura di G. Petrocchi, Milano, Mondadori, 1966-1967, 4 vols. (and subsequent editions).

Perplexities about Petrocchi's stemma, text and editorial canon have been voiced chiefly by F. Sanguineti, "Per l'edizione critica della 'Comedia' di Dante", *Rivista di Letteratura Italiana* 12, 1994 (but: 1996), 277-292; P. Trovato, "Intorno agli stemmi della 'Commedia' (1924-2001)", in NP, 615-624; A.E. Mecca, "Il canone editoriale dell'antica vulgata di Giorgio Petrocchi e le edizioni dantesche del Boccaccio", in NP₂, 119-182.

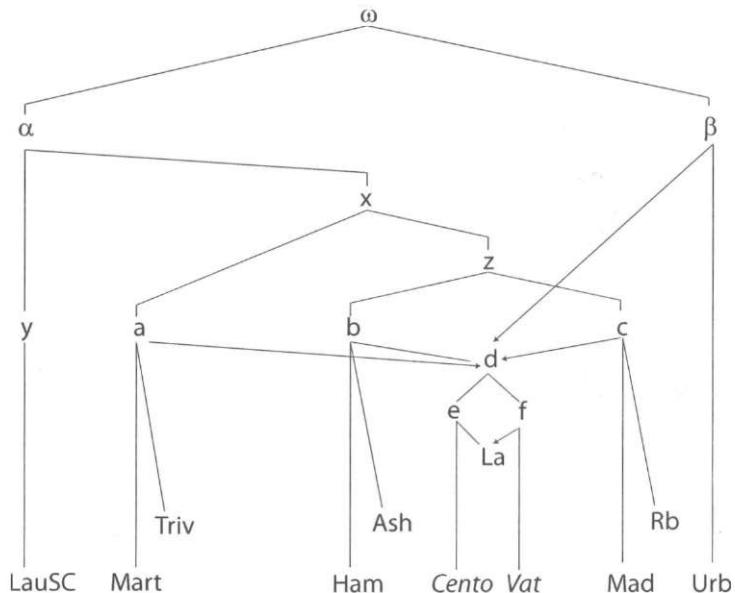
The article by Gabriella Pomaro is "Codicologia dantesca 1. L'officina di Vat", in *Studi danteschi* 58, 1986, 343-374.

9.3. The state of the question (1996-2001). The contribution of Federico Sanguineti

Barbi (*loci selecti*), Bédier (*bon manuscrit*) and Lachmann (*stemma*) are to be thanked for the method followed here. Any flaws are imputable to the editor of the present edition, who is *minimus* among philologists.

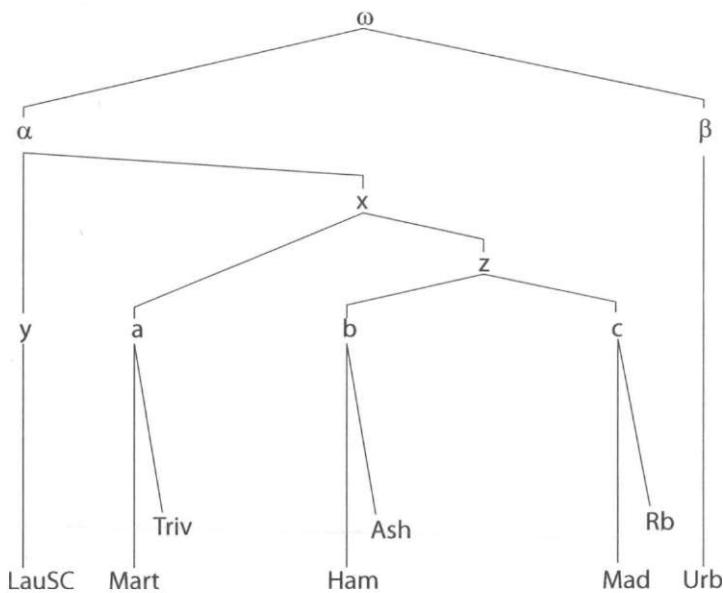
Federico Sanguineti, "Premessa", in *Dantis Alagherii Comedia...*, IX

After a few years of preparatory studies, in 1996 (but the year given in the journal is 1994) Federico Sanguineti published a short essay with a shocking title and content. In the essay, which is simply titled "Per l'edizione critica della 'Comedia' di Dante" [Towards a critical edition of Dante's *Comedy*], Sanguineti explains that Ham is not a *descriptus* but an independent relative of Ash, and Co and Gv (as well as Ga and La and Lau) are misplaced in the Petrocchi stemma. Consistently with his Neo-Lachmannian approach, Sanguineti also offers a couple of proofs of the existence of an archetype. He thus proposes a stemma that is inevitably very different from Petrocchi's:

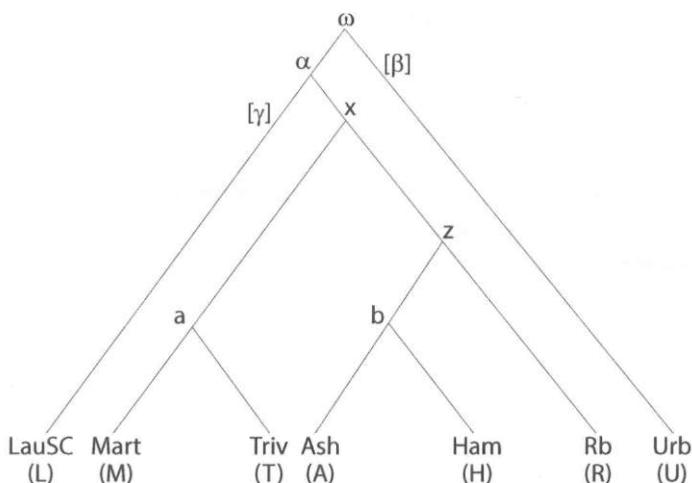


For now, I will only highlight its most momentous innovations, referring the reader to NP and SHAW for a detailed discussion of the positions of LauSC, Mad and Rb. First of all, after discarding presumed hyparchetypes, such as Co and Gv, Sanguineti persuasively traces La, the *Cento*, the Vatican group, Fi, Po, and the so-called “complementary witnesses” (Petrocchi) Eg, Laur and Pa to a single ancestor *d*, “corresponding to Casella’s old β ” (Sanguineti, “Per l’edizione critica...”, 279). Furthermore, his relocation of the northern witnesses Mad and Rb from branch β to branch α of the stemma has major stemmatic and textual implications.

The hypothesis that *d* is a descendant rather than an independent relative of *b* is less convincing, since the conjunctive errors Sanguineti points out only prove beyond doubt that *b* and *d* are relatives (besides, La and others have good readings corresponding to some errors in *b*). But the highly contaminated nature of *d* would in any case authorize, in my opinion, the discarding of the witnesses to it, not as *codices descripti*, but as *codices inutiles* (\rightarrow 3.4). After this broad *eliminatio*, the stemma of 1994 appears as follows:



After a few more years, during which Sanguineti collated about a third of the manuscripts in Barbi's 400 *loci* (the remaining two thirds of mss. were collated in a very restricted number of passages only), his edition came out (2001). Unless I am mistaken, it was the first edition of the *Commedia* to call itself (in my opinion, with reason) an *edizione critica* [critical edition]. In the edition, which is introduced by extremely concise *Prolegomeni* and supplemented by two levels of apparatus (on which more below), Sanguineti proposes an even slimmer diagram, where for just a few errors we lose Mad, i.e., the Genoese relative of Rb (*Dantis Alagherii Comedia...*, LVIII).



The extreme slimming down of Sanguineti's stemmas—but from now on I will only refer to the more complete 1994 one—is reached at the cost of a not exactly textbook but even heterodox *eliminatio codicum descriptorum* (→ 1.4), based on very few errors. This has caused opposite reactions, from the appreciation of some reviewers to the barely concealed irritation of others, curiously forgetful of the fact that the history of Dante philology is also a history of attempts to devise clever short cuts to avoid getting bogged down in a mire of almost 600 non-fragmentary witnesses.

Let us try, for the sake of expediency, to define what historians of science would call Petrocchi's paradigm. This is negatively characterized by the impossibility of:

1. proving the existence of the archetype;
2. classifying the 27 manuscripts selected on the basis of a series of mono-genetic errors, and thus reducing them to relatively stable families;

as well as by:

3. renunciation of the stemma as a tool for choosing between the great number of equally acceptable variants (“the classification of witnesses of the early vulgate tradition does not yield a fully-fledged reconstructive canon”: Inglese, *Come si legge un’edizione critica...*, 153).

Today, less than forty years later, the situation has radically changed. Sanguineti's work has confirmed, precisely because its premises are different, that some decisions and several details in Petrocchi's reconstruction were reasonable (all of Sanguineti's principal witnesses except one belong to the *antica vulgata*; U—in Petrocchi's opinion, the most authoritative exponent of family β—appears even more valuable in the light of Sanguineti's re-examination, etc.). But the new "working hypothesis" questions some of the basic assumptions of Petrocchi's edition (e.g., the importance of some of the earlier dated manuscripts, La, of 1336, Gv, of February 1337 *more fiorentino*, i.e., 1338 [to be emended, however, to 1387-1388 or 1437-1438], Triv, of 1337) and substantially modifies the shape of the stemma, that is, the reconstruction of the relationships between the witnesses. In the new paradigm, as represented by Sanguineti's edition:

1. the transmission of the *Commedia* is traced back to an archetype;
2. with few exceptions, the overwhelming majority of the witnesses carrying at least one Cantica (that is, some twenty mss. of the *antica vulgata* along with several hundred discarded by Petrocchi, not counting the fragments) is traced back to a single, already highly contaminated ancestor *d* and discarded, on the basis of a few conjunctive errors.

Finally:

3. unlike Petrocchi's stemma (very often ignored by its own author in the constitution of his text), Sanguineti's (to paraphrase Inglese, quoted above) "yields a fully-fledged reconstructive canon".

In other words, whether it is right or wrong (I shall be addressing this issue in the next section), the stemma of the latest editor of the *Commedia* "works". This allows the substance of the text to be reconstructed largely on the basis of binding, mechanical decisions, through the application of methods developed in the positivistic nineteenth century by the founding fathers of Romance textual criticism (→ 1). The result of the reasonably argued "promotion" of the northern ms. U to sole representative of one of the two families is that (save for a limited number of exceptions, the so-called polygenetic trivializations) when an acceptable—although not necessarily irresistible—reading in U is confirmed by members of the other branch, this reading is accepted as the most probably correct one on the strength of the majority principle.

Furthermore, Sanguineti has adopted the healthy mistrust of late-twentieth-century philology for excessively carefree reconstruction of the textual substance. He nearly always adheres to the base witness, even in the not infrequent cases when the other branch of the tradition has what are, at least apparently, equally acceptable readings.

Actually, most of the objections of the reviewers of the Sanguineti edition are not aimed at his stemma, but at his almost Bédierist approach to the critique of linguistic features. Having adopted as his base witness the northern ms. U, which today still seems to be the best representative of the branch closest to the archetype, β , Sanguineti also uses it as his model for the linguistic form of his text. He does edit out the numerous forms in U that are unacceptable in Florentine, but he takes care to highlight all these changes by placing them in italics or square brackets. While Sanguineti points out substantive divergences from his text in his highly formalized but ultimately traditional second level of apparatus (thank God a positive apparatus), his adjustments to the linguistic *facies* of U are detailed in the first level, which is reserved for the forms that occur in the base witness (Figure 5).

Bibliographical notes. F. Sanguineti, "Per l'edizione critica della Comedia di Dante", *Rivista di Letteratura Italiana* 12, 1994 (but: 1996), 277-292; *Dantis Alagherii Comedia*, edizione critica per cura di F. Sanguineti, Firenze, Edizioni del Galluzzo, 2001.

Some of the more wide-ranging reviews and discussions include: R. Abardo, *Rivista di Studi Danteschi* I, 2001, 153-162; T. Brückner, *Deutsches Dante Jahrbuch* 82, 2007, 201-207; C. Caruso, *Modern Language Review* 98/3, 2003, 729-731; E. Fenzi, *Soglie* 2, 2002, 73-77; G. Inglese, "Per il testo della Commedia di Dante", *La Cultura* 40, 2002, 483-505; A. Longoni, *Strumenti critici* 17, 2002, 305-311; P.V. Mengaldo, *La Parola del Testo* 5, 2001, 280-289; C. Segre, "Postilla sull'edizione Sanguineti della Commedia di Dante", *Strumenti Critici* 17, 2002, 312-314; M. Veglia, "Sul testo della Commedia (da Casella a Sanguineti)", *Studi e Problemi di Critica Testuale* 66, 2003, 65-119.

The quotation from Giorgio Inglese is from *Come si legge un'edizione critica. Elementi di filologia italiana*, Roma, Carocci, 1999, 153 (the whole section on the *Commedia* is suppressed in the 2nd ed., published in 2006).

On Sanguineti's stemma: NP, especially 611-649; SHAW ("Introduction").

such that no task, even those we may regard as purely scientific, is ever fully completed [...]. We may feel satisfied at one moment with what we have accomplished, but soon we will find it in need of redoing, just as others will have to do it in their own ways, and then do it again. In the ninety-eighth chapter of *Moby-Dick*, Melville describes the process of scrubbing down the decks after the oil has been extracted from a whale; but no sooner is this activity finished than another whale is sighted, and the whole sequence, from killing the whale to cleaning up the ship afterward, must be performed again (TANSELLE, "Textual Criticism at the Millennium", 78-79).

After all, as Contini, among others, observed in a very polite refutation of an argument by his mentor, Bédier, this character of work in progress is common to the evolution of all sciences:

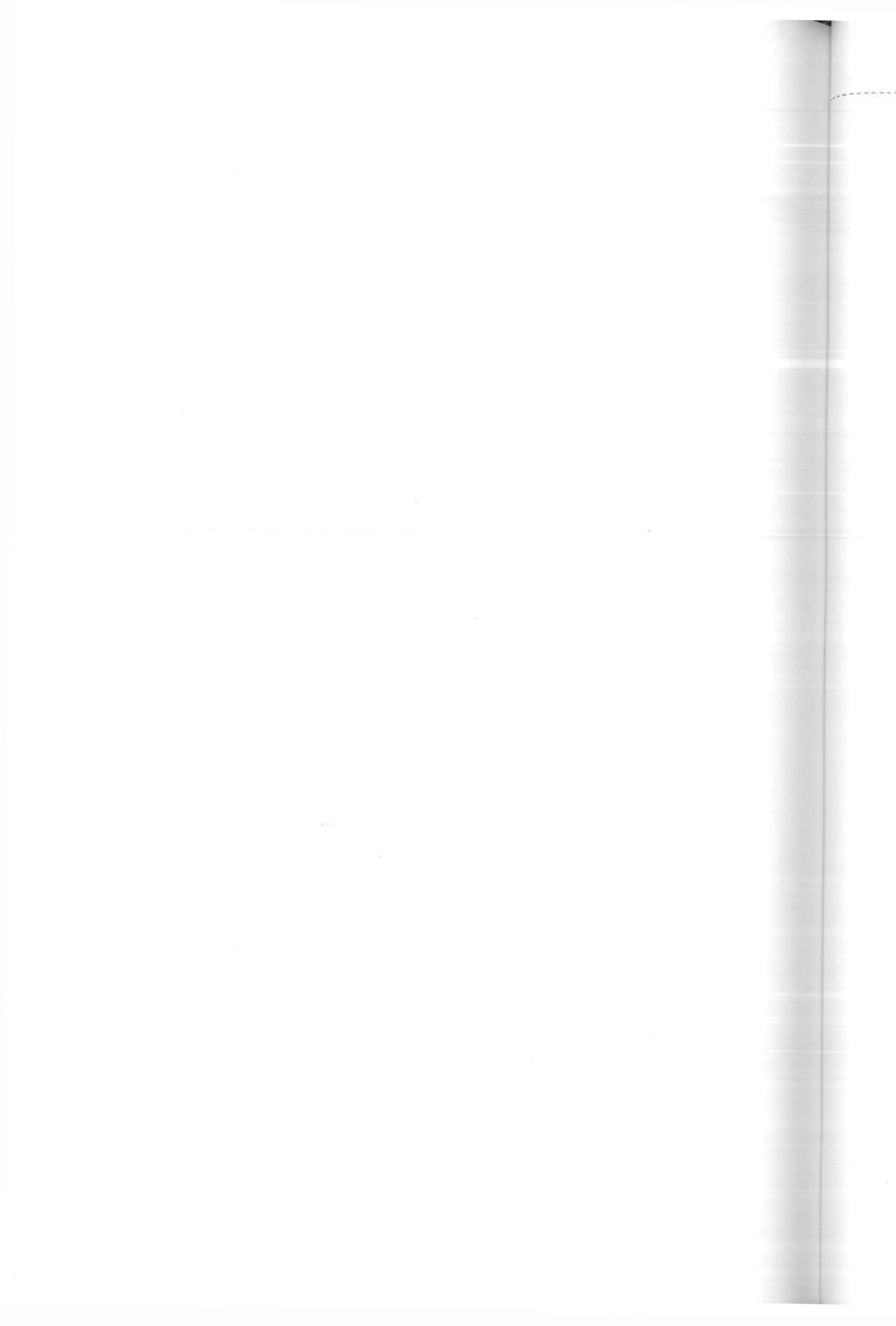
Bédier sees the absence of a definitive reading [*sc.*, the fact that in the critical text changes may be introduced by editors in the light of the discovery of new witnesses] as a flaw [...] but I do not see how continuous dynamic improvement could be anything but a positive quality. This progress towards truth—a fractional truth, so to speak, as opposed to the alleged organic truth of individual witnesses, truth as reduction of error—appears to be an approach worthy of science (CONTINI, 33 [= ID., *Frammenti*, I, 30]).

Certainly, what is really at stake in the training of philologists capable of rigorous investigation, like the best twentieth-century textual critics, is too important for us to allow it to become outmoded, like an old dress or style of furniture. I conclude by subscribing to what Alberto Varvaro stresses in quite clear terms:

In my years of teaching, I never deemed it important that my students should learn who Juan Ruiz, archpriest of Hita, or any other writer of the Romance Middle Ages, was, and what he had written. Neither did I find it so essential that they should remember in the future what the lenition of intervocalic consonants in some Romance languages is. To know these things may be worthwhile and commendable, but it is much more important to realize that *a text, any text, harbours an interpretative problem, and that this problem needs to be determined in its correct form. Awareness of these two problems is essential for the good functioning of human society, which is indeed founded on textual transmission. This, in my opinion, is what justifies the very existence of philology and its cultural and social relevance* (VARVARO, *Prima lezione*, 144; my emphasis).

Bibliographical notes. F. Bowers, *Textual and Literary Criticism*, Cambridge, Cambridge UP, 1966; Rupert T. Pickens “The Future of Old French Studies in America: The “Old” Philology and the Crisis of the ‘New,’” in *The Future of the Middle Ages*, 53-86. Here some references to old or new philologies obviously allude to the late twentieth-century enthusiasm, not devoid of naïveté, of the so-called New Philologists.

While I do not agree with the rest of the essay, whose linguistic foundation I find weak, I fully concur with the following observations of R. Howard Bloch: “I will argue not only that there is nothing new in the term ‘New Philology’ (viz. Michele Barbi’s *Nuova Filologia*, Florence, 1938), but that the old philology was in fact a new philology (viz. the Neo-Grammarians) with respect to that which had preceded. *Use of the labels ‘new’ and ‘old,’ applied to the dialectical development of a discipline is a gesture sufficiently charged ideologically as to have little meaning in the absolute terms—before and after, bad and good—that it affixes [...].* We must always bear in mind Dumézil’s witty truism: ‘aujourd’hui, j’ai raison; mais demain, j’aurai tort’” (R. Howard Bloch, “New Philology and Old French”, *Speculum* 65, 1990, 38 and footnote; my emphasis).



GENERAL INDEX

The General index comprises a register of personal names and a selection of terms, concepts and themes among those discussed in the book. Whenever appropriate, specific notions are collected under main entries (e. g.: "authorial variants" is under "variants"; "contamination of readings" under "contamination"; "indicative error" under "error"). The symbol → means *see*. The symbol ➔ means *see also*. Non-English terms are given in italics. Page numbers referring to definitions or substantive discussions of key concepts and themes are given in bold.

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